The Relation Between Formal Science and Natural Science

Pedro M. Rosario Barbosa

This document was used using exclusively free software, the word "free" meaning here "freedom" (http://www.gnu.org/philosophy/free-sw.html). It was made using LaTeX 3 , available under the LaTeX Project Public License, and LyX, which is available under the GNU General Public License 2.0. It was created in a GNU/Linux operative system (Gentoo Linux distro 2005.0), Linux version 2.6.11-vidalinux1.

CHAPTER 1

On the Nature of Formal Science

To be able to understand adequately the relation between the formal sciences and natural sciences, we need to explore the very nature of formal sciences themselves, which are indispensable to the practice of natural sciences. I wish to address one of the most important problems ever risen in the XXth century and still continues today, among them the division between analytic and synthetic propositions, and then the problem between Platonism vs. Antiplatonism in philosophy of mathematics.

1.1. Quine's Rejection of Analytic and Synthetic Distinction

1.1.1. Historical Background of "Two Dogmas". One of the most widely known philosophies which reject the traditional analytic/synthetic distinction is presented by the distinguished philosopher Willard Van Orman Quine in his famous essay "Two Dogmas of Empiricism" (Quine 1953, 20-46). This attack must be understood within a historical context.

Before Immanuel Kant, there was something very similar to the analytic and synthetic distinction. For example, Gottfried Wilhelm von Leibniz made the distinction between *vérités de raison* and *vérités de fait*; David Hume made also a similar distinction between *relations of ideas* and *matters of fact*. For them, analytic judgments are *a priori* while synthetic judgments are *a posteriori*.

For Kant, there are two criteria to establish the difference between analytic and synthetic judgments[1]:

(1) The first one is evaluating it from a Subject-Predicate point of view: If the concept of the predicate is included already in the concept of the subject, then the judgment is analytic; if not, then it is synthetic. For example, if I say "Bodies are extended", the concept of extension is already included in the concept of body, therefore it is an analytic judgment. If I say "Bodies are heavy", the concept of heaviness is not included in the concept of body, therefore it is a synthetic judgment (Kant A6-7/B10-11).

(2) The second one is evaluating the judgments using the logical principles of identity and no-contradiction. For instance, if I say "Bodies are extended", a body that is not extended is not possible and would not even be imaginable, therefore the judgment is analytic. If I say "Bodies are heavy", I can account the fact that a body can indeed be conceivable as weightless, therefore this judgment is synthetic (Kant A6-7/B10-11).

Also Kant distinguishes between *a priori* and *a posteriori* knowledge. A judgment is *a priori* if it expresses universality and/or necessity. It is *a posteriori* if it refers

[1]For more details see López 63-64.

to facts of experience. For him, all analytic judgments are *a priori*, but there are two kinds of synthetic judgments.

(1) Synthetic *a posteriori* judgments: These judgments are about particular experiences (Kant A7-8/B11-12).
(2) Synthetic *a priori* judgments: These judgments which refer not only to experience, but also express universality. There is a necessity in the sense that it refers to the way objects will behave under *all* circumstances (Kant A9-10/B13-B14).[2]

For Kant, synthetic *a priori* judgments are possible and they constitute the propositions of science (theories, laws, hypotheses, etc.) For example, a law such as "The gravitational force of two objects is proportional to the objects' masses and inversely proportional the square of the distance between them" would be synthetic *a priori* judgment. Although it refers to objects of experience, it states something that science takes to be universally true. Kant places the value of syntheticity and analiticity in our forms of intuition and the categories of understanding. If I say: "All bodies are hot when heated", that would be a synthetic judgment because it establishes a causal relation between body and heat (the category of causality), and it would be necessary or *a priori*, because it establishes an universality (Kant B17-18).

However, there are other kinds of synthetic *a priori* judgments, like: "7+5=12". Kant says that numbers are constructed thanks to pure intuition. Once these objects are constructed, we must relate them in such a way that it gives us that specific mathematical truth. There is nothing in the 7 and the 5 that implies 12. It is during the process of adding five to seven (maybe with the help of our fingers) that we slowly arrive to the number 12. Hence "7+5=12" is a synthetic *a priori* judgment, and the same goes to the judgments of geometry. But, these arithmetical judgments are not *universal*, in the sense that they are applied to *all* objects of possible experience, but *singular*, in the sense that they can be applied *only* to individual mathematical objects: there can be only one 7, one 5 and one 12 (Kant B14-B17).

For many, there was definitely something wrong with this Kantian picture: Let's accept for the sake of the argument that there are synthetic *a priori* judgments. It seems, though, that the judgments of arithmetic are more apodictic (logically necessary) than objects of our pure intuition (in Kant's sense). Gottlob Frege wondered if we can ever use fingers or such pure intuition to find out if 135664+37863=173527 (Frege 6). Other great philosophers like Bernard Bolzano, Lotze, Husserl, etc. would severely criticize this position on arithmetical judgments as being synthetic *a priori*. Evidently, for all of them, arithmetical judgments are not synthetic *a priori*, but analytic *a priori*.[3]

---

[2]Contrary to opinions of many philosophers and scholars, I hold that in this case Kant is not talking about logical universality or necessity, which would apply only to analytic judgments.

[3]Unfortunately, even in the late XX century, there have been efforts by many who hold a Kantian view of mathematics to revive it, needless to say that they have failed miserably. The last try I ever saw of providing a philosophical argument in favor of the syntheticity of the judgments of arithmetic was: Neri Castañeda. "7+5=12 as a Synthetic Proposition." *Philosophy and Phenomenological Research.* Vol. XXI. No. 2. December 1960: 141-158. The entire basis of the argument rests on the same problem that Kant had during his time. At least, in Kant's case, there was not yet a distinction between subjective representations, meanings and reference. However, Castañeda, who wrote the article in 1960, did live in a time where such a distinction

What about scientific laws? It seemed plausible to many that the scientific laws are indeed synthetic *a priori* in Kant's sense. However, there were some problems. For Kant, from an epistemological point of view, synthetic *a priori* judgments don't express logical necessity, and they must fulfill two conditions for them to be acceptable within science. First, they must refer to objects of possible experience, and must be the basis for synthetic *a posteriori* judgments. The truths of the latter are determined exclusively by experience (Kant A 425, see also Kant A 239-240; B 146; B 166; A 296/B 352-353; A 792/B 820). Second, they must express necessity, but not logical necessity (Kant A2). This all sounds beautiful, but after the XIX century, and philosophers' reevaluation of what the words "*a priori*" and "*a posteriori*", to think about synthetic judgments being *a priori* slowly was regarded as not being plausible. Bolzano, Frege and Husserl made the difference between subjective representation, meaning (sense) and reference, and many philosophers began paying attention to the meaning and reference of judgments, rather than the subjective representations and activities. This was necessary to determine if a judgment is *a priori* or *a posteriori*. If the judgment doesn't contain (meaning) a reference to experience, then the judgment is *a priori*. If the judgment contains (meaning) a reference to experience or objects of possible sensible experience, then it is *a posteriori*. Therefore, all *a priori* judgments must be analytic, and all laws and theories of science are synthetic *a posteriori*.

Also Kant's statements that there are physical laws are *a priori*, had a very big problem in terms of establishing its necessity. For example, Karl Popper, in his *Two Problems of Epistemology*[4], stated the following: "There are, no doubt, synthetic a priori judgments, but they are often a posteriori false" (Coffa 332). There was no room for synthetic *a priori* judgments anymore.

Obviously the next question was: what makes the difference between an analytic and a synthetic judgment? Logical positivists, who rejected the notion of synthetic *a priori* judgments, made a way to disntinguish both kinds of judgments. First, they distinguished between logical and mathematical laws which they regarded as conventional and indispensable for science[5]. A second way, proposed by Rudolf Carnap, was to establish the sameness of meaning among terms. For Quine, this is where all the fun begins!

1.1.2. The Argument of "Two Dogmas". Quine saw this division of analytic and synthetic as purely artificial, and "Two Dogmas" was directed mostly as a criticism to Carnap's way of distinguishing both kinds of judgments. Quine's empiricism aspires to reject all kinds of intensionalism. To focus on meanings as if they were objective entities would be to invite intensions into empirical science. He

---

was very well made by Frege, Husserl, the logical empiricists, among many others. Castañeda's statements confuse the ontological aspect of meanings and the subjective acts of representing them, making numbers and arithmetical relations as activities of constructing and relating them. It makes the truth of "7+5=12" dependent on the subject's ability to grasp them. What makes propositions analytic and synthetic is hardly psychological conditions, but the apodictic nature of the judgments.

[4]Originally published in German under the title: *Die beiden Grundprobleme der Erkenntnistheorie*, which after an editing process was called: *Logik der Forschung* (translated to English as *The Logic of Scientific Discovery*).

[5]Some used the traditional terminology of "analytic" and "synthetic", but other philosophers like Hans Reichenbach used the terms "axioms of coordination" and "axioms of connection" (Reichenbach 1965, 34-47; Coffa 192).

would say: "Meaning is what essence becomes when it is divorced from the object of reference and wedded to the word." (Quine 1953, 22). A physicalist and empiricist Quine would reject intensionalism and focus more on extensions, since all of science is extensional in nature. To refute this analytic and synthetic distinction, he criticizes the view of synonymy as the basis for analiticity of propositions. He says that such a criterium would inevitably lead, not exactly to a circularity, but something like a closed curve in space (Quine 1953, 30).

Quine distinguishes between two classes of of analytic judgments according to philosophers. There are those which are *logically true*, like: "No unmarried man is married" or in terms of symbolic logic: $\neg(\exists x)(\neg F(x) \leftrightarrow F(x))$, but there are also analytic propositions which are analytic *in virtue of their meanings*, such as "No bachelor is married". This is due to the synonymy of terms; for example, I can replace "bachelor" with "unmarried man". Carnap defended the position of state descriptions or an assigment of truth values to atomic statements of language, while the rest of the language is built up using them. This is unsatisfactory because of the fact that it depends on extralogical factors such as establishing synonym-pairs, such as "bachelor" and "unmarried man". That way, any analytic statement built on them would be a kind of a "second class" of analytic statements. This can only serve to reconstruct logical truth, but not analyticity as such (Quine 1953, 23-24).

Other philosophers state that there are judgments which are analytic in virtue of their *definitions*, for example "bachelor" is defined as "unmarried man". However, the way that the word "bachelor" is defined depends greatly on linguistic usage, a way in which people in their daily lives use those words. Another apparent criterium of analycity is the interchangeability of terms *salva veritate*, as Leibniz suggested. But, it is not clear that the words "bachelor" and "unmarried man" can be interchangeable *salva veritate*. Let's take this case:

Bachelor has less than ten letters.

In this case, we see that the word "bachelor" cannot be substituted by "umarried man". Also cases like "bachelor of arts" present us counterinstances for interchangeability of terms. It can be argued that in the former case we are meaning the word "bachelor", while in the latter case the word "bachelor" means a different thing from "unmarried man". We must take into account *cognitive* synonymy. To claim that "bachelor" and "unmarried man" are synonymous means that the proposition "All and only bachelors are unmarried men" is analytic. This is to say that "Necessarily all and only bachelors are unmarried men." Let's carry out the substitution and say: "Necessarily all and only bachelors are bachelors." Both propositions, even though they interchanged both terms "bachelor" and "unmarried men", the cognitive information they offer is very different. We could try saving the argument by appealing to extensions. For example, two terms are interchangeable *salva veritate* if they have the same extension. However, extensions that fall under concepts depend greatly on accidental matters of fact. For example the concepts of "creature with a heart" and "creature with kidneys" have the same extension (presumably), but they are not interchangeable *salva veritate*. It seems that the only way to assert the synonymy is by supposing that the terms "bachelor" and "umarried man" are analytic. And Presto!, we have our "closed curve in space". In order for us to distinguish between analytic and synthetic we must appeal to synonymy; at the same time, we should also understand synonymy with interchangeability *salva veritate*. However, such a condition to understand synonymy is not enough so we not only argue that the terms should be interchangeable, but necessarily so. And to explain this logical necessity we must appeal to analyticity once again (Quine 1953, 24-32).

It could well be that the failure for establishing the interchangeability *salva veritate* is due to the vagueness of language. We could use instead, as Carnap certainly tried, an artificial language to avoid such vagueness, and establish semantical rules to distinguish some propositions from others. Now, we have to explain why there are semantical rules that distinguish analytic and synthetic propositions is different from other semantical rules. The answer to this is that it is adopted because they can indeed pick up analytic propositions and distinguish them from synthetic ones. Here we find a circular reasoning: What distinguishes analytic propositions from synthetic ones is the supposition of semantical rules, which are themselves adopted because they can distinguish between analytic and synthetic propositions. In seeking the definition of analytic, it leads us to presuppose the notion we wish to define (Quine 1953, 32-39).

From this, Quine presents this analytic/synthetic distinction as a kind of article of faith held by logical empiricists, along with the dogma of verification. I will leave the rest of the discussion of "Two Dogmas" for another discussion. However, we must point out the fact, that, for him, propositions which are logically true ("No unmarried man is married" or $\neg(\exists x)(\neg F(x) \leftrightarrow F(x))$) along with propositions of science and experience are only posits. They are myths, no different epistemologically to the Greek gods of antiquity which were also posited to explain what people at that time perceived (Quine 1953, 17-19, 42-46)[6]. For the Quine of "Two Dogmas", mathematics can be indeed revised in light of recalcitrant experience (an issue we will deal next chapter). Therefore, the difference between the posits of formal laws and the posits of science is only of degree of abstraction, hence there can be no analytic/synthetic distinction between them.

1.1.3. Reply to Quine's Arguments. One of the most known replies to Quine with respect to this criticism to the analytic/synthetic distinction came from H. P. Grice and P. F. Strawson and their article "In Defense of Dogma". In there, Grice and Strawson state that there is a *non sequitur* in Quine's reasoning, that is that if some criteria are insufficient to establish the analytic/synthetic distinction, then that means that there is no analytic/synthetic distinction. In fact, there can be also many other criteria to establish this distinction.

First of all, they say, there is a factor of philosophical *use* which has to be accounted for. For instance, for many centuries we have made the differences between: *a priori* and *a posteriori*, necessary and contingent (Aristotle), truths of reason and truths of fact (Leibniz), truths of reason and matters of fact (Hume), analytic and synthetic (Kant, Frege, Husserl, Carnap, etc.), a tradition still being held by logical empiricists. Therefore, the fact that we haven't arrived to clear-cut criteria to establish the boundary between analytic and synthetic, doesn't mean that there are no such objective criteria (Grice and Strawson 464). Although I agree with their *non sequitur* reply to Quine, I don't find the argument of philosophical cases completely convincing, exactly because of what they say in that same text: "They apply the term 'analytic' to *more or less* the same cases, withold it from *more or less* the same cases, and hesitate over *more or less* the same cases" (Grice and

[6]Of course, we must clear up that by this Quine is not dismissing the possible ontological existence of such laws of nature, etc. Quine stated very wisely: "Posited objects can be real. As I also wrote elsewhere, to call a posit a posit is not to patronize it" (Quine 1953, viii).

Strawson 464, my italics). This mere fact actually reinforces Quine's view, precisely because the criteria to make such distinctions has changed very much throughout history, and that the apparent difference between analytic and synthetic judgments seem to be a difference of abstraction. You can't actually draw a line that distinguishes both in a gray area between both kinds of judgments. For instance, Kant considered synthetic some propositions that Leibniz and Frege considered analytic. Some other philosophers denied that there was such a difference, and that they are all abstracted from experience (John Stuart Mill).

Quine, unfortunately, is one of those philosophers like Hume: you know they are wrong, but it is difficult to prove they are wrong. I only limit myself to say the following. Let's assume for the sake of the argument that analytic propositions are as much posits as theories of science. However, Quine is one of those philosophers who refuse to believe that logical laws and mathematic propositions are abstracted from experience. Posits about the world also are not abstracted from experience either, they only serve to explain the data we receive from our senses[7]. But, nothing prevents me from making a difference among posits, just as I can make a difference, for instance, between imaginary objects which I can't represent mentally (the round square or Hegel's universal spirit) and objects which I can represent mentally (a table, a unicorn, a gnome, Pegasus, etc.), none of these objects actually exist in the physical world or at least I have no good reason to believe they exist, specially when most of them have no scientific use whatsoever. This is not merely artificial, this distinction can be made very clearly and legitimately.

The same we can say concerning posits that have one characteristic and posits that have another characteristic. For example, I can distinguish posits like this one:

$$((\forall x)(F(x) \rightarrow G(x)) \wedge (\forall x)(G(x) \rightarrow H(x))) \rightarrow ((\forall x)(F(x) \rightarrow H(x)))$$

or like this one

$$\sin\frac{x}{2} = \sqrt{\frac{\cosh(x-1)}{2}} = \frac{\sinh x}{\sqrt{2(\cosh(x+1))}}\ , \text{ if } \quad x \geq 0$$

which have nothing to do with material objects or material concepts (meaning sensible objects or general concepts which refer to sensible objects). They are only logical truths and mathematical relations, whose variables can represent in the first case propositions, or in the second case any real number greater or equal to zero. The way they are shown to be true according to logical rules and mathematical axioms does not depend at all on sensibility.

