# Structural Idealism: Quantum Probabilities and Selective Subjectivism

Cameron Beebe


**Abstract**

An understanding of structuralist philosophical foundations for physics as *idealist* rather than *realist* is warranted. I aim to reconstruct and understand justification for such a position by examining texts from influential proponents like Eddington, Cassirer, and Weyl among others. I will pursue this research project using quantum probabilities as a first case study of an example where *structural idealism* may make more sense than structural realism or alternatives like "participatory realism" (which might represent more of a difference in terminology and historical coherence than a difference in substance).

I will attempt to argue that structural idealism is not only consistent with a Bayesian approach to quantum probabilities, it bolsters Bayesian accounts of probability generally by showing why they are **epistemologically compelling beyond traditional normative accounts**. This is arguably a central point of Eddington's "selective subjectivism", which can be considered an influential and early view of structural realism because of the focus on group structures central to an agent's phenomenological experience. However, Eddington contends that the position is *idealist*, and so I think a position of this sort can justifiably be characterized as *structural idealism*.

At present I believe I have also identified an interesting second case study, that of open systems as advocated for in Hartmann and Cuffaro. Currently I have identified several passages from Hermann Weyl's work which seem to support an open systems view. This view might be compared also with other notions from transcendental idealism and process philosophy.

So, while it is clear that Eddington's and Weyl's views are central to this project, I will also follow relevant leads in the work of Poincare, Cassirer, and perhaps Husserl and Brouwer among others (although not sure how deep into the philosophy of math I need to go). I expect there might be good reason to expand beyond, but to begin with I will prioritize to avoid scope creep. Secondary literature discussing this cluster of idealist views (which is arguably consistent with Eddington's) will also be prioritized, for example the extensive discussion by Ryckman in the context of

relativity theory, and I would like to know to what extent I can apply the views for the case studies.

# Contents

# 1 How to Read This Document

**This document is a work in progress, and very rough around the edges.** I am updating it when I have time to work on it, and pushing version updates to a github repository. Much of the content is currently typed out quotes from literature and my notes. No doubt a lot will need to be corrected. In this document I am trying to outline where I am at, and where there are still open questions I would like to address. I am sure some of my open questions have been addressed in the literature, but I am including an honest status of my project in the interests of transparency and completeness.

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### 1.1 Generative Model Affidavit

I, Cameron Beebe, swear none of the text in this report was the result of prompting a generative model, such as ChatGPT. As far as I can tell, the content was generated wholly or nearly wholly by my own biological neural nets, and any interactions I have had with a generative model were not particularly insightful or meaningful with respect to the overall content, direction, and literature that is discussed. (In fact, to the contrary, I have been sent on a wild goose chases after an article that did not exist...)

## 2 Project Outline

Within the overarching framework of the cluster of ideas we might summarize as "structural idealism", I think it is warranted to compare what is now called participatory realism in the context of quantum bayesianism with the position advocated by Eddington (and also Cassirer, Weyl, and Husserl as discussed in Ryckman (2005)). What Eddington called "selective subjectivism", for example, should be explicitly be compared detail by detail with participatory realism.

### 2.1 Literature

I've already had a first pass over a few of these, and determined there may be a payoff to spend more time on them in the light of the scope of this proposal. I am also omitting literature in my personal library on philosophy/foundations of physics, thus I'm listing literature that is new to me.

#### 2.1.1 Read or Underway

The core of the ideas in this proposal stem from a long-time interest in the philosophy and foundations of quantum theory, as well as recent discussions on idealism with Ryan Mulally, and the (potential) connections between Husserl and Hegel and the views of Errol Harris. The literature I have prioritized so far on this project:

- Poincaré (1952 [1905]) ✓
- Eddington (1939) ✓
- Ryckman (2005) $\sim \frac{1}{2}$
- Harris (1965) $\sim \frac{1}{2}$
- Weyl (1949) $\sim \frac{1}{2}$
- Cassirer (1963 (1927)) $\sim \frac{1}{2}$

- Riezler (1940) ✓
- Carnap (1966) ✓
- Kant (1977) $\sim \frac{1}{2}$

### 2.1.2 Prospective and Supplementary

As I've done preliminary searching for relevant literature on the project, the following have stood out as important for me to become familiar with:

- M. Cuffaro papers re: Kant/Idealism/Quantum
- Fuchs et al on "participatory realism"
- Weyl collection of papers
- Schilpp (1949), e.g. II.1-II.8, II.21-II.23
- Adorno (1940)
- Clifford (1878)
- Ewing (1974 [1934])
- Kilmister (1994)
- Atmanspacher and Rickles
- Cartwright (1983)
- Torretti (1999)
- van Atten book Brouwer meets Husserl
- Husserl Crisis Collection
- Zahar (2001)
- Zahar (2007)

