Truth, Falsity and Experimental Science

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Abstract

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The arguments presented in this thesis aim to show that a Kantian model in which a theoretical framework is constitutive of empirical reality denies the possibility of a full grown experimental science. In contrast, a logical empiricist model according to which empirical reality is described by means of a theoretical language such as Euclidian geometry or Newtonian mechanics allows for a possible disagreement between experiment and theory. One of the main strengths of this model consists in its ability to handle cases in which theory is shown to be false in respect to empirical reality. However, in order to achieve this compatibility with experimental science, I argue that we must abandon the assumption that there is a single, unified language of science and replace it with the more general assumption of a plurality of languages. Under this new formulation, a perfect unity of science cannot be granted solely by a coherent formulation of all scientific knowledge in a unique language of science, but must be complemented by the common reference of all scientific theories to the same empirical reality.

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Introduction

One of the distinguishing features of natural science is its experimental character. Knowledge is gained through a process of hypothesis-formation and hypothesis-confirmation, the former pertaining to the formulation of a theory, the latter to the experimental confirmation of that theory. Despite the widespread popularity of this view among scientists and philosophers alike, few attempts have been made to establish the conditions making it possible for a knowing subject to gain access to experimental knowledge. In my opinion, experimental knowledge requires a distinction between two independent faculties of the mind, one pertaining to our ability to form hypotheses, the other to our ability to observe, or perceive empirical reality. These two faculties are responsible for mental presentations which, when found to be in agreement, amount to true knowledge of empirical reality. Accordingly, the main goal of the present work is to show that experimental science is impossible without the agreement or disagreement between two distinct sets of mental presentations.

We can immediately observe that among epistemologists it is Kant who provides us with a dual-faculty model of the mind and with a theory of truth whereby truth and cognition amount to the agreement between these two faculties. Unfortunately, despite these assets, without major modifications Kant's theory fails to provide a workable account of experimental science. Most notably, Kant's epistemology makes Newtonian mechanics, as well as the conceptions of space and time attached to it, constitutive, that is, unrevisable elements of our knowledge. Second, Kant's notion of intuition overlaps with what we would call today perception, yet it is not identical with it; similarly, what

Kant considered the domain of discursive thought is split nowadays between perception of objects and events, which occurs automatically without involving any conscious thought, and theoretical concepts required for scientific knowledge. Third, Kant lacks a naturalised account of perception, such that even though he introduces the notion of an agreement between intuition and concept, this agreement remains strictly internal to our minds and has no connection with external reality. Finally, Kant's theory of schematisation entails a necessary agreement between concept and intuition, conclusion that ultimately amounts to a pre-established harmony incompatible with the notion experimental falsification. All these difficulties have been answered by subsequent generations of epistemologists, yet, as I already remarked earlier, little attention has been paid to the kind of mind the possibility of experimental science presupposes.

In light of these criticisms and their respective solutions, the arguments presented in Chapter 1 aim to show that in order to make experimental science possible, knowledge must amount to a predication whereby theory (the predicate) is not always true, but can also be false in respect to empirical reality (the subject). Since such a predication cannot always be interpreted as a Kantian synthesis in which theory subsumes, organises and ultimately becomes constitutive of empirical reality, scientific knowledge must consist at least in the case of experimental falsification in a dual presentation of empirical reality. One kind of presentations is derived from perception, the other stems from our ability to formulate hypotheses. An agreement between these two mental items amounts to scientific truth, a disagreement, to falsity.

Chapter 2 further explores the nature of perceptually derived presentations. My two main arguments are 1) that a constructivist approach is best suited for accounting for the fact that we do not have detectors specialised for each individual entity or type of entity existing an ontological level, and 2) that a construction based on logical and simple arithmetical operations is not enough to account for our perception of objects. Hence the necessity of harnessing the process of construction with a process of verification, both implemented physiologically at the level of perception. This way we can maintain a distinction between empirical reality and external reality, yet keep the two connected via mechanisms of perception.

Finally, in Chapter 3 I argue that we construct theories, yet this construction does not simply mirror or imitate structural elements empirically accessible. We don't just invent theories on the spot, but construct them much as we construct formal languages and logico-mathematical systems, while verification and falsification are absolutely required because these theoretical constructions may or may not emulate specific aspects of the construction effected at the level of perception. This minimal amount of information conveyed by a constructivist approach allows a discussion about how facts and theory fit together beyond the mere statement that they must somehow "agree" or "disagree". Further investigation of the nature of this possible agreement and disagreement suggests that the only unity that can be established experimentally is that derived from the common reference of all scientific theories to the same empirical reality, while a complete unity hinges on the possibility of a meta-knowledge of empirical reality.

Chapter 1

Overcoming the limitations of Kant's framework of possible experience

1.1 Introduction

In the *Critique of Pure Reason*, Kant starts from the premise that we know and aims to establish the theoretical conditions that made possible our knowledge in the first place. Consequently, his transcendental philosophy has the task of uncovering the necessary presuppositions grounding Newtonian physics in particular and scientific knowledge in general.[1] In Kant's day Newtonian mechanics was considered the most fundamental natural science, serving as underlying theoretical basis for all other sciences, Kant regarded these presuppositions not only as conditions of possibility for Newtonian mechanics, but for natural science in general. In fact, Kant went much further, claiming that in as much these presuppositions underlie all empirical knowledge, we can safely conclude that they also constitute the absolute, fixed framework of all possible experience of empirical reality. Thus, for Kant, the conditions granting the possibility of scientific knowledge are by no means mere postulates of Newtonian physics, but constitutive principles of experience.[2]

It is clear to us who have witnessed the development of new mechanical theories that Kant's framework of all possible science is obsolete. Among several other philosophers, Cassirer and Poincaré understood very well the huge difficulty that the development of non-Euclidian geometries and, even more problematical, the

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[1] Friedman, M. *The Dynamics of Reason* (Stanford: CSLI Publications, 2001), 37.

[2] The concepts of the understanding are schematized, which is to say that they subsume and organize intuition, thereby giving it an a priori determined form. In other words, we experience reality not merely as sensations in space and time, but also as causal events, substances etc. Kant, I. *Critique of Pure Reason* (Hackett, 1996; translated by W. S. Pluhar), B237.

development of relativistic mechanics posed to Kantian epistemology. Nevertheless, they thought that we can keep Kant's fundamental idea that scientific knowledge has a formal structure while fully understanding that Kant's famous framework of possible experience is nothing but a particular example of a framework applying solely to Newtonian mechanics. Interestingly enough, neither undertook the task of defining the precise framework elements underling the science of their day; most probably, such a task, more akin to Hilbert's axiomatization project, was seen as belonging to mathematics and theoretical physics rather than philosophy. Instead, they concentrated their efforts in defining a framework flexible enough that can apply not only to a particular geometry or mechanics, but to science in general. Their main concern was to preserve the general idea of a framework of scientific knowledge, while avoiding any strong commitment to a particular formulation of this framework. Cassirer proposed a replacement of Kant's unique and essentially immutable framework of knowledge with a dynamic one, while Poincaré advocated the existence of a plurality of frameworks among which we can choose which ever best serves our cognitive interests.

The purpose of the present chapter is twofold. First, I will explain how Cassirer and Poincaré succeed in overcoming the shortcomings of Kant's constitutivist interpretation of space, time and Newtonian mechanics in order to make Kant's general notion of a framework of experience compatible with the development of new geometries and mechanical theories. By preserving this fundamental element of Kantian epistemology, Cassirer and Poincaré also maintain a distinction between the form and the content of knowledge.[3] This distinction is important in as much as it grants philosophy the status of a formal discipline of knowledge, to be distinguished from natural science,

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[3] Friedman, M. *Reconsidering Logical Positivism* (Cambridge Univ. Press, 1999), 45.

which applies the form of knowledge to an empirical content. Second, I make a note of the fact that although a relativization of the framework of scientific knowledge solves the problem at a formal level, it is also important to specify how this relativization is achieved in the actual scientific practice. I think this second requirement is important in as much the epistemological accounts offered here claim to be accounts of scientific knowledge. Cassirer proposes to us a rule or instrument of knowledge, while Poincaré appeals to conventionalism. I argue that neither of these particular instantiations of the formal solution captures the actual process through which science progresses, which, as far as most scientists are concerned, is always experimental.

As mentioned before, Kant generalizes the conditions of possibility of Newtonian mechanics in two steps, first by claming that these conditions apply to all science and second, that they are constitutive of all possible experience. The first generalization is quite natural given the status of the science of his time. The second generalization is much more subtle and hinges on Kant's notion that there are such things as synthetic a priori judgments. The solutions proposed by Cassirer and Poincaré constitute a remedy to Kant's first generalization. Thus, even though the same empirical data can be explained by several scientific theories and, more so, even if the same empirical data can be better explained by some theories rather than others, the said models and theories remain constitutive of empirical data. But if this is the case, it follows that a theory can never be in absolute contradiction with empirical experience and as such no theory can ever be refuted on experimental grounds. Given the implausibility of this conclusion, I argue that a Kantian-like distinction between intuition/perception and understanding/scientific knowledge must be preserved if experimental science is to be made possible.[4] My arguments aim to show that in order to make experimental science possible, knowledge must amount to a predication whereby theory (the predicate) is not always true, but can also be false in respect to empirical reality (the subject). Since this predication cannot be always further interpreted as a Kantian synthesis in which theory subsumes, organises and ultimately becomes constitutive of empirical reality – counterexamples being precisely such cases of falsification, when a theory is shown to be false about empirical reality (i.e., knowledge of what or how reality is not) – scientific knowledge must consist at least in these cases in a dual presentation of empirical reality, as a set of direct, perception-based observations and as a theory failing to explain these observations.

1.2 Poincaré's conventionalism

1.2.1 Overview of Poincaré's conventionalism

In *Science and Hypothesis*, Poincaré develops an approach known as conventionalism. His position constitutes a direct response to Kant's claim that space, understood here as Euclidian space, is the pure form of intuition and therefore must be constitutive of all experience of empirical reality. Poincaré dismisses this view on two grounds. First, he argues that intuitive space, that is, the experience of space conveyed by our senses, is different from the Euclidian space postulated by Newtonian mechanics.

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[4] I say "Kantian-like" because this distinction does not amount to a separation of intuition from concept, but to a separation of perception from theoretical/scientific concepts. Intuition is a flux of primary sensations and reflects the typical model of perception prevalent in the 17th and 18th centuries; Kant's contribution is to point out that this flux always occurs in space-time. Perception, or what is nowadays defined as perception in any introductory psychology textbook, further organizes primary sensations in objects and events. The distinction is grounded on the fact that perception is a given, for we cannot perceive empirical reality else than already organized in objects and events manifesting themselves in space-time, and thought, which further organizes perceptions according to laws of conservation, causality, action and reaction and many other theoretical concepts. I think this is very similar to what Kant had in mind when he distinguished between intuition and thought (Kant, *Critique of Pure Reason*, B33).

According to Poincaré, intuitive space comes in many flavours. There is a "visual space", a "tactile space", a "motor space" etc. None is identical with Euclidian space. For instance, visual space is two-dimensional, finite and non-homogenous; in contrast, Euclidian space is three-dimensional, infinite and homogenous.[5] Based on this argument, Poincaré concludes that Euclidean space is a theoretical construct rather than a form constitutive of sense data.[6] Second, Poincaré continues, we can always replace Euclidian space with a non-Euclidian space and reformulate empirical laws in accordance with this newly postulated geometrical configuration of the universe. In order to illustrate this point, Poincaré gives the example of a finite, spherical universe of radius R whose temperature at any distance r from the center of the universe is proportional with $R^2 - r^2$. He further postulates that all objects within this universe are subjected to the same coefficient of dilation/contraction. Poincaré argues that this universe would appear infinite to its inhabitants. Due to the temperature drop, the length of any measuring rod would become smaller and smaller as one moves from the center to the periphery of the universe. While progressing towards the periphery, one's steps become smaller and smaller, ultimately making it impossible to ever reach the edge of the universe. Given this setup, it becomes clear that we cannot distinguish between our universe, in which "*geometry is only the study of the laws according to which invariable solids move*" and this imaginary universe in which geometry is the "*study of the laws of motion of solids deformed by the differences of temperature alluded to*".[7] In formulating the laws of motion, we appeal to two independent postulates, one defining the behaviour of the measuring unit (what counts as a solid body), the other, the geometry by the means of

[5] Poincaré, H. *Science and Hypothesis* (New York: Dover, 1905), 52.
[6] Poincaré, *Science and Hypothesis*, 57.
[7] Poincaré, *Science and Hypothesis*, 66.

which we choose to formulate these laws. Since there are two variables, we can never definitively and absolutely prove that Euclidian geometry is the right geometry, but only that Euclidian geometry is the right geometry describing this universe in as much as there is such a thing as a rigid body; conversely, we can prove that there are rigid bodies only by assuming that Euclidian geometry is the geometry of our universe.

In more general terms, Poincaré argues that axiomatic geometry contains no assertions in respect to empirical reality and as such it cannot predicate anything about physical objects. It follows from here that geometry must be complemented with empirical laws in order to obtain knowledge about empirical reality. Based on this view, according to which knowledge about external reality arises only when a formal foundation is complemented with empirical laws, Poincaré argues that we could choose any geometry and alter the empirical laws in such a way that they agree with the formal foundations of science. Einstein summarises Poincaré's standpoint as follows: "*Geometry (G) predicates nothing about the relations of real things, but only geometry together with the purport of physical laws (P) can do so. Using symbols, we may say that 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 contradictions is to choose the remainder of (P) so that (G) and the whole of (P) are together in accord with experience*".[8]

The immediate consequence of this view is that all empirical laws describing the actual behaviour of physical objects are necessarily grounded on a set of premises assumed to be true a priori. In particular, geometry, and with it our conception of space, underlies empirical reality at an a priori level, with the difference that, unlike Kant,

[8] Einstein, A. *Sidelights on Relativity* (New York: Dover, 1921), 35.

Poincaré gives us the option to constitute empirical reality in accordance to more than one conception of space. Space is therefore an a priori element of cognition, yet it is not constitutive in a Kantian sense. Rather, it is a convention, that is, the postulating of a particular geometry by the means of which we choose to formulate empirical knowledge. Since nothing forbids us to describe empirical reality by means of another geometry, we can always abandon the whole of our natural science and construct a new one on the a priori basis of this other geometry. In fact, it may very well be the case that some geometries make possible simpler, more concise and elegantly formulated empirical laws allowing for easier mathematical computation. At the very extreme, it may even be the case that a particular geometry may be well suited for quick computations of solutions to specific problems, while other geometries may entail heavier mathematical formulation yielding very difficult or even impossible to compute solutions. Nevertheless, despite all possible practical and mathematical virtues, no geometry can ever be shown to be in itself a truer, more faithful representation of reality.

1.2.2 The problem of experimental falsification in connection to Poincaré's conventionalism

We can see quite easily how conventionalism overcomes the limitations of a straightforward Kantian interpretation of space. Poincaré explicitly distinguishes between intuitive space, such as the "visual space", and theoretical space, such as the Euclidian space postulated by Newton. He argues for the existence of a space inherent to our perception in order to point out the fact that this space is by no means the Euclidian space posited by Kant as the pure form of our intuition. If this is the case, then Euclidian space can be safely removed from the sphere of perception and transferred into that of theoretical knowledge such that instead of necessarily intuit or perceive phenomena occurring according to the laws of Euclidian geometry, we rather choose to conceive them so. Without any doubt, Poincaré continues, our choice is guided by pragmatic concerns, yet, strictly speaking, nothing forbids us to conceive phenomena as occurring into a non-Euclidian space.

Conventionalism becomes problematic the moment we make an explicit move from geometry to physics. It is quite easy to imagine how conventionalism answers the difficulty raised by relativistic mechanics. The conventionalist would argue that we can choose Euclidian geometry and describe the consequences of the fact that light has a finite space in accordance to the laws of Newtonian mechanics (these would be Lorentz's transformations), or we can choose a non-Euclidean geometry and embrace the special theory of relativity.[9] In other words, we can formulate empirical laws and describe empirical reality in terms of Newtonian mechanics (and therefore appeal to Euclidian geometry), or in terms of relativistic mechanics (and therefore work on the premises of a non-Euclidian space); which mechanics we choose to formulate empirical laws is a matter of convention.

The solution works. Kant's fundamental notion of a framework of scientific knowledge is altered as to accommodate new geometries and mechanics. However, there is no overlap between this solution to Kant's problem and the actual scientific practice. This is not how physicists see physics. Physicists do not believe that they chose by convention relativistic mechanics over classical mechanics, but claim loud for anyone to hear that they have experimental proof that relativistic mechanics is truer and Newtonian

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[9] Friedman, *The Dynamics of Reason*, 62. Friedman, *Reconsidering Logical Positivism*, 22.

mechanics is an approximation. To make things worse, under a conventionalist model of scientific knowledge experience can never falsify fundamental claims about empirical reality. Ultimately, there is never a dialogue between theory and experience, but merely a formulation of empirical observations in the language of one conventional theory or another. Conventionalism reduces science to a description of the universe in accordance with theory rather than the confirmation or falsification of theory. As a former experimental scientist, I cannot but argue that such a conception of science is fundamentally mistaken. While Aristotelian physics or 19^{th} century zoology might have been primarily descriptive, contemporary science is essentially experimental. Science does not merely aim to describe phenomena in light of a theory. In the eyes of the contemporary scientist, this is not science, but at worst speculation and at best classification. For the contemporary scientist, science aims primarily to show that the descriptions it provides are true because the theory underlying them is true. What matters, at least in as much we are not directly engaged in a practical application of our knowledge, is not how accurate our picturing of reality is, but rather whether a theory is true or false about predicting observed phenomena.

1.2.3 Conventionalism relies on the assumption that we cannot distinguish between fact and theory

Poincaré's conception of scientific knowledge is modeled after axiomatic geometry. First we decide by convention the kind of geometry we want to use and then apply ourselves to the task of formulating accurate physical laws describing empirical reality in conformity with this geometry (i.e., by obeying the rules of our description language). The idea is quite fascinating in its practical simplicity. For those acquainted with computer programming, it is called "patching". What Poincaré tells us is that we can choose whichever geometry we want, even if it is not the true geometry of the universe, and correct the inadequacies of our geometry by appending further clauses under the form of empirical laws. Unfortunately, Poincaré did not confine himself to the sphere of the practical, but argued that since truth can sometimes be approximated by choosing an arbitrary initial position which we correct afterwards by trial and error, our knowledge has something arbitrary about it. The problem is that if conventionalism is right, then experimental science is impossible. If empirical laws are always formulated inside a given geometry, they can never confirm or falsify our choice of one geometry rather than another. In contrast, experimental science aims to show that a hypothesised theory agrees with the empirical description of reality. Thus, it is assumed that empirical reality already has an underlying structure of its own and that theory can agree or disagree with this structure.