On the other, we find abstract laws but which refer to material objects (objects of the physical world) or at least they refer to the way they behave, like:

$$F_g = G\frac{m_1 m_2}{d^2}$$

which is Newtonian formula to find the gravitational force between two masses. They differ significantly from the previous examples in the fact that the variables F, G, m, d, refer to any material force or theoretical object applied to the phenomenical world. These are not formal relations like in the case of logic and mathematics. Why can't we establish, for instance, logical and mathematical relations (completely empty of material content) to be the criteria for establishing what is analytic, while those which express the relations among material concepts be synthetic? The

[7]He expresses this in different forms: "stimulation of sensory receptors", "firings of our sensory receptors" or "triggerings of extroceptors" (Nelson and Nelson, 14-15).

difference between analytic and synthetic in this case is very clear and not subject to ambiguity or vagueness.

Also there can be an intervention here of the notion of "necessity". Why not? Just because Quine finds the notion of "necessity" suspicious, why would "necessity" be *necessarily* an illegitimate concept? The notion of "necessity" (logical necessity) may not be defined adequately, but people at least intuitively can understand its difference with contingency. For example, "all bodies are extended" is necessary, while "all bodies are heavy" is contingent. I can say, "the circle must be round" is necessary, while "the circle must be green" is contingent. This intuition of necessity, called by the philosopher Edmund Husserl "eidetic intuition", is an intuition of understanding which intuits a *sine qua non*. Bodies can have no weight, but they can't exist without extension, or a circle can be of any color whatsoever, but it cannot exist without it being round. To reject the notion of necessity because it cannot be definable falls into the fallacy of believing that a notion to be valid must be defined.

If we translate this to the scientific realm, propositions like:

$$\overrightarrow{F} = m\overrightarrow{a}$$

would be analytic *a priori* because the product of mass and accelleration *is* force within the Newtonian paradigm, *it is a definition of force*. Without mass and/or accelleration there can't be any force as such. There can be no possible exception to this law of motion. Propositions like this, are legitimate scientific laws which cannot be falsifiable in any way because they express necessary relations among material scientific concepts, that the only way to "refute" them is really adopting another scientific paradigm. The Newtonian second law of motion is one of them. This formula can be distinguished in its own nature from the formula of the law of gravity, which expresses contingency (it can be logically possible to find counterinstances).

Even if we were to believe that the posits of logic and mathematics we have shown are as much posits as the Newtonian law of gravity, clearly they are essentially very different from each other. This difference is legitimate for two reasons:

(1) Because the difference between analiticity of logical and mathematical laws from the syntheticity of the law of gravity is due to their own nature. Analytic judgments deal only with formal relations or formal objects, and the necessity of material and formal concepts.
(2) Even from a pragmatic point of view, *it is very useful* to understand the nature of logic and mathematics, versus the nature of scientific theories and other kinds of propositions about the physical realm, or about the cultural realm, or about the psychological realm.

1.2. Husserlian Philosophy of Logic and Mathematics

1.2.1. Why do Many Analytic Philosophers Reject Edmund Husserl's Philosophy of Logic and Mathematics?[8] It would surprise to many that I take

[8]At the very beginning I did not wish to include this subsection, but I was very surprised to see how very well rooted are the myths against Husserl, and how deep is the rejection of Edmund Husserl in the Analytic tradition. Therefore, I wish to exorcise these misconceptions on Husserl in this section. For more thorough study on this subject, and a full demystification of wrong conceptions about Husserl see: Claire Ortiz Hill. *Word and Object in Husserl, Frege and Russell*; Claire Ortiz Hill and Guillermo E. Rosado Haddock. *Husserl or Frege?: Meaning, Objectivity and Mathematics*; Rudolf Bernet, Iso Kern and Eduard Marbach. "Mathematics, Logic, and

Husserl's philosophy of logic and mathematics as a starating point to clear up the relation between formal sciences and natural sciences. Practically most analytic philosophers completely ignore Husserl's philosophy of mathematics, even if it is the one that best describes mathematics today in the XXI century. Even in the case of mathematicians, they regard Gottlob Frege far superior to Edmund Husserl and much more worthy of recognition.

I want to explain briefly why this is so in the Analytic world. First, there is an unfounded myth which began with Dagfinn Føllesdal's Master's Thesis: *Husserl and Frege: A Contribution to Elucidating the Origins of Phenomenological Philosophy* and his article "Husserl's Notion of Noema", where he states that Husserl began as a psychologist and wrote his *Philosophy of Arithmetic* (1891) from that perspective, and later changed his mind due to Frege's review (1894) against that book. The myth further states that Husserl became a kind of Fregean semanticist and that his phenomenological notions (like the notions of *noema* and object (*Gegenstand*)) are nothing more than an extension of Fregean distinction between sense (*Sinn*) and reference (*Bedeutung*). And if that was not enough, many other authors accuse Husserl of falling into the claws of psychologism once again.

None of this is true. As it turns out, apparently Frege's review of Husserl's *Philosophy of Arithmetic* had nothing to do with him changing his mind. In fact, as some studies have shown, some of Frege's criticisms were valid, but others were not. He indeed exaggerated Husserl's position to the point of caricaturizing it. Many authors consider Frege's criticism against Husserl's notion of abstraction as accurate (e.g. Coffa 68-69), when in reality Husserl did not expouse the silly assertions which Frege accused him of saying. It can be possible that Frege's attack was not directed to Husserl, but to Georg Cantor, who was Husserl's close friend, colleague and mentor, and, like Husserl, was a disciple of the renowned mathematician Karl Weierstrass, whom Frege opposed (Hill and Rosado 96-97). Frege would charge Cantor exactly of the same errors that he charged against Husserl (Hill and Rosado 98).

The myth that Husserl adopted Fregean semantics after the famous Fregean review (1894) is also false. Husserl already had his own theory of sense (meaning) and reference (object), by 1890. Yes, *Philosophy of Arithmetic* was published in 1891, but that only means it was published that year. Husserl finished writing it in 1890. So, *Philosophy of Arithmetic* represents Husserl's thinking on mathematics up to 1890. Mohanty adds to this:

> the basic change in Husserl's mode of thinking which by itself could have led to the *Prolegomena* conception of pure logic had already taken place by 1891. [. . .] If pure logic is defined in the *Prolegomena* in terms of the concept of ideal objective meanings, then already the 1891 review of Schröder's work contains this concept. If the major burden of Frege's 1894 review of [*Philosophy of Arithmetic*] is the lack of distinction, in that work, between the subjective and the objective, between *Vorstellung* and *Begriff*, then Husserl already had come to distinguish

Phenomenology." *An Introduction to Husserlian Phenomenology*: 13-57; J. N. Mohanty. " Husserl and Frege: A New Look at their Relationship"; J. N. Mohanty. *Husserl and Frege*; and Iaakko Hintikka. "The Phenomenological Dimension." in *Cambridge Companion to Husserl*: 78-105.

between *Vorstellung* meaning and object in his 1891 review (Mohanty 1974, 22-23).

Ironically, it was Frege, in a letter he wrote to him (May 24, 1891), who recognized that Husserl made a distinction between sense and reference, and he (Frege) compares correctly both theories of sense and reference of concept words. Husserl himself recognized that by the time *Philosophy of Arithmetic* was published, he already disagreed with its content. He says that he began having doubts about psychologism from the very beginning (Hill 16-17). He attributes his change from psychologism to his reading of Gottfried Wilhelm von Leibniz, Bernard Bolzano, Rudolf Hermann Lotze, and David Hume. He makes no mention of Frege as being decisive for the change. In fact, in his *Logical Investigations* he mentions Frege only twice: the first one in a footnote to point out that he retracted three pages of his criticism of Frege's *The Foundation of Arithmetic*[9], and the other one was to question his use of the word "*Bedeutung*" to denote reference rather than meaning (sense) (see *Log. Invest.* Vol. II. Inv. I. Sec. 15).

Finally, contrary to what many people think, Husserl did not fall again into psychologism. Husserl maintained basically the core of the criticisms against psychologism throughout his life, and we can find them in works as late as *Formal and Transcendental Logic* (1929). That accusation usually is product of a misunderstanding between Husserlian semantics and his phenomenological doctrine. But even the ideal realm has a very important role in phenomenology, *noemas* are the irreal (ideal) correlates of real *noetic* activities of consciousness.

Another source of people not knowing about Husserl's doctrine of formal sciences has to do with the fact that many Phenomenologists have not paid attention at all to it, either because they don't know it, or because they are not acquainted enough with logic and mathematics to notice it. They mostly focus on Husserl's phenomenological doctrine in general as well as his doctrine of the crisis of the European sciences.

Finally, there are those Husserlian scholars who make some distorted expositions of Husserl's philosophy of mathematics. For example, Richard Tiezsen, who has been recognized by many to be an authority on Husserl's philosophy of mathematics, gives the impression that Husserl stated that through eidetic intuition we are able to abstract numbers, sets, etc. This is false. For Husserl, numbers and sets are constituted by consciousness through categorial intuition (which we'll discuss later). I do agree with Rosado Haddock that not to mention categorial intuition in Husserl's philosophy of mathematics would be like trying to explain Newtonian mechanics without mentioning the three laws of motion[10].

Up to now, we have presented most of the reasons why people have dismissed Husserl's philosophy of mathematics. So, to finish exorcising some philosophers' image of Husserl, I'll present his background in mathematics and philosophy of mathematics.

---

[9]Notice that it is only three pages, not eight as the typographical mistake of the old English edition seems to imply. This typographical mistake, unfortunately, has fueled the myth about Husserl being dramatically changed by Frege. In reality Husserl leaves most of his criticisms to Frege intact *after* his turn from psychologism to Platonism (see. *Log. Invest.* Vol. I. "Prolegomena", Sec. 46; see also Ortiz and Rosado 4-5).

[10]For a thorough criticism of Tiezen's exposition, see Guillermo E. Rosado Haddock. "Edmund Husserl: A Philosopher for all Seasons?": 388-395.

(1) While Husserl was a student in the University of Berlin, he studied with great mathematicians such as Leopold Knoecker and Karl Weierstrass (1878-1881). Later, Husserl became Weierstrass' assistant (1883-1884) (Hill and Rosado xi, Verlade 3).
(2) After being disciple of Franz Brentano (1884-1886), he went to the University of Halle, where he was under the supervision of Carl Stumpf, who was Brentano's disciple and to whom Husserl will later dedicate his *Logical Investigations*. It was through Carl Stumpf that Husserl increasingly became interested in Platonic ideas and led him away from Brentano's philosophy (Hill 17). Stumpf was the one who convinced Gottlob Frege to elaborate a philosophy to clear up the purpose of his recently created conceptual notation (*Begriffsschrift*), which led Frege to write his masterpiece *The Foundation of Arithmetic* (Hill and Rosado 3).
(3) It was during his years in Halle where he befriended Georg Cantor, the father of set theory, who would become his mentor (1886-1901) (Hill and Rosado xi).
(4) Husserl's first philosophical works were precisely about mathematics. In Halle his doctoral dissertation was called "On the Concept of Number", and later in 1891 he published his *Philosophy of Arithmetic: Psychological and Logical Investigations*. Even we can count the first volume of *Logical Investigations* called "Prolegomena of Pure Logic" where Husserl exposes his definitive doctrine on logic and mathematics.
(5) Later, when Husserl went to the University of Götingen, he was a colleague of David Hilbert, the great mathematician who was looking for a definitive proof of the completeness of mathematics. Husserl also believed in the completeness of arithmetic and formed part of Hilbert's Circle (1901-1916) (Hill and Rosado xi).
(6) Husserl was deeply interested in the paradoxes of set theory. Ernst Zermelo, who worked with set theory, found a paradox and in 1902 communicated this to Husserl. The reason for this is that Husserl had discussed a similar paradox in a review he wrote in 1891. Bertrand Russell also found this very same paradox and told Frege that his logical foundation of arithmetic as presented in *Basic Laws of Arithmetic* made possible this paradox. This is the reason why the world knows it as the "Russell paradox", even though Zermelo found it first. In here we'll call it the Zermelo-Russell paradox. Many of Husserl's works on set paradoxes (which are hundreds of pages) still remain unpublished (Hill 2-3).
(7) Even in later works such as *Formal and Transcendental Logic* (1929) and *Experience and Judgment* (1938), Husserl elaborated his philosophy of mathematics in its final form.
(8) Apparently Husserl contributed to the field of logic more than Analytic philosophers realize. For example, some suspect that the difference between the "laws of formation" and the "laws of transformation" proposed by Rudolf Carnap, really were proposed originally by Husserl, but with other names ("rules to prevent non-sense" and "rules to prevent countersense"). We must remember that Carnap, in his *Der Raum* includes much of Husserl's thinking in his philosophy of space (Friedman 1999, 46-48),

> and at one time during 1924 to 1925 he assisted advanced seminars given by Husserl during three semesters (Hill and Rosado 202-203).

Therefore, a philosopher of mathematics would have to think twice or thrice before dismissing Husserl's philosophy of logic and mathematics.

1.2.2. Husserl's Philosophy of Logic and Mathematics. The reason why Husserl changed from psychologism to Platonism was precisely because of the mathematical and logical way of thinking (Hill 17). He was very different from Frege in terms of interest. Frege's main interest was to demonstrate that arithmetic could be founded in logic. In Husserl's case, he was interested in logic, mathematics, and also in epistemological issues. Not only did Husserl provide a Platonist view of logic and mathematics, but also provided an epistemology of mathematics, something Frege never did.

Another basic difference between Husserl and Frege is that for Husserl logic and mathematics are two separate different fields, which deal with very different relations and objects. However, for him, they are both related: one is the ontological correlate of the other. He describes mathematics as logic's "fat sister" (*Log. Invest.* Vol. I. "Prolegomena". Sec. 46). Mathematics is never reducible to logic (as Frege would believe), nor it can be reduced to set theory. Let us make an exposition on Husserl's philosophy of logic and mathematics as well as an exposition of his epistemology of mathematics.

1.2.2.1. *Husserl's Conception on "Mathesis Universalis".* One of the authors who greatly influenced Husserl and his turn to psychologism was G. W. von Leibniz, and his notion of *mathesis universalis*. His definitive view on logic and mathematics can be found first in the eleventh chapter of the first volume of *Logical Investigations*, the "Prolegomena", and he continued elaborating it until it culminated in *Formal and Transcendental Logic*.

Husserl notices that there are two levels of logic which we need to distinguish. One is the *theoretical level* of logic which tells us what *is*, the other one is the *normative level* of logic which tells us what we should think. Throughout history, says Husserl, philosophers have confused both levels of logic. The normative side of logic is not founded on our psychological processes, but on the theoretical side of logic: the truths of logical propositions themselves. Psychologists have tried to justify the principle of no-contradiction through some process of abstraction (or cultural dispositions, or products of evolution, etc.). However, this only explains how we can psychologically grasp such a notion, but doesn't itself *justify* what we think, it doesn't explain its objective truth independently from all subjects.

His whole criticism to psychologism leads to the necessary difference between the *real* [*reell*] and the *irreal* [*ireell*] or *ideal* [*ideel*]. The real realm has to do with the physical world and the psychological realm, i.e. all those realms which are subject to temporal succession. There is also the realm of the irreal or the ideal unities of meanings, which are "multitemporal" (remain identically the same even if they are recalled many instances)[11]. He is essentially proposing a Platonist view of logic and mathematics.

Once he establishes the importance of the ideal unity of the formal sciences such as logic and mathematics, these turn out to be the *a priori* conditions for every science. For him, pure logic does not deal at all with the physical or psychological realm (the *real* realm), but itself is universally valid and ideal. It is made up of the *formal theory of judgments* (what we call "logic"), and also the formal theory of object or *formal ontology* (what we call "mathematics"). These are constituted by

---

[11] We must point out, however, that Husserl began in the "Prolegomena" with a very naïve Platonist view of meanings, and characterized them as ideal or irreal *species*. Throughout his life he went through a process of correcting much of his thoughts, including the conception of meanings as species. He also believed initially that numbers were species themselves, an idea he seems to have rejected in *Experience and Judgment* (Part III. Chap. 3, Sec. 96). However, he did remain a Platonist as late as *Formal and Trascendental Logic.* He says: "Pure logic has as its thematic sphere ideal formations. But they would have had to be clearly seen, and apprehended, as such ideal objectualities, before transcendental questions about them and about pure logic could have been asked. The eighteenth century and the age that followed were so strongly actuated by empiricism (or better, by anti-Platonism) that nothing was remoter from them than recognition of ideal formations as being objectualities – in the manner and in the good and never-relinquishable sense whose legitimacy we have established in detail" (*Form. Trans. Logic* Sec. 100: p. 228-229). Also, the expression which describes ideality of meanings, which Husserl chose for the ideal realm is "omnitemporal", and he talks about it in many of his works. In *Cartesian Meditations* he says the following:

> How does one of my own subjective processes acquire for me the sense and status of an existent process, something existing with its identical temporal form and identical temporal content? The original is gone; but, in repeated representations, I go back to it and do so with evidence: "I can always do so again." But these repeated representations are evidently themselves a temporal sequence; and each is separate from the others. In spite of that, however, an identifying synthesis connects them in the evident consciousness of "the Same"– which implies the same, never repeated temporal form, filled with the same content. Here, as everywhere else, "the Same" signifies therefore and *identical intentional object of separate conscious processes*, hence an object immanent in them only as something *non-really* inherent. Another case, very important in itself, is that of the constitution of objects that are ideal in the pregnant sense – for example: all logically ideal objects. In a living, many-membered thinking action I produce a structure: a theorem or a numerical structure. Subsequently I repeat the producing while recollecting my earlier producing. At once, and by essential necessity, an identifying synthesis takes place; furthermore a new identifying synthesis occurs with each additional repetition [. . .] It is identically the same proposition, identically the same numerical structure, *but repeatedly produced* or, this being equivalent, repreatedly made evident. [. . .] With that, moreover, the supremely significant *trascendental problem of ideal objectualities* ("ideal" in the specific case) is solved. Their supertemporality turns out to be *omnitemporality*, as correlate of free produceability and reproduceability at all times (*Cartes. Med.* Fifth Medit: 155-156).