## 2.2 Deadlines for Papers

- Nov. 15, 2024 Perspectivism and Quantum Mechanics Collection

## 2.3 Structural -isms

The story or dialectical summary that was uncritically familiar to me was that structural realism is a potential route for the scientific realist to respond to the pessimistic meta-induction (PMI). Scientific theory changes, and so the entities or objects referred to by the theory should better be taken lightly, or as useful fictions, or else we should expect to be disillusioned just as we have been in the past for previous theory referents. Humors, phlogiston, and luminiferous aether are typically given examples of theoretical objects that scientists once (mistakenly) thought corresponded to *real* objects *in the world*.

So, if we want to justifiably believe in some objects, which are rather permanent entities that potentially carry the veridical load of our scientific attention, the objects should be somewhat immune to shifts in that attention over time. The focus of our beliefs should therefore be adjusted towards the invariant referents.

Thus, the realist, wanting to uphold the referential connection between theory and reality independent of any particular individual's beliefs or changes in theoretical perspective, focuses attention on the relational objects devoid of individuality. Like turning a kaleidoscope until the edges all line up and the image is distorted the least, the realist supposes that the structural beliefs have lodged themselves into the joints of the real world like a cam. Whatever way our theoretical perspectives may shift, the cams will stay lodged in the bedrock of reality.

Surely these invariant footholds in reality, which we have good reason to suppose will persist through theory change, are the prime candidates to be realists about.

It is a nice story, and it is one that I am sure many like myself would find compelling like the narrative of a fairy tale is compelling. But, of course, the fairy tale is full of untruths, exaggerations, and absurdities, assuming that the frame of mind of the reader will be detached enough. The reader knows there is a distance between reality (whatever that may be) and the fiction depicted. Is there such a gap in structural realist stories?

The purpose of this project is to take a look at how

### 2.3.1 OSR vs. ESR

Two strains that seem to be obvious. Ontic Structural Realism. Epistemic Structural Realism.

Structural Idealism tries to split the difference.

### 2.3.2 From Structural Realism to Selective Subjectivism

Eddington's group theoretical structuralism is combined with a 'subjective' or mental apprehension fundamental to the structure's existence. In other words, if one is trying to be a realist about these structures, for Eddington, then one is asserting the existence of properties in the mind. These properties are about the world, but they are only *in* the world to the extent that the world is constituted *in* the mind. [CITE FRENCH]

One could perhaps distinguish between external structural realism, and internal structural realism. I'm not sure that the external picture makes sense, and neither would Eddington. If one is going to assert fundamental structural properties about the world, it seems to make little sense to locate these properties externally.

Is this an inescapable slide into a form of idealism? Can a structural realist stop at a realist position about structures in the phenomenological border of experience? An empiricist like van Fraassen would, presumably, very much like to find pragmatic reasons to stop not just at the border for *external* existence of objects, but also *internal* or *idealist* objects (whether these objects are structural or not). [CITE LADYMAN]

If 'entities' as they appear to us are defined by (or constituted from) their group structure, as defined mathematically, it seems hard to separate the existence of the theoretical process of the mind in this construction from the apparently external world that supposedly instantiate these structures. In this sense, the idealist (as I am reconstructing tentatively) simply takes seriously the phenomenological border, and this drives not just Eddington but others as well to see it as *necessary* to formulate physics in a rationalist or idealist fashion.

The relations which can be characterized by group operations (e.g. transformational symmetries) are held and understood by a mind, and this mind is what constitutes the symmetries, shifting the epistemological burden onto the empiricist, onto the external structural realist, onto the anti-realist of various stripes. Structural idealism, as exemplified by Eddington, can be considered a form of structural realism, but it aims for even more metaphysical consistency.

While there have been objections to Eddington's particular application of these philosophical and metaphysical motivations, he was not by any means alone in pursuing this strategy.

Cassirer

## 2.4 Objective vs. Subjective Idealism

DOES THIS MAKE SENSE

Idealism, in the present context, should not be confused as identical with some fundamental subjectivism or skepticism about the external world (or even solipsism).

Certain strains of idealism identify substance as being a more sophisticated form of universal *Mind*. I am not an expert, but it is obvious enough on a first pass that many positions are perfectly fine with there being a substance, and unifying the nature of that substance with the apparent subjective nature of the mind in e.g. a physicist's mind. Whether this is tenable and coherent is beyond the current proposal, but there are attempts (e.g. [HEGEL PROBABLY]).