Einstein attempts to overcome Poincaré's conventionalism by appealing to the notion of "rigid body", that is, the notion that if the size of two rods is determined to be equal here and now, those rods are of equal size everywhere and at any time in the universe. Poincaré argues that we choose a particular geometry and then proceed to formulate all our knowledge of the physical world in accordance with that geometry, although, under the same conventionalist frame of thought, nothing forbids us first to choose the definition of the solid body and, on the basis of this assumption, prove or refute one geometry as being the true geometry of our universe. So far, it seems that one way or another, we decide upon some fundamental premises which automatically become a priori constitutive of our subsequent knowledge. However, if our choice of geometry is not something immediately verifiable, our choice of what counts as a rigid body is, to use Einstein's own words, "*a principle which is accessible to experience*".[10] Phenomenally speaking, objects preserve their appearance in space and time. Mere translation in space and time does not alter their size, that is, the observed fact that the edges of the two rods continue to coincide. What we observe is two rods that conserve their lengths; this is the empirical datum. From here on, the task of physics is to establish the relationships between the two rods once we move them apart. It turns out that in a universe populated by massive bodies these spatial relationships are better matched by the Riemannian space underling relativistic mechanics rather than by standard, Euclidian geometry assumed by classical mechanics.

The other question is "Do we observe a dependence of space on temperature?". According to Poincaré, length is seemingly independent of temperature in a curved-space universe because we are considering bodies with identical coefficients of dilation moving across a gradient of temperature such set that the curvature of space is automatically corrected as to make all local empirical measurements indistinguishable from measurements within a Euclidian space. Poincaré's explanation takes for granted that the length of a material body is dependent on temperature; I assume this can be tested empirically. Then by increasing or decreasing temperature we can alter the empirically established geometry of space, as determined by local measurements, such that we can specifically distinguish the effect of temperature on geometrical measurements and subtract it accordingly. The real difficulty that remains to be settled is whether temperature alters the structure of space rather than the mere behaviour of material

[10] Einstein, *Sidelights on Relativity*, 37.

bodies. Doing local measurements will not be of much help here. What we need to do is monitor the overall geometry of space. We need to watch the sky for some colossal explosion, say, a supernova, and see if during this event the apparent geometry of space changes – for instance, see if the position of some background stars is the same during the explosion of the supernova. Presumably, the experiment will establish that, unlike mass, temperature does not affect the structure of space. But if space is not curved by temperature, then we can rely on the experimentally established co-variation of temperature and length in order to subtract the local effect of temperature onto our local measurements and establish the geometrical structure of space.[11]

The difficulty of Poincaré's objection stems from the fact that he offers two equivalent theoretical scenarios: whether we abide with one or the other, space will be experienced as non-curved and infinite. This is true, yet this does not mean that what we have here are two competing theories about empirical reality. Our experience of empirical reality does not reduce to measurements, but must also take into account what we can and cannot alter about each of the variables involved in our theories. Poincaré's first theory, about our universe as we know it, refers to a reality in which space does not depend on temperature. The second, about the round-shaped universe, refers to a reality in which either space is altered by temperature or no temperature changes are possible or there is a further clause prohibiting us from dissociating temperature and space. But we just established that this second clause doesn't fit empirical reality, therefore the two

[11] Friedman argues for the same result in a similar manner: "*In the context of classical mathematical physics, Poincaré is perfectly correct: physical geometry belongs to the a priori part of our theoretical framework and hence to the conventional part. In the context of the general theory of relativity, however, Poincaré is incorrect: in this context physical geometry belongs rather to the empirical part of our theoretical framework and hence to the nonconventional part*" (*Reconsidering Logical Positivism*, 85-6).

theories cannot possibly have the same referent, but are true in respect to two distinct possible realities.

Experimental confirmation of a non-occurring phenomenon requires a modification of the way the universe is experienced at an empirical level, else the explanation has merely the value of a counterfactual statement, i.e. should the universe be so and so, then such a phenomenon would occur. Given this distinction between actual and counterfactual, it becomes clear that the main difference between Poincaré's conventionalism and Einstein's experimental approach hinges on a sharp distinction between observation and theory. Theory is something we can always change, while observation stays fixed (or again, we can change solely by means of speculative, counterfactual thought experiments; however, in this case we remove the actual observation from the realm of perception and reproduce it, with some specific alterations, in our imagination). It follows from here that we do not have two actual variables, but only one. We cannot not observe that two rods equal here are also equal three meters or a million meters from here; likewise, we cannot observe a dependence of space on temperature. We can only imagine what the entailments for our scientific theories would be, should they ever turn out not to be equal or should space be affected by temperature; only in the case of such a counterfactual thought experiment we would have two variables. But if in actual experience there is only one variable, it follows that, from an experimental point of view, it is up to theory (the actual variable) to match observation, not of observation (the constant) to match theory. Thus, the question whether Euclidian or non-Euclidian geometry is true about empirical reality makes sense only in as much it aims to establish the spatial relationships between the two rods in those conditions in which we have already empirically established that they remain equal and that temperature does not alter the structure of space. To use Einstein's formulation, in G + P, G is not up to us, but has to account for these two facts. Neither of these two facts are laws of physics already presupposing a geometry; they are just that to which G truthfully refers. But if G is so constrained, P (including the law of dilation) must be likewise constrained, for all empirical laws, in addition to the standard requirement of matching facts, must also be formulated on the assumption of this G and no other.

1.3 The formal structure of objective knowledge according to Cassirer

1.3.1 Brief introduction of Cassirer's model of knowledge, as presented in *Substance and Function*

In *Substance and Function* (1910/1923), Cassirer offers a more sophisticated strategy for the relativization of the a priori. For Cassirer, the object of knowledge must be in agreement with the criteria of constancy of our judgment; thus, objectivity and truth amount to the formulation of universal laws.[12] What we know objectively are not individual conscious presentations, but their constant relationships.[13] It follows from here that knowledge is not a passive receiving of information, a mirroring or picturing of the world, but, in order to count as knowledge, it must satisfy certain demands of constancy.[14] Reason demands that each judgment (S is P) holds not only for the present moment, but for the whole succession of moments in time; this constitutes "*the fundamental schema for the concept of an empirical object*".[15] Because knowledge is

[12] Cassirer, E. *Substance and Function*, 305.
[13] Cassirer, *Substance and Function*, 290.
[14] Cassirer, *Substance and Function*, 295.
[15] Cassirer, *Substance and Function*, 294.

knowledge only in as much it satisfies the formal demands of reason, it follows that what counts as real, true and objective, that is, the object of our knowledge, must also be in agreement with the formal demands and criteria of constancy of our judgment.

1.3.2 Knowledge as a rule-based operation

It would be unfair to judge Cassirer's epistemology as a purely coherentist or pragmatist one. He does not say that it is true whatever coheres with our already formed or assumed knowledge of reality, although this seems to be an inevitable consequence (this is particularly obvious is his assessment of the Copernican model of the solar system, which I shall consider in a moment). Coherence, if we are to understand by it a thoroughgoing unity of systematic mathematical relationships binding all empirical data into a whole governed by universal laws, is neither a goal to be attained, nor a criterion for judging retrospectively whether what we believe to know counts indeed as knowledge[16], but a method guided by a logical principle through which we acquire knowledge in the first place. Cassirer argues at length that judgment does not alter or create reality. The fact that knowledge conforms to a formal principle or rule set by our reason does not entail any kind of arbitrary subjectivity, for it is not a question here of altering the content of knowledge, but only of judging if this or that piece of empirical data has the structure of objective knowledge or not.[17]

Second, Cassirer also argues that knowledge is by no means relative to some criterion of satisfaction external to knowledge, but to a logical principle constitutive of knowledge itself. In this respect, he argues that his "critical view" differs from Dewey's

[16] Cassirer, *Substance and Function*, 303.
[17] Cassirer, *Substance and Function*, 297.

pragmatism in as much as the formal structural elements of knowledge are not determined by subjective needs, but only by "*the universal intellectual postulate of unity and continuity*". Truth is not guided by our subjective feeling of satisfaction towards this or that system of knowledge, but by an objective logical principle (a purely theoretical goal, i.e., a formal unity) through which we are grasping the empirical being in "*the form of rational mathematical order*". It follows from here that knowledge has nothing to do with the fruitfulness of our hypotheses and theories, but merely with the absolute demand stating that what we can grasp must be solely in the form of relation. The demand remains fixed, it is an absolute principle or a fixed "*method of experience*", and only the means of achieving it, the various attempts to achieve objective unity in respect to empirical data change throughout history of science.[18] Thus, the difference between Dewey's pragmatic and Cassirer's critical theory of truth hinges on the difference between "goal of knowledge" vs. "method of knowledge". Usefulness is a goal that can be achieved by various methods among which we can choose according to subjective principles, as there is no explicit rule dictating the choice of method; more so, an obvious element of arbitrariness is present when more than one method can achieve the same result, as in the case of the Copernican and Ptolemaic models of the Solar System, which were equally successful in accommodating the same astronomical observations. In contrast, objective unity is a method, a rule-based activity leaving no place whatsoever to an arbitrary and subjective element. In order to illustrate this distinction, Cassirer reminds us that the Copernican model of the Solar System can be grounded in Newtonian

[18] Cassirer, *Substance and Function*, 318.

mechanics, therefore being in logical agreement with a larger, systematic "whole" of knowledge.[19]

It seems obvious that Cassirer distances himself from coherentism and pragmatism in order to avoid any charge of subjectivism. His argument is very simple and powerful: the knowing subject does impose formal constraints upon knowledge, yet these formal constraints are not to be seen as subjective and arbitrary, but as a very rigorous and objective following of a rule. Cassirer's account of knowledge allows the objective structure of the world as we know it to change over time without letting this change being guided by some subjective principle. Thus, knowledge can be revised and augmented, something which was impossible under Kant's framework of experience, yet this revision does not entail an artificial change operated solely in virtue of our subjective inclination or arbitrary will.

1.3.3 Objective knowledge and experimental science

Contrasted with Poincaré's conventionalism, Cassirer's solution constitutes a more appealing solution to the scientific mind. Nevertheless, I still have to object that his purely constructivist approach, according to which knowing is following of a rule, remains in sharp disagreement with experimental science. According to such an account there can never be an open, explicit clash between theory and experiment. Or such a clash was, to consider an example dear to Cassirer, the very motivation behind the development of relativistic mechanics.

Of course, I am ready to admit that the special theory of relativity is in a sense the synthesis of Newtonian mechanics with the fact that light speed is a constant, as

[19] Cassirer, *Substance and Function*, 320-1.

demonstrated by Lorenz's transformations. Two invariants, two laws of nature are unified, thereby giving us an overall system which closely resembles Einstein's relativistic mechanics. This historical, retrospective account is in perfect agreement with Cassirer's epistemology. There is however another point of view from which we can consider the development of relativistic mechanics. Prior to the solution, there was a problem. Relativistic mechanics is not only an enhanced version of classical mechanics, a wider web of invariants so to speak, but also a reaction to experimental data. Prior to Einstein's theory of relativity, there was an obvious clash between the predicted results of Newtonian mechanics and the actual empirical fact that light speed is finite. Again, I cannot but admit that the empirical fact that light speed is constant acquires significance only in reference to Newtonian mechanics, for it would have made no difference, say, for Aristotelian physics to say that the speed of light is constant or not. In this respect, it seems clear that Cassirer's notion of a separation and reciprocal determination of the objective and subjective within the sphere of knowledge must hit upon some fundamental truth about scientific knowledge.[20] Nevertheless, if the fact that the speed of light is constant acquires scientific significance only in reference to the systematic whole of a Newtonian model of the universe, it still remains an empirical fact that does not fit a Newtonian understanding of the universe. Given this tension between fact and theory, the more general question I am raising now is "How can empirical discoveries that disagree with out current understanding of the universe be objective knowledge?"

[20] Cassirer, *Substance and Function*, 271.

1.3.4 Cassirer's account of error and its inability to encompass experimental falsification

Just as in the case of Poincaré, the deeper problem I am trying to uncover pertains to a collapsing of the Kantian intuition and understanding. Historically speaking, their unification seemed necessary in as much the theory of relativity implies a conceptual remodelling of time and space. Space and time changed with the development of relativistic mechanics meaning that they cannot be fixed, constitutive principles of our faculty of perception as Kant believed.[21] In fact, this discovery entails not only a collapsing of intuition with the understanding, but also a collapsing of the two with reason, for space and time are not only conceptual, but they must also be more akin to Kant's regulative principles of reason then the constitutive concepts of the understanding. As Friedman[22] notes on several occasions, for Cassirer, conceptual requirements are not constitutive, but remain essentially regulative in respect to knowledge. Such a mollifying of the formal structure of knowledge proved indeed necessary. If we adopt a strictly Kantian point of view and begin by determining the a priori conditions that made our present knowledge possible in the first place, we cannot but conclude that these conditions are constitutive of our knowledge and therefore knowledge remains forever enclosed within the framework of these conditions. Since it turns out that this framework changes as science progresses, Cassirer's remodelling of Kant is not unjustified, yet we must be aware that his strategy entails a homogenisation of the Kantian faculties of the mind. Or the moment we conceive knowledge as the product of an essentially unique

[21] Howard, D. "Einstein, Kant and the Origins of Logical Empiricism", in *Logic, Language, and the Structure of Scientific Theories: Proceedings of the Carnap-Reichenbach Centennial, University of Konstanz*, 46.
[22] Friedman, M. *A Parting of the Way: Carnap, Cassirer and Heidegger*, 91-2.

faculty of the mind, it becomes very hard to explain how we can present to ourselves a theory that does not agree with observed reality.

For Cassirer, the presentation of the invariants of experience takes time, and as such it is clear that the constant, formal web of relationships permeating, connecting and unifying objective experience is not presented at once, but gradually.[23] Our mind "discovers" objective reality little by little, gradually incorporating new invariants of experience as science progresses. Thus, to get back to the example of mechanics, it can be said that classical and relativistic mechanics constitute two successive stages of the same process of separating the constant elements of experience from the variable ones. There is no single opposition between the objective, that is to say constant, and the subjective, i.e., variable, elements of experience, but a successive series of oppositions through which knowledge progresses in accordance with the same formal principle of universality driving it. As I remarked earlier, this principle constitutes that which is common to science at all times, that is, to the series successive series of relational structures of the objective. Cassirer argues that error consists in confusing the changing and constant elements of experience such that we end up by attributing a constancy contingent on particular circumstances to the whole of experience.[24] In particular, since there is a distinction to be made between the unchanging method, instrument or principle of knowledge and the series of systematic models of reality it produces, it follows quite clearly that a common, and, I would think, quite inevitable error consists in taking our present knowledge to be the ultimate knowledge of nature. For instance, Newtonian mechanics can be viewed now as relativistic mechanics applied to some special

[23] Cassirer, *Substance and Function*, 323.
[24] Cassirer, *Substance and Function*, 275.

situations, as for example if the velocities considered are significantly smaller than the speed of light, or again, as a model of a universe devoid of immensely massive bodies. Judging things from this point of view, we can say that Newtonian mechanics is an example of a hasty generalisation matching Cassirer's definition of error.

Yet, to get back to my initial complaint, I also note that this account of error works solely retrospectively, as "an after the fact" comparison of current and past knowledge of the universe.[25] In other words, error is represented here as the difference between two consecutive mechanical models of the universe. In contrast, from the standpoint of experimental science, there is also a representation of error which does not involve the contrast of two theoretical constructs.

After Einstein, it is obvious in which way Newtonian mechanics constituted a hasty generalisation. However, when Michelson discovered that the speeds of the light and the earth do not add, what we had in terms of knowledge was the representation of an empirical phenomenon unaccountable by Newtonian mechanics and not the contrast between two theories. In more general terms, I would say that it is perfectly possible at any point in the history of science to have a systematic explanatory model to which are appended a number of clauses limiting the universality of the model, thereby leaving some empirical facts either unexplained or explained by different models. Notoriously, this has been the case for relativistic and quantum mechanics, applying respectively to the "infinitely big" and the "infinitely small"; or, to give a less popular example, of biology, which is still a rather diverse collection of models, explanations and brute facts irreducible to a complete, thoroughgoing systematic unity. We may very well continue to

[25] "*Since we never compare the totality of hypotheses with the naked facts, but can only oppose one hypothetical system of principles with another, more comprehensive and radical [system]...*"; Cassirer, *Substance and Function*, 268.

view science as the result of that formal instrument or principle which Cassirer calls "*fundamental schema of the concept of object*", yet, in light of the abovementioned counterexamples, it seems we must also admit that there is a kind of scientific error which does not oppose two theoretical concepts, but fact and theory, or, to use Kantian terminology, there is a kind of error stemming from a disagreement between intuition and understanding.

1.4 Arguments in favour of keeping perception distinct from theoretical knowledge

1.4.1 Review of the arguments so far presented

In light of the above criticisms of Cassirer's and Poincaré's constitutivist approaches, I argue that empirical reality has its own spatial structure and whether any proposed geometry matches this structure or not can only be determined a posteriori. This agrees with the general distinction between "intuitive" and "theoretical" space drawn by Carnap, Schlick, Russell and, in the end, by Poincaré himself.[26] Such a view, in my opinion the only one compatible with the demands of experimental science, entails a sharp distinction between our faculty to formulate hypotheses, amounting in this example to the construction of various geometrical systems, and that of perception. By this I do not mean to say that geometry itself is derived from experience. Presumably, pure, unapplied geometry is constructed axiomatically and as such it belongs to the realm of the a priori. What is determined experimentally is only which geometry better matches the relative positions and movements of perceived objects. I also do not claim that mere

[26] Poincaré, *Science and Hypothesis*, 52-4.

perceptual observation confers the whole universe an underlying spatial-temporal structure. I think it is quite obvious that perception presents the spatial-temporal relationships between a very limited set of objects.[27] It is the task of physics to propose an overall spatial-temporal structure of the universe which is in agreement with the partial relational structures presented in our perception of objects.