This formulation of omnitemporality of meanings is cleared up when Husserl makes a difference between "free" idealities (such as logical and mathematical truths along with essences), and linked idealities (meanings which are subject to the temporality of physical events, psychological events or social (cultural) events (for example the works of Goethe or the discovery of a mathematical theorem)) (*Exper. and Judgm.* Part II, Ch. 2, Sec. 65). For more on all of this subject of this change in Husserl's thinking see Castilla Lázaro's "La idealidad de los significados en Husserl". Castilla argues that the term "omnitemporal" is too strong, "multitemporal" is more adequate (Castilla 158).

categorial forms which are the formal components of all propositions which relate objects or simpler propositions. Husserl identifies among these categorial forms the *categories of pure logic*.

Formal theory of judgment has as objective to find the essential structures of all propositions, the conditions of truth of propositions and how these relate with each other. These are made up of *meaning categories*, which are a subclass of categories of pure logic, and they are all the elementary forms of combinations of propositions or judgments. They form a deductive unity of propositions. Husserl mentions some of these elementary forms: copulative, disjunctive, and hypothetic combination of propositions into new propositions. It also includes the different subject-predicate forms, to all forms of copulative and disjunctive union, forms of plural, etc. In this manner we could derive an infinite number of new forms and propositions (*Log. Invest.* Vol. I. "Prolegomena" Sec. 67). In other words, we could qualify as meaning categories the following logical relations: "no" ($\neg$), "and" ($\wedge$), "or" ($\vee$), "implies" ($\rightarrow$), "iff" ($\leftrightarrow$), among many other logical connectives. Their role is to relate propositions into new propositions, and those new propositions are related to form new ones. In principle we can form new propositions indefinitely.

But besides meaning categories, Husserl also recognizes the existence of what he called *formal-ontological categories*[12], which are the basis of formal ontology (mathematics). If meaning categories relate propositions, formal-ontological categories relate objects. These categories include: unity, plurality, order, magnitude, cardinal numbers, ordinal numbers, sets (groups), relation, parts and wholes, among many others (*Log. Invest.* Vol. II. Inv. III. Sec. 11; *Ideas* Chap. 1. Sec. 10). Notice that for Husserl all of these are very different formal-ontological categories, and none of them are reducible to the other, *they all have equal importance*. All mathematics (with the exception of usual geometry) is constituted by formal-ontological categories.

For Husserl, both of these different fields come together to form what he calls a "pure theory of manifolds", or *mathesis universalis*, which considers *a priori* all possible forms of theories and laws which govern their relations. Husserl saw Rieman's and Helmholtz's theories about Euclidean and non-Euclidean n-dimensional theories, Grassman's theories of extension, W. Rowan-Hamilton's theories, Lie's theory of group transformation, Cantor's investigations about numbers and multiplicities, among many others, as partial realizations of this theory of manifolds (*Log. Invest.* Vol. I. "Prolegomena", Sec. 70)[13].

---

[12] At the very beginning, Husserl called them "formal-objective categories" due to a problem with the term "ontological", much later he proposed "formal-ontological categories" as being most appropriate (*Ideas*. Chap. 1. Sec. 11).

[13] Notice how advanced is Husserl's view of mathematics compared to Frege's. For example, Frege would not accept as true non-Euclidean Geometry. To admit formal-ontological categories as different from those of logic, wouldn't have satisfied Frege at all. Also notice the fact that several philosophers have tried (and are still trying) to see if indeed if arithmetic can be reduced to logic (which is essentially Frege's and Russell's programs). However, their failures apparently show that Frege was wrong, and apparently Husserl was right. Also very little has been said about "part and whole" being formal-ontological categories, and there is also a deep neglect of Husserl's third investigation of *Logical Investigations* which deals about this subject. According to Kit Fine, this part-whole philosophy seems to foreshwadow the structure of a relative closure algebra, and thus, that of topological space (Fine 475, 477; see also Rosado 384-385). So, definitely, we cannot say that Husserl held Fregean views on logic and mathematics. He did agree with Frege

1.2.2.2. *Husserl's Epistemology of Logic and Mathematics.* We have seen that categories of pure logic constitute this *mathesis universalis* and why it is objective and independent of any subject, or independent of the physical or psychological world. Their truth doesn't depend at all on anyone's capacity to grasp them. They are independent of us in every way. So, we are left with one huge problem. How do we know them? Husserl also addresses this problem. Remember, he was not only interested in logic and mathematics, but also he was concerned with epistemological problems, one of them being a proper epistemology of the abstract ideal realm.

We notice that in all sentences we see words that have sensible correlates, and others which have no sensible correlates. For example, I can have a sentence talking about "table", "Mary", "John", "computer", etc. which are words which have meaning and do have sensible correlates. The way we can know these objects is through what Husserl called *sensible intuition*, which includes *sensible perception* and *sensible imagination*. *Sensible perception* is the faculty through which we can obtain all our sensible data "in person" (what we see, hear, smell, taste and touch). *Sensible imagination* is the way we can represent in our mind or with our imagination a sensible object, such as an imaginary table, or Pegasus, a fairy, etc. So, words which have sensible correlates can be known through sensible intuition. Apparently no problem there.

However, there are words which Husserl calls "formal words", which have apparently no sensible correlates: "no", "the", "a", "is", "under", "greater", "over", "or", "and", etc. I can say, "The glass is over the book" or "The book is under the glass". We have the sensible correlates "book" and "glass", but we have no sensible correlates of "is under the" or "is over the". These are different ways with which we relate objectively sensible objects. Just as I can have five pencils on the table, and I can relate them as a "set" of pencils, or as "3+2" pencils, or "4+1" pencils or "5" pencils, etc. These are ways we relate objectively the pencils on top of the table. We can only perceive sensibly the pencils, but not the "set", the "1", the "2", the "3", the "4", the "5", and the "+".

All of these non-sensible correlates in propositions is what Husserl calls *categorial forms*, and the way they are known is through a faculty of understanding called *categorial intuition*: which also includes *categorial perception* and *categorial imagination*. Through categorial intuition we are able to relate any objects or propositions whatsoever (*Log. Invest.* Vol. II. Inv. VI. Sec. 40-52).

On this basis, Husserl makes a very important distinction between "state of affairs" (*Sachverhalt*) and "situation of affairs" (*Sachlage*). For Husserl, on the basis of sensible intuition, our consciousness constitutes passively a situation of affairs, on which we can actively constitute many states of affairs. For example, let's consider the following two propositions or judgments:

(1) "Mary is greater than John."

(2) "John is smaller than Mary."

Both of these sentences express different propositions, which refer to different states of affairs. Both sentences contain words that refer to two sensible objects: John and Mary; but the categorial relation between (1) and (2) is different, hence the different states of affairs. However, both states of affairs are founded on the *same* situation of affairs: being Mary greater than John, or being John smaller than

in some things (for example, that logical and mathematical truths are analytic, and that usual geometry is synthetic *a priori*) but the reasons for these agreements are completely different.

Mary. Husserl gave the example that if a and b are objects given in a situation of affairs, then "$a > b$" and "$b < a$" refer to two states of affairs, but with the same situation of affairs as a reference base[14] (*Exper. and Judg.* Part II. Ch. 2. Sec. 58-60).

Husserl calls *objectual act* to this act of constitution by consciousness either of a situation of affairs or categorial acts of states of affairs. He also pointed out that despite that states of affairs can be constituted on a situation of affairs, in principle, a state of affairs can also serve as basis for other objectual categorial acts. The most clear example of this is sets:

> In the domain of receptivity there is already an act of plural contemplation in the act of collectively taking things together; it is not the mere apprehension of one object after the other but a retaining-in-grasp of the one in the apprehension of the next, and so forth [. . .]. But this unity of taking-together, of collection, does not yet have *one* object: the pair, the collection, more generally, the set of the two objects. In a limited consciousness, we are turned toward one object in particular, then toward another in particular, and nothing beyond this. We can then, while we hold on to the apprehension, again carry out a new act of taking-together [of, let us say,] the inkwell and a noise that we have just heard, or we retain the first two objects in apprehension and look at a third object, as one separate from the others. The connection of the first two is not loosened thereby. It is another thing to the combination or to take a new object into consideration in addition to the two objects already in special combination. and then we have a unity of apprehension in the form of {{A,B},C}: likewise {{A,B},{C,D}}, etc. It is necessary to say again here that each apprehension of a complex form has as objects A B C . . . and not, for example {A,B} as *one* object, and so on (*Exp. and Judg.* Part II. Ch. 2. Sec. 61).

So, a set of objects can serve as a basis for sets, and these new sets can serve as basis for more sets, and we can establish an kind of hierarchy of objectualities. If we want to know the ultimate sensible substracts of those sets, we can trace down until reaching the sensible components of those objectual acts[15].

---

[14] Husserl calls this difference phenomenological, but it also semantic. Notice how different is Husserlian semantics from Fregean semantics. For Frege, the sense of a sentence is a *thought* (we call it "proposition"), and its reference is a truth value. In Husserl's case, the sense of a sentence is a proposition, its reference is a state of affairs, and its reference basis is a situation of affairs. If the proposition is fulfilled by the state of affairs, then it is true; if not, then it is false. Notice also that for the Frege of "On Sense and Reference", propositions such as "$a > b$" and "$b < a$" express different thoughts, while in "The Thought" Frege says that they express the same thought. It seems that in "On Sense and Reference" the Fregean notion of thought is close to Husserl's propositions, while in "The Thought" he is closer to Husserl's "situation of affairs" (see Rosado's article on this subject called "On Frege's Two Notions of Sense" in Hill and Rosado 53-66).

[15] Notice that this is a considerable advantage over Frege. Frege's logicist proposal in his *Basic Laws of Arithmetic* falls in the Zermelo-Russell paradox. Rosado Haddock has pointed out, however, that Husserl's proposal avoids two very important set paradoxes, one of them being the Zermelo-Russell paradox. The Zermelo-Russell paradox states that we can constitute a set of all sets that don't belong to themselves. The question is, is the set an element of itself or not? If it is,

However, mathematical propositions don't have to do at all with sensible objects. They refer to sets, numbers and many other mathematical entities, but not the sensible objects these categories relate. Husserl also noticed a faculty of understanding he called *categorial abstraction*. This consists on getting rid of the sensible components of propositions and just attend to the categorial forms (*Log. Invest.* Vol. II. Inv. VI. Sec. 60). Then, through eidetic intuition we are able to discover the *necessary* relations between them, which is what logic and mathematics is all about (*Ideas* Ch 1. Sec 10-11, 13-14). As Rosado sums up correctly: "Without fear of paradox, it can be claimed that, although categorial intuition builds on sensible intuition, there is no trace of sensible foundation in mathematical intuition" (Hill and Rosado 281).

In the case of logic, our categorial intuition, as faculty of understanding, reveals us essential structures of propositions so that they can express a meaning. This is what Husserl called the first stratum of logic, a *morphology of meanings*. It is purely a syntactic level, a pure grammar which is universally valid. With the help of meaning categories we can make possible an ideal "construction" of infinity of possible forms of judgments. It's laws also serve to relate subject and predicate forms. For example, if I say "S is p", I can also use it to form another proposition "S(p) is q", or "S(p,q) is r", etc.

The *a priori* laws that make this possible are what he calls "laws for avoiding non-sense". In this level, only the meaning or the sense of propositions is important, not their truth value (*Log. Invest.* Vol. II. Inv. IV. Sec. 10-12; *Form. and Transc. Logic.* Sec. 12-13).

Logic's second level is "logic" properly speaking, because it has to do with all possible forms of true judgments. It is basically syllogistic classic logic, that studies the apodictic relations among propositions. This has nothing to do with the truth of the propositions themselves, but only deals with the consistency between them. He called this level the *logic of consequence* or *logic of no-contradiction*. He includes in this level what he called "logic of truth" which consists of formal laws of possible truth and its modalities. The laws of no-contradiction are essential to the laws of possible truth.

If the previous level of formal logic is syntactic, this second level is semantic. In the previous level, we could accept this proposition as meaningful:

$$(\alpha \rightarrow \beta) \wedge \neg(\neg\alpha \vee \beta)$$

But in the second level of logic it would be regarded as a counter-sense or contradiction. The laws that rule this level of logic is what he called *laws to prevent*

---

then it is not an element of itself, because it is the set of all sets that don't belong to themselves. If it is not, then it is an element of itself for that very same reason. Husserl's hierarchical view of sets blocks the Zermelo-Russell paradox, because it is impossible to constitute a set that belongs to itself.

The second paradox it overcomes is the Cantor paradox. Can we establish a set of *all* sets? Then the problem is that the set does not include itself as an element, hence it is not a set of all sets. This paradox is blocked by the fact that for Husserl, in principle, all sets can serve as the basis for more sets, and this hierarchy of objectualities makes it is impossible to constitute a set of *all* sets (Hill and Rosado 235-236). Rosado further states: "In the same vein one can show that the supposed entities that originate the rest of the paradoxes of naive set theory cannot be constituted in any categorial intuition" (Hill and Rosado 236).

counter-sense (*Log. Inv.* Vol. II. Inv. IV. Sec. 12; *Form. and Transc. Logic.* Sec. 14-22) [16].

Husserl recognizes a third level, a meta-mathematical level, which he calls *theory of manifolds* which is the supreme form of all mathematical disciplines. In this level, logic and mathematics come together to form a theory of all possible forms of theories. In it logicians and mathematicians are allowed to posit any entities whatsoever and axioms which govern mathematical as long as no-contradiction or consistency of the system is guaranteed (*Form. and Transc. Logic* Sec. 28-36). This way, we hold as true the completeness of mathematics and its consistency, which Husserl held as self-evident.[17] Rosado points out that General Topology, Universal Algebra, Category Theory and other mathematical disciplines can be seen as "important partial realizations of Husserl's view on mathematics as ultimately the theory of all forms of possible multiplicities or, to use a more frequent term nowadays, forms of possible structures" (Hill and Rosado 205).

1.3. Is Platonist Formal Intuition A Twilight-Zone Faculty?

On the essay "Mathematical Truth", Paul Benacerraf makes a challenge to philosophers of mathematics. The challenge consists in formulating a Philosophy of Mathematics which comply with the following requirements:

(1) First, to reach the objectivity and high level of consistency of mathematics, and an adequate account of mathematical truth. This has been accomplished by Platonism (Benacerraf 408).
(2) Second, to provide an adequate epistemology of mathematics, which Platonism is apparently not able to provide (Benacerraf 409).

The second requirement is precisely the reason why most people reject Platonism as an answer to philosophers. But before replying to this rejection, I wish to first portray a more accurate description of what Mathematical Platonism is all about.

1.3.0.3. *What Is Mathematical Platonism and What It Is Not.* There are lots of misunderstandings concerning about what Platonism is about, and what it proposes. For example, Michael Dummett says the following about Platonism:

> If mathematics is not about some particular realm of empirical reality, what, then, *is* it about? Some have wished to maintain that it is indeed a science like any other, or, rather, differing from others only in that its subject-matter is a super-empirical realm of abstract entities, to which we have access by means of an intellectual faculty of intuition analogous to those sensory faculties by means of which we are aware of the physical realm. Whereas the empiricist view tied mathematics too closely to certain of its applications, this view generally labeled "platonists," separates it too widely from them: it leaves it unintelligible how the denizens

[16] Many suspect that Carnap was acquainted with Husserl's doctrine of the three levels of logic, and that these ideas were used in his famous *Logical Syntax of Language*. Carnap makes a distinction, previously made by Husserl, between the syntactic level and the deductive level of logic. He called the former "syntax", and its rules were called "formation rules" (what Husserl called "laws to prevent non-sense"); while the latter has "transformation rules" (what Husserl called "laws to prevent counter-sense") (Carnap 1937, 1-2; Hill and Rosado 203).

[17] After Gödel's proofs of the incompleteness of arithmetic, this aspiration to a pure consistency of mathematics remains a kind of Kantian ideal (Hill and Rosado 205).