I see an analogy with subjective vs objective Bayesianism. While ultimately both positions can define probabilities as subjective degrees of belief, it is not incoherent to say that there are properties of 'the world' (as it is, or as experienced) which should be reflected by any agent's DoB. I.e., there are, in an important sense, *objective* subjective degrees of belief. There are certain distributions, properties, or relations that *will hold* because of rational constraints regardless of whether these rational conditions are concerned with reality as it is or as it is experienced.

## 2.5 Weyl and Transcendental-Phenomenological Idealism

Perhaps we can characterize positions regarding quantum foundations also along the lines of the foundations of mathematics as outlined by Weyl:

> The stages through which research in the foundations of mathematics has passed in recent times correspond to the three basic possibilities of epistemological attitude. The set-theoretical approach is the stage of *naive realism* which is unaware of the transition from the given to the transcendent. Brouwer represents *idealism*, by demanding the reduction of all truth to the intuitively given. In axiomatic formalism, finally, consciousness

> makes the attempt to 'jump over its own shadow,' to leave behind the stuff of the given, to represent the *transcendent*—but, how could it be otherwise?, only through the *symbol*. Basically, the idealist viewpoint in epistemology has been adhered to by occidental philosophy since Descartes; nevertheless, it has searched again and again in metaphysics for an access to the realm of the absolute, and Kant, who meant to shoot the bolt once and for all, was yet followed by Fichte, Schelling, and Hegel. It cannot be denied that a theoretical desire, incomprehensible from the merely phenomenal point of view, is alive in us which urges toward totality. Mathematics shows that with particular clarity; but it also teaches us that that desire can be fulfilled on one condition only, namely, that we are satisfied with the symbol and renounce the mystical error of expecting the transcendent ever to fall within the lighted circle of our intuition. So far, only in mathematics and physics has symbolical-theoretical construction gained that solidity which makes it compelling for everyone whose mind is open to these sciences. Their philosophical interest is primarily based on this fact.
>
> (Weyl, 1949, p. 65-66)

Ryckman explains Husserl briefly in 5.3, noting Husserl also distinguishes phenomenology (as a form of idealism: transcendental-phenomenological idealism) is very different from e.g. Berkeleyan subjective idealism.

# 3 Structural *Idealism*

Many associate one of the first explicit structural realists in the work of Eddington, who talks about the group theory structure of experience. These structures are persistent and indicate somehow some (Kantian?) transcendental reality that is intersubjective. However, in chapter 7 of Ryckman (2005), he argues (as anyone should after reading Eddington's Eddington (1939)) that it is perhaps not correct to try to pull Eddington's views into the modern tent of structural *realism*. Rather, it is explicitly idealist.

So the objects in our scientific theories, including the formal mathematical objects, and their reference to the world, are expressed not in realist terms but in idealist ones. It isn't that the persistent group structures correspond in some important way to the real structure of the (external) world, but that they are epistemologically normatively compelling from the lights of a subjective agent. The structure is composed of relations partly or wholly determined by the subjective mind, but rejecting solipsism they are intersubjective for other minds, and this intersubjectivity is the core of the persistent structure that is synthesized from a complex of agents. Structural realist persistence of course supposes t

Consider, as Ryckman does for Cassirer, Weyl, and Eddington in the context of relativity, that quantum probabilities may be of a similar status. They are subjective in Eddington's sense, and in the sense of Bayesian subjective degrees of belief, but they are also synthetic or ideal in the sense that they are not arbitrary and they are intersubjective in important ways. They are synthetic objects in the sense that if they refer, they refer to the structure of the physical world which is (per Eddington) an ideal and subjectively determined structure (physical knowledge is a subset of general knowledge which are all determined by conscious experience first and foremost). And not necessarily to the actual physical world, and they do so in virtue of their synthetic mind-dependent character and not because they correctly pick out

## 3.1 Neo-Kantianism?

For sake of brevity, I am lifting a hasty summary from Ladyman:

> The subtler idealism of Kant is a more palatable strategy aimed at avoiding scepticism. Whereas Berkeley collapses the external world onto our impressions to solve the problem of how we get from knowledge of the latter to knowledge of the former, Kant agrees with metaphysical realists that there is a mind-independent world, but he agrees with sceptics that we can't have knowledge of it. Instead, he argues, all our knowledge is of the world as it is *for us*. The world in itself he calls the *noumenal* world, and the world as we experience it he calls the *phenomenal* world. Much of our knowledge is of particular facts about the phenomenal world learned through the senses, but Kant thought that some of our knowledge is *a priori*. Although we can only know the measurements of a specific triangle through our senses, we can know that any triangle we experience will have internal angles summing to 180°, by the use of reason alone. According to Kant, arithmetic, geometry, and Newtonian mechanics are *a priori* forms of knowledge, not about the noumenal world, but about the form our experience must take. (Ladyman, 2002, p. 146)