Of course, as presented so far, my position remains a gross simplification abstracting from a series of very important problems. For instance, in order to speak of positions and movements, one has to establish a point of reference, while it is clear that mere perception is usually not enough to distinguish, say, between an inertial and an accelerated system of reference. Nevertheless, in the case of relativistic mechanics, science was able to specify the conditions necessary for experimental verification and these conditions turned out to be fulfilled almost perfectly in some particular, empirically actual situations. My critical analysis of the solutions proposed by Cassirer and Poincaré does not concern their ability to save the Kantian notion of a formal framework of knowledge, but merely their ability to propose solutions that are also in agreement with the actual scientific practice or, to be more precise, capable of satisfying the demands of experimental science. In this respect, my arguments aimed to show that a Kantian-like distinction between "intuition" and "understanding" is a necessary condition for the possibility of experimental science. By appealing to this distinction I hope to provide a viable alternative to the view according to which all observations are theory-laden, a view which most often entails an idealism according to which a scientific theory is not merely true or false about empirical reality, but somehow, once formulated, it constitutes and sometimes even substitutes for the empirical phenomenon whose explanation it is. It

[27] As Poincaré points out, visual space is finite, while the theoretical space underlying physics is infinite.

should be noted that my arguments do not concern the metaphysical status of theoretical entities, and as such they do not directly engage current debates concerning scientific realism and antirealism, but concentrate on the formal structure of knowledge. As indicated in the introduction, my goal is to show that in order to make experimental science possible, knowledge must amount to a predication whereby a theory (the predicate) is shown to be true or false in respect to empirical reality (the subject). This predication should not be automatically interpreted as a logical equivalence such that theory can be substituted to empirical reality (like 'bachelor' can be substituted to 'unmarried man') or as a theoretical constitutiveness whereby theoretical entities are already part of the empirical datum. Substitution entails an impossibility to revise a theory in light of new observations, while an openly Kantian consitituvist approach makes experimental verification impossible.

1.4.2 Back to Kant

Cassirer's model works on the assumption that science never stops progressing, such that error can at any single time be accounted as the contrast or difference between two successive concepts of objective experience; that is to say, between two theories, such as classical and relativistic mechanics. As I remarked earlier, this model of science is not in itself untenable for as long as we consider science from an essentially historical point of view. However, if we choose to consider science from the scientist's point of view, a point of view that can never be a historical, retrospective one, but that of the present-day science as it advances toward a yet unknown and uncertain future, the picture changes. While agreeing with the fact that, thus far, science never came to a complete stop, we have to acknowledge the fact that it has and still is marked by a series of crisis whereby theories fail to match facts. The same comment applies to Poincaré. Science is not always a description of empirical facts by means of some theoretical framework, but is also confronted with situations in which a theory fails to accurately describe a phenomenon.

In order to account for the experimental point of view, we need a second "framework of natural perception" in addition to a theoretical framework. In other words, we must seriously consider a return to Kant's distinction between intuition and understanding; not necessarily a return to Kantian intuition and understanding, but merely a return to a multi-faculty model of the mind. I think this general line of thought is indeed one of the main characteristics of logical empiricism. Although one could always retort that no logical empiricist ever devoted himself specifically to the task of defining a framework of intuition, I think they all worked on the implicit assumption that theory must somehow match perceptually accessible facts. Schlick, for example, was ready to admit that what we know are relationships, yet these relationships are not true in and by themselves, but in respect to empirical reality. They match something which pre-exists and can be observed empirically. Relationships and laws of nature are true about empirical reality in as much there is a "coincidence" between the relational framework of objective knowledge and the empirical manifestation of phenomena in the space-time of our perception.[28] Early Carnap follows a similar idea when he distinguishes between a theoretical and an intuitive space.[29]

[28] Schlick, M *General Theory of Knowledge*, 61.
[29] R. Carnap, *Der Raum*, cited in Howard, "Einstein, Kant and the Origins of Logical Empiricism", 79.

1.4.3 Beyond Kant

With post-Kantian epistemology, it is no longer a question of just distinguishing invariants within experience or of formulating empirical laws on the premise of a geometry, but also of matching these structural elements with something we could broadly view as the "real structure of the universe". Experimental science works in virtue of the same general assumption. There is a perceptual object and it is this object that our mind attempts to match in its mathematical reconstruction of the world. This is not to say that we know what the transcendent, real object is, but only if the reconstructed object science has to offer agrees or disagrees with the observed object. Shortly put, what we know is a theoretical object plus experimental knowledge telling us if the known object is true or false about empirical reality. If true, the object can be said to be constitutive of experience and we can declare ourselves satisfied with constitutivist interpretation of our theoretical assumptions. If not, then observation and theory, the experimental counterparts of the Kantian intuition and understanding, are in disagreement. Falsity and experimental falsification still count as knowledge, for we obviously know how reality is not, yet this empirical knowledge of theoretical error has no place in a constitutivist epistemology.

The main difference between the present account of experimental science and standard Kantian epistemology stems from the fact that Kant considered knowledge to be knowledge only in as much intuition and understanding are in agreement with one another, which is to say, only in as much understanding is true about and "*subsumes the manifold of intuition*" under its concepts.[30] Kantian knowledge is synthetic not only in the sense that concept has to be true in respect to intuition, but, in virtue of being true about

[30] Kant, *Critique of Pure Reason*, B294, B349.

intuition, concept also organises the intuitive content in a specific structure (such as causality, substance and so on). It becomes obvious that when a concept is false about intuition, no such subsuming can occur and, as a consequence, Kant never considered the disagreement between intuition and understanding as being knowledge, although he did conceive of intuitions unsubsumable under the concepts of understanding in his *Critique of Judgment*.[31] Experimental science disagrees on this particular aspect of Kantian epistemology. Experimental falsification still counts as knowledge. We still have a logical predication telling us something about empirical reality even though the synthesis whereby concept organises intuition is impossible. Granted, scientific models of reality are not merely taken to be logically true about empirical reality, but, in order to have any practical value, scientists also assume that reality is organised according to the structure dictated by the said model. Nevertheless, when a model is shown to be false about empirical reality, this also counts as knowledge. We know how reality is not, papers get published on this topic and prestigious prizes are sometimes awarded to people that succeed in showing how things are not. Or the moment we admit falsification as a form of knowledge, we must also admit a separation of intuition and understanding beyond that usually operated by Kant, for now knowledge is no longer a unified conceptual whole, but a set comprising two distinct pieces of information, an empirical datum, and a theory which is not true about that datum. In Kantian terms, we intuit in absence of all conceptual apparatus, which is not to say that concept is absent, but only that it is false, and therefore unable to subsume and organise intuition.

In the case of experimental science, truth follows the synthetic (or transcendental) logic of Kant, hence all the talk about models of reality, while falsity implies a purely

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[31] Kant, I. *Critique of Judgment* (Hackett, 1987; translated, with an introduction by, W. S. Pluhar), Ak385.

logical predication (what Kant considered to be a matter of "general logic") which operates a separation of intuition and understanding proper to the positivist accounts of knowledge. Truth in experimental science implies a space-like mathematical universe in which all elements stand in specific relations not only to the whole, but also with each other, while falsity requires a switch to a set-like logical universe in which all elements stand in a relationship to the set, but not with each other within the set.[32] These two alternative formal assumptions about the universe mirror the two states in which intuition and understanding stand in respect to each other within the knowing subject: either they stand in a synthetic relationship to one another, or they exist separately as two distinct presentations grounded in two distinct faculties of the same subject.[33]

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[32] A logical set {A, B} entails A, B, A and B. In contrast, the subset {1, 2}, entails not only 1, 2, 1 and 2, but also implies the mathematical relationship (the order already constitutive of the set) $1 < 2$ linking 1 and 2.

[33] Although this is a bit too simplistic, I venture to say that we have Kant on one side and logical empiricism on the other, both applying to experimental science, yet neither being entirely right about experimental science, Kant in as much he wants all knowledge to be synthetic, logical empiricism in as much he treats truth and falsity as being equally a question of mere logical predication.

Chapter 2

Mechanisms of perception

2.1 Introduction

In the previous chapter, I argued that in order to satisfy the demands of experimental science we need to distinguish between theory and perception. At this point, the reader may ask himself why theory should refer to perceived reality instead of external reality. As I tried to argue in my critical overview of Cassirer's account of error, in the case of experimental falsification, we must have two distinct sets of conscious presentations of the world, one drawn from empirical observations, the other from theory. It follows from here that reality needs 1) to be presented in our consciousness and 2) presented so independently of any theoretical concerns. The first condition is likewise needed in order for science to consistently refer to empirical reality, a requirement for verification which will be discussed in more detail in Chapter 3. If empirical reality would not amount to a mental presentation, then knowledge of falsity would not be entirely in our heads, but reflect a transcendent relationship linking something in our mind to something in the external world. Knowledge would still be present, although, according to this externalist account, we would never be able to say that a theory is true or false. Since this conclusion doesn't apply to scientific knowledge, I will simply abandon the externalist assumption underlying it.

The question that arises now is whether this presentation of reality constituting what Kant calls empirical, or phenomenal reality, is perfectly equal to and indistinguishable from external reality.[34] Since theory can be false, it follows that our theoretical knowledge is not a priori identical with reality, but can only be shown a posteriori to be true or false in reference to empirical reality. According to this account, theoretical knowledge is not necessarily a faithful picturing of reality as it is in itself; instead, in its more general form, theory is propositional and discursive, consisting in true and false predications. But the referent of scientific knowledge, is it not some faithful picture of external reality? Could we not safely assume that perceived empirical reality is identical with external reality? In as much we are interested exclusively in providing an account of experimental confirmation of scientific knowledge, we can equate perceived reality with external reality. In this particular situation, we are interested in providing an account of the matching of theory and facts through experimentation, and as such we are concerned solely in providing a theory of empirical verification. Nevertheless, current developments in the neuro-psychology of perception show quite clearly that sensuous presentations are the result of complex processing of sensory inputs by various mechanisms of perception. For instance, it is clear that we do not directly perceive objects, but reconstruct them, that we do not passively perceive a tri-dimensional space, but actively calculate depth, etc. Presumably, the perceived appearance of reality is a reliable, and maybe even a faithful picturing of what is really out there, yet, on the other hand, it is also quite clear that there is no immediate identity between perceived and external reality, but only an equality mediated by mechanisms of perception. Accordingly, one argument in favour of distinguishing perceived empirical reality from ontological reality can be stated as follows: if empirical appearance coincides with

[34] I prefer to use the term "external reality" instead of "noumenal reality" in order to avoid any confusion: I agree with Kant that there is something beyond appearance or empirical reality, yet I do not embrace his epistemological anti-realism.

ontological reality, then no place is left for mechanisms of perception; but perception is mediated by mechanisms of perception, therefore such a coincidence is highly implausible.[35]

2.2 Distinguishing between perception and theoretical knowledge at a formal level

2.2.1 Mechanisms of perception as mediators between external and empirical reality

At the beginning of the century, epistemologists became increasingly aware of the fact that perception does not consist in a direct correspondence between reality and conscious presentation, but rather a reconstruction of reality via physiological mechanisms of perception, reconstruction in some ways similar to theoretical constructions mediating our scientific knowledge of the world. The difference between mechanisms of perception and Kant's faculty of intuition responsible for the empirical manifestation of things in space and time is, of course, the difference between a transcendental psychology, considering the problem at a formal level, and an "incarnated", or empirical neuro-psychology, for which perception is not merely a faculty, but a physically implemented processing device.

[35] Searle, J. R. *Mind. A Brief Introduction* (Oxford Univ. Press, 2004), 261-6. A second argument comes from the fact that, if perception is a direct picturing of reality as it is in itself, then theoretical knowledge is superfluous, for it provides only a second-hand grasp of reality. This phenomenological conception according to which scientific knowledge proper is not genuine knowledge, but falls short in respect to the authentic sensuous experience of reality, seems rather unconvincing. As I shall argue in the third section of this chapter, perception reconstructs objects starting from elementary sensations such as colours, positions and shapes via a continuous process of active construction followed by experimental verification. In other words, in addition to passive detection, perception also consists of experimental behaviours testing for a rather limited number of hypotheses, presumably essential for our survival. Scientific knowledge involves a similar testing, but of different constructs and in reference to the partially reconstructed structure of the world effected at the level of perception. If this is the case, then an important segment of the knowledge gained via perception, such as the perception of objects is not in any way more unmediated or authentic than scientific knowledge.

Given this realisation, it became important to specify the status of perception in respect to scientific knowledge on one hand, and that between perception and external reality on the other. Poincaré, for instance, explicitly distinguishes between intuitive space, and theoretical space. Unfortunately, after drawing this very important distinction between perception and theoretical science essential for his refutation of Kant's notion that Euclidian space is constitutive of our perception of reality, he makes no further use of intuitive space; at any rate, he does not attribute to it any role in so far as scientific knowledge is concerned. Even worse, he completely dismisses the possibility that the disparity between geometrical constructs underlying our theoretical knowledge and the relational elements conveyed via our sensuous experience of reality might entail a disagreement between observation and theory.[36] Needless to add, if perception is epistemically inert, then our choice of one geometry rather than another remains a matter of theoretical convention, thus putting an end to any future experimental investigation on this topic.

Cassirer takes a symmetrically opposed strategy. Unlike Poincaré, he incorporates perception into the overall body of scientific knowledge, yet he achieves this integration by downplaying differences between perception and theoretical knowledge. Cassirer sees scientific knowledge as an extension and further universalization of structural elements already picked up by perception. For example, he maintains that Kantian space and time are already invariants of experience. What differentiates these invariants picked up at the level of perception from more theoretical objects, such as the laws and principles of Newtonian mechanics is only a question of degree: Newtonian mechanics picks up more invariants and integrates them into a wider, more universal and more complex system of

[36] Poincaré, *Science and Hypothesis*, 70.

relationships. In this respect, Kantian intuition is nothing but a stage in the series of progressing science towards absolute universality. Cassirer's argument is quite convincing. Even assuming a more modern perspective on perception, it is indeed very difficult to deny the fact that perceived things are constants of experience, for, unless we are ready to admit that we have sensory detectors for each real object, we must conceive perceived objects as the result of primary sensory elements bound together by certain logical and mathematical relationships as dictated by the computational algorithms inherent to our mechanisms of perceptual recognition. Given the fact that perception is already "objective" (i.e., constructed in accordance to some principle of constancy), it seems quite natural to assume a progression toward a more and more universal objectivity moving from primary sensory inputs to perceived things to natural laws governing the interactions between things. Any significant distinction between perceptive and theoretical knowledge is thus abolished and perceived empirical reality becomes an intermediate stage between the initial input of sensuous information hitting our sense organs and the more and more elaborated systems of theoretical knowledge.

2.2.2 Argument against an unbroken continuity between intuition and understanding

As discussed in the previous chapter, for Cassirer, knowledge amounts to a successive incorporation of new invariants of experience into more and more comprehensive relational systems suggesting a progressive view of knowledge. At any given stage, experience is separated into constant and variable aspects. At the next stage, if science is to progress, among that which was considered as variable, new constants are detected and synthetically integrated into a more universal system; and so on, and so forth. The border separating the constant-objective and the variable-subjective aspects of experience is gradually sliding on the side of the objective. For instance, if we take the standard, early modern view of the universe, in as much all things have mass, shape and quantity of motion, it is obvious that they must all obey the laws of classical mechanics. Nevertheless, it is just as clear that not a single thing is ever entirely explained by mechanics. Things have chemical, biological, and psychological properties in addition to mass, bulk and quantity of motion. Although these additional properties do not prevent them from obeying the laws of mechanics, they are responsible for phenomena unaccountable form a strictly mechanical point of view. Thus, we could say that the laws of mechanics capture a universally "mechanical aspect" of reality. I define this aspect as being "horizontally universal" in as much it applies to all things of the universe without completely explaining the total phenomenal manifestation of any single thing in this universe; in contrast, non-mechanical properties are "vertical" and not bound by universal laws.

Or again, to use Cassirer's terminology, the laws of mechanics constitute the invariant relationships, while non-mechanical properties are "subjective" variables that do not fit our current schema of objectivity. Cassirer argues that these variables of experience do not require a special mode of presentation. They are merely that which is left over once we extract the invariable aspect of experience. He considers these leftovers to be the matter or content of knowledge, which he contrasts with the constant relationships constituting the form of knowledge. The idea is that these extra properties unaccounted by the laws of nature serve to empirically individuate things. Sensible properties are phenomenal signs standing for noumenal realities. There is no resemblance between these signs and their corresponding realities, hence their purely subjective status. What is objectively true is the relationship in which they stand, which is the same for signs and noumenal realities.[37] According to this particular interpretation of Helmholtz's sign theory, there cannot be any contradiction between observation and the laws of nature.

To get back to my example, the mechanical aspect of reality is absolutely universal and applies to all things. One thing could be red, another blue, one made of hydrogen and the other of sulphur, but they would all obey to the letter the laws of mechanics.[38] Cassirer does not deny that at one time or another we might subsume these subjective properties under some objective concept, as laws of chemistry for example, thereby shifting the border between objective and subjective. But if this is the case, then this further level of lawfulness of nature will always be in perfect agreement with the laws of mechanics. Unifying various levels of universality gives us more and more thoroughly synthetic relational systems slowly advancing towards a final, unified science of all things, or what I would call an "absolute universality", spanning both the "horizontal" and "vertical" dimensions of empirical reality in which sciences layer one on the top of the other as further additions of laws and principles. In this example, mechanics is at the bottom, for all things are subjected to its laws, then chemistry adds its lot of laws which we must take into account when the bodies studied are not made of the

[37] Cassirer, *Substance and Function*, 288.
[38] This is very similar to Cassirer: "*Space, but not color, is 'a priori' in the sense of the critical theory of knowledge because only it constitutes an invariant of every physical construction*" (*Substance and Function*, 270).

same material, then biology might come into play, assuming that we are concerned with living bodies, and so on and so forth.

Given Cassirer's understanding of the distinction between the subjective and the objective, I assume that it is this kind of smooth, continuous unification that he has in mind when he considers the transition from Newtonian to relativistic mechanics.[39] Under his interpretation, error consists in either appealing to Newtonian mechanics in order to explain non-mechanical properties or in confusing non-mechanical properties as constant and the mechanical ones as variable. What cannot be is an error concerning the "horizontal" universality of mechanics, i.e., that universality in virtue of which Newtonian mechanics must be true about all things despite the fact that it does not explain everything.

I think it becomes quite obvious how this picture is defective. If it were not for the experimental contradiction between empirical reality and the laws of nature, it would have been reasonable to accept Cassirer's account of scientific knowledge without any further ado. But this is not the case. Light does not obey the laws of Newtonian mechanics. Electrons do not obey the laws of Newtonian mechanics. The phenomenal manifestation of light and electrons cannot be mere leftovers, subjective signs analogous to colours and other such seemingly subjective elements of experience. They oppose an objective resistance to the objectively universal laws of classical mechanics. If there is a transition from Newtonian mechanics to relativistic and quantum mechanics, this transition involves a conflict rather than a mere augmenting in universality. It is not a

[39] When Cassirer talks about invariants, it does not follow necessarily that these invariants must be universal invariants. They could be local invariants. However, at some point these invariants reach the status of laws of nature, and thereby that of a thoroughgoing universality, and it is at this point that the objection I am raising becomes relevant.

continuous extension of universality "vertically" through a successive unification of various levels of universality, but, before such extension occurs, universality is challenged "horizontally": not all things of this universe obey the laws of Newtonian mechanics. There is a crisis element involving the realisation that universal laws are not universal, and that not in the sense that they do not explain all empirical manifestation of things (that they are not "vertically" universal), but that not all things are bound to respect them (which is to say that there is no such thing as a "horizontal" universal aspect of reality). Cassirer's division between the constant-objective and variable-subjective elements of experience fails to capture the difference between mere signs individuating the content of knowledge and empirical facts contradicting the assumed objective structure of the universe.