> of this atemporal, supra-sensible realm could have any connection with, or bearing upon, conditions in the temporal, sensible realm that we inhabit.
>
> Like the empiricist view, the platonist one fails to do justice to the role of proof in mathematics. For, presumably, the supra-sensible realm is as much God's creation as is the sensible one; if so, conditions in it must be as contingent as in the latter (Dummett 20).

Unfortunately this view of Platonism is inaccurate. If we follow Husserl, the reason why we are able to know certain mathematical objects is because they have sensible intuition as the basis for mathematical intuition. The way we relate these objects makes us being acquainted with formal mathematical objects. This explains why we are able to use these mathematical objects and operations in the physical world. But it is perfectly possible that we don't know certain mathematical objects which we are not able to constitute, and which we'll never know because of the limits of our faculty of understanding. As Husserl once argued, maybe it will take the intelligence of angels (or God's intelligence) to know them all.

Another point I wish to bring to this is the fact that these mathematical objects as well as logical relations are not God's creations. Quite the contrary. Logic deals with the conditions of possibility of truth, while mathematics deals with the conditions of possible formal relations among objects, even among mathematical objects themselves. It is in *this* sense that they exist. If there is a God with supreme intelligence, and his nature has to do with truth and goodness, then his supreme intelligence should have a way to know (not to create) these truths. In case He doesn't, and wants us thinking lies, God could make us *think* that the principle of no-contradiction is false, but that doesn't mean He can make that principle *be* false. "2+2" *is* "4", it doesn't matter if God wants us to think otherwise. There is no divine power which can make the principle of no-contradiction false, nor "2+2" to be "3"[18].

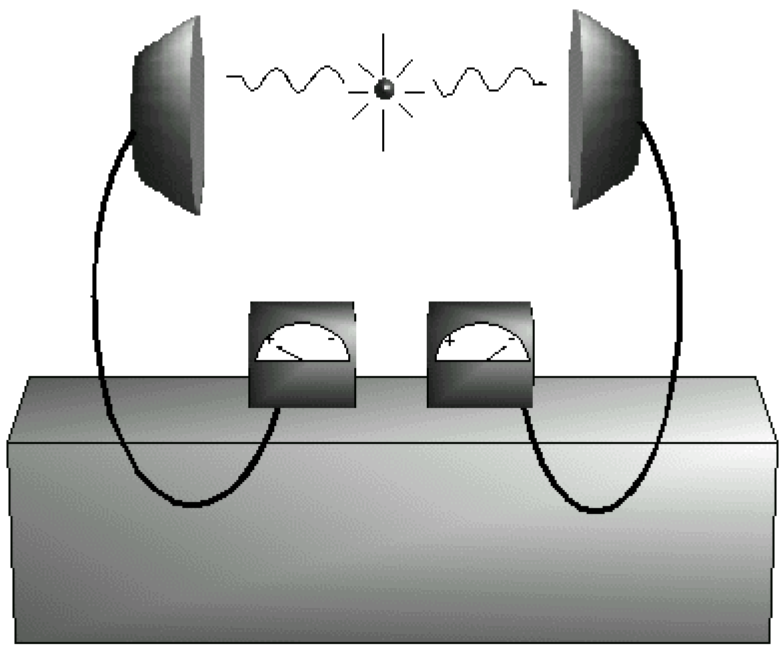
1.3.0.4. *Causal Knowledge for Legitimate Epistemology?* So, Platonism as such is not positing objects "floating in the middle of the air" or "creatures of God" or anything of the sort. We are talking about an abstract reality which establishes the condition of possibility of all being, which is *a priori* and universal in the all sense of the word. Therefore, we are talking about an ideal realm which is totally independent of human psychology and the physical world.

---

[18]Before fundamentalists and not-well-read people begin accusing me of being a "heretic" I have to remind that this was a very well treated in the Middle Ages, specially by St. Augustine and St. Thomas Aquinas. The reason for the discussion was the famous passage that says: "... for nothing is impossible to God" (Luke 1, 37). What does this mean? St. Augustine as well as St. Thomas Aquinas stated that this passage has to be understood within a more restrictive view of "omnipotence". For them, God cannot make something that happened not happen in the past; God cannot create anything that is essentially self-contradictory (for example, "2+2=6" or a round square). God cannot sin, because sinning is impotence, and would imply the non-omnipotence of God. God also cannot suffer any evil, etc. To sum it up St. Augustine said: *Non potest mori, non potest peccare, non potest mentiri, non potest fally: tanta non potest, quae, si posset, non esset omnipotens* (St. Augustine *Sermo* 213,1; 214,4; St. Thomas Aquinas *Summa Theologicae* I. Ch. 25. a. 3). What is meant with the word "omnipotent" in theology is that for God everything that doesn't imply a logical contradiction is perfectly possible. The phrase "... for God nothing is impossible" should be understood only in this sense. This shows even the extent of how God can also be subjected to formal truths.

So, the question of how do we know entities which are not psychological nor physical becomes relevant. Mathematical Platonists in general have had a very huge problem explaining mathematical intuition to become acquainted with those abstract independent objects. Gottlob Frege talked about "grasping" them. James R. Brown has talked about the "mind's eye" (Brown 13), but these methaphors don't help much clearing up *how* do we know mathematical objects. At best all the Platonist can do is to provide a non-causal epistemological explanation, which is rejected by the vast majority of empiricists. Many philosophers today require that knowledge *has* to be causal.

The requirement of causality for an adequate epistemology of mathematics doesn't seem convincing to us, specially because of the fact that it is not self-evident. Even sometimes in the case of science it is not theoretically possible to establish a causal relations among certain objects, and yet we can establish legitimate scientific knowledge. The following example shows one case:



This is an what is called an EPR machine. Let's suppose we have a positronium[19] which annihilates and two photons are released in opposite directions. According to experiments carried out by scientists, the spin of one photon will be in one direction, and the other in the opposite direction. Apparently there are no exceptions to this rule. We could conjecture why this happens, but it seems we cannot establish a causal connection which might explain why the spins are always different. We could say that perhaps it is because one photon seems to affect the other. They travel at the speed of light, therefore the effect of one on the other should be faster than the speed of light, going against special relativity, which states that nothing can travel faster than the speed of light. We could perhaps think that what affects their spin is at the very source of the photons at the moment of annihilation, but quantum physics states that if such a thing would be true, the outcome would

[19]A positronium is an atom composed of an electron and a positron.

be very different. Therefore, *we can know non-causally* the spin of photon B, just by knowing the spin of photon A. The causal requirement for legitimate knowledge is refuted (Brown 16-17).

1.3.0.5. *Non-Twilight Zone Explanation of Formal Knowledge.* Platonist epistemology has been accused of being mysterious and mystical. As Katz has pointed out, the fact that something is mysterious doesn't make it illegitimate. Philosophy is replete with mysteries and it thrives in solving those mysteries rationally (Katz 1998, 33). Also it has been accused of mysticism, since it requires a kind of an extra-eye to "see" those mysterious entities floating in the air waiting for someone to know them. Mysticism proposes that we can obtain knowledge beyond our cognitive faculties (Katz 1998, 33). But as I'll show here, categorial intuition as well as eidetic intuition are perfectly natural faculties of understanding (of our mind).

Husserl proposed a non-causal epistemology of mathematics, using categorial intuition and categorial abstraction as ways of knowing these abstract objects and truths. Eidetic intuition only plays a role of finding apodictic truths about these formal categories (logical and mathematical entities). Jerrold Katz, who wanted to provide an adequate non-causal epistemology of mathematics did not provide anything similar to categorial intuition, but he did recognize something similar to eidetic intuition (which he called mathematical intuition).

First, we will demonstrate that categorial intuition is not a mystical intuition, but actually a very natural faculty of understanding. In fact, humans are not the only living beings to have categorial intuition. George Ifrah explains how non-human living things are able to use what we call categorial intuition:

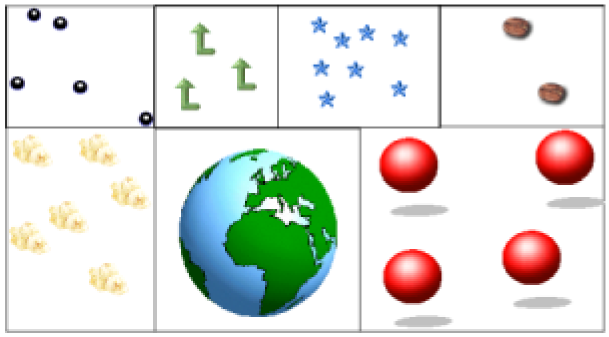
> Some animal species possess some kind of notion of number. At a rudimenatry level, they can distinguish concrete quantities (an ability that must be differentiated from the ability to count numbers in abstract). For want of a better term we will call animals' basic number-recognition the *sense of number*. [. . .]
>
> Domesticated animals (for instance, dogs, cats, monkeys, elephants) notice straight away if one item is missing from a small set of familiar objects. In some species, mothers show by their behaviour that they know if they are missing one or more than one of their litter. A sense of number is marginally present in such reactions. The animal possesses a natural disposition to recognise that a small set seen for a second time has undergone a numerical change.
>
> Some birds have shown that they can be trained to recognise more precise quantities. Goldfinches, when trained to choose between two different piles of seed, usually manage to distinguish successfully between three and one, three and two, four and two, four and three, and six and three.
>
> Even more striking is the untutored ability of nightingales, magpies and crows to distinguish between concrete sets ranging from one to three or four. [. . .]
>
> What we see in domesticated animals is a rudimentary perception of equivalence and non-equivalence between sets, but only in respect of numerically small sets. In goldfinches, there is something more than just a perception of equivalence – there

> seems to be a sense of "more than" and "less than". Once trained, these birds seem to have a perception of intensity, halfway between a perception of quantity (which requires an ability to numerate beyond a certain point) and a perception of quality. However, it only works for goldfinches when the "moreness" or "lessness" is quite large; the bird will almost always confuse five and four, seven and five, eight and six, ten and six. In other words, goldfinches can recognize differences of intensity if they are large enough, but not otherwise.
>
> Crows have rather greater abilities: they can recognise equivalence and non-equivalence, they have considerable powers of memory, and they can perceive the relative magnitudes of two sets of the same kind separated in time and space. Obviously, crows do not count in the sense that we do, since in the absence of any generalising or abstracting capacity they cannot conceive of any "absolute quantity". But they do manage to distinguish concrete quantities. They do therefore seem to have a basic number-sense (Ifrah 3-4).

If we are consistent with Husserl's doctrine of mathematics being built on formal-ontological categories, we notice here that Ifrah is confusing three very different formal-ontological categories, namely cardinal numbers, ordinal numbers and sets. Numbers are very different from sets, and what he seems to describe as "number-sense" is nothing more than the capacity of the act of conceiving objects as one set (or group). Notice also that these animals can also relate categorially as "more than" and "less than", which are other categorial relations[20]. He notices correctly that the reason why they are not able to have a more abstract notion of numbers is because they have not been able to separate these formal categories from the perceived objects. In other words, they have not reached the level of carrying out categorial abstraction.

Not carrying out categorial abstraction can explain why animals (including humans) cannot see beyond three or four objects. In the case of humans, Ifrah makes an interesting experiment which I'll reproduce here with different graphics, but it is essentially the same experiment. What I wish the reader to do now is to look at the next graphic, a series of objects. Try to know how many there are in each frame *without counting them*, just glancing at them:

[20] It might be objected by some that maybe animals don't have such a faculty of categorial intuition, because they are "taught" or trained to relate certain objects. However, if this is true, then it is a big problem for those who don't posit the existence of categorial intuition. To teach an animal to notice an element of a set would be fruitless if in principle there was no categorial intuition as its faculty of the mind. To be able for an animal to learn something, it must first have all the mental faculties with which it is capable of accomplishing a certain task.



You may have noticed that you can know just by glancing one object, two objects, three and maybe four. But by glancing five objects or more you are not able to know the quantities of objects there are without counting them (using ordinal faculty). Since animals don't know how to count, they have a very limited capacity for numbering. Not only animals, but also in some civilizations around the world, people are not able to count above three or four. For example the Murray islanders use the numbers "one", "two", "three" or "four", but above that they call "a crowd of...". The same thing happens with the Torres Straits islanders, among others (Ifrah 6).

However, we have reached a level of being able to separate sensible experience from formal judgments. We don't need to see 99,999+1 sensible objects to know that it equals 100,000. Our mind already has the capacity to grasp this judgment on the basis of carrying out our apodictic faculty of the mind. We are even able to conceive all kinds of mathematical entities, many of them not grasped using sensible basis like irrational numbers, imaginary numbers (negative roots), ultrafilters, etc.

The other one, the eidetic intuition in mathematics is also very natural and there is a very simple example of how it works.

> Consider the pigeon-hole principle. Even mathematically naive people immediately see that, if m things are put into n pigeon-holes, then, when m is greater than n, some hole must contain more than one thing. We can eliminate prior acquaintance with the proof of the pigeon-hole principle, instantaneous discovery of the proof, lucky guesses, and so on as "impossibilities." The only remaining explanation for the immediate knowledge of the principle is intuition (Katz 1998, 45).

This is eidetic intuition in action, the "perception" (figuratively speaking) of possibilities, impossibilities, necessity, etc. is also what is behind mathematical and logical reasoning. Both, mathematical intuition (categorial intuition and categorial abstraction) and eidetic intuition, are perfectly natural faculties of the mind.

1.3.0.6. *Platonism and the Fallibility of Formal Knowledge.* One of the most misunderstood aspects of Platonism is the aspect of the fallibility of mathematical knowledge. For many Antiplatonists, the infallibility of conditions of knowledge of truths and abstract relations of all objects whatsoever, implies necessarily infallibility of logical and mathematical knowledge. Also they make the mistake of equating *a priori* knowledge with infallibility of *a priori* knowledge. This is all *non sequitur* reasoning. It somehow implies that for that object to be infallible, it has to follow that our knowledge of it is infallible. The study of empirical science shows us that this is not so. Uranus has existed millenia before humans did, so it didn't depend at all in our knowledge of it. The same way, for this abstract *a priori* ideal realm to exist, it doesn't depend at all on our knowledge of it.

In fact, for Platonists, logic and mathematics *are* science, but they are *formal* sciences: they provide knowledge in a process that is essentially very different from that of natural science. For example, in the case of natural science it is not enough to formulate consistent theories, we need to find the empirical correlates that might indicate that our theories are indeed correct. But logic and mathematics don't proceed in such a way. Let me give two very simple examples, one of logic and one of mathematics.

THEOREM. *Modus Ponens preserves truth; so if α and $\alpha \rightarrow \beta$ are true in an interpretation I, then β has to be true in I.*

PROOF. Let's suppose that $\alpha$and $\alpha \rightarrow \beta$ are true in I, but that β is false in I. If $\alpha$is true and β false in I, then that would mean that $\alpha \rightarrow \beta$ should be false according to the definition of implication. This, however, would contradict the theorem's position that $\alpha \rightarrow \beta$ is true. Therefore β should be true in I. □

Notice that in the case of logic we don't have to appeal at all to experience in any way, its truth relies solely in its own definitions, rules and/or axioms.

Let's see now an example of mathematics:

THEOREM. *There is no rational number c which satisfies $c^2 = 2$, so $\sqrt{2}$ is an irrational number.*

PROOF. An irrational number is that number which cannot be expressed as a fraction (a ratio). Suppose that there is a fraction $\frac{p}{q}$ reduced to its lowest terms, in such a way that

$\left(\frac{p}{q}\right)^2 = 2$

$\therefore\ p^2 = 2q^2$

This means necessarily that independently of q's numerical value, p has to be an even number. So, therefore the value of p is $2a$. If we substitute p for $2a$ then we will obtain the following equation:

$(2a)^2 = 2q^2$

Therefore:

$2q^2 = 4a^2$

$\therefore\ q^2 = 2a^2$

This means necessarily that q is an even number. If p is even and q is even, then that means that the fraction $\frac{p}{q}$ is not reduced to the lowest terms, because both can be simplified dividing them by 2. This would contradict the hypothesis of the theorem which states that the fraction is reduced to the lowest terms. Hence, there is no rational number c which satisfies $c^2 = 2$. □

We see here also that there is no appeal at all to sensible experience in order to prove that the $\sqrt{2}$ is an irrational number.

For Platonists, both of these theorems are true and have always been timelessly (eternally) true. What we have done is not an "invention" or "construction" of the truth of the theorems. What we have done is to *discover* their absolute truth. The very notion of discovery implies these were truths that existed, but were not previously known, and they constitute mathematical knowledge. If Platonists argue the *discovery* of such objects, then the Antiplatonist's view that we posit an infallible knowledge is completely invalid.