## 3.2 Poincaré

## 3.3 Cassirer

### 3.3.1 Schilpp

> The transition from the level of sense impressions to the mediated world of spatial representations is made possible by the fact that within the sequence of fluctuating impressions certain constant relations can be fixed and defined and can be asserted as something permanent and independent of the flux itself. Each impression must then prove its objective significance by becoming an integral element of these relations or, rather, of the totality or the system of these relations. Its fusion into the systemic context, and this alone, gives objective meaning to the individual impression. From the very first, every formation of concepts, irrespective of the field in which it

is carried through, points toward one ultimate goal, the one goal of all cognition, namely, the fusion of all specific "positings," of all particular conceptual structures, into one unique and univocal all-comprehensive context of thought. This complete synthesis, this absolute systemic unity, is the goal and driving force impelling the process of cognition—and it is this for Cassirer no less than for Natorp and Cohen. Only at the end of the process of integration is the object of experience fully determined, and only then has truth, absolute and unchanging truth, been attained.

However, before the contents of experience can be integrated as here demanded they must be transformed. The "sense data" or "immediate impressions" must be "resolved" into elements of theoretical thinking. They must be "posited" as such elements. Without this transformation, carried through most effectively in the physical sciences, it would be impossible to formulate the laws which describe and determine the context of experience. And without attaining at least in some measure the systemic unity according to law we could not even speak about "nature." "Nature"—for Cassirer as well as for Natorp—is the unity according to law, the systemic context of all particulars in experience, the totality of progressive integration and objective determination of the "objects" of experience.

In the field of physics the "transformation" referred to involves a change from the quale of immediate impressions to concepts of measure and number. The laws of physics are stateable only with respect to such transformed "objects." And, *vice versa*, the significance of an "object" *as object* now depends exclusively upon the clarity and univocality with which it reflects or represents the law or the determinateness of the context, upon its inclusion within a system of law. Cassirer and Natorp would agree on this point. They differ, however, in their reference to the specific laws determining the content. Natorp based his interpretation exclusively upon Newtonian mechanics; Cassirer, on the other hand, took into consideration more recent developments in the field of physics. For Natorp, "existence" means the complete and absolute determination of an "object" with respect to space and time. Cassirer, contemplating the conclusions reached by quantum mechanics, knows that such complete determination is impossible even in theory, and that we must rest satisfied with a less rigid demand. Let us follow Cassirer's arguments in greater detail.

Cassirer finds that quantum mechanics has confirmed rather than disproved the general position of the neo-Kantians. At least it has cut the ground from under all realistic interpretations of reality. "Things" no longer provide the starting-point of cognition but are the ultimate goal of our interpretations. Laws can no longer be derived from "things" through a process of abstraction; they constitute the basis upon which alone we can assert the existence of "things." The concept of law is logically prior to the concept of thing. Our knowledge of "nature" extends only so far as does our concept of law. Objective "reality" can be asserted only in so far as there is order in accordance with law. "Things" are the "limit," the ultimate goal toward which the process of cognition moves but which it never actually reaches. The "object" of experience is not something completely determined in itself but something determinable without end in the process of cognition. Except through the medium of laws, no "object" is "given" or known.

So far Cassirer is in complete agreement with Paul Natorp. But, whereas Natorp believed that Newtonian mechanics provided the means, in principle, for a complete determination of an object in space-time, Cassirer points out that this thesis involves an over-simplication of the facts. "Field" theories, the phenomena of "entropy," and the "uncertainty relations" of quantum mechanics involve problems which cannot be solved within the framework of classical mechanics.

"Fields of forces," for example, are not "entities" in the classical sense of "material bodies." As far as such "fields" are concerned, "mass" can no longer be regarded as "ponderable reality" but must be resolved into electric "charges." The whole conception of a "physical body" must therefore be redefined. The "field" is not a "thing," but a "system of effects."

Similarly, the "atom" can no longer be conceived as a "thing." It has turned out to be an intricate "system" of dynamic relations and can be described only through the laws which express its effects. To assert that "electrons" exist within the atom and that they move in definite orbits actually means only that certain laws, formulated to describe observable phenomena of cathode rays and line spectra, are valid; that they are descriptive of the phenomena. Neither the "electrons" nor their "orbits" are "things-in-themselves," mere "stuff," "given" to us prior to cognition. They are, rather, terminals within the integrative process of cognition. The electron "exists" only relative to a "field," relative, that is, to a "system of effects," and as a particular "place" of that field.

The laws of classical mechanics are so formulated that they are valid only under two assumptions. First, all physical objects must be reducible to