2.2.3 Experimental science is possible only in as much as there is an absolute point of reference to which all scientific theories can refer

This view according to which any significant distinction between perceptive and theoretical knowledge is abolished fails to comply with the demands of experimental falsification. If theoretical knowledge is in direct continuity with perception, it seems reasonable to doubt that theory can ever be in contradiction with facts. On the contrary, if theory is in itself nothing else than a further processing of observed facts according to a rule, it seems that theory cannot but always be confirmed in experience. On Cassirer's account, because there is an unbroken continuity from perception to classical mechanics to contemporary mechanics, it follows that somehow both classical and contemporary mechanics must be true in respect to empirical reality. In other words, we are confronted once again with the view that Newtonian mechanics was never abandoned, but rather upgraded to, or maybe incorporated into a more universal mechanical theory; at any rate, it was never falsified. In sharp contrast with this position, from an experimental point of view, Newtonian mechanics was abandoned not because it failed to explain everything, but because it failed to apply to all things; in other words, it was explicitly falsified.[40]

This difference between Cassirer's "progressivist" account and an experimental point of view becomes more obvious if formulated as follows: for Cassirer, error is something that can only concern two successive stages in the series of objective knowledge, as for instance, classical and relativistic mechanics; in contrast, in an experimental setup, relativistic mechanics does not refer back to Newtonian science, for we are not concerned with their mutual agreement or disagreement, but to empirical data, to which they both refer and in respect to which they agree or disagree. These two points of view are not equivalent. If the universe were different from what it is – the example I gave earlier was that of universe devoid of high-velocity and high-mass bodies – then relativistic mechanics would collapse into Newtonian mechanics. Through a process of mathematical simplification, we would apply relativistic mechanics to a specific case and stumble back upon the laws of mechanics as they are taught in high school. We can adopt Cassirer's retrospective account of scientific error, yet we must keep in mind that this account compares two theories and counterfactually changes empirical reality to provide a unity and means to move from one theory to the next. As I tried to show in the previous chapter, this is not how experimental science works. Experimental science holds

[40] Newtonian mechanics was abandoned not because it was not horizontally and vertically universal, no scientific theory is, but because its horizontal universality was challenged. According to this interpretation, what science struggles to achieve is not a complete horizontal and vertical universality, a total knowledge, but a horizontal universality necessary in order preserve universal laws of nature.

empirical reality fixed. But the moment we take empirical reality as an absolute point of reference, we can plainly see that there is a discontinuity between Newtonian and contemporary mechanics. The experimental discontinuity is actual, while the retrospective, historical continuity is only counterfactual.

I am trying to mark two points here. First, it is obvious that we cannot have a historical point of view for a theory which we do not posses yet, as it must have been the case of physics after Michelson, but preceding Einstein. Since this moment actually existed, science must have also existed as knowledge of disagreement of Newtonian mechanics in respect to empirical data. Second, it is just as obvious that even from a retrospective point of view we do not have a thorough agreement, unity and progressive movement from one concept of objectivity to the next, as it is clear that Newton's and Einstein's mechanics agree only counterfactually, which is to say for alternative empirical realities (and therefore for alternative perceptions of the world). We cannot assume, as Cassirer does, that we have a progression from perceptual objectivity, to a Newtonian concept of objectivity to a relativistic concept of objectivity. Either we keep perception fixed and we experience a conflict and therefore a falsity relationship between Newtonian mechanics and perception, or we alter perception and preserve the continuity between Newtonian and relativistic mechanics. One way or another, knowledge cannot be a continuous growth of truths into more and more comprehensive synthetic models, but knowledge of false predications must also be taken into account.

It becomes clear now that from an experimental point of view perception represents an absolute point of reference to which all subsequent scientific models of the universe must refer back. The agreement, and possible disagreement, concerns not as much every two successive stages in the series of objective knowledge (such a step by step agreement gives us only a counterfactual coherence), but each stage of objective knowledge in respect to that particular stage of objective knowledge called perception. If we accept Cassirer's notion that our natural perception of the world already implies a separation and distinction of invariables and variables of experience, our perception of things turns out to be a stage like any other in the series of the concepts of objectivity. In light of the argument for the existence of perception mechanisms, I think he is right in assuming that perception involves a construction, yet I also argue that this construction is not merely a stage in the series of objective knowledge, but must also serve the role of an absolute point of reference. There might not be any formal necessity in upgrading this particular stage as a point of reference rather than another, yet just by having a fixed point of reference, the formal structure of knowledge changes radically.

2.2.4 Concluding remarks

Conceiving both perception and scientific knowledge as having a similar structure tells us that perception, despite its privileged position as point of reference, is still phenomenal, rather than absolute knowledge. I think this is a very important point, for if we were to admit that perception involves a "seeing" of things as they are in themselves, then further knowledge would not be knowledge, but a mere rearrangement of what we know according to some regulative principles. Further knowledge would be an illusion, maybe a useful illusion, but still an illusion. Second, since perception itself distinguishes and separates invariants form variants within experience, we can conceive a mechanism of perception. This would be impossible if perception were somehow a direct knowledge of noumenal reality. Again, I think this aspect is of some importance. On the other hand, I also think that Cassirer's notion of truth and objectivity must be modified in order to accommodate the demands of experimental science. I think it is just as important to reintroduce a clear-cut distinction between intuition and understanding within the overall structure of knowledge. I argued therefore for the existence of an absolute point of reference within the overall structure of objectivity. This absolute point of reference is the empirical reality we access via our senses and it is to this unique empirical reality that all theories refer. For Cassirer knowledge is arranged in a linear, progressive manner, such that each theory refers back to its previous, less universal version. In contrast, by fixing an absolute point of reference, knowledge becomes a circular structure in which each theory refers back to a single set of empirical facts; continuity between theories is still possible, yet it is by no means necessary.

2.3 Towards a naturalized, science-friendly account of perception

2.3.1 Arguments for a constructivist approach to perception

Following Kant, I believe that empirical reality, the reality we perceive through our senses, is not necessarily identical with external or metaphysical reality. However, I do not endorse an idealist account of intuition (or that which we could roughly equate with perception nowadays). Rather, I believe that if intuition presents an appearance of reality, it is due to the fact that our perception is mediated by mechanisms of perception. Owing to Russell (1956, 1973), Carnap (1928/1967), as well as the contemporary theories of the mind of Fodor (1998) and other adepts of computational models of the mind, I argue for a compositional structure of perception according to which objects are constructed from simpler elements by means of logical and simple mathematical operations.[41] While there is a considerable disagreement as to what should be considered a complex object and what are the simplest elements of which complex objects are constructed, this model of perception assumes: 1) that there are primitive, atomic elements that perception (Russell, Carnap) or thought (Fodor) uses as building material for more complex mental items; 2) that the truth value of these primary elements is granted from the very beginning via a direct, or at least very simple and reliable mechanisms of detection[42]; 3) that complex objects are the result of logical operations on primary elements.

From 2) and 3) it follows that the truth value of complex constructs depends solely on the truth values of the simpler elements and on the nature of the logical operations used to combine the simpler elements into more complex constructs. For instance, if 'brown' is true and 'square' is true, then 'brown and square' is true. Russell makes further use of existential quantifiers in order to yield such statements as "if 'brown' is true and 'square' is true then 'there is an x such that x is brown and x is square'", thereby reformulating our perception of objects on the basis of our perception

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[41] It is in this sense only, namely as a construction of more complex structures from atomic elements according to the rules of a logical/mathematical system, that I understand the use of the term "constructivism". This form of logical constructivism is, of course, very different from radical and social constructivism. For instance, the fact a certain shade of orange is detected and encoded as a certain amount of stimulation of "green", "red", "yellow" and "blue" neurons in the retina does not involve any arbitrary construction of perception. In this particular case, there is a specific wavelength out there capable of stimulating the various neurons in those precise ratios. Concepts are not arbitrarily projected, but rather correspond to a given state of affairs in the external world.

[42] These simple, elementary pieces of sense data might directly correspond to an ontological entities ("*I regard sense-data as not mental, and as being, in fact, part of the actual subject-matter of physics*"; Russell, *Mysticism and Logic*, 149), or, they might represent our first and most elementary point of access to reality (they are "*epistemically primary*" if we choose an autopsychological basis; Carnap, *The Logical Structure of the World*, §54, 61), or again they might be triggered by corresponding external realities (Fodor, *Concepts: Where Cognitive Science Went Wrong*, 19).

of elementary sensations.[43] Carnap offers a similarity metrics method of composition yielding classes of objects which, once established, can be converted into properties individuating objects via what he calls "the method of quasi analysis".[44] Finally, Fodor provides a computational account of inference, such that if we currently entertain the concepts BROWN and SQUARE, we can entertain the concept BROWN SQUARE and this concept will refer to some external reality.[45]

This said, it matters to observe that these authors do not make specific claims about mechanisms of perception. The distinction between the construction of objects operated automatically by perception and the further construction of scientific concepts effected at a theoretical level varies from one author to the next. Their main point of focus is the general problem of construction considered from a purely formal, or at least from a quasi-formal point of view.

In this respect, Russell explicitly states that "*Whenever possible, logical constructions are to be substituted for inferred entities*".[46] Obviously enough, such a reductionism is solely meant as a method useful in solving problems at a formal level. It cannot necessarily be the case that, just because we can reformulate complex objects as logical constructions of simpler elements, complex objects must have been generated by constructing them in perception or thought, or that simple sensuous presentations have a direct ontological counterpart, or again that mechanisms of perception are physical

[43] Russell, B. *Mysticism and Logic* (New York: Longmans, Green & Co., 1914), 175.
[44] Carnap, R. *The Logical Structure of the World* (Los Angeles: Univ. of California Press, 1967), §71, 76-77.
[45] Fodor, J. *Concepts: Where Cognitive Science Went Wrong* (Oxford Univ. Press: Clarendon Press, 1998), 20, footnote 16.
[46] Russell, *Mysticism and Logic*, 155.

implementations of logical algorithms of compositionality.[47] Nevertheless, of extreme interest for a theory of perception is the fact that not only logical structures can be substituted for complex objects, but unless we are committed to the implausible view that we have detectors for each object or kind of object, we must also assume that perception actually constructs complex objects from simpler data elements provided by the detectors present in sense organs.

Carnap confronts us with a more subtle interpretation. In the *Aufbau*, he struggles to maintain a neutral language: complex structures are "*neither 'generated' nor 'recognised', but rather 'constituted'*"[48]. For Carnap, the individuation of types of objects is based on purely syntactic elements.[49] He is not as much concerned in telling us how perception constructs objects starting from sensory inputs, but aims to prove that we can distinguish between perception objects based on their formal structure independently of any "phenomenal properties", such that their formal structure can apply indifferently to perceived qualities, in order to produce perceived objects, or to physical properties, in order to generate physical objects. Abstracting from Carnap's philosophical goals and commitments, this task of assigning each object a syntactic structure is essential for any computational account of perception and thought. In particular, if a logical or logico-mathematical processing is to be operated on mental presentations of objects, this

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[47] Russell, B. "Logical Atomism." In J.H. Murihead, ed., *Contemporary British Philosophy* (London: George Allen & Unwin Ltd., 1956): 357-83, 367.

[48] Carnap, *The Logical Structure of the World*, §5.

[49] Most interestingly, and somewhat perplexing too, Carnap explains at great length how visual presentations can be distinguished from auditory ones on purely syntactic grounds: visual presentations have 5 dimensions, that is they are arrays of 5 variables, while auditory ones have only 3. Carnap, *The Logical Structure of the World*, §86. It seems though Carnap troubled himself too much with such details: our brain cannot distinguish the origin of sensory inputs based only on the formatting of the information received; instead, it relies simply on the fact that visual inputs come from the eyes while auditory ones come from the ears, meaning that if one should amuse to "switch cables" and send auditory inputs to the visual cortex, we would start seeing what we hear.

processing can be operated only in virtue of the syntactic properties of the said presentations. The upshot of this view is that, internal to perception mechanisms, sensory data must be encoded in a symbolic form.

Fodor's informational atomism complements computational models of the mind with a naturalised account of intentionality. For Fodor, lexical concepts are unstructured symbols.[50] They have no components which are semantically interpretable, hence the "atomism", while their content is defined solely by counterfactual causal relationships between the concepts and the real properties to which they refer, hence the "informational" aspect. The two main advantages of this account are its ability to explain intentionality and compositionality. Since each concept is linked via a nomological causal relationship to a real property, a concept of P always refers to P independently of our knowledge and theories about P; thus intentionality is preserved even if we entertain false beliefs about P. This would be the advantage of the "informational" part of the account. Atomism is extremely useful in explaining compositionality. If concepts are unstructured symbols, then they are compatible with computation. For instance, symbols can be combined into complex concepts via Boolean operators. Thus we obtain "NOT-concepts", such as NOT A CAT, "AND-concepts", such as PET FISH etc. Combining the atomist and informational aspects of the account, we obtain concepts which are indicators of real properties of the world combined according to a language of thought which is none other than first order logic (or something closely resembling it).

Although none of these accounts refers explicitly and exclusively to perception, they all include among the most basic elements simple presentations usually associated

[50] Fodor, *Concepts: Where Cognitive Science Went Wrong*. Fodor expounds his arguments for informational semantics in chapters 1 and 4.

with unprocessed or low-level processing of sensory inputs at the level of physical detectors present in our sense organs. Similarly, among the more complex objects constructed from these simpler elements are included common, perceived objects associated with high-level processing of sensory inputs in the brain. Considering the fact that there is gap between the information gathered at the level of sense organs and our perception of the world while keeping in mind that constructivist accounts claim to bridge this gap at a formal level by treating complex, perceptual objects as rule-based combinations of simpler elements, it becomes quite reasonable to postulate the existence of perception mechanisms responsible for operating such combinations. Converting formal constructivist accounts into naturalised models of perception requires only the possibility of physically implementing the algorithms of composition; I think that in as much we are concerned here only with simple logical and arithmetical operations, this is perfectly possible given our current knowledge of the nervous system.

We can still speak of a language of perception describing external reality as syntactic constructions of primary sensory data elements. This possibility is granted by the fact that the same algorithm could be implemented in more than one physical system, such as a computer program emulating perception. However, in light of my criticism of Cassirer's progressive model of knowledge, I also argue that in order to allow for the possibility of experimental science, there cannot be an unbroken continuity between the language of perception and any further theoretical language describing empirical reality in terms of necessary substructures of objects or vast systems governed by universal laws. This limiting clause does not really hinder any of the three combinatorial accounts presented so far, and that for the very simple reason that, except for some very sketchy extrapolations, they all start with relatively simple sensuous data and end with objects as perceived by us independently of our scientific education. Thus, although the claim would be that all knowledge can be ultimately accounted as a progressive construction towards more and more complex objects, what is actually offered in any significant detail is mainly a possible construction of common objects, which is to say an account of the processing operated at the level of perception.

2.3.2 Arguments against a purely combinatorial model of perception

The main advantage of purely combinatorial accounts of perception resides in their ability to preserve a straightforward correspondence between the input fed into the perceptive system and the conscious output. Based on an interpretation by Friedman, I believe that these accounts are grounded in the combinatorial logic of Wittgenstein's *Tractatus*. Once we agree upon a set of elements and their respective truth values, we can a priori determine the truth values of all possible logical combinations of these elements.[51] Friedman appeals to the *Tractatus* in order explain the notion of syntax, which plays a very important role in Carnap's philosophy. Since among the logical combinations of atomic elements there are some that are tautologies, i.e., combinations which are true independently of the truth value of the elements, there must be purely analytic truths. Friedman argues that under the influence of Hilbert and his axiomatization project, Carnap altered this view, trying to show that there are analytic truths relative to any given language, where by language we are to understand various logical and mathematical systems. While I will make use of this observation in the next chapter, the main focus of this chapter remains the general structure of perception.

[51] Friedman, *Reconsidering Logical Positivism*, 177, 185-6.

Because this structure is based on a combinatorial model, it corresponds to what I like to call an "input-only constrained" processing unit. By this I mean to say that the only empirical constraint on the system is at the level of the input. Once the truth values of the primary elements is determined, the truth value of complex objects, inferences and statements follows from the mere logical relationship between the primary elements and does not require any further confrontation with a reality external to the system.

Since most of the examples considered in *Concepts* are constructed via simple logical operations such as negation or conjunction, I think it is quite clear how this applies to Fodor. Russell makes use of more sophisticated logic and Carnap combines logic with basic arithmetic, yet I think it is still true that we only need to determine the truth values of atomic propositions and the numeric values of similarity relationships, while the rest is automatically entailed in virtue of subsequent computation. Cassirer follows a somewhat different pattern. For him, objective knowledge consists in constant relationships of any kind. However, it is still the case that knowledge is an activity according to a rule, namely the fundamental principle or instrument of knowledge in virtue of which the constant, objective elements of experience are progressively separated from the variable, subjective ones. The truthfulness, or, to use his own words, the "objectivity" of subsequent knowledge stems from the fact that it is constructed according to a rule. Nothing is arbitrarily invented, but merely further processed. Presumably, there is an absolute starting point preceding all processing according to the fundamental rule of knowledge, and there might be an absolute ending point, when the application of the rule cannot yield any further knowledge (i.e., any further invariables). If this picture approximates to an acceptable degree Cassirer's thought, then it becomes clear that knowledge consists in distinguishing invariables – perceptual objects in the beginning, then regularities and universal laws of nature – within some primary flux of sensations. In other words, knowledge is a huge construction whose only point of contact with reality is an initial given consisting in experience preceding any separation of constant and variable elements.