Now, we have to confront an usual objection against Platonists concerning mathematical knowledge, that is the difficulty of proving certain theorems because their proof is too long and sometimes something in it is missing. Philip Kitcher, for instance, presents this argument:

> We suppose, with the apriorist, that when we follow a proof we begin by undergoing a process which is an a priori warrant belief in an axiom. The process serves as a warrant for the belief so long as it is present to mind. As we proceed with the proof, there comes a stage when we can no longer keep the process and the subsequent reasoning present to mind: we cannot attend to everything at once. In continuing beyond this stage, we no longer believe the axiom on the basis of the original warrant, but rather because we recall having apprehended its truth in the appropriate way. However, this new process of recollection although it normally warrants belief in the axiom, does not provide an a priori warrant for the belief. So, when we follow long proofs we lose our a priori warrants for their beginnings (Kitcher 44-45).

But this argument does not refute at all the fact that logic and mathematics considered in themselves are *a priori*, nor does it refute the fact that we have some *a priori* knowledge. All this argument shows is that our knowledge and psychological processes are limited, and we are not fully able to grasp the truth of certain logical or mathematical propositions. But through a process of check shared by all mathematicians we can show the flaws of such a "long proof" and see what went wrong and how to make it right. Sometimes there *is* no proof because the mathematical conjecture *is* wrong but *we don't know* yet it *is* wrong. Brown shows an example of Euler's conjecture, which is a generalization of Fermat's Last Theorem. It states the following:

> [. . .] if $n \geq 3$, then fewer than n nth powers cannot sum to an nth power. As a special case, this means that there are no solutions $\{w,x,y,z\}$ to the equation $w^4 + x^4 + y^4 = z^4$. The conjecture was well tested by examples, and for about two centuries was as widely believed as [Fermat's Last Theorem]. However, counter-examples have been found recently, for example, $2,682,440^4 + 15,365,639^4 + 18,796,760^4 = 20,615,673^4$ (Brown 166).

So, Euler's conjecture was definitely false, even when everyone thought it was true. The point Platonists wish to make is not that our knowledge of the logical-mathematical realm is infallible, rather we wish to point out the fact that many of these *a priori* truths sometimes can be discovered, either by proving a certain conjecture or providing its refutation. But our belief on them being true or false has nothing to do with its objective truth value.

1.3.0.7. *Where and Why Do mistakes Occur?* Most of the mistakes being made in mathematics are due to three main reasons (Brown 18-23):

(1) We could formulate false mathematical conjectures, but not know that they are actually false. This is the case of Euler's generalization of Fermat's Last Theorem which we have shown above, or David Hilbert's belief in the completeness of arithmetic. In both cases, it was through apodictic certainty of mathematics that led to the discovery of these conjectures' falsity. These refutations of conjectures don't mean at all that mathematics is doomed to be uncertain as fallible inventions of humans (Klein 391-394), nor does it mean that because this sole aspect has a similarity in conjecturing and refuting like in science, it belongs to natural science (contrary to what Kline states in 395-427). It is an exaggerated claim to state that because some of these conjectures were false there are "disasters" in mathematics (Kline 3-7). On the contrary, due to the refutation of these conjectures we have a more *certain* knowledge of mathematics: certainly it is more certain to say that arithmetic is incomplete than to state that arithmetic is complete, I would add that the certainty of the former is absolute.

(2) The use of wrong and naïve concepts is also a source of mathematical mistakes. For example, some of the ancient Greeks associated numbers with geometrical objects, and adhered to these concepts the notion of perfection (circles, squares or equilateral triangles as perfect shapes). Of course, the number 3 has nothing to do with the equilateral triangle itself, nor is the 4 related at all ontologically to the square, etc. Nor are these numbers expressing perfection of any kind. Also the misconception of numbers as distances prevented many in history to adopt negative numbers, or conceiving negative roots as contradictory to certain axioms of mathematics prevented many to develop a mathematical theory of complex numbers.

(3) Also, incorrect application of accepted principles. I'll show here an example:

Let's suppose that $x = 1$. Let us follow the rules of algebra and let's multiply both sides of the equation with the same variable x:

$$x^2 = x$$
$$x^2 - 1 = x - 1$$
$$(x-1)(x+1) = x - 1$$

If we divide both sides by $x - 1$ the result will be:

$$x + 1 - 1 = 1 - 1$$
$$x = 0$$

Therefore, $x = 1$ and $x = 0$. This is a mathematical imposibility because x cannot be 1 and 0 simultaneously. Although this demonstration seems to follow correctly all algebraic laws, in reality it does not. If the premise is that $x = 1$, then $x - 1$ would be zero, and division by zero is strictly forbidden in algebra. The mistake was to divide both sides of the equation by $x - 1$ (taken from Bunch 13).

Therefore, we must be careful in not confusing Platonism with certainty of knowledge. *Our* knowledge of abstract objects is indeed affected by our limits and fallibility, but that doesn't mean that there is no *a priori* knowledge of logical and mathematical objects. Mathematical conjectures, to attain absolute certainty, must be proven or refuted in some apodictic way, if they are not then they are just that: conjectures.

1.4. Conclusion

Formal sciences and natural sciences both are disciplines which formulate problems, seek for solutions, and aspire to attain more knowledge about their respective subjects of interest. Even though some philosophers have tried to make the validity of formal sciences to depend on natural sciences, it is evident that this is not valid reasoning. Logic and mathematics are also different fields, but closely related, and together they become an *a priori* realm and condition of possibility for the truth of any proposition or any formal relations whatsoever among objects. The faculties by which we get to know this realm are completely natural and shared not only by humans, but also by many living beings. *A priori* knowledge doesn't mean infallible knowledge. Logic and mathematics are infallible themselves, but our knowledge of them is indeed fallible; therefore, the posit of such a realm is not at all incompatible with our lack of knowledge about it or our mistakes.

CHAPTER 2

The Relation of Formal Science and Natural Science

Now that we have established the difference between formal sciences and natural sciences, in this chapter we'll explore the relation between them. There have been very different philosophical positions concerning this issue, and I'll only discuss the most important aspects of it. This chapter will deal with the issues on the dependence of formal sciences on natural sciences, how far can scientific underdetermination affect formal sciences, among others.

2.1. The Quine-Putnam Theses

One of the most controversial aspects of Philosophy today is what has to do with the problem of underdetermination in science. Most people speak of the Duhem-Quine thesis as one of its foundations. I would like to mention the fact that *there is no such thing as the Duhem-Quine thesis.* As famous as this "thesis" may be, Pierre Duhem and W. V. O. Quine really stated two very different things concerning science and how its theories affect other branches completely unrelated to science.

I wish to correct this because this term has been widely used in the fields of Epistemology and Philosophy of Science. Donald Gillies made an excellent exposition about the similarities and differences concerning the Duhem thesis and the Quine thesis. Pierre Duhem said that *in the case of Physics* an experiment can never condemn a hypothesis but a whole theoretical group. That what is really put to the test is no merely a hypothesis, but a whole bunch of hypotheses, laws and theories that the tested hypothesis supposes (Duhem 183-188; Gillies 98-99). This underdetermination of Physics *does not extend* to other branches such as Medicine and Physiology, and definitely doesn't extend to formal sciences (logic and mathematics) (Duhem 180-183). Evidence of this is the fact that he refused to think that the General Theory of Relativity was legitimate because of its use of non-Euclidean Geometry, going against our intuition that space is Euclidean (Curd and Cover 377; Gillies 105).

Quine holds a very different point of view, in "Two Dogmas of Empiricism". He denied the distinction between analytic and synthetic, and hence there is no actual distinction between formal sciences and natural sciences except in degrees of abstraction. Since there is no distinction among both sciences, all of these formal and natural posits are nothing more than convenient fictions to give experience a meaning. Quine says the following in "Two Dogmas":

> If this view is right, it is a misleading to speak of the empricial content of an individual statement – especially if it is a statement at all remote from the experiential periphery of the field. Furthermore it becomes folly to seek a boundary between synthetic statements,

> which hold contingently on experience, and analytic statements, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? [. . .]
>
> As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries – not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer. For my part I do, qua lay physicist, believe in physical objects and not Homer's gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits. [. . .]
>
> The over-all algebra of rational and irrational numbers is underdetermined by the algebra of rational numbers, but is smoother and more convenient; and it includes the algebra of rational numbers as a jagged or gerry mandered part. Total science, mathematical and natural and human, is similarly but more extremely underdetermined by experience. The edge of the system must be kept squared with experience; the rest with all its elaborate myths or fictions, has its objective the simplicity of laws (Quine 1953, 43-45)

As we can see, Quine's proposal is much more radical than Duhem's, and it does extend to logic and mathematics, specifically he presents the case of quatum logic, which we'll discuss later.

Hilary Putnam says that propositions such as "2+2=4" without a doubt are true and not subject to revision. However, Putnam states that there are mathematical propositions which are "quasi-empirical", which may be revised. Therefore there is no such thing as *a priori* knowledge (Putnam 124-126). Putnam seems to equate revisability with empirical experience, and in his mind empiricism implies revision of supposed *a priori* knowledge. I have argued against Kitcher in the first chapter that *a prioricity* is not incompatible with revisability[1]. Also Hilary Putnam shows the example of Quantum Logic as an instance of revisability of classic Logic. He says:

> [in rejecting] the traditional philosophical distinction between statements necessary in some eternal sense and statements contingent

[1]See Bob Hale's comments on Putnam's view on revisability of *a priori* disciplines in Hale 143.

> in some eternal sense [. . .] could some of the 'necessary truths' of logic ever turn out to be false *for empirical reasons*? I shall argue that the answer to this question is the affirmative (Putnam 174).
>
> I am inclined to think that the situation is not substantially different in logic and mathematics. I believe that if I had the time I could describe for you a case in which we could have a choice between accepting a physical theory based upon a non-standard logic, on the one hand, and retaining standard logic and postulating hidden variables on the other. In this case, too, the decision to retain the old logic is not merely the decision to keep the meaning of certain words unchanged, for it has physical and perhaps metaphysical consequences. In quantum mechanics, for example, the customary interpretation says that an electron does not have a definite position measurement; the position measurement causes the electron to take on suddenly the property that we call its 'position' (this is the so-called 'quantum jump'). Attempts to work out a theory of quantum jumps and of measurement in quantum mechanics have been notoriously unsuccesful to date. It has been pointed out that it is entirely unnecessary to postulate the absence of sharp values prior to measurement and the occurrence of quantum jumps, if we are willing to regard quantum mechanics as a theory formalized within a certain non-standard logic, the modular logic proposed in 1935 by Birkhoff and von Newmann, for precisely the purpose of formalizing quantum mechanics (Putnam 248).

Therefore, we have here two philosophers who seem to argue that empirical science can indeed revise formal *a priori* science.

Their statement can be summerized in two theses, which shall be called here the Quine-Putnam Theses[2]:

(1) *First Quine-Putnam Thesis*: Mathematics and logic can be revised in light of recalcitrant experience as well as changes in scientific theories.
(2) *Second Quine-Putnam Thesis*: Mathematics as such exists by the fact that it is indispensable to science. This is the so-called *indispensability argument*.

In here I shall discuss both theses in light of the case they present and the refutation of such claims.

2.2. The Case of Quantum Logic

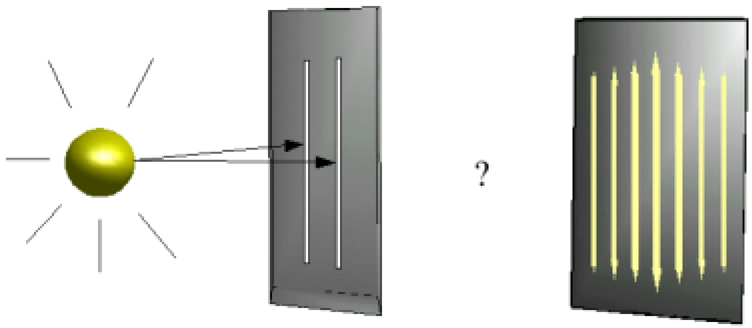
One of the most cited cases of supposed revision of Classic Logic is the famous Quantum Logic which has as its basis the empirical data on quantum behavior.

According to classic logic, this well formed formula is a tautology according to De Morgan's Laws:

$$(\alpha \wedge (\beta \vee \gamma)) \leftrightarrow ((\alpha \wedge \beta) \vee (\alpha \wedge \gamma))$$

[2]This is a phrase I borrow from Jerrold Katz who used it to refer to the statement made by both W. V. O. Quine and Hilary Putnam concerning the revisability of formal sciences in light of experience (Katz 1998, 50).

If this is a tautology, this means that it is always true, independently of the truth value assigned to the propositional variables. However, this logical truth would not seem to be true at all in Quantum Logic. Curd and Cover give us an example of how this is so. Let's look at the following illustration:



This illustration presents us the famous double-slit experiment. If we have a light-source at one side of a panel with two slits, we will be able to see an interference pattern in a screen at the other end of that panel. If we conceive light as a wave, we will be able to account for the interference pattern in the screen. If we conceive light as made up of photons (light-particles) we are not able to account for the interference pattern; at least according to our experience with particles the interference pattern should not appear, we should see only a set of two light bands corresponding to the two slits in the panel. If we cover one of the slits, the interference pattern is not shown anymore; if the two slits are open, then it appears once again. How does the photon "know" that the other is going to pass through the other slit to then form an interference pattern? It might even seem that the same photon can pass through both slits to show the interference pattern.

Now, let's suppose that p is the proposition: "The photon is in region R of the screen", q_1 is the proposition "The photon went through slit 1", and q_2 stands for the proposition "The photon went through slit 2". If the photon goes through slit 1 or slit 2, we don't see the interference pattern formed in the screen. However, if they go through *both* slits simultaneously, then we can see the pattern. Therefore this means that:

$$p \wedge (q_1 \vee q_2)$$

should not interderive with:

$$(p \wedge q_1) \vee (p \wedge q_2)$$

This is true in the quantum world, because a photon is assumed to pass through *both* slits, not one. (Curd and Cover 380). Is this a refutation of classic logic on empirical grounds, just as the Quine-Putnam Theses suggest?

2.3. The Refutation of the First Quine-Putnam Thesis

One of the ironies of philosophy is that one of the philosophers who proposed this First Quine-Putnam Thesis was precisely the one who refuted it: W. V. O.

Quine. In his *Philosophy of Logic*, Quine presents many cases of the so-called "deviant logics", one of them being Quantum Logic[3]. Ironically many Quineans nor his opponents pay attention to this very important retraction from statements he made in "Two Dogmas".

For Quine, to deny a law of logic or redefine logical connectives according to quantum phenomena would be "changing the subject". In classic logic, the meaning of logical connectives is defined by their truth values. Quantum logic is not truth functional and we cannot determine through truth tables whether conjuction, disjunction, implication mean the same as in Classic Logic. Therefore Quantum Logic can hardly be considered as a refutation of Classic Logic. At most it is an alternative logic, but not a refutation of the older one (Quine 1970, 83-86). He further adds the following:

> [. . .] I would cite again the maxim of minimum mutilation as a deterring consideration. [. . .] let us not underestimate the price of a deviant logic. There is a serious loss of simplicity, especially when the new [quantum] logic is not even a many-valued truth-functional logic. And there is a loss, still more serious, on the score of familiarity. Consider again the case [. . .] of begging the question in an attempt to defend classical negation. This only begins to illustrate the handicap of having to think within a deviant logic. The price is perhaps not quite prohibitive, but the returns had better be good.
>
> [. . .] Now the present objection from quantum mechanics is in a way reminiscent of this, though without the confusion. [. . .] Certainly a scientist admits as significant many sentences that are not linked in a distinctive way to any possible observations. he admits them so as to round out the theory and make it simpler, just as the arithmetician admits the irrational numbers so as to round out arithmetic and simplify computation; just, also as the grammarian admits such sentences as Carnap's 'This stone is thinking about Vienna' and Russell's 'Quadruplicity drinks procrastination' so as to round out and simplify the grammar. Other things being equal, the less such fat the better; but when one begins to consider complicating logic to cut fat from quantum physics, I can believe that other things are far from equal. The fat must have been admirably serving its purpose of rounding out a smooth theory, and it is rather to be excused than excited (Quine 1970, 86).

To add to this rejection from Quine, many other philosophers and scientists have objected the fact that Quantum Logic does not at all help us understand what happens in the quantum world. For many of them, such a logic only shifts the mystery from Quantum Theory to logic (Curd and Cover 380). It is for this reason that many philosophers of science as well as many scientists themselves have completely rejected the Copenhagen interpretation of Quantum Physics.

In fact, the norm in science is never to change formal sciences on empirical grounds, nor revise it, but actually to change the theory so it is consistent *according*

[3]The others being intuitionistic logic (which rejects the principle of excluded middle) and various multi-valued logics.

to the formal laws of logic and mathematics. There is a very simple but good example shown by Carl G. Hempel about this subject:

> [. . .] consider now a simple "hypothesis" from arithmetic: 3+2=5. If this is actually an empirical generalization of the past experiences, then it must be possible to state what kind of evidence would oblige us to concede the hypothesis was not generally true after all. If any disconfirming evidence for the given proposition can be thought of, the following illustration might well be typical of it: We place some microbes on a slide, putting down first three of them and then another two. Afterward we count all the microbes to test whether in this instance 3 and 2 actually added up to 5. Suppose now that we counted 6 microbes altogether. Would we consider this an empirical disconfirmation of the given proposition, or at least as a proof that it does not apply to microbes? Clearly not; rather, we would assume we had made a mistake in counting or that one of the microbes had split in two between the first and second count. *But under no circumstances could the phenomenon just described invalidate the arithmetical proposition in question* (Hempel 4, my italics).