In more general terms, I like to draw a parallel with computer science. In all these accounts, it seems that perception, as well as thought, science and knowledge in general, amount to the logical processing of an input. If this is the case, then the output will always be true in respect to the input. For instance, as remarked above, if the input consist of impulses A and B or of symbols 'brown' and 'square', then any computational processing consisting of Boolean operations will yield outputs which are necessarily correct given the input (i.e., yield 'brown table' if 'brown' is true and 'table' is true, or not yield it if one or both of the elements is missing). We can conclude therefore that in as much scientific theory says something true about perceived objects, scientific theory must also say something true about the simpler sensuous elements from which objects are constructed. If we further assume a perfect, or at least a reliable correspondence between primary sensuous presentations and external reality, then we can reasonably assume that in as much as scientific theory is true about perceived empirical reality, scientific theory is also true about external reality responsible for the input fed to our sense organs.[52]

Although this straightforward model seems to simplify matters to a certain degree, I think it is incomplete as it stands. First, it is incompatible with the fact that perception is

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[52] This last observation does not apply to Cassirer. For Cassirer, only the constant elements of experience have a direct correspondence with metaphysical reality (*Substance and Function*, 304-5). Presumably, this is due to the fact that primary experience, in which invariants are not separated from variables, is purely subjective (in this case, objective experience amounts to zero, as there are no constants yet).

not purely passive, but has a distinctive experimental dimension attached to it. For instance, Poincaré argues that our geometrical notions are derived from group transformations in visual space.[53] Poincaré is very careful in specifying that our concepts of geometry do not stem from a direct detection of the relationships between sensations in our visual field.[54] Instead, we extract some basic principles of Euclidian geometry, as well as the general notion of a Euclidian space by studying the reciprocal relationships between the motion of things in our visual space and the motion of our eyes and bodies. Similarly, in order to determine if an object is in front of another object, perception needs to compute the change in the apparent positions of the two objects as the angle of vision changes. In both examples perception not only constructs objects or structures according to a set of rules, but also verifies them back in experience. Presumably, the group transformations postulated by Poincaré are valid in the case of motions with which we are concerned in our everyday life. If perception were to compute group transformations associated with the motion of celestial bodies, or, in general, in the case of motion on long distances in non-inertial systems of reference, it would not come to the same results and therefore would not necessarily present objects in a Euclidian-like space. The same goes for depth perception: before presenting an object as being situated in front, behind or at the same distance from the observer, it has to confirm the expected parallax deviations corresponding to each case.[55]

The second reason for rejecting a purely combinatorial account of perception stems from the fact that, should objects constructed in perception be always true in

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[53] Poincaré, *Science and Hypothesis*, 63.
[54] Poincaré, *Science and Hypothesis*, 59.
[55] Of course, in computing the parallax, perception works on the presumption of a Euclidian space. This is not problematic in as much depth perception is concerned with everyday life situations.

respect to the perceptual input, alternative theoretical reconstructions of the perceptual input would be entirely superfluous. In saying this I have in mind the notorious "bent spoon in a glass of water" illusion cases. Something is bent, namely the light rays traveling to the observer's eyes, yet perception associates the bent not with the medium transmitting information, but with the object about which light transmits information. In a small number of cases, scientific theory is true in respect to simpler sensuous elements, such as the pixel-by-pixel retinal image of a bent spoon in a glass filled with water, but not with the more complex perceptual presentation of an object which is bent. For whatever reason, perception does not automatically confirm the constructed object, such that it is only after conducting the willed experiment of removing and putting back the spoon in the glass that we realise that the spoon must have been straight all along.

Aside these general difficulties associated with purely compositional accounts of perception, there are also much more serious difficulties associated with a compositional account in which the rules of composition reduce to logical operations. For instance, if we consider the case of a white sheet of paper lying on a brown table, it becomes obvious that the conjunction 'white' AND 'brown' AND 'table' is true despite the fact that perception does not refer to a white and brown table, but to a white something on a brown table. The only way to distinguish between a white and brown table and a white sheet lying on a brown table is by conducting an experiment, for instance, by trying to physically separate the white part form the brown part. Even more striking, 'brown' AND 'table' refers to a table which is brown not to a brown which is table. The use of existential quantifiers alleviates the problem, but does not explain why we associate table with the subject and brown with the predicate. In this case, the subject/substrate is determined experimentally, by changing the colour of the table. Given the gap between purely logical constructions and perceived objects, our carving of reality into the right objects must be a process subjected to trial and error. Presumably, perception is usually right because it must have been subjected to the negative pressure of natural selection and also because it automatically makes use of past experience (as for example, of the fact that the white spot changes position here and now while the brown spot remains is the same position as the square shape of the table).

If we further assume that perception is implemented by a set of independent modules, each responsible for processing the same input in order to produce a specific presentation, the situation becomes even more complicated. For example, given our current knowledge of visual perception, we can assume that the same visual input, that is, light conveying information about external world, is simultaneously processed along independent perception pathways according to specific algorithms in order to produce a set of parallel primary presentations, one pertaining to colour, another to shape, movement, position, depth and so on. All these outputs refer back via the computational processing of their respective cognitive pathways to exactly the same input, thereby each is saying (or predicating) something different about the same subject. The system seems to emulate consciousness at a purely computational level, such there is no need for a final synthesis of these perceptual elements into an output presentation of the object. However, once we try to physically implement such a system, we realise that it is not at all necessary that all presentations are produced simultaneously. It could be the case that computing depth takes longer than computing colour. The two presentations would then be asynchronous, meaning that in the supposed consciousness of the perceptive device, colour presentation of moment t_0 is simultaneous with depth presentation of moment t_1, thereby resulting in a cacophony of presentations referring to different moments in real-time and ultimately to different realities. Presumably, perception cannot know a priori how long it takes to process the various presentations (although we can know this from an external point of view, by counting the number of operations required by each process and designing a synchronizing device analogous to the "clock-speed" of microprocessors), but can only determine that a posteriori, by attributing an object to various combinations of these presentations and test it in experience. Once again, it seems that object perception must be closely linked to object learning and object recognition if an organism is to adapt to its external environment.

2.3.3 Arguments for an experimental model of perception

A straightforward compositional account of perception according to which objects are produced by combining simpler perceptual presentations via logical operations is particularly attractive because it is relatively easy to implement both at formal and physical levels. It offers an uncomplicated and quite accessible way of providing a naturalised account of object perception and in this sense it constitutes a viable option. However, given the complications discussed above, I argue that mere combinatorial composition of elementary sensory presentations is not enough to account for the objects we actually perceive.[56] In order for such a model of perception to work, we must further

[56] I am considering here algorithmic models of perception. An important part of the research in cognitive psychology involves the study of input-output systems, as for example, the ability of subjects to detect causal regularities given a sequence of inputs. The relationships between inputs and outputs can modeled by algorithms, which in some cases are very similar for both human and animal subjects. This strongly indicates that there are mechanisms at work responsible for the processing of the inputs. Whether these mechanisms are physically implemented as symbolic computational devices or as neural (connectionist)

suppose a testing mechanism selecting the right combinations among the set of all possible logical combinations. One such mechanism might be natural selection. Organisms that fail to reconstruct real objects in the normal conditions of their environment are condemned to mass extinction; as a consequence, it can be argued that only organisms endowed of mechanisms of perception combining into objects the right elementary presentations have survived. Another alternative is learning: there seem to be mechanisms automatically confronting visually constructed objects to tactile and motor information; more so, our perception of objects is never constructed from instantaneous sensory inputs, but requires the processing of inputs collected over intervals of time.

From a more formal point of view, the model proposed here differs from the models of Russell and Carnap in as much it conceives perception as a dual, input-output constraint system. According to this model, perception is not reduced to passive detection, the equivalent of a "one-shot picturing" of reality as some empiricists seem to imply, but a continuous analysis of sensory inputs over longer periods of time. Such a system, although built on truth-preserving logical operations, remains nevertheless reliable and not infallible. One consequence of this conclusion is that external reality remains distinct from empirical reality despite the fact that there is, most of the time, a practically perfect correlation between the two. As discussed earlier, this is important in as much it leaves place for mechanisms of perception and allows for alternative, theoretical reconstructions in those cases in which object perception is subjected to illusion.

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networks is, of course, open to debate. It is worth noting that in order to allow for a comparison of perceptual and theoretical reconstructions of reality, symbolic computational devices should be favoured.

A second consequence of some importance is the relativization of our ontological commitments. On one hand, it seems that most of us are committed to the existence of the things we perceive; at any rate, it is at least true that we function at a biological level on the unconscious assumption that what we see is real. On the other hand, building an ontology based on a purely combinatorial model of perception is often problematic. For example, it must be the case that primary sensations refer to an actual state of affairs of external reality, yet it seems implausible to assume that external reality is made of entities corresponding to colours, shapes, sound and other such data elements. Another problem pertains to a conflict between an ontology based on elementary presentations and one based on complex objects constructed from these presentations. More so, if we abide to the view that object perception constitutes an automatic extension of primary sensations, itself an extension of inputs detected by relatively simple detectors, then perception constitutes a single, unbroken causal event going from external reality to perceived objects. The move from elementary sensations to complex objects is not justified by an additional influx of information, but constitutes a further processing of the information provided by the same initial sensory input. This is problematic since it becomes impossible for us to distinguish between an ontology based on primary sensory presentations and an ontology based on perceived objects: the only way to pacify the two seems to require a commitment to the view that the realities corresponding to primary sensory presentations compose at an ontological level in order to produce of the realities corresponding to perceived objects.

An experimental model of perception displays an increased flexibility allowing for revisable and more plausible ontologies. In light of the arguments presented in the previous section, I proposed a modified model for perception which makes use of the computational notions of "parallel processing" and "input-output constrained processing systems" in addition to the standard constructivist assumption. The thesis of parallel processing, inspired directly from research in the field of visual perception and related to the concept of modularity as developed by Fodor (2000) and Cosmides and Tooby (1994), states that the same perceptual input, for example, light hitting the retina, is processed in parallel via different neural pathways in order to produce various visual presentations such as colour, shape, movement and so on. Most probably, external reality is organised in objects, yet what we "see" at the most basic level are not objects, but only a set of primary sensations referring back via the mechanisms of perception to one and the same input corresponding to a "snapshot" of whatever happens to fall into our visual field at any given moment in time. At the level of passive detection, reference is not oriented towards individual properties, but towards sets of properties of particular chunks of space at a given time or interval of time. Thus, an ontology inspired from a parallel-processing model of perception differs greatly from an ontology based on a standard constructivist model. Although it is true that primary sensations remain the simplest, most elementary mental items, this does not suggest in any way that the world consists in simple realties corresponding to colours, shapes, movements etc. Rather, the ontological commitment inherent to this level of perception is a commitment to the existence of time-space continuums endowed with the phenomenal properties ascribed to it by our primary sensory presentations. In other words, we are simply asked to believe that external reality is extended in space-time and that different "chunks" of space-time may display different physical properties. To give an example, the set of primary presentations changes as we move our head from right to left. Presumably, this is due to the fact that external reality is not the same at all points in space; given the currently accepted knowledge of the universe, this does not seem an implausible ontological commitment.

Beyond this very elementary level of presentation, perception has the task of reconstructing the objects of reality starting form primary sensations. This reconstruction involves a combinatorial process as described by Russell, Carnap and Fodor, yet, although all combinations are possible and valid in respect to the logical operations performed by perception processing onto primary sensations, not all complex objects thus obtained refer to something actually existing in reality. Logical processing of primary sensations produces a set of possible complex objects (a disjunctive set of all possibilities), out of which only some are verified through trial and error either at the level of the organism, or that of the species.[57] A correspondence with external reality is required not only at the level of the sensory input, but the complex, output presentations obtained via the processing of the input are also tested in order to verify if they correspond to a state of affairs in external reality. Once again, I think this conclusion is of some relevance in developing an ontology. The grouping together of primary sensations into objects requires an additional input of information; this is not the case of the standard, input only constrained model of perception. But if more information is injected in the system, it is not unreasonable to assume that perception revises our ontological commitments in order to accommodate newly gained information about external reality.

[57] As discussed above, this is obvious in the case of depth perception (we need to move our head to the right or left in order to calculate the parallax and thereby determine if an object is in front or behind another object; the same is achieved by perception by a continuous, involuntary movement of the eyeballs). According to this model, it can be argued that the evolution of perception does not consist merely in the selection of various detection mechanisms, but also in the selection of the right algorithms of composition and the right experimental behaviour required for confirmation of reconstructed objects.

External reality retains the status of an unknown x that perception reconstitutes given the information to which it has access.[58] Furthermore, because information is acquired in two distinct steps, one presenting the distribution of detected properties in space, the other testing a posteriori the grouping of these properties into objects, we can speak of a mapping of ontological objects on the initial framework of elementary sensations distributed in a space-time continuum. The final ontological commitment towards an existence of physical objects does not conflict with the initial ontology of a non-homogenous distribution of properties in space. Quite on the contrary, it seems that the two add together, giving us properties attached to objects extended in space-time: given the lack or presence of further information, we may remove or add objects, yet, with or without underlying objects, properties still display exactly the same distribution in space-time.

Finally, of particular interest for my overall account of experimental knowledge is the fact that an experimental model treats perception as an explicitly "closed", functional "as is" system, while a standard combinatorial model presents it as a potentially "open" system. The distinction hinges on the fact that in an experimental model, perception constructs objects and verifies them in experience; the process ends here, for in order to be truthful, any further construction would require a further verification. In contrast, in a purely combinatorial model, truth is achieved at any given level of the construction process, hence the lack of any evident limit as to how far perception should go with the

[58] We know external reality in as much as it appears to us as empirical reality and in as much as our theories are true about empirical reality. In principle, we can always test new theories and therefore we can always gain more knowledge.

processing of the initial input.[59] This distinction between "open" and "closed" may seem a small detail, yet I think it's not without some importance. In the context of a "closed" system it makes sense to talk about further theoretical constructions which are distinct from previous perception-based constructions. What is of particular interest is the fact that these further constructions are distinct, meaning that they may operate on the grounds of different sets of rules than those of perception. In light of this observation, we can view perception as a particular language by the means of which we succeed in providing a true reconstruction of external reality; beyond perception, scientific knowledge succeeds in saying something true about perceived reality using its own, potentially distinct language. We don't need to worry whether the language of perception reduces to the language of science or vice versa. In contrast, because it admits a never-ending construction yielding more and more complex objects – although this complexity is more like that of fractals, that is, a complexity obtained by the never ending repeating of the same motif at various levels of construction – purely combinatorial models usually assume an unbroken continuity between the language of perception and that of science and therefore an a priori agreement between the reconstruction of reality operated at the level of perception and the theoretical reconstruction hypothesised by science. Thus, when the bottom line is drawn, an experimental account of perception seems to be in better agreement, or at least is more apt to suggest the possibility of experimental falsification than a purely combinatorial one.

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[59] Cassirer explicitly states that the series of objective knowledge is a never-ending series (*Substance and Function*, 269). The same is particularly obvious in the case of Carnap, whose model allows a never-ending construction of types, types of types, types of types of types, etc.

Chapter 3

Experimental verification of scientific theories

3.1 Introduction

In Chapter 1, I argued for the necessity of introducing two sets of mental presentations in order to account for experimental falsification. One kind of presentations is derived from perception, the other stems from our ability to formulate hypotheses. An agreement between these two mental items amounts to scientific truth, a disagreement, to falsity. Chapter 2 further explored the nature of perceptually derived presentations. My two main conclusions were 1) that a constructivist approach is best suited for accounting for the fact that we do not have detectors specialised for each individual entity or type of entity existing an ontological level, and 2) that a construction based on logical and simple arithmetical operations is not enough to account for our perception of objects, hence the necessity of harnessing the process of construction with a process of verification, both implemented mechanically at the level of perception.

Thus far, I have proposed an account according to which perception reliably presents an input coming from external reality as objects and events constituting empirical reality. In this chapter I will focus on the relationship between theory and empirical reality. For the purposes of providing an account of experimental science, I don't have to embrace the view that theories are derived from experience. All that experimental science requires is that theories are confronted against empirical reality via experimentation. Accordingly, my account does not involve any explicit or implicit commitments to a traditional empiricist epistemology, but merely a verificationist theory of truth. Theories can be constructed a priori, derived from experience via "association", "abstraction", "habit" or any other such empiricist devices, or can even come from above through divine revelation. All that matters for experimental science is that theory can agree or disagree with facts.

This said, I don't believe that the above presented options are equally plausible. On one hand, it makes more sense to harness verificationism to a "spontaneous" faculty capable of constructing theories. If theories were indeed induced from empirical observation in such a way that they followed necessarily from the observed facts, I don't see the need of verifying them back in experience. I presume verification is necessary only in as much it is not impossible for us to come with false theories, in which case induction must incorporate something foreign to empirical reality. On the other, to say that we just come up with theories from nowhere is equally unsatisfying. In lack of a better alternative, I think that the most plausible choice is the proposal that we construct theories, yet this construction does not simply mirror or imitate structural elements empirically accessible. All questions are not answered, but, if asked, I can at least say that we don't just invent theories on the spot, but construct them much like we construct formal languages and logico-mathematical systems, while verification and falsification are absolutely required because these theoretical constructions may or may not emulate specific aspects of the construction effected at the level of perception. This minimal amount of information conveyed by a constructivist approach allows a discussion about how facts and theory fit together beyond the mere statement that they must somehow "agree" or "disagree".

3.2 Science as knowledge of empirical reality

3.2.1 Knowledge outside the agreement or disagreement of facts and theory

Kant defines truth as an agreement between concept and intuition.[60] Although we are concerned with the agreement between two mental items, we must not forget that for Kant intuition amounts to phenomenal reality, such it is question here of harmonizing concepts and empirical reality. What severely cripples Kant's account is the fact that, according to his theory of schematisation, this agreement takes place a priori, thus forfeiting the possibility of falsification and experimentation in general. With the verificationist thesis, we don't speak only of an agreement, but also of a possible agreement setting the limits of scientific knowledge. Even more importantly, we speak of a possible disagreement between theory and facts, which gives us the philosophical tools needed in order to account for experimental falsification. Knowledge is defined now as a possible agreement or disagreement between theory and facts. Unfortunately, these upgrades come pre-packed with a number of considerations about the meaningfulness of statements and the absolute limits of knowledge in general which seem to be in direct continuity with Kant's view that there is no knowledge outside the agreement between concept and intuition. For most verificationists, reference within the domain of possible scientific knowledge is meaningful; reference that transcends this domain is meaningless. One way to formulate this distinction is to claim that meaning is testability. However, by setting such narrow constraints on knowledge and meaning, verificationism makes it

[60] For Kant, truth and error are the agreement or disagreement between cognition in respect to the laws of the understanding (*Critique of Pure Reason*, B350). The upshot of this view is that truth requires that intuition is presented in accordance to the rules or concepts of the understanding. On its own, understanding is by definition in accordance with its own laws; similarly, intuition, if taken separately, has nothing to do with truth and error, for it doesn't agree or disagree with anything. Only when intuition and understanding meet in judgments of experience can we speak of truth and error.

impossible to insert mechanisms of perception in between external reality and empirical reality. Consequently, the difficult task of this section is to argue for a relaxed version of the principle of verification capable of accommodating a mechanistic account of perception.

3.2.2 Arguments for a more neutral formulation of the principle of verification

I have argued previously that science does not "get metaphysical", as Poincaré seems to imply. Just as a quick reminder, Poincaré argues that we cannot distinguish between a universe in which geometry is the study of the laws according to which invariable solids move and a universe in which geometry is the study of the laws of motion of solids deformed by the differences of temperature.[61] The idea here is that there is an extra term we must take into account in our equations, namely temperature, yet because the universe is built in such a way that we can never empirically observe a correlation between geometry and temperature, we will never be able to establish the true, metaphysically valid structure of the universe. In Poincaré's setup, there is a knowledge of the form G_1P_1 referring to reality R_1, namely a reality that has the underlying structure of Euclidian geometry, and there is another knowledge of the form G_2P_2 that refers to reality R_2 that has the structure of a non-Euclidian geometry. In some special conditions $G_1P_1 = G_2P_2$ (they are indistinguishable), therefore we don't know if our knowledge refers to R_1 or R_2.

I don't know if science should worry about these metaphysical complications. What I know as a fact is that it doesn't. To put it in experimental terms, "Should there be an observable dependence of motion on temperature, we would change the laws of

[61] Poincaré, *Science and Hypothesis*, 66.

motion and our geometrical commitments accordingly; there isn't, therefore we don't". In other words, science is not concerned with Poincaré's scenario whereby G_1P_1 refers to R_1 and G_2P_2 refers to R_2, but only with a holistic setup in which knowledge is composed of two theories G and P, such that, facing some experimental inconsistency, we have the choice to alter either G to G' or P to P' in order to make our knowledge agree with the facts. However, whether true or false, both G'P and GP' refer to the same empirical reality and not to two alternative realities.

This does not mean that we cannot or should not think of alternative structures of the universe, but only that the referent of our knowledge is different. Poincaré sees knowledge as knowledge of something which absolutely transcends the mind. Science takes matters more practically, and makes of itself knowledge of that which we "see", that is, of something which is already presented in our consciousness at the level of perception. It is not concerned with absent phenomena unless those phenomena should occur given our current models, theories and general knowledge of nature, in which case there is an experimental disagreement between theory and facts. As pointed earlier, the two scenarios are different in as much a theory aiming to explain a non-occurring phenomenon is explicitly directed towards an alternative reality, i.e., something which is not presented at the level of perception, but rather in our imagination, while a theory that predicts phenomena that should, but don't occur is a false theory about empirical reality.

Accepting as a matter of fact that science is concerned only with empirical reality is enough to bypass such metaphysical complications. It is solely a question here of understanding what science is a knowledge of. On the other hand, it is just as important to realise that a matter of fact is not a necessary reason. Most verificationists jump to the other extreme and make out of a mere fact a question of meaningfulness, which they tie to an even more bizarre conclusion, whereby "what science is" becomes "what knowledge must be".[62] In Ayer's standard formulation, the principle of verification is as follows: "*We say that a sentence is factually significant to any given person, if and only if, he knows how to verify the proposition which it purports to express—that is, if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false*".[63] In other words, meaning is testability, where testability is defined by the possible extent of our scientific knowledge (that is, the sum total of possible true and false statements about empirical reality).

I am well aware that most verificationists like to challenge bigger, yet more vulnerable fish, such as God and faith. Unfortunately, their arguments also hit less glamorous, but considerably harder to dismiss metaphysical complications such as those raised by Poincaré. The problem is that I don't see a scenario in which we change reference but keep knowledge identical any less or more meaningful than a scenario in which we keep reference fixed and alter knowledge. Given this dilemma, the solution I proposed is to take it as a fact that science has one fixed referent and therefore constitutes a body of knowledge telling us something about one thing, namely the empirical reality amidst which we live our lives. According to this account, mentioning God or, more relevantly, mentioning temperature-dependent laws of motion in the middle of a discussion about mechanics while failing to understand that we do not always refer to and therefore talk about the same underlying reality renders the discussion meaningless. In order to make sense of it, we need to refer each set of laws of motion to empirical reality

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[62] Carnap, R. *The Unity of Science* (London: Kegan Paul, 1934), 50-1. Carnap, R. *Pseudoproblems in Philosophy* (Los Angeles: Univ. of California Press, 1967), §7.
[63] Ayer, A. J. *Language Truth and Logic* (London: Gollancz, 1936), 17.

and, respectively, to a an alternate state of empirical reality in which all bodies have the same coefficient of dilation and move across a temperature gradient.[64]

I agree with the verificationist thesis that knowledge must have an absolute point of reference, yet I do not see why the uniqueness of the point of reference should entail a uniqueness of knowledge. Presumably, whether the point of reference is metaphysical reality, empirical reality or alternate, fictional versions of empirical reality, the formal structure of knowledge remains unchanged: knowledge is knowledge of something. Different kinds of knowledge have different points of reference, although it is absolutely true that each kind of knowledge has one and only one point of reference. According to this relaxed conception of knowledge, meaning must be relative to a particular theory-referent knowledge-like structure.

In order to conclude that only knowledge of empirical reality is knowledge, the verificationists make the further assumption that empirical reality is the one and only possible reality. Taken as an absolute truth rather than a practical constraint, this assumption is problematic. The fact that the only reality against which we can actually test our beliefs is empirical reality has nothing necessary about it. Things could have been otherwise. For instance, it seems reasonable to assume that should our perception be different, should evolution ever alter our perception, or again, should we ever be able to alter our perception or get rid of it altogether, the point of reference for scientific knowledge would change. Should any of these ever happen, on a standard verificationist account, mixing knowledge before and after the alteration of perception would still yield

[64] I employed a similar strategy when I argued against Cassirer that Newtonian mechanics coheres with relativistic mechanics only counterfactually, that is, by assuming alternate structures of the universe. Because only one of these two alternate structures matches observed reality, relativistic mechanics is meaningful as a true theory about empirical reality while Newtonian mechanics is meaningful as a false theory about (the same) empirical reality.

meaningful discourses. According to a relativized account, meaningfulness would be preserved only if we are provided with rules of translation from one point of reference to the other. I think this reflects the commonsensical idea that unless we understand how our perception is altered, lots of meaningless disagreements will follow. To give a quick example, instruments of measurement and observation are useful only in as much they present empirically detectable changes. The instrument itself is a mechanism linking to some external reality, yet what we see, is still something accessible to our perception: the mechanism of the instrument is the equivalent of the mechanism of perception, while the detectable change is the equivalent of a sensory presentation. However, because two mechanisms map onto the same sensory presentation, one needs rules of translation in order to refer to the right reality. The layman, unaware of the mechanism of a barometer, refers to a black pin slowly moving left or right; in contrast, the scientist refers via the mechanism of the barometer to atmospheric pressure rising or dropping. If we were to follow a dispute between the layman and the scientist where each of them sticks to what he "sees", we would find their disagreement meaningless and that for the very simple reason that this disagreement has no common object of dispute.

Conversely, according to a relativized account, both perception and science are knowledge, although they are not explicitly knowledge of the same realities, and therefore are not explicitly parts of the same body of knowledge.[65] Of course, by

[65] It should be noted though that perception refers to external reality twice, once via the mechanisms of perception linking the initial sensory input to primary sensory presentations, and a second time via the experimental behaviour verifying the reconstructed object. I do not explicitly propose a similar account for theoretical knowledge, but prefer to keep silence as to what the initial input (if any) of our faculty of formulating hypotheses might be. What I know, is that theory says something about perceived objects; saying that theory is also derived from empirical reality would require me to give examples of "hypothesis-formulation mechanisms", i.e., some combinatorial process through which objects are linked into lawful structures later verified experimentally. While it is true that science consists quite often in a testing of relationships between objects, relations which are sometimes purely statistical (meaning that, strictly

postulating the existence of mechanisms of perception, a unity, or rather a telescopic continuity of the referent is also postulated (i.e., empirical reality is in continuity with external reality via physically implemented mechanisms of perception), such that, ultimately, both perception and theory refer to external reality. Nevertheless, I think it is important that perception is allowed to remain knowledge independently of any theoretical considerations. The moment we deny perception the status of knowledge, we reduce it to a mere set of presentations. But if perception amounts to presentations which are not explicitly presentations of something, then there must be something essentially arbitrary about it, for in this case perception doesn't have to agree with anything, but simply to produce presentations. This seems absurd. Even in the absence of a theoretical apparatus, animals know something. They may not know much about empirical reality, but they must know something about external reality via mechanisms of perception. The same goes for us. In as much I remove my hand from a hot stove before I can even formulate the thought "My hand rests onto a hot stove", mere perceptions of things must amount to some form of knowledge.

3.2.3 Arguments for keeping empirical reality distinct from external reality without completely disconnecting one from the another

I start from the assumption that science aims to explain that which we perceive. In other words, I assume, following Kant, Duhem, Einstein, Carnap, as well as the general consensus of the scientific community, that scientific knowledge refers to, aims to explain and has no other object of study than empirical reality. In Kantian language, this

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speaking, they are all one and the same kind of relationship, namely statistical), I still hesitate to claim that we are endowed with a limited set of relationships we can possibly imagine, and therefore test, as is the case of perception.

can be translated roughly as "scientific concept is predicated of intuition, not noumenal reality". Thus, I endorse the Kantian notion of truth as an "agreement" between intuition and understanding.[66]

This thesis may or may not entail an idealist/coherentist account of knowledge, depending on whether empirical reality is or is not connected to external reality. For Kant, the gap between empirical and metaphysical reality is unfathomable, hence his idealist position: truth consists in an agreement between concept and intuition, yet because both intuition and concept are ultimately just mental items, this agreement amounts to an internal coherence of our mental presentations. More so, in virtue of Kant's synthetic logic, concept is constitutive of our cognitions, therefore making it impossible to have cognitions in which concept and intuition disagree. We speak in this case of an a priori agreement between concept and intuition, which of course, leaves no space for experimental science. It matters however to observe that although the distance that separates metaphysical and empirical reality is presumed infinite, there still are two distinct realities, such that the gap could be breached in principle. Likewise, if Kant abides with his synthetic logic throughout the *Critique of Pure Reason*, he does conceive of a possible disagreement between concept and intuition in the *Critique of Judgment*. By the very way in which he formulates his solution, Kant immediately suggests alternate solutions which he dismisses on dogmatic grounds (i.e., the gap between metaphysical and empirical reality is infinite and concepts are constitutive of our cognitions; no reasons are given as to why things are this way and cannot be any other way, meaning that these are either postulates or premises of his epistemology).

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[66] Kant, *Critique of Pure Reason*, B296, B350.

At the other extreme, the positivists equate external reality with empirical reality. As I observed earlier, the position is not untenable, yet it does have the disadvantage of ignoring the existence of mechanisms of perception. From a philosophical point of view, the most common ensuing complication is that of illusions.[67] Besides this commonly acknowledged difficulty, other problems arise when we try to understand how the process of experimental verification might function at a practical level. If we abolish the difference between external reality and empirical reality, but still keep them as two identical copies, one on the side of the physical, the other on the side of the mental, it follows that perception does not involve a construction, but a direct, 1:1 mirroring of reality. Leaving aside the difficulty of providing a biologically sound model for this "mirroring", it follows that the difference between perception and scientific knowledge would be that between two radically different kinds of mental presentations, one involving a "mirroring" and the other a reconstruction of reality in light of a theory. But if we talk about fundamentally different kinds of mental presentations, then how are they confronted to each other in the process of experimental verification and what exactly constitutes their agreement or disagreement? In other words, how does one compare a conscious picture devoid of any structural features with a rule-governed construct explicitly defined by its structural features? It seems more reasonable to compare constructs with constructs. When applied to a particular situation, a theory, presumably built around a set of universal laws, gives us objects behaving in a certain way;

[67] This difficulty is usually solved by appealing to intersubjectivity. Naturalised accounts are also available. (Ex.: If it would ever happen that all scientists are intoxicated all the time, their theories would be in agreement with their distorted perceptions and their perceptions in disagreement with external reality. They would all have right theories yet these theories, once applied, would have the most disastrous practical consequences, meaning that they would all die without ever understanding why. Given the process of natural selection, we can assume that only organisms endowed of reliable mechanisms of perception survive.)

perception also gives us objects behaving in a certain way, but according to different rules and elements of construction as dictated by mechanisms of perception; the theory is true about empirical reality in as much objects and their behaviours coincide. Thus, the idea is that if two structures that coincide over some crucial features (i.e., those features that are tested by the experiment), they are assumed to be in continuity with one another.[68]

In light of the above difficulties, I choose to defend a middle position: there is a difference between external reality and observed empirical reality, yet the two are linked by reliable mechanisms of perception. From an epistemological point of view, we begin by adopting a positivist position, according to which all our theoretical knowledge is knowledge of empirical reality, and take for granted our faithfulness of our perceptions. From here, we gradually move to uncover the nature of the mechanisms of perception, showing how external realty is linked to empirical reality and prove that, indeed, our perception of external reality is trustworthy. The two projects intertwine, physics and chemistry providing theories about empirical reality, biology and psychology making use of these theories and their applications in order to investigate the underlying mechanisms of perception.

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[68] As Carnap points out in *Testability and Meaning*, experiments do not verify theories, but rather confirm them. Obviously, an experiment cannot prove a theory and that for the very simple reason that a theory is never entirely derived from an experiment while it is always the case that an experiment is designed on the grounds of some theory. As a consequence, an experiment merely confirms a theory, that is, it allows us to continue to believe in it. Similarly, under my particular interpretation of verificationism, when two structures have points of identity, they are assumed to be in continuity; this assumption amounts to the same idea that we can continue to rely, practically and epistemologically, on an experimentally confirmed theory. Carnap, R. "Testability and Meaning" In R. Ammerman, ed., *Classics of Analytic Philosophy* (New York: McGraw-Hill, 1965), 134. This is different from Popper's "falsificationism"), which seems to entail that we can continue to believe in a theory for as long as that theory has not been falsified. Popper, K. R. *Conjectures and Refutations* (New York: Basic Books, 1965), 285. Granted, falsification is logically sounder than confirmation, yet I still think that Carnap's "confirmationism" reflects much better the actual scientific practice. It seems to me that most of our knowledge consists in true statements about empirical reality. At any rate, making knowledge consist of false statements surely renders impossible any practical application of scientific knowledge.

Among the most prominently negative consequences of this intermediate position is the fact that most scientific knowledge is, properly speaking, just knowledge of perceived reality and not necessarily of reality as it is in itself. However, this position does not yet collapse into a purely coherentist account, but hangs onto a thin thread, namely the possibility of proving the existence of reliable mechanisms of perception. Similarly, metaphysical scepticism does not dissolve, but, on the other hand, it does not prove right either. It is merely kept in an indeterminate state, frustrating for philosophy, but profitable to science, which can pursue its activities.

On the positive side, my account preserves an explicit distinction between perception and scientific knowledge while preserving common structural elements that make more plausible a comparison between the two. As argued in chapter 2, if we allow a continuous flow from external reality to perception to scientific knowledge, either as a "highlighting" of more and more universal objective invariants or as continuous construction process following essentially the same rules for both perceptual objects and scientific concepts, perception can never disagree with theory, therefore making experimental science impossible. On my account, both perception and science have the same general structure of experimental knowledge: both predicate something about a referent, both involve constructions and they both involve a verification of these constructs. Nonetheless, the referent of perception is different from that of scientific knowledge. Perception refers to metaphysical reality and presents it as empirical reality, while science refers to empirical reality and presents it as theoretical knowledge. Second, because they are both constructions, we can further distinguish them on the basis of the rules of construction involved and the elements on which they operate. Perception seems to involve rather basic logical operations and simple arithmetic computations. In contrast, scientific knowledge makes use of "richer" mathematical languages. Thus, the kind of constructions, or "hypotheses" elaborated at the level of scientific knowledge are very different from those elaborated at the level of perception. The elements on which these rules operate also differ. Perception works with primary sensations produced via an early, low-level processing of the sensory input either at the level of the detectors present in the sense organs or very early along the overall perception pathway. In contrast, scientific knowledge concerns itself primarily with perceived objects constructed by perception from primary sensory presentations. Finally, in addition to the rules and elements involved, it might also be worth considering the fact that each form of cognition appeals to its own set of experiments aiming to verify their respective reconstructions of reality. Because of its mechanical nature, perception can only rely on a limited set of experimental behaviours. These behaviours make heavy use of constants usually present in the normal environment of the organism; these assumed constants may vary if the organism is removed from its normal environment, such that the experimental behaviours initiated by perception may fail to adequately test the constructions they were meant to test. As a general rule, scientific verification does not share the same shortcomings. Possible variables are carefully identified and their impact on the output of the experiment assessed. Not only that, but science is often concerned with unusual, but nevertheless physically possible situations, thus making it possible to test for hypotheses unverifiable in the usual circumstances. Summing up all these difference, it follows that even though perception itself involves a reconstruction of metaphysical reality in terms of sensuous presentations, this reconstruction is to be regarded as distinct from whatever reconstruction is operated at the level of scientific knowledge.

3.3 The logic of experimental verification

3.3.1 Theoretical languages

In *Physical Theory and Experiment*, Duhem talks at length about a "translation" or "rendering" of empirical data in a mathematical format. He gives the examples of Kepler's geometrical formulation of celestial dynamics and contrasts it with Newton's algebraic formulation of the law of gravity.[69] The upshot of this conception, already reminiscent of Carnap's idea of a language of science, is the view that science is in fact constituted of a set of different "translations" of the same empirical data into various theoretical formulations. In as much these theoretical formulations rely on mathematical systems susceptible of axiomatization, we can further speak of a "translation" into various theoretical languages. Presumably, one of the most important tasks of science is to unify all these parallel descriptive languages into one overarching language, such as that of Newtonian mechanics. If the unity of science depends on the unity of the languages used by its various theoretical formulations, it follows that, practically speaking, the unity of science hinges ultimately on the unity of mathematical languages used by science, that is, on the possibility of a complete axiomatization of mathematics[70]

[69] Duhem, P. *The Aim and Structure of Physical theory* (New York: Atheneum, 1977), 191-5.
[70] Hilbert, D. "The Foundations of Mathematics." In Jean van Heijenoort, ed., *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931* (Cambridge: Harvard Univ. Press, 1967): 464-79.

and on the possibility of reducing it to classical logic[71] or some other common denominator.