This illustrates very well the relation between formal sciences and natural sciences. Some events in natural sciences seem to revise mathematics, when in reality they do not. Let us see two more cases where this only *seems* to happen.

2.4. General Theory of Relativity and Non-Euclidean Geometry

One of the most cited argument in favor of revision of revision of mathematics is the General Theory of Relativity and its adoption of non-Euclidean Geometry. It could be said, for instance, that Einstein's discovery of physical space-time being non-Euclidean refuted Euclidean Geometry (Putnam xv-xvi). However, we must look carefully at these claims to understand well what Einstein really did in the case of Non-Euclidean Geometry and his theories of relativity.

To be able to understand well what happens in the relation between Geometry in general, as well as the General Theory of Relativity, we should examine the reason why Non-Euclidean Geometry was formed. One of the most significant axioms in Euclidean Geometry was the axiom of the parallels which says that given a line and a non-collinear point, there is one and only one line that goes through that point which is parallel to the given line. For many, this was self-evident axiom, but for others it was not. The same assumptions were necessary for the proof of the theorem that stated that the sum of the angles of a triangle is 180°. As more mathematics became to a point of abstraction, the more mathematicians questioned the necessity of the axiom of the parallels, as well as the theorem of the angles of the triangle. In the XVIII and XIX centuries, there was the conviction by several mathematicians that to deny such an "axiom" would not lead to any logical contradictions.

The Jesuit priest, Gerolamo Saccheri (1667-1773), after trying to prove the axiom of the parallels, he discovered accidentally that Non-Euclidean Geometry is possible. He could not show through the method of *reductio ad absurdum* that the negation of the axiom of the parallels was false. So, without he knowing about it, he showed that non-Euclidean Geometry could be consistent and that it could be perfectly possible to conceive the angles of a triangle being less than 180°. He rejected this conclusion on intuitive grounds, but later Carl Frierchich Gauss (1777-1855) realized that Non-Euclidean Geometry is as valid as the Euclidean one.

It was not until János Bolyai (1802-1860) and Nikolai Lobachevsky (1793-1856) when a variant of Non-Euclidean Geometry called Hyperbolic Geometry was developed, which were mostly ignored or rejected by other mathematicians at the time for being counter-intuitive. This Hyperbolic Geometry denied the axiom of the parallels and assumed not a flat kind of space, but a pseudo-spherical kind of space. In this kind of space, the sum of the angles of a triangle is less than 180°, it was possible to "draw" more lines going through a non-collinear point which are parallel to a given line. Another mathematician, Bernhard Riemann (1826-1866), developed another kind of Non-Euclidean Geometry called Spherical Geometry, where the sum of the angles of a triangle is greater than 180°, and where the shortest distance in space between points lies in a great circle (the great circle being the line which divides the spherical space in two halves). It also makes possible that more than one lines pass through two points, contrary to Euclidean Geometry, where only one can pass.

As we can see, the development of these Non-Euclidean Geometries was not grasped by experience in any way, they came about as a direct result on the reflection of centuries of mathematicians, specially those mathematicians in the XVIII and XIX century. Such discoveries did revise mathematics, but not refuting Euclidean Geometry. In fact, Euclidean space became *one* of infinite possible mathematical spaces. It didn't refute at all that in Euclidean space, the sum of the angles of a triangle is 180°, or that the Pythagorean theorem is true. What it did refute was the assumption that the *only* valid Geometry is Euclidean Geometry. Natural science, definitely, had absolutely nothing to do with this revision.

Then what did Einstein do? Einstein was acquainted with the philosophy of the famous mathematician Henri Poincaré, who is today considered one of the fathers of the General Theory of Relativity. Poincaré accepted the mathematical validity of Non-Euclidean Geometry (Poincaré 50). For him, a Non-Euclidean world is perfectly possible, and it is very different from Euclidean space (Poincaré 64-68), but he further states the following:

> It is seen that experiment plays a considerable rôle in the genesis of geometry; but it would be a mistake to conclude that from that that geometry, is, even in part, an experimental science [. . .] [Geometry] is not concerned with natural solids: its object is certain ideal solids, absolutely invariable, which are but a greatly simplified and very remote image of them. The concept of these ideal bodies is entirely mental, and experiment but the opportunity which enables us to reach the idea. The object of geometry is the study of a particular "group"; but the general concept of group pre-exists in our minds, at least potentially. It is imposed on us not as a form of our sensitiveness, but as a form of our understanding; *only, from among all possible groups, we must choose one that will be the* standard*, so to speak, to which we shall refer natural phenomena.*
>
> Experiment guides us in this choice, which it does not impose on us. It tells us not what is the truest, *but what is the most convenient geometry.* It will be noticed that my description of these [Non-Euclidean] worlds has required no language other than that

> of ordinary geometry. Then, were we transported to those worlds, there would be no need to change that language. Beings educated there would no doubt find it more convenient to create a geometry different from ours, and better adapted to their impressions; but as for us, in the presence of the same impressions, it is certain that we should not find it more convenient to make a change (Poincaré 70-71, my italics).

As we can see in Poincaré's statement concerning Non-Euclidean Geometry is that it is as valid as Euclidean Geometry, but it would not serve well at all to adopt Non-Euclidean Geometry as a convention in this world. It is perfectly conceivable that it would make sense to adopt Non-Euclidean Geometry as a way to make the theories of the world simpler, even if Non-Euclidean Geometry itself is not as simple as Euclidean Geometry. But due to the fact that our world seems to be Euclidean, he rejects the possibility of the eventual adoption of Non-Euclidean Geometry to make scientific theories simpler.

That's where Einstein came in. One of the consequences of Lorentz Transformations and the adoption of the independence of the constancy of light from all inertial reference frames, is that nothing can travel faster than light's speed. This left a significant problem: according to Newtonian Mechanics, the speed of gravity among massive objects is infinite, and there is no account for Lorentz's spatial contraction. Einstein tells us about the way Poincaré influenced his way of thinking, and in his adoption of Non-Euclidean Geometry:

> I attach special importance to the view of geometry which I have just set forth, because without it I should have been unable to formulate the theory of relativity. Without it the following reflection would have been impossible: – In a system of reference rotating relatively to an inert system, the laws of disposition of rigid bodies do not correspond to the rules of Euclidean geometry on account of the Lorentz contraction; thus if we admit non-inert systems we must abandon Euclidean geometry. The decisive step in the transition to general co-variant equations would certainly not have been taken if the above interpretation had not served as a stepping-stone. If we deny the relation between the body of axiomatic Euclidean geometry and the practically-rigid body of reality, we readily arrive at the following view, which was entertained by that acute and profound thinker, H. Poincaré: – Euclidean geometry is distinguished above all other imaginable axiomatic geometries by its simplicity. Now since axiomatic geometry by itself contains no assertions as to the reality which can be experienced, but can do so only in combination with physical laws, it should be possible and reasonable – whatever may be the nature of reality – to retain Euclidean geometry. For if contradictions between theory and experience manifest themselves, we should rather decide to change physical laws than to change axiomatic Euclidean geometry. If we deny the relation between the practically-rigid body and geometry, we shall indeed not easily free ourselves from the convention that Euclidean geometry is to be retained as the simplest. Why is the equivalence of the practically-rigid body and the body of geometry – which

> suggests itself so readily – denied by Poincaré and other investigators? Simply because under closer inspection the real solid bodies in nature are not rigid, because their geometrical behaviour, that is, their possibilities of relative disposition, depend upon temperature, external forces, etc. Thus the original, immediate relation between geometry and physical reality appears destroyed, and we feel impelled toward the following more general view, which characterizes Poincaré's standpoint. Geometry (G) predicates nothing about the relations of real things, but only geometry together with the support (P) of physical laws can do so. Using symbols, we may say that only the sum of (G)+(P) is subject to the control of experience. Thus (G) may be chosen arbitrarily, and also parts of (P); all these laws are conventions. All that is necessary to avoid the contradictions is to choose the remainder of (P) so that (G) and the whole of (P) are together in accord with experience. Envisaged this way, axiomatic geometry and the part of natural law which has given a conventional status appear as epistemologically equivalent [. . .] (Einstein 33-35).
>
> The question whether the struture of [the space-time] continuum is Euclidean, or in accordance with Riemann's general scheme, or otherwise, is, according to the view which is here being advocated, properly speaking a physical question which must be answered by experience, and not a question of a mere convention to the selected on practical grounds. Riemann's geometry will be the right thing if the laws of disposition of practically-rigid bodies are transformable into those of the bodies of Euclid's geometry with an exactitude which increases in proportion as the dimensions of the part of space-time under consideration are diminished (Einstein 39).

What is Einstein telling us here? He is not telling us that he "looked at" space and saw its Non-Euclidean shape. What Einstein confronted in light of the Lorentz Transformations, light's speed limit, etc. was a dilemma:

(1) If we assume an Euclidean space-time, then we choose the simplest kind of geometry only at the expense of complicating the Theory of Relativity
(2) If we assume a Non-Euclidean space-time, then we choose a more complicated kind of geometry, but with the benefit of having a much simpler scientific theory.

By choosing the latter, Einstein not only could formulate a very consistent general theory of relativity, but also was able to predict and include a series of phenomena which were not accounted for in Classic Newtonian Mechanics: the second paradox of the twins, the deviation of light near massive objects, the motion of Mercury's Perihelion, among others (see also Carnap's comments in Reichenbach 1958, v).

So, what we see here in the case of General Theory of Relativity, is not that it refuted Euclidean Geometry. In its pure aspect, pure mathematical Euclidean Geometry is not itself a contradiction of Non-Euclidean Geometry, and an Euclidean space is one of an infinity of spaces. Secondly, Non-Euclidean Geometry came to be because of internal considerations *within* mathematics itself, and its historical origin has nothing to do with its adoption or rejection within empirical sciences.

Therefore, the General Theory of Relativity did not revise mathematics at all. Quite the contrary. Einstein chose one of many mathematical models of space which were available, in order to formulate the simplest theory possible that takes into consideration the mathematical implications of the special theory of relativity, as well as other phenomena.

2.5. Chaos Theory and Mathematics

2.5.1. What is Chaos Theory? Recently there has been quite an enthusiasm about Chaos Theory, even to the point of the absurd[4], and, needless to say, too propagandized. However, some have presented Chaos Theory as a refutation of empirical sciences to mathematics in general. What is Chaos Theory? Alan Sokal and Jean Bricmont explain this very well:

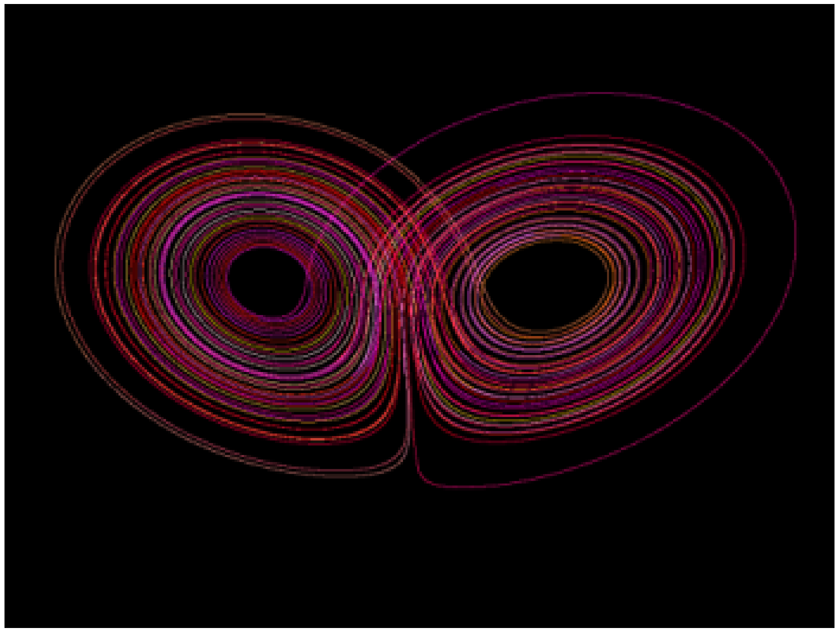
> What is chaos theory about? There are many physical phenomena governed by deterministic laws, and therefore, predictable in principle, which are nevertheless unpredictable in practice because of their "sensitivity to initial conditions". This means that two systems obeying the same laws may, at some moment in time, be in very similar (but not identical) states and yet, after a brief lapse of time, find themselves in very different states. This phenomenon is expressed figuratively by saying that a butterfly falpping its winds today in Madagascar could provoke a hurricane three weeks now from now in Florida. Of course, the butterfly by itself doesn't do much. But if one compares the two systems constituted by Earth's atmosphere with and without the flap of the butterfly's wings, the result three weeks from now may be very different (a hurricane or not). One consequence of this is that we do not expect to be able to predict the weather more than a few weeks ahead. Indeed, one would have to take into account such a vast quantity of data, and with such a precision, that even the largest conceivable computers could not begin to cope (Sokal and Bricmont 138).

This is illustrated best with what occured to one of the fathers of Chaos Theory, Edward Lorenz. He was the first to discover what would later be known as the "butterfly effect", specially in relation to the weather. He used attractors in order to describe the behavior of certain systems. Chaotic behavior, *in Chaos Theory*, doesn't mean just pure disorder or pure random. A system is chaotic if it depends greatly in the sensitivity of initial condicions. Lorenz, studying the weather and picking up data, eventually showed with an attractor, the behavior of a chaotic

---

[4]I need not emphasize the huge problems of certain "thinkers" using chaos theory to support the latest non-sense that comes to their mind. Alan Sokal and Jean Bricmont illustrate very well the overwhelming confusions concerning so-called "thinkers" like Jean-François, Lyotard, Jean Baudrillard, Gilles Deleuze and Félix Guattari (Sokal and Bricmont 147-168). For a very sober research on Chaos Theory, I might suggest the reader the following references: Kadanoff, Leo P. "Fractals: Where's the Physics?" *Physics Today.* 39. (February 1986): 6-7; Matheson Carl and Evan Kirchoff. "Chaos and Literature." *Philosophy and Literature.* 21. 1997: 28-45; Ruelle, David. *Chance and Chaos.* Princeton: Princeton University Press, 1991; Ruelle David. "Where Can One Hope to Profitably Apply the Ideas of Chaos?" *Physics Today* 47. 7. (July 1994): 24-30; Van Peer, Willie. "Sense and Nonsense of Chaos Theory in Literary Studies." *The Third Culture: Literature and Science.* Ed. Elinor S. Shaffer. Berlin and New York: Walter de Gruyter, 1998: 40-48.

system. The following is a display of what has come to be known as the famous *Lorenz attractor*:



The attractor shows that even in what is apparently disorderly behavior, there is an inherent structure within the stream of data, even when the system never really repeats itself, because the trajectories never intersect. Such a view of chaotic patterns have been very useful in explaining, for instance, Jupiter's Red Spot, which is essentially a self-organizing system within a chaotic system. This seems to apply, not only to nature, but also to economy, population growth among others (Gleick 55).

Chaos Theory also includes the idea of the fractal aspect of such behavior. Among many scientists and mathematicians, it was Benoit Mandelbrot who discovered accidentally that a diagram of income distribution can be correlated with the diagram of eight years of cotton prices. Gleick tells more of this story:

> [. . .] when Mandelbrot sifted the cotton-price data through IBM's computers, he found the astonishing results he was seeking. The numbers that produced aberrations from the point of view of normal distribution produced symmetry from the point of view of scaling. Each particular price change was random and unpredictable. But the sequence of changes was independent of scale: curves for daily price changes and monthly price changes matched perfectly. Increibly, analyzed Mandelbrot's way, the degree of variation had remained constant over tumultuous sixty-year period that saw two World Wars and a depression (Gleick 86).

So, we see not only that there is an order within the pattern as discovered by Lorenz, but also, in a sense, there was a correlation between the whole and the parts of a chaotic system. Mandelbrot could in fact calculate the fractional dimensions of real objects according to shape or any other irregular patterns. And it doesn't matter which dimensional fraction we reduce it to, we are able to see that irregular pattern again and again. He created the word *fractal* to refer to these fractional dimensions, and *Fractal Geometry* is the discipline which has fractals as its objects of study.