3.3.2 A formal point of view on experimental verification

Although my knowledge of logic and mathematics prohibits a more profound discussion of this topic, I note that in *Logical Syntax* Carnap (1934/1937) considers the same problem from a purely formal standpoint. We are no longer concerned with the essentially epistemological problem of translating Kepler's laws in the language of Newtonian mechanics, but with the general problem of translating any logical/mathematical language into any other such language. The problem of the unity of science is very important for Carnap and will be addressed in more detail in the last section of the present chapter. For now, I will simply take for granted that science involves a "translation" involving one object, i.e., empirical reality, and several descriptive languages based on logic and/or mathematics. What is important for an account of experimental verification is the idea that if empirical reality itself relies on a logical construction we can consider the possibility of translating empirical reality into a theoretical language and *vice versa*.

As suggested earlier, scientific theories hypothesise possible structures of empirical realty. These possible structures are needed as means of experimental confirmation: in order to prove that a theory is true about empirical reality, the structure of reality postulated by that theory must coincide with the structure of observed reality. Thus, what at an epistemological level amounts to the application of the theory to a

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[71] Gödel, K. "On Formally Undecidable Propositions of *Principia Mathematica* and Related Systems." In Jean van Heijenoort, ed., *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931* (Cambridge: Harvard Univ. Press, 1967): 596-616.

particular situation is, at a more formal level, a translation of a theoretical language into the language of empirical reality. Granted, in as much neither the language of theory, nor that of perception is completely formalised, such a translation remains purely hypothetical. What I say is only that if we accept a constructivist approach to both perception and science, we can make a rapprochement between what is otherwise a purely epistemological account of experimental science and Carnap's "linguistic turn" in the philosophy of science. I think this rapprochement can yield a number of interesting observations allowing a partial integration of the present account into the linguistic debates dominating philosophy of science after Carnap.

3.3.3 Epistemic reductionism

As Friedman[72] points out in his interpretation of Gödel's theorem of incompleteness, a translation takes the form of a reduction in as much as the object language is shown to be part of a so-called "richer" descriptive language. On the other hand, if the description language is richer than the object language, no reduction is being operated. To my understanding, reduction means that all the items of the object language can be constructed according to the rules of the descriptive language and all the proofs of the object language reduce to proofs in the descriptive language. According to this somewhat sketchy interpretation, if arithmetic were ever to be reduced to logic, then we could translate all arithmetic operations in terms of logical operations. In contrast, if reduction cannot take place, that is, if the object language is "richer" that the descriptive language, this means that we have to introduce new axioms to the descriptive language in

[72] Friedman, *Reconsidering Logical Positivism*, 196.

order to prove and construct all that can be proven and constructed within the object language. The idea suggested here by Friedman, and also by Goldfarb and Ricketts[73], is that of a formal hierarchy of languages.

We can immediately observe that in as much a theoretical language is "richer" than the language of perception, we can always translate the language of empirical reality into the language of theoretical knowledge without any need of further assumptions[74] yet we can never reduce a theoretical language to the language of pure observation. In epistemological terms, induction requires the introduction of some extra formal elements, while verification doesn't. I think this is in good agreement with experimental science, which is very careful not to claim that theories are derived from experience, but only tells us that they can and must be verifiable in experience. This very important difference between empirical epistemology and experimental epistemology, one still a philosophical project, the other, a realised fact, might thus be grounded at a formal level.

3.4 Quinean concerns

3.4.1 Quine's argument against the analytic-synthetic distinction

Carnap's formalisation project faced from the very beginning some very serious criticisms. Gödel showed that classical mathematics required for most scientific theories does not reduce to Carnap's candidates for a universal language of science. Thus, the idea of a universal language ran out of steam relatively early after its initial formulation. Twenty years later, the general idea of an a priori basis of scientific theories came under

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[73] Goldlfarb, W. and T. Ricketts. "Carnap and the Philosophy of Mathematics." In D. Bell and W. Vossenkhul, eds., *Science and Subjectivity* (Berlin: Akademie, 1992): 61-78.

[74] All this could be possible in principle, of course, for we have to assume here that both the languages of theory and perception are formulated in their more basic, axiomatic form and then prove that one language is stronger than the other.

questioning too. In the *Two Dogmas of Empiricism*, Quine argues most famously that if a translation cannot be operated in a reductionist sense, we loose the distinction between analytic and synthetic.[75] For instance, if we fail to reduce the "richer" language of classical mathematics to logic, first order logic cannot possibly constitute the most fundamental syntax of classical mathematics, and therefore cannot constitute the a priori, analytic element of the language of classical mathematics. What happens is that there are mathematical truths which cannot be proved by logical means, thereby rendering Carnap's distinction between analytic and synthetic useless. It turns out that a true reductionism is in fact impossible because the proposed universal language of science cannot account for classical mathematics needed by mechanical theories, meaning that, ultimately, we cannot distinguish between the laws of physics and the a priori foundations of the language of science. Based on this conclusion, Quine argues for a holism which, in Friedman's opinion[76], is highly reminiscent of Poincaré's conventionalism: "*the totality of our so-called knowledge [...] is a man-made fabric which impinges on experience only on the edges. [...] A conflict with experience at the periphery occasions readjustments in the interior of the field. [...] But the total field is so undetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to re-evaluate in the light of any single contrary experience. [...] Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system.*"[77]

[75] Quine, W. V. "Two Dogmas of Empiricism." *Philosophical Review* 60 (1951): 20-43.
[76] Friedman, *Reconsidering Logical Positivism*, 67.
[77] Quine, *Two dogmas of Empricism*, 39-40.

3.4.2 Holism does not conflict with experimental science

Although I can plainly see the similarity between Quine and Poincaré, I don't believe their conclusions stem from an essentially similar approach to the problem. According to the Quine-Duhem thesis of verification holism[78], only a theory as a whole can be tested by an experiment. This fits with the "model-based" approach of experimental science: what are tested are structures and not elements of structures. For instance, Rutherford's experiment shows that matter is composed of tiny, negatively charged particles dispersed around denser, positively charged particles. In as much matter is so structured, it follows that electrons must exist. Nevertheless, if it would turn out at some point that electrons exist, but nuclei don't, we could not possibly continue to believe that Rutherford's experiment proved the existence of electrons. In more general terms, whenever an experiment succeeds, it shows that the structure of reality as defined by the whole body of our beliefs closely emulates the real structure of reality.

Problems arise when experiments falsify our knowledge. When Friedman argues for a similarity between Quine with Poincaré, he does not have in mind verification holism. He is mainly concerned with the question of how we come to modify theories when they fail to match observed reality. I think though that the simple fact that Quine has a verification theory sets up for the very beginning a huge difference between Quine and Poincaré. For Poincaré, there aren't really any facts. There are only descriptions of facts in light of a theoretical apparatus. Quine is more subtle in his approach. There are facts which explicitly disagree with our theories. However, because a theory has more than one component and our knowledge is made from more than one theory, we can always choose to alter one part of a theory or one theory rather than another in order to

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[78] Duhem, *The Aim and Structure of Physical theory*, 185. Quine, *Two dogmas of Empricism*, 39-40.

bring it in agreement with the facts. There are a number of possible options open to our consideration and we are free to choose whichever suits us best. If this interpretation is correct, then holism doesn't enter in any blatant contradiction with the demands of experimental science. Facts remain facts, constituting a fixed point of reference, and whatever changes is on the side of theoretical knowledge.

3.4.3 The individuation of theories

Unfortunately, despite the general agreement of verification holism with the experimental procedure and the lack of obvious contradictions between Quine and the most basic demands of experimental science, problems can arise. Let me illustrate the situation as follows. In a practical setting, if one experiment verifies two theories, we cannot but conclude that they are both true despite our inability to unify and reduce them to some common denominator. In this case, Quine would most likely argue that we choose the theory which is in better agreement with the rest of our accepted knowledge, although, from a strictly formal point of view, we could have just as well chosen the other theory and get rid of the rest of our knowledge. Even though counterproductive, the latter is just as valid as the former. Thus, according to Quine, the overall body of knowledge is layered between more central and more peripheral elements of knowledge, yet there is no significant difference between them in as much both can be modified should we ever need to revise our knowledge of empirical reality. Most notably, this layering cannot possibly amount to a distinction between analytic and synthetic elements of knowledge.

At this point we already have a divergence between Quine and experimental science. Quine assumes that if more than one combination conforms to empirical data, for example, if we need to alter our knowledge composed of theories T_1 and T_2 to $T_1'T_2$ or T_1T_2' in order to bypass some experimental disagreement, then we must make a choice between $T_1'T_2$ and T_1T_2'. It follows form here that if two theories T and T' explain some phenomenon P, we must likewise choose between T and T'. Experimentally speaking, if two theories or two combinations of theories are true about the same phenomenon, then we must keep both of them despite whatever incoherence and lack of unity of our total body of knowledge they may entail. So far, the disagreement seems of little consequence, for science does its best to achieve an absolute coherence and a unified picture of reality. I note however that it does not always succeeds in achieving this goal and despite this fact it nevertheless still provides knowledge of empirical reality.

Let us imagine now that we want to prove a theory and falsify the other. Duhem argues quite convincingly that in order to make sure that we do not run into any contradictions, we would have first to show that none of the components of the falsified theory and none its grounding theories overlap with the components and grounding theories of the theory we want to prove right.[79] We can prove one theory correct and falsify the other only in as much the two theories are completely independent. If not, by falsifying one we might actually falsify an element common to both theories, which would be contradictory. Shortly put, the logic of experimentation is such that not only we cannot prove a theory true by proving its alternatives false, but also doesn't allow us to conclude that if a theory is true then competing theories must be false; instead, if we want

[79] "*Unlike the reduction to absurdity employed by geometers, experimental contradiction does not have the power to transform a physical hypothesis into an indisputable truth; in order to confer this power on it, it would be necessary to enumerate completely the various hypotheses which may cover a determinate group of phenomena; but the physicist in never sure he has exhausted all the imaginable assumptions*". Duhem, *The Aim and Structure of Physical theory*, 189-90.

to prove that only one theory is true, we either have to conduct separate experiments or make sure that the theories involved do not overlap with one another.

Even though holism warns us of possible complications, this does not entail a contradiction between experimental science and the Quine-Duhem thesis. Strictly speaking, Duhem does not tell us that we cannot distinguish between the various elements of theory, but only that we must take into account these elements if we ever want to prove a theory and, at the same time, falsify competing theories. Holism becomes problematic the moment we link the basic thesis of holism to Quine's claim that there is no analytic-synthetic distinction. Given the inability of Kantian epistemology to cope with new geometries and mechanical theories, I cannot but agree that even a priori statements must and can be changed. However, I am quite puzzled as to how we can continue to distinguish between two theories as being two different theories rather than two inseparable elements of the same theory if we completely abolish any distinction between analytic and synthetic.

The problem is most obviously one of choice and has its deeper roots in an ungranted assumption concerning the overall unity and coherence of scientific knowledge. If our knowledge is not composed of distinct theories, then, to give a quick example, when it comes to revise our knowledge, rather than changing Rutherford's model of matter, we can decide just as well to group together two elements pertaining to two different theories, say, our belief that there are electrons and our belief that the gravity constant is 6.754 x 10-11 Nm^2/kg^2, and modify them as we would modify a theory. In lack of distinct theories, we can choose to combine any elements of our knowledge and alter them as if they were a theory. It seems to me that we can rely on such a strategy only if we assume beforehand that despite our fragmentary understanding of nature, all the elements of our knowledge must somehow be related to one another such that altering any two arbitrarily chosen elements will somehow result in a meaningful restructuring of our knowledge. The discrepancy between Quine and experimental science becomes now much more obvious. Science aims to an absolute unity of our knowledge, but does not take it for granted, for, obviously enough, scientists alter theories, not arbitrary collections of beliefs.

If this is the case, then how do we individuate theories? In the case of perception, I assumed an externalist point of view. Each presentation is mechanically individuated by its referring back to a specific input via a specific processing pathway. There is no need, as Carnap suggested, to imagine syntactic features proper to each kind of presentation. In the case of theoretical knowledge however, I cannot but assume an internalist point of view. At any rate, it would seem strange to postulate the existence of mechanisms linking hypothesised theories to a world external to our minds, although some have tried, most notably, empiricists like Locke, Berkeley and Mill, who suggested that through abstraction, understood here as some kind of mental operation, we can transform empirical inputs into numbers and other such highly theoretical concepts.

For example, how can we distinguish between Kepler's three laws and Newton's law of gravity as being two different theories about celestial dynamics? There are some disagreements between their respective predictions, yet they are not significant enough to differentiate them as one being false and the other true about empirical reality. Then how do we know they are two different theories? Or, should we maintain the opposite, how do we know they are parts or different formulations of the same theory? Appealing to the process of “abstraction” or “empirical induction” is pointless. This process would have the same input (i.e., the same astronomical observations) and, in lack of any further elaboration, would consist of exactly the same operation of “abstraction”, therefore yielding exactly the same output no matter what theories about celestial movements we have in mind. Needless to add, such a continuity, or, even worse, an identity between all theories of celestial dynamics is highly implausible. By this account, Ptolemy, Copernicus, Kepler, Newton and Einstein have all proposed the same theory of which they highlighted different parts or offered different, but equivalent formulations.

On the other hand, if we distinguish, as Duhem does, between Kepler’s laws as being formulated geometrically and Newton’s law of gravity as making use of an algebraic formulation, we can argue that they are formulated in two different languages. Each language has its own distinctive syntactic features and it is on the basis of these syntactic features that we can tell them apart. We distinguish the two theories in as much we cannot mathematically derive Newton’s law of gravity from Kepler’s laws or *vice versa*. All we have is an identical (or significantly identical) mapping of the two theories onto empirical reality, hence their acceptance as being both true about empirical reality despite the fact that they remain two distinct and thus far impossible to unify theories.

I think it is safe to assume that Quine would agree with this conclusion. Kepler’s three laws of celestial dynamics and Newton’s law of gravity are not synonymous. Only if all theories could be reduced to a unified language of science allowing us to navigate, paper and pencil only, from Kepler to Newton, would we be able to prove that two theories about the same phenomenal reality are synonymous. In this respect, Carnap’s dream of a universal language of science falls short of reality. On the other hand however, it also seems that Quine's claim concerning the complete abolition of the analytic-synthetic distinction is a bit premature. There is no analytic-synthetic distinction across the total body of knowledge for the very simple reason that there is no unified body of knowledge, yet within each distinguishable part of knowledge such a distinction must be held, else we loose the individuation of theories as distinct parts of knowledge.[80]

3.4.4 Friedman against Quine

Following Reichenbach, Friedman argues that if it is true that we can no longer accept a rigid, absolute distinction between analytic and synthetic, a relativized version of it is still plausible.[81] The idea here is that there is more than one possible framework grounding a priori our knowledge; or again, there are no fixed, unrevisable truths, but there still are truths independent of empirical reality. In a more elaborated version of this argument, Friedman agrees with Quine that there is no such thing as an "analytic for any language" (in Quine's formulation, "[statement] *S is analytic for* [language] *L, with variable 'S' and 'L'*"). However, he argues that we don't need such a notion in order to preserve the distinction between analytic and synthetic. Friedman grounds Carnap's distinction between analytic and synthetic in the combinatorial logic of Wittgenstein's *Tractatus*, to which he appeals in order explain the notion of syntax and tautology.[82] As mentioned in the previous chapter, once we agree upon a set of elements and their respective truth values, we can a priori determine the truth values of all possible logical

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[80] The only other solution would be to sacrifice the analytic-synthetic distinction and replace it with meanings. However, if Frege appeals to meanings in his famous morning star - evening star example, it is solely because reference and syntactic features are not enough to individuate the two concepts. But theories are not unstructured symbols, they have syntactic features and, more so, they seem to have distinctive syntactic features, such that appealing to meanings is unnecessary.

[81] Friedman, *Reconsidering Logical Positivism*, 82.

[82] Friedman, *Reconsidering Logical Positivism*, 177, 185-6.

combinations of these elements. Since among the logical combinations of atomic elements there are some that are tautologies, i.e., combinations true independently of the truth value of the elements, there must be purely analytic truths. Friedman argues that under the influence of Hilbert and his axiomatization project, Carnap altered this view, trying to show that there are analytic truths relative to any given language, where by language we are to understand various logical and mathematical systems. It follows from there that if there is no analytic in general, there still is, or at least could be in principle an analytic level for each language in particular and that it is all we need in order to preserve the analytic-synthetic distinction (and with it the a priori - a posteriori distinction as well).

According to this argument, it seems quite reasonable to assume that in as much we adopt a constructivist approach, there must be truths derivable solely from the syntax of the language used to construct scientific theories. It is not at all clear whether Quine is committed to a constructivist approach, so I wouldn't know if this counterargument is meant to bring Quine down on his knees. What I struggled to show is that Quine distinguishes theories from one another and that a classical empiricist account à la Mill is insufficient for an individuation of theories. It followed from there that either we defend an empiricist position, in which case the total body knowledge is not divided in distinguishable theories, or we give up a straightforward empiricism and think of some other way to individuate theories. If we choose the former, it follows that our knowledge as a whole amounts to one theory comprising different parts, all elements of the same over-arching structure. This further entails that there must be a unified body of knowledge and therefore there must be an explicit continuity not only between theories on various aspects of empirical reality, but also between theories explaining the same phenomenon. Presumably, we can still distinguish between two theories about two different phenomena based on a difference in the referent, but when it comes to theories about the same phenomenon, we are faced with the implausible, yet necessary conclusion that they are all one and the same theory. I gave the example of Newton's and Kepler's laws of celestial dynamics in order to illustrate my point. Given the unhappy conclusion of this hasty marriage between radical empiricism and holism, I did my best to show that individuation of theories is most likely to occur on syntactic grounds. But if this is the case, then we have to adopt a constructivist approach and, once we adopt it, we are further justified in accepting Friedman's defence of the analytic-synthetic distinction.

3.4.5 The relevance of Quine-Duhem holism in the context of experimental science

This said, I want to make a very important observation which will further clarify my exact position concerning this issue. As one might expect, from a strictly experimental point of view, analytic statements are of no direct concern, for they don't say anything about empirical reality. Nevertheless, in as much they say something about the language in which science expresses itself, they do have the formal structure of knowledge. Carnap's idea of a knowledge of the scientific knowledge, or meta-knowledge of empirical reality is without any doubt one of the most daring ideas with which philosophy ever confronted our minds. In the *Dynamics of Reason*, Friedman depicts a most fascinating account in which knowledge is layered into a straightforward knowledge of empirical reality constituting the domain of natural science, and a theory of theories or meta-knowledge which would constitute the proper domain of philosophy.

When scientific knowledge fails to fall in agreement with empirical facts, philosophy comes to the rescue by altering the core structure of scientific knowledge, thus enabling it to overcome its otherwise a priori set limitations.