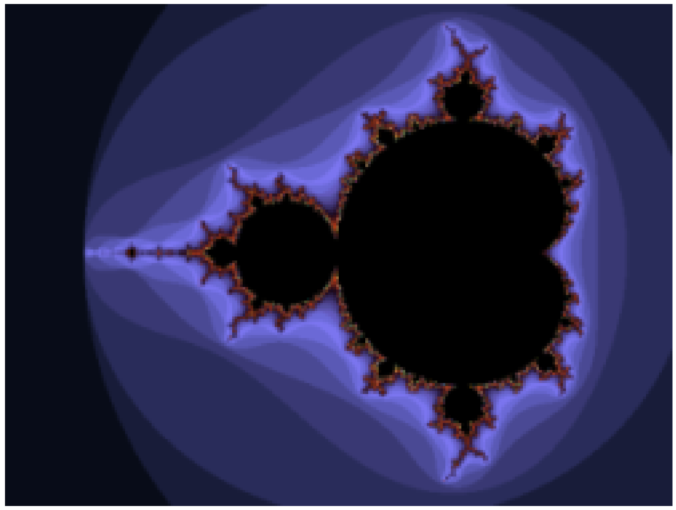
This non-conventional way to look at the world and the creation of such a mathematical discipline was a very important step in understanding the behavior of the physical world. Up to now we have seen chaotic systems and fractals in relation to the physical world. What about the mathematical realm? Many of these views apparently apply also for pure mathematical objects, such as the very well known *Mandelbrot Set*. Gullberg explains in full clear detail what the Mandelbrot Set is:

> The fractal behavior in the complex number plane is demonstrated by iterating a nonlinear function whose variables *includes its own result*. If a set of an infinite sequence $f(z)$, $f[f(z)]$, $f\{f[f(z)]\}$..., where z is a complex number, is plotted on a graph, the sequence of iterates may
>
> 1. be unbounded; or
> 2. jump around within a bounded region
>
> If (2) holds, we say that z lies in the "filled-in Julia set for f" [. . .]
>
> The Mandelbrot set is related to the Julia set, but for it the defining variable is the c in $f(z) = z^2 + c$, where z and c are complex numbers. Starting with $z = 0{+}0i$, we look for the complex numbers c, such that 0, $f(0)$, $f[f(0)]$,... remain bounded.
>
> If we let $z = 0 + 0i$ in $f(z) = z^2 + c$, then
>
> $$f(z) = f(0 + 0i) = (0 + 0i)^2 + c = c$$
>
> $$f[f(z)] = f(c) = c^2 + c$$
>
> $$f\{f[f(z)]\} = f(c^2 + c) = (c^2 + c)^2 + c$$
>
> and the process may be iterated *ad infinitum*.
>
> [. . .]
>
> We are now in a position to define the **Mandelbrot set** as the set of all complex numbers c for which the iterated $f(z) = z^2 + c$ remains bounded. The initial value of z is $0 + 0i$ and each subsequent value of z is used to find the next one (Gullbert 633).

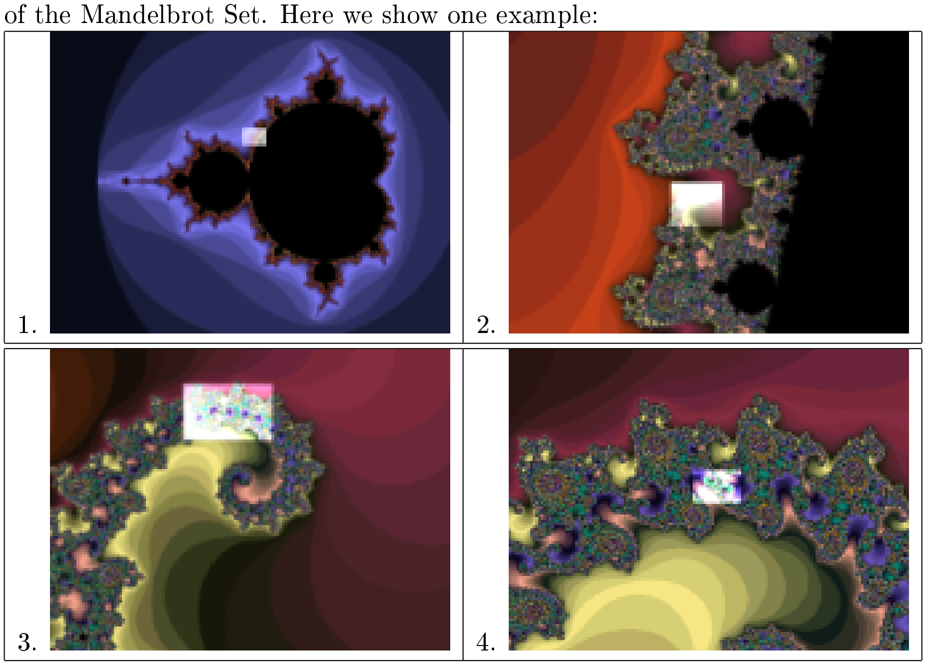
A more formal definition of the Mandelbrot set is the following:

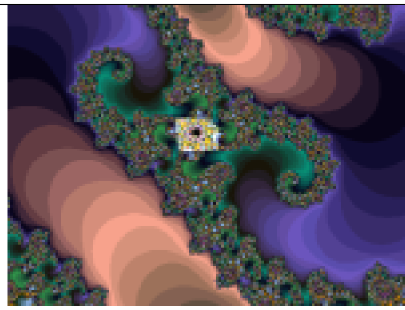
DEFINITION. The *Mandelbrot set* M is the set of complex numbers c such that the sequence $\left\langle f \begin{smallmatrix} n \\ c \end{smallmatrix} (0) \right\rangle$ does not approach ∞ as n gets larger.

Through the process of iteration in a computer program, we can actually produce this image, which is the graphical representation of the Mandelbrot Set:

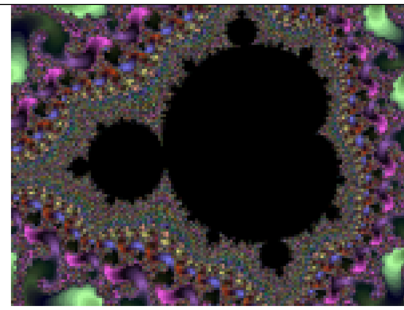


This image, as complex and irregular as it may appear, it has also in it an intrinsic pattern. Using a certain sequence of finding fractions of the Mandelbrot Set, we are able to find again a fraction that contains once more the original image of the Mandelbrot Set. Here we show one example:





5.



6.

We can see that the lighter rectangles show how we can “zoom in” the Mandelbrot Set, and find gradually the same image pattern of the Mandelbrot Set, and if we repeat that process, we will find another image of the Mandelbrot Set, etc.

So, it seems that once again we are apparently faced with a possibility that empirical sciences have revised mathematics. The same reasoning we apply in empirical experience is the same we apply to mathematical objects, since fractal reasoning applies to objects of sensible experience, it also does apply to certain mathematical objects. This reasoning is itself based on experience, and hence on the basis of sensible experience, mathematical reasoning has been revised.

2.5.2. Reply to Chaos Revisability. To be able to understand this well, we have to actually distinguish here two very important sides of Chaos Theory, one that has to do with natural and empirical sciences, and then one solely having to do with pure mathematics. Some people have alleged that Chaos Theory adds uncertainty to mathematics, specially on the basis of experience. First, I wish to ask: if mathematics is uncertain on the basis of experience, then what guarantees the certainty of Chaos Theory in the first place? If Chaos Theory in a sense refutes mathematical certainty, then why does it use mathematical certain rules to be able to understand this with certainty?

Alan Sokal and Jean Bricmont have pointed out the confusion of those who argue this way. It confuses determinisim with predictability, but this aspect applies only to empirical sciences, not to pure mathematics. Chaos Theory doesn’t refute determinism at all, all chaotic systems in the world are determined according to physical laws. They just have the peculiar aspect of its determination depending on the sensitivity of initial conditions. However, due to the fact that we are not able to account each and every single variable that intervenes in a chaotic system, we are not able to predict with 100% accuracy certain phenomena. That’s why Edward Lorenz discovered that we are able to predict the weather only in short term within a statistical model, and such a prediction loses its certainty as time goes by (Sokal and Bricmont 140-146).

But none of this revises mathematics at all, on the contrary, as I’ll show, the application of certain mathematical chaotic notions such as attractors or fractals is no different from the application of Non-Euclidean Geometry to physics. So, for this discussion we will have to look at the part of Chaos Theory which deals with pure mathematics.

In general, there are some misuses of the notion of fractals which should be mentioned here. Notice that many of the advocates of fractals as being a more accurate representation of reality. As Gleick would say about Mandelbrot:

> Clouds are not spheres, Mandelbrot is fond of saying. Mountains are not cones. Lightning does not travel in a straight line. The new geometry mirrors a universe that is rough, not rounded, scabrous, not smooth. It is a geometry of the pitted, pocked, and broken up, the twisted, tangled, intertwined (Gleick 94).

The classic way in which Mandelbrot confronts a certain problem concerning the usual way we do geometry, is to ask: "How long is the Coast of Britain?" The data about the length of the coast of Britain we see in many encyclopedias and other references are only approximations to its real one. In his research, Lewis F. Richardson found discrepancies about 20% in the estimated lengths of the coasts of Spain, Portugal, Belgium and the Netherlands. Mandelbrot wanted to take another approach: the fractal approach. He arrived to the conclusion that a coastline is infinitely long. In theory, if you continue "zooming in" the coastline, not only will everyone discover how long it is, but also discover the same irregular shapes as the original (as the famous fractal shore illustrates) (Gleick 94-96). But is this true?

Gullberg comments in this aspect:

> The universe is replete with shapes that repeat themselves on different scales within the same object. In Mandelbrot terminology, such objects are said to be **self-similar**.
>
> In the idealized world of mathematics, there are several well defined figures that are self-similar and an infinite number of such figures may be generated through iteration of functions [. . .] The word fractal – coined by Mandelbrot – was intended to describe a dimension that could not be expressed as an integer, today, "fractal" is generally understood to mean a set that is self-similar under magnification.
>
> Unlike mathematical fractals, no object in nature can be magnified an infinite number of times and still present the same shape of every detail in successive magnifications – one reason being the finite size of molecules and atoms. Yet fractal models may provide useful approximations of reality over a finite range of scales.
>
> Mandelbrot and others have applied fractals as explanatory models of natural phenomena involving irregularities on different size scales. This technique is used in graphical analysis in such diverse fields as fluid mechanics, economics, and linguistics and the study of crystal formation, vascular networks in biological tissue, and population growth (Gullberg 626).

The word "models" here is key to understand exactly what is going on. As we can see, Fractal Geometry is as much as an approximation to reality as Euclidean and Non-Euclidean Geometry is. What Chaos Theorists do essentially is to use Fractal Geometry, and many other mathematical notions and apply them to experience, the very same way the Non-Euclidean Geometry (even though it is complex) was applied to simplify scientific theory and explain the physical world more accurately; so does Fractal Geometry help us understand better chaotic systems in the physical world. None of this refutes the validity of Euclidean geometry at all. The choice of the mathematical models depends greatly on which kind of theory we choose, how is that model pertinent to it, and if it really simplifies scientific theory in such a way that actually makes possible a better understanding of the world.

So, attempts to make Chaos Theory as a way which we revise formal sciences basing ourselves on empirical experience is doomed to failure.

2.6. Refutation of Second Quine-Putnam Thesis

The Second Quine-Putnam Thesis is what has been known as the "indispensability argument", this means that mathematics is meaningful because of the fact that it is indispensable to science, and that if it was not indispensable to science, then mathematics would have no reason to be. This is related to the First Quine-Putnam Thesis, that somehow formal sciences are revised in light of recalcitrant experience. Even though we have refuted this at length, I wish to point to some strange consequences of the indispensability argument.

Rosado Haddock says that if mathematics is subordinated to Physics, it is strange that mathematics does not refer at all to physical entities or theories of any kind. In fact, it seems that mathematics, most of the time, is self-evident and true in every possible world, while Physics is not.

> Now, although applicable to the physical (and other) sciences, mathematical theorems seem to be true even if all actually accepted physical theories were false and, thus, the claim that only after the advent of modern physical science can we argue that mathematical theorems are true seems really amazing, to say the least. It is also extremely unreasonable to think that before the advent of modern physical science there was no way to establish the existence of mathematical entities, thus, e.g., that there exists an immediate successor of 3 in the natural number series. Moreover, it is perfectly conceivable that there exists a world in which all mathematical theorems known to present-day mathematicians are true (supposing that current mathematics is consistent), and that mathematicians know as much mathematics as they actually know, but in which none of the physical laws accepted as true nowadays were known to humanity. What is not possible is a world in which physical science were as developed as it actually is, but in which our present mathematical theories (especially those applicable to present-day physical science) were not valid, or, at least, were not considered to be valid (Hill and Rosado 269).

Katz also made his criticisms along this line, that we can establish the existence of these mathematical entities even without empirical science (Katz 1998, 50-51). So, can we really remain with a straight face when we state that the validity of mathematics depends on the validity of scientific theories? As we have shown, it seems the other way around. Mathematics and logic provide the theoretical basis and models for science to be able to formulate theories about the natural world. They are in every sense *a priori*, prior to any knowledge: they are the condition of possibility for any science about the world.

2.7. Conclusion

The Quine-Putnam theses are essentially wrong when they try to explain the objective relation between formal sciences and natural sciences. They practically misunderstand this relation because those who hold these Antiplatonist beliefs do not exactly reflect on what has happened historically to logic, mathematics and natural sciences, nor do they realize the fact that natural science is totally incapable of revising formal sciences, nor does logic and mathematics depend on natural science to have a meaning.

At most, natural sciences can choose several logical or mathematical models in order to provide explanations which can satisfy the factors of simplicity and logical consistency of scientific theories, as well as their power to predict phenomena. There can't be any empirical instance that can revise logic and mathematics themselves, but with empirical criteria, we can choose one mathematical model over another. This doesn't mean that the latter is no longer valid in itself mathematically speaking, it just means that the latter cannot provide the strongest model to explain empirical experience.

As a result, we must simply dismiss the Quinean Thesis that everything (even logic and mathematics) is underdetermined by science. We can legitimately consider formal sciences as completely independent of natural sciences. On the contrary, as we have stated in last chapter, the dependence of natural science on formal sciences is due to the fact that logic and mathematics together constitute the condition of possibility of the truth of every proposition and the formal relation among many objects whatsoever. Without this *a priori* foundation, not only natural sciences, but also any other kind of science is simply impossible.

Bibliography

[1] Benacerraf, Paul. "Mathematical Truth." *Philosophy of Mathematics*. Ed. Paul Benacerraf and Hilary Putnam. 1964. UK, US, Australia: Cambridge University Press, 1983: 403-420.

[2] Bernet, Rudolf, Iso Kern and Eduard Marbach. *An Introduction to Husserlian Phenomenology*. IL: Northwestern University Press, 1999.

[3] Brown, James Robert. *Philosophy of Mathematics: An Introduction to the World of Proofs and Pictures*. 1999. US, UK, Canada: Routledge, 2000.

[4] Bunch, Bryan. *Mathematical Fallacies and Paradoxes*. NY: Dover Publications, 1982.

[5] Bunge, Mario. *Epistemología*. 1980. México: Siglo XXI editores, 1997.

[6] Carnap, Rudolf. *The Logical Structure of the World and Pseudoproblems in Philosophy*. 1967. IL: Open Court, 2003. Orig. *Der Logische Aufbau der Welt*.1928.

[7] - - -. *The Logical Syntax of Language*. IL: Open Court, 2002. Orig. *Logische Syntax der Sprache*. 1937.

[8] Castilla Lázaro, Ramón. "Sobre la idealidad de los significados en la filosofía de Husserl." *Diálogos*. Num. 69. 1997. Departamento de Filosofía. Facultad de Humanidades. Universidad de Puerto Rico: 97-173.

[9] Coffa, J. Alberto. *The Semantic Tradition from Kant to Carnap: To the Vienna Station*. 1991. UK, US and Australia: Cambridge University Press, 1998.

[10] Curd, Martin and J. A. Cover (Editors). *Philosophy of Science: The Central Issues*. US: W. W. Norton & Company, 1998.

[11] Cushing, James T. *Philosophical Concepts in Physics: The Historical Relations Between Philosophy and Scientific Theories*. 1998. UK: Cambridge University Press, 2000.

[12] Duhem, Pierre. *The Aim and Structure of Physical Theory*. 1954. UK and US: Princeton University Press, 1982. Original Title: *La Théorie Physique: Son Objet, Sa Structure*. 1906.

[13] Dummet, Michael. "What is Mathematics About?" *Philosophy of Mathematics: An Anthology*. US and UK: Blackwell, 2002: 19-29.

[14] Einstein, Albert. *Sidelights on Relativity*. NY: Dover Publications, 1983.

[15] Fine, Kit. "Part-Whole." *Cambridge Companion to Husserl*. Cambridge: Cambridge University Press, 1995: 463-485.

[16] Finocchiaro, Maurice A. (ed.). *The Galileo Affair*. Berkeley, Los Angeles, London: University of California Press, 1989.

[17] Føllesdal, Dagfinn. "Husserl's Notion of Noema." *Journal of Philosophy* 66. 1969: 680-687.

[18] Frege, Gottlob. *The Foundations of Arithmetic*. 1950. IL: Northwestern University Press, 1999.

[19] Friedman, Michael. "Physics, Philosophy and the Foundations of Geometry." *Diálogos*. Año XXXVII. Num. 79. Enero 2003. Departamento de Filosofía. Facultad de Humanidades. Universidad de Puerto Rico: 121-143.

[20] - - -. *Reconsidering Logical Positivism*. US, UK and Australia: Cambridge University Press, 1999.

[21] Gillies, Donald. *Philosophy of Science in the Twentieth Century*. UK and US: Blackwell, 1993.

[22] Gleick, James. *Chaos: Making a New Science*. US: Penguin Books, 1987.

[23] Grice, H. P. and P. F. Strawson. "In Defense of Dogma." *Analytic Philosophy: An Anthology*. Editors: A. P. Martinich and David Sosa. US and UK: Blackwell Publishers, 2001: 463-472.

[24] Gullbert, Jan. *Mathematics: From the Birth of Numbers*. New York and London: W. W. Norton & Company, 1997.

[25] Hale, Bob. *Abstract Objects*. Oxford: Basil Blackwell, 1987.

[26] Hempel, Carl G. "On the Nature of Mathematical Truth." *The Philosophy of Carl G. Hempel.*1945. Oxford University Press, 2001: 3-17.
[27] Hill, Claire Ortiz. *Word and Object in Husserl, Frege and Russell.* 1991. US: Ohio University Press, 2001.
[28] Hill, Claire Ortiz and Guillermo E. Rosado Haddock. *Husserl or Frege? Meaning, Objectivity and Mathematics.* 2000. IL: Open Court, 2003.
[29] Hintikka, Jaakko. "The Phenomenological Dimension." *The Cambridge Companion to Husserl.* Editors: Barry Smith and David Woodruff Smith. Cambridge: UK, US and Australia: Cambridge University Press, 1995: 78-105.
[30] Husserl, Edmund. *Cartesian Meditations: An Introduction to Phenomenology.* The Hague: Martinus Nijhoff, 1960.
[31] - - -. *Experience and Judgment.* London: routledge and Kegan Paul, 1973. Original: *Erfahrung und Urteil.* 1939. 5th. ed. Hamburg: Meiner, 1976.
[32] - - -. *Formal and Transcendental Logic.* Trans. Dorion Cairns. The Hague: Martinus Nijhoff, 1978. Original: *Formale und transzendentale Logik.* 1929. 2nd. ed. Husserliana, vol. XVII. The hague: M. Nijhoff, 1974.
[33] - - -. *Ideas: General Introduction to Pure Phenomenology.* New York: Colliers, 1962. Original: *Ideen zu einer reinen Phänomenologie und einer phänomenologischen Philosophie I.* 1913. Rev. ed. Husserliana. Vol III. The Hague: Martinus Nijhoff, 1976.
[34] - - -. *Logical Investigations.* 2 vols. 1970. London and New York: Routledge, 2003. Original title: *Logische Untersuchungen.* 1900/1901. Rev. ed. Halle: Niemeyer, 1913.
[35] Ifrah, Georges. *The Universal History of Numbers: From Prehistory to the Invention of the Computer.* 1981. US: John Wiley & Sons, 2000.
[36] Kant. Immanuel. *Critique of Pure Reason.* Trans. Paul Guyer and Allen W. Wood. 1998. UK, US and Australia: Cambridge University Press, 2000.
[37] Katz, Jerrold J. *Realistic Rationalism.* 1998. Cambridge and London: The MIT Press, 2000.
[38] - - -. "What Mathematical Knowledge Could Be." *Philosophy of Mathematics: An Anthology.* US and UK: Blackwell, 2002: 128-146.
[39] Kay, Keith. *The Little Giant Book of Optical Illusions.* NY: Sterling Publishing, 1997.
[40] Kitcher, Philip. *The Nature of Mathematical Knowledge.* NY: Oxford University Press, 1984.
[41] Kline, Morris. *Matemáticas: la pérdida de la certidumbre.* 1985. España: Siglo XX Editores, 1998. Orig. *Mathematics: The Loss of Certainty.* NY: Oxford University Press, 1980.
[42] Lakatos, Imre. *Proofs and Refutations.* Cambridge: Cambridge University Press, 1976.
[43] Laudan, Larry. "Demystifying Underdetermination." *Philosophy of Science: The Central Issues.* Editors. Martin Curd and J. A. Cover. New York and London: W. W. Norton & Company. 1998: 320-353.
[44] - - -. "Dissecting the Holist Picture of Scientific Change." *Philosophy of Science: The Central Issues.* Editors. Martin Curd and J. A. Cover. New York and London: W. W. Norton & Company. 1998: 139-169.
[45] López Fernández, Álvaro. *Conciencia y juicio en Kant.* Río Piedras, Universidad de Puerto Rico, 1998.
[46] McMullin, Ernan. "Rationality and Paradigm Change in Science." *Philosophy of Science: The Central Issues.* Editors. Martin Curd and J. A. Cover. New York and London: W. W. Norton & Company. 1998: 119-138.
[47] Mohanty, J. N. *Edmund Husserl's Theory of Meaning.* The Hague: Martinus Nijhoff, 1982.
[48] - - -. *Husserl and Frege.* Bloomington: Indiana University Press, 1982.
[49] - - -. "Husserl and Frege: a New Look at their Relationship." *Research in Phenomenology.* 4. 1974: 51-62.
[50] Nelson, Lynn Hankinson and Jack Nelson. *On Quine.* US: Wadsworth, 2000.
[51] Poincaré, Henri. *Science and Hypothesis.* US: Dover Publications, 1952.
[52] - - -. *The Logic of Scientific Discovery.* London and New York: Routledge, 1959.
[53] Putnam, Hilary. *Mathematics, Matter and Method.* New York: Cambridge University Press, 1975.
[54] Quine, W. V. O. *From a Logical Point of View.* 1953. Cambridge and London: Harvard University Press, 1999.
[55] - - -. *Philosophy of Logic.* 1970. US: Cambridge and London: Harvard University Press, 1998.
[56] Reichenbach, Hans. *The Philosophy of Space & Time.* NY: Dover Publications, 1958.

[57] - - -. *The Theory of Relativity and A Priori Knowledge*. Berkeley and Los Angeles: University of California Press, 1965.

[58] Rosado Haddock, Guillermo E. "Edmund Husserl: A Philosopher for all Seasons?" *Modern Logic*. Vol. 7. nos. 3/4. July/December, 1997: 380-395.

[59] Sokal, Alan and Jean Bricmont. *Fashionable Nonsense: Postmodern Intellectual's Abuse of Science*. US: Picador, 1998.

[60] Tieszen, Richard. "Mathematics." *The Cambridge Companion to Husserl*. Ed. Barry Smith and David Woodruff Smith. US: Cambridge University Press, 1995.

[61] Verlade-Mayol, Victor. *On Husserl*. US: Watsworth, 2000.

Appendix

Creative Commons

Legal Code

Attribution-NonCommercial-NoDerivs 2.0

CREATIVE COMMONS CORPORATION IS NOT A LAW FIRM AND DOES NOT PROVIDE LEGAL SERVICES. DISTRIBUTION OF THIS LICENSE DOES NOT CREATE AN ATTORNEY-CLIENT RELATIONSHIP. CREATIVE COMMONS PROVIDES THIS INFORMATION ON AN "AS-IS" BASIS. CREATIVE COMMONS MAKES NO WARRANTIES REGARDING THE INFORMATION PROVIDED, AND DISCLAIMS LIABILITY FOR DAMAGES RESULTING FROM ITS USE.

License. THE WORK (AS DEFINED BELOW) IS PROVIDED UNDER THE TERMS OF THIS CREATIVE COMMONS PUBLIC LICENSE ("CCPL" OR "LICENSE"). THE WORK IS PROTECTED BY COPYRIGHT AND/OR OTHER APPLICABLE LAW. ANY USE OF THE WORK OTHER THAN AS AUTHORIZED UNDER THIS LICENSE OR COPYRIGHT LAW IS PROHIBITED.

BY EXERCISING ANY RIGHTS TO THE WORK PROVIDED HERE, YOU ACCEPT AND AGREE TO BE BOUND BY THE TERMS OF THIS LICENSE. THE LICENSOR GRANTS YOU THE RIGHTS CONTAINED HERE IN CONSIDERATION OF YOUR ACCEPTANCE OF SUCH TERMS AND CONDITIONS.

1. Definitions. a. "Collective Work" means a work, such as a periodical issue, anthology or encyclopedia, in which the Work in its entirety in unmodified form, along with a number of other contributions, constituting separate and independent works in themselves, are assembled into a collective whole. A work that constitutes a Collective Work will not be considered a Derivative Work (as defined below) for the purposes of this License.

b. "Derivative Work" means a work based upon the Work or upon the Work and other pre-existing works, such as a translation, musical arrangement, dramatization, fictionalization, motion picture version, sound recording, art reproduction, abridgment, condensation, or any other form in which the Work may be recast, transformed, or adapted, except that a work that constitutes a Collective Work will not be considered a Derivative Work for the purpose of this License. For the avoidance of doubt, where the Work is a musical composition or sound recording, the synchronization of the Work in timed-relation with a moving image ("synching") will be considered a Derivative Work for the purpose of this License.

c. "Licensor" means the individual or entity that offers the Work under the terms of this License.

d. "Original Author" means the individual or entity who created the Work.

e. "Work" means the copyrightable work of authorship offered under the terms of this License. f. "You" means an individual or entity exercising rights under this License who has not previously violated the terms of this License with respect to the Work, or who has received express permission from the Licensor to exercise rights under this License despite a previous violation.

2. Fair Use Rights. Nothing in this license is intended to reduce, limit, or restrict any rights arising from fair use, first sale or other limitations on the exclusive rights of the copyright owner under copyright law or other applicable laws.

3. License Grant. Subject to the terms and conditions of this License, Licensor hereby grants You a worldwide, royalty-free, non-exclusive, perpetual (for the duration of the applicable copyright) license to exercise the rights in the Work as stated below:

a. to reproduce the Work, to incorporate the Work into one or more Collective Works, and to reproduce the Work as incorporated in the Collective Works;

b. to distribute copies or phonorecords of, display publicly, perform publicly, and perform publicly by means of a digital audio transmission the Work including as incorporated in Collective Works;

The above rights may be exercised in all media and formats whether now known or hereafter devised. The above rights include the right to make such modifications as are technically necessary to exercise the rights in other media and formats, but otherwise you have no rights to make Derivative Works. All rights not expressly granted by Licensor are hereby reserved, including but not limited to the rights set forth in Sections 4(d) and 4(e)

4. Restrictions. The license granted in Section 3 above is expressly made subject to and limited by the following restrictions:

a. You may distribute, publicly display, publicly perform, or publicly digitally perform the Work only under the terms of this License, and You must include a copy of, or the Uniform Resource Identifier for, this License with every copy or phonorecord of the Work You distribute, publicly display, publicly perform, or publicly digitally perform. You may not offer or impose any terms on the Work that alter or restrict the terms of this License or the recipients' exercise of the rights granted hereunder. You may not sublicense the Work. You must keep intact all notices that refer to this License and to the disclaimer of warranties. You may not distribute, publicly display, publicly perform, or publicly digitally perform the Work with any technological measures that control access or use of the Work in a manner inconsistent with the terms of this License Agreement. The above applies to the Work as incorporated in a Collective Work, but this does not require the Collective Work apart from the Work itself to be made subject to the terms of this License. If You create a Collective Work, upon notice from any Licensor You must, to the extent practicable, remove from the Collective Work any reference to such Licensor or the Original Author, as requested.

b. You may not exercise any of the rights granted to You in Section 3 above in any manner that is primarily intended for or directed toward commercial advantage or private monetary compensation. The exchange of the Work for other copyrighted works by means of digital file-sharing or otherwise shall not be considered to be intended for or directed toward commercial advantage or private monetary compensation, provided there is no payment of any monetary compensation in connection with the exchange of copyrighted works.

c. If you distribute, publicly display, publicly perform, or publicly digitally perform the Work, You must keep intact all copyright notices for the Work and give the Original Author credit reasonable to the medium or means You are utilizing by conveying the name (or pseudonym if applicable) of the Original Author if supplied; the title of the Work if supplied; and to the extent reasonably practicable, the Uniform Resource Identifier, if any, that Licensor specifies to be associated with the Work, unless such URI does not refer to the copyright notice or licensing information for the Work. Such credit may be implemented in any reasonable manner; provided, however, that in the case of a Collective Work, at a minimum such credit will appear where any other comparable authorship credit appears and in a manner at least as prominent as such other comparable authorship credit.

d. For the avoidance of doubt, where the Work is a musical composition:

i. Performance Royalties Under Blanket Licenses. Licensor reserves the exclusive right to collect, whether individually or via a performance rights society (e.g. ASCAP, BMI, SESAC), royalties for the public performance or public digital performance (e.g. webcast) of the Work if that performance is primarily intended for or directed toward commercial advantage or private monetary compensation.

ii. Mechanical Rights and Statutory Royalties. Licensor reserves the exclusive right to collect, whether individually or via a music rights agency or designated agent (e.g. Harry Fox Agency), royalties for any phonorecord You create from the Work ("cover version") and distribute, subject to the compulsory license created by 17 USC Section 115 of the US Copyright Act (or the equivalent in other jurisdictions), if Your distribution of such cover version is primarily intended for or directed toward commercial advantage or private monetary compensation.

e. Webcasting Rights and Statutory Royalties. For the avoidance of doubt, where the Work is a sound recording, Licensor reserves the exclusive right to collect, whether individually or via a performance-rights society (e.g. SoundExchange), royalties for the public digital performance (e.g. webcast) of the Work, subject to the compulsory license created by 17 USC Section 114 of the US Copyright Act (or the equivalent in other jurisdictions), if Your public digital performance is primarily intended for or directed toward commercial advantage or private monetary compensation.

5. Representations, Warranties and Disclaimer. UNLESS OTHERWISE MUTUALLY AGREED BY THE PARTIES IN WRITING, LICENSOR OFFERS THE WORK AS-IS AND MAKES NO REPRESENTATIONS OR WARRANTIES OF ANY KIND CONCERNING THE WORK, EXPRESS, IMPLIED, STATUTORY OR OTHERWISE, INCLUDING, WITHOUT LIMITATION, WARRANTIES OF TITLE, MERCHANTIBILITY, FITNESS FOR A PARTICULAR PURPOSE, NONINFRINGEMENT, OR THE ABSENCE OF LATENT OR OTHER DEFECTS, ACCURACY, OR THE PRESENCE OF ABSENCE OF ERRORS, WHETHER OR NOT DISCOVERABLE. SOME JURISDICTIONS DO NOT ALLOW THE EXCLUSION OF IMPLIED WARRANTIES, SO SUCH EXCLUSION MAY NOT APPLY TO YOU.

6. Limitation on Liability. EXCEPT TO THE EXTENT REQUIRED BY APPLICABLE LAW, IN NO EVENT WILL LICENSOR BE LIABLE TO YOU ON ANY LEGAL THEORY FOR ANY SPECIAL, INCIDENTAL, CONSEQUENTIAL, PUNITIVE OR EXEMPLARY DAMAGES ARISING OUT OF THIS LICENSE OR THE USE OF THE WORK, EVEN IF LICENSOR HAS BEEN ADVISED OF THE POSSIBILITY OF SUCH DAMAGES.

7. Termination. a. This License and the rights granted hereunder will terminate automatically upon any breach by You of the terms of this License. Individuals or entities who have received Collective Works from You under this License, however, will not have their licenses terminated provided such individuals or entities remain in full compliance with those licenses. Sections 1, 2, 5, 6, 7, and 8 will survive any termination of this License.

b. Subject to the above terms and conditions, the license granted here is perpetual (for the duration of the applicable copyright in the Work). Notwithstanding the above, Licensor reserves the right to release the Work under different license terms or to stop distributing the Work at any time; provided, however that any such election will not serve to withdraw this License (or any other license that has been, or is required to be, granted under the terms of this License), and this License will continue in full force and effect unless terminated as stated above.

8. Miscellaneous. a. Each time You distribute or publicly digitally perform the Work or a Collective Work, the Licensor offers to the recipient a license to the Work on the same terms and conditions as the license granted to You under this License.

b. If any provision of this License is invalid or unenforceable under applicable law, it shall not affect the validity or enforceability of the remainder of the terms of this License, and without further action by the parties to this agreement, such provision shall be reformed to the minimum extent necessary to make such provision valid and enforceable.

c. No term or provision of this License shall be deemed waived and no breach consented to unless such waiver or consent shall be in writing and signed by the party to be charged with such waiver or consent.

d. This License constitutes the entire agreement between the parties with respect to the Work licensed here. There are no understandings, agreements or representations with respect to the Work not specified here. Licensor shall not be bound by any additional provisions that may appear in any communication from You. This License may not be modified without the mutual written agreement of the Licensor and You.

Creative Commons is not a party to this License, and makes no warranty whatsoever in connection with the Work. Creative Commons will not be liable to You or any party on any legal theory for any damages whatsoever, including without limitation any general, special, incidental or consequential damages arising in connection to this license. Notwithstanding the foregoing two (2) sentences, if Creative Commons has expressly identified itself as the Licensor hereunder, it shall have all rights and obligations of Licensor.

Except for the limited purpose of indicating to the public that the Work is licensed under the CCPL, neither party will use the trademark "Creative Commons" or any related trademark or logo of Creative Commons without the prior written