I am afraid though that my practical and essentially epistemological understanding of science and its problems will forever prohibit me walking that high. I cannot but agree with Carnap and Friedman that there must be analytic truths. On the other hand, if we were to specify what these truths are, we would immediately find out that the problem of reductionism cannot be easily avoided. Historically speaking, most of mathematics and science were not explicitly constructed as languages, meaning that in order to prove the existence of analytic truths we need to operate a reduction to a constructed language in which the existence of such truths is provable. Since Carnap's formulation of the language of science is not rich enough for the purposes of such a reduction, it follows that we must rethink the language of science, emulating it after a "richer" language capable of describing all the fundamental theories of models science has to offer. This language has not been designed yet. Lacking this crucial element, we can only defend a lesser version of the analytic-synthetic distinction.

For instance, if we adopt the commonly accepted view that Newtonian mechanics is built on the premises of Euclidian geometry, it seems reasonable to assume that there are geometrical statements which are true not only in the context of Newtonian mechanics, but in whatever theory that includes among its rules the axioms of Euclidian geometry. We have here, in these axioms of geometry, something closely resembling an a priori basis for Newtonian mechanics. However, even though this approach suggests the existence of an a priori basis, we must realise that this basis cannot be exhaustively spelled out by means of mere reduction. Newtonian mechanics does not reduce to Euclidian geometry. Among many other significant differences, Newtonian mechanics does not deal with geometrical points (three-dimensional coordinates), but with material points (an array of two sets of coordinates, three for spatial position, and another separate one for the quantity of inertial mass; the value of this last coordinate is 0 for empty space). To use the terminology of Kant's partial formalisation of natural science, mechanics deals with both extensive and intensive quantities.[83] But if this is the case, then Newtonian mechanics has its own set of additional rules and axioms, such that there might be additional analytic truths that can never be proved by a mere subordination of Newtonian mechanics to Euclidian geometry. Shortly put, we have good reasons to believe that there are analytic truths inherent to Newtonian mechanics, but failing to identify all of them, we are reduced to a practical state of affairs in which we cannot fully distinguish between analytic and synthetic truths. It follows form there that we cannot take for granted that there are perfectly well defined analytic truths grounding each theory at an a priori level, but only that there are some syntactic properties inherent to each theory and that these syntactic properties may be enough for individuating theories.

On one hand, this conclusion merely reflects the general attitude of scientists towards the idea of a meta-knowledge of empirical reality. Even though knowledge of syntactic properties constitutes the object proper of a formal meta-knowledge, from a strictly experimental point of view, having to worry about syntactic properties is an unwanted epistemological complication. Syntax limits theories, sometimes in a purely formal manner, in which case there is nothing to worry about, sometimes in such way that prohibits them for ever agreeing with observed facts. Science takes into account

[83] Kant, *Critique of Pure Reason*, B202 (Axioms of intuition), B207 (Anticipations of perception).

formal constraints only if pressured seriously enough by empirical data, which is not to say that for this reason formal constraints are any less formal, like Quine seems to suggest, yet this certainly demonstrates a rather unsystematic and tangential interest in anything even remotely resembling a priori truths.

On the other hand, the same conclusion might provide an explanation as to why some elements of knowledge are preferentially changed over others. In the context of a partial reductionism, not all a priori elements can be properly identified. It follows from here that what is left after we remove the a priori elements of knowledge cannot constitute the a posteriori gained content of knowledge, but a mixture of a priori and a posteriori elements.[84] This division between a priori and the leftover mixture of a priori/a posteriori elements mirrors the distinction between axiomatized and non-axiomatized segments of scientific knowledge. Paradoxically, in this less than ideal context it is much safer to change the axiomatized segment of knowledge. First, it is only in as much we know how the language of theory is constructed as that we can specifically construct it differently. Second, it is only by making specific changes of this highly formalised basis of theory that we can be sure that our changes affect the theory globally. In short, by changing the formalised segment of knowledge we can make specific changes and track their impact onto the theory as a whole. Changes in the non-formalised segment of a theory can be as unpredictable as program patches. Such changes are meant to correct a theory locally by adding further clauses, yet it is never clear if these clauses change something belonging to the way in which the theory is constructed at a formal level (i.e., changes that affect a theory's identity) or merely adjust some superficial empirical law in

[84] This is very different from Poincaré's conventionalism, which clearly separates a priori and a posteriori elements of knowledge. With Poincaré it is always clear whether we alter a theory at the formal level or merely adjust a description of facts while continuing to work on the same formal basis.

order to better fit some particular circumstances. Thus, while Quine might be right that our first impulse is to change the seemingly "a posteriori" segment of a theory, it is precisely because of holistic concerns that I argue that changes in the a priori segment are always preferable. Strictly speaking, Quinean holism remains intact: there is no absolute distinction between analytic and synthetic elements of knowledge. Nevertheless, from a practical point of view, a partial distinction is established and this partial distinction may be enough to justify the overall directionality of scientific knowledge, which doesn't spread out in all possible directions every time a revision is required, but advances in a quasi-linear progression.

3.5 The unity of science

My criticism of Quine hinges onto the notion that science is not necessarily a unified body of knowledge. Ironically, it is precisely this lack of unity that goes against both Quine's and Carnap's views that fuels my argument for preserving the analytic-synthetic distinction. Thus, even though I argue against Quine that we cannot afford to abandon the analytic-synthetic distinction, I still cannot return to Carnap's commitment to an epistemic reductionism whereby all theories reduce to a unified language of science or to a more basic language of empirical verification. An interpretation of Gödel theorems of incompleteness by the means of which Goldfarb and Ricketts[85] aim to show that Carnap's project is essentially devoid of any reductionist/foundationalist ambitions suggests that theories layer themselves according to the "richness" of the language in which they are formulated. If this is the case, then it can be argued that since the language

[85] Goldfarb. and Ricketts. *Carnap and the Philosophy of Mathematics*, 78.

of theoretical science is "richer" than the language of perception, we cannot possibly reduce theory to observation, although we could, at least in principle, always theoretically emulate all objects construable in perception. The best we can hope for in a practical context is a partial reduction providing an incomplete set of analytic-like statements. Likewise, in light of Quine's criticism, I don't believe that there is a unified language of science and a single, absolute analytic-synthetic distinction spanning the whole body of scientific knowledge. Designing such a language may not be impossible, yet thus far no such language is available, hence the distinctions we make between various theories and the inevitable holistic concerns that ensue.

Both conclusions are in contradiction with Carnap's epistemic reductionism, but in agreement with experimental science. In an experimental setup, knowledge amounts to true and false theories about empirical reality. Except for the necessity of verification or falsification in reference to empirical reality, no other conditions are imposed onto knowledge. This minimalism contrasts quite violently with the wide-spread philosophical conception that knowledge must be an internally coherent and thoroughly unified body of beliefs. Kant and Carnap thought so, for they both advocated the existence of structural elements permeating all our knowledge about empirical reality. From an experimental point of view, whether knowledge acquired experimentally crystallises into a coherent, unified body of knowledge is irrelevant. That theory T_1 is true about empirical reality and theory T_2 is true about empirical reality does not entail an equality or continuity between T_1 and T_2. A unity of knowledge is never assumed beforehand, but, if unity is what we want, it is an extra we have to fight for; as I shall argue in a moment, this battle cannot be won only by trying to find more about empirical reality. Some sciences, like physics or chemistry, are highly, although not completely unified. Biology, on the other hand, is but a collection of very diverse theories on the same topic. Formal, non-experimental sciences are not spared either. Mathematics is not exactly one science, but as, the name suggests, we have quite a lot of different mathematics which are not all in happy continuity with one another. More so, on a constructivist assumption, it is precisely because we don't have a unified logico-mathematical formal science that experimentally-gained knowledge remains fragmented into different sciences where each science is further fragmented among many distinct theories.

Likewise, experimental science does not need our knowledge to be governed by the principles of simplicity and certainty. Just because we cannot see how two theories can be both true does not mean that one must be true and the other false in respect to empirical reality. For Poincaré, there is a disjunction stating that T_1 is true or T_2 is true, but not both; the only way to make them both true is by referring them to two distinct universes, in which case one theory is always outside the scope of science.

The only kind of unity we can rely on in an experimental setup is the unity of reference, not because this kind of unity is verified in experience, but because scientific knowledge is by definition knowledge of empirical reality. Thus, what keeps science together at the most basic level, providing a unity to the whole corpus of scientific knowledge, is the common reference of all theories to one and the same empirical reality. I distinguish this unity as being "external" – i.e., achieved by reference to something external to the hypothesised theory; the same kind of unity is granted by the co-reference of primary sensations to one and the same sensory input – and oppose it to the internal unity of science that Kant, Cassirer and Carnap explicitly advocate and even an empiricist like Quine seems to implicitly take for granted.

The emerging picture is that of a knowledge in which we have a set of theories $\{T_1, T_2, T_3, \ldots T_n\}$ truthfully or falsely referring to phenomenal reality P via experimental verification. $T_1, T_2, T_3, \ldots T_n$ and P are all conscious presentations, yet the only unity that can be established experimentally is that derived from the relationships linking each theory to phenomenal reality. A complete unity transcends the scope and means of experimental science, for it is obvious that it depends on more than mere correspondence with empirical reality. Presumably, such a unity is the object of study of a meta-knowledge of empirical reality. Assuming a hierarchy of formal languages and the possibility of partial reductions of "richer" languages to "simpler" ones, this meta-knowledge can provide a further unity beyond that operated at the level of experimental science, but never the complete and thoroughgoing unity postulated by most philosophical accounts.

Conclusion

With the development of new geometries and mechanical theories it became clear that Kant's framework of possible experience is nothing but a particular example of a framework applying specifically to Newtonian mechanics. In an attempt to save Kant's fundamental idea that scientific knowledge has an a priori formal structure, both Cassirer and Poincaré concentrated their efforts in defining a framework flexible enough so it can apply not only to a particular geometry or mechanics, but to science in general. Cassirer proposed a replacement of Kant's unique and essentially immutable framework of knowledge with a dynamic one, while Poincaré advocated the existence of a plurality of frameworks among which we can choose which ever serves our interests. The critical assessment of the solutions offered by Cassirer and Poincaré conducted in Chapter 1 showed that a mere relativization of the a priori is not enough for accounting cases of experimental falsification. In order to satisfy the demands of experimental science, knowledge must amount to a predication whereby theory is not always true, but can also be false in respect to empirical reality. I showed that such a predication cannot always be interpreted as a Kantian synthesis in which theory subsumes, organises and ultimately becomes constitutive of empirical reality, scientific knowledge must consist at least in the case of experimental falsification in a dual presentation of empirical reality, one derived from perception, the other, from our ability to formulate hypotheses. An agreement between these two mental items amounts to scientific truth, a disagreement, to falsity. I have also showed that in the actual scientific practice an agreement between two theories cannot possibly supersede the requirement of experimental verification: perception of empirical reality constitutes the absolute point of reference for all theories, such that each scientific theory must agree or disagree with empirical reality before we can concern ourselves with the overall agreement and coherence of all theories within a thoroughly unified body of knowledge.

In accordance with this conclusion, I argued in Chapter 2 that perceptually derived presentations must be independent of any theoretical concerns. One way of achieving this independence is by appealing to mechanisms of perception, which, as mere physiological mechanisms, are immune to the influence of conscious thought. In light of the fact that we do not have detectors specialised for each individual entity or type of entity existing an ontological level, I further argued that perception must involve a construction of perceived objects and events from simpler data elements. A critical assessment of several constructivist models (Cassirer, Russell, Carnap and Fodor) that could be implemented mechanically showed that a construction based on logical and simple arithmetical operations is not enough to account for our perception of objects. I solved the problem by harnessing the process of construction with a process of verification, both implemented physiologically at the level of perception. As a conclusion to the second chapter, I proposed an experimental model of perception simple enough to be implemented mechanically – and therefore apt to keep perception independent of any theoretical activity of thought – yet capable of overcoming the shortcomings associated with purely constructivist models.

Finally, under the influence of Poincaré, Duhem and Carnap, I argued that we construct theories. One argument in favour of adopting a constructivist approach of our ability to formulate theories stemmed from the necessity of explaining how facts and theory fit together beyond the mere statement that they must somehow "agree" or "disagree" in order for a theory to be verified in experience. I argued in Chapter 3 that constructivist approach makes it possible to distinguish between perceptual and theoretical presentations based on the referent, the rules of construction involved, the elements upon which these rules operate and the means of verification, while still allowing a means of comparison required for experimental verification. However, in order for this account to work, a distinction between the a priori and the a posteriori elements of knowledge must be possible. Consequently, I argued against Quine that at least some syntactic, a priori features of a theory must be distinguishable in order to make possible an individuation of theories within the overall body of scientific knowledge. I showed that such an individuation actually occurs in the actual scientific practice and suggested that this individuation is most likely to occur on syntactic grounds. An overall assessment of Carnap's "linguistic approach" in the context of the experimental practice enabled me to conclude that the only unity that can be established experimentally is that derived from the common reference of all scientific theories to the same empirical reality, while a complete unity hinges on the possibility of a meta-knowledge of empirical reality.

To summarise, throughout this thesis I tried to show that experimental falsification is possible only in as much our ability to perceive reality is separated from our ability to formulate theories. On the basis of this necessary requirement I have argued that both perception and science most likely involve structured presentations constructed according to a set of rules. This constructivist approach makes it possible to distinguish between the two kinds of presentation, while still allowing a means of comparison required for experimental verification. In parallel, I tried to show that experimental science takes empirical observation as the absolute point of reference for all its theories. By combining this essential feature of scientific knowledge with a criticism of the possibility of a universal language of science allowing for a reformulation and reduction of all theories to a common denominator, I concluded that what keeps science together is first and foremost the common reference of all theories to empirical reality, while an internal, theory-to-theory unity of science remains thus far incomplete. This overall model of scientific knowledge in which each theory stands in a specific relationship with perceptual presentations, but not necessarily in a relationship with every other theory suggests the picture of a consciousness incapable of achieving absolute internal unity and whose "hanging together" relies heavily on its continuous connection with empirical reality, itself kept together by a continuous connection with the external world.

Bibliography

Ayer, A. J. *Language Truth and Logic* (London: Gollancz, 1936)

Carnap, R. "Der Raum. Ein Betrag zur Wissenschaftslehre." *Kant-Studien* 30 (1922): 90-107

Carnap, R. *The Logical Structure of the World* (Los Angeles: Univ. of California Press, 1967)

Carnap, R. *The Logical Syntax of Language* (London: Kegan Paul, 1937)

Carnap, R. *The Unity of Science* (London: Kegan Paul, 1934)

Cassirer, E. *Substance and Function* (New York: Dover, 1923)

Carnap, R. "Testability and Meaning" In R. Ammerman, ed., *Classics of Analytic Philosophy* (New York: McGraw-Hill, 1965), 130-195

Cosmides, L. and Tooby, J. "Origins of Domain Specificity: The Evolution of Functional Organization", *Mapping the Mind* (Cambridge University Press, 1994)

Duhem, P. *The Aim and Structure of Physical theory* (New York: Atheneum, 1977)

Einstein, A. *Sidelights on Relativity* (New York: Dover, 1921)

Fodor, J. *Psychosemantics* (MIT Press, 1987)

Fodor, J. *Concepts: Where Cognitive Science Went Wrong* (Oxford Univ. Press: Clarendon Press, 1998)

Fodor, J. *The Mind Doesn't Work That Way* (MIT Press, 2000)

Friedman, M. "Logical Truth and Analyticity in Carnap's *Logical Syntax of Language*." In W. Aspray and P. Kitcher, eds., *History and Philosophy of Modern Mathematics* (Minneapolis: Univ. of Minnesota Press, 1988): 82-94

Friedman, M. "The Re-evaluation of Logical Empiricism." *Journal of Philosophy* 88 (1991): 505-19

Friedman, M. "Carnap and Weyl on the foundations of Geometry and Relativity Theory." *Erkenntnis* 42 (1995): 247-60

Friedman, M. *Reconsidering Logical Positivism* (Cambridge Univ. Press, 1999)

Friedman, M., *A Parting of the Ways: Carnap, Cassirer and Heidegger* (Chicago: Open Court, 2000)

Friedman, M. *The Dynamics of Reason* (Stanford: CSLI Publications, 2001)

Gödel, K. "On Formally Undecidable Propositions of *Principia Mathematica* and Related Systems." In Jean van Heijenoort, ed., *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931* (Cambridge: Harvard Univ. Press, 1967): 596-616

Goldfarb, W. and T. Ricketts. "Carnap and the Philosophy of Mathematics." In D. Bell and W. Vossenkhul, eds., *Science and Subjectivity* (Berlin: Akademie, 1992): 61-78

Hempel, C. G. "On the Logical Positivists' Theory of Truth." *Analysis* 2 (1935): 49-59

Hilbert, D. "The Foundations of Mathematics." In Jean van Heijenoort, ed., *From Frege to Gödel: A Source Book in Mathematical Logic, 1879-1931* (Cambridge: Harvard Univ. Press, 1967): 464-79

Howard, D. "Einstein, Kant and the Origins of Logical Empiricism", in *Logic, Language, and the Structure of Scientific Theories: Proceedings of the Carnap-Reichenbach Centennial, University of Konstanz* (Pittsburg-Konstanz Series in the Philosophy and History of Science, No. 2, 1994, pp. 45-105)

Kant, I. *Critique of Pure Reason* (Hackett, 1996; translated by W. S. Pluhar)

Kant, I. *Critique of Judgment* (Hackett, 1987; translated, with an ntroduction by, W. S. Pluhar)

Poincaré, H. *Science and Hypothesis* (New York: Dover, 1905)

Popper, K. R. *Conjectures and Refutations* (New York: Basic Books, 1965)

Popper, K. R. *The Logic of Scientific Discovery* (New York: Harper & Row, 1965)

Quine, W. V. "Two Dogmas of Empiricism." *Philosophical Review* 60 (1951): 20-43

Reichenbach, H. *The Theory of Relativity and A Priori Knowledge* (Los Angeles: Univ. of California Press, 1965)

Russell, B. "Logical Atomism." In J.H. Murihead, ed., *Contemporary British Philosophy* (London: George Allen & Unwin Ltd., 1956): 357-83

Russell, B. *Mysticism and Logic* (New York: Longmans, Green & Co., 1914)

Russell, B. "The Theory of Logical Types." In D. Lackey, ed., *Essays in Analysis* (New York: George Braziller, 1973): 215-52

Schlick, M. *General Theory of Knowledge* (La Salle: Open Court, 1985)

Searle, J. R. *Mind. A Brief Introduction* (Oxford Univ. Press, 2004)

Wittgenstein, L. *Tractatus Logico-Philosophicus* (London: Routledge, 1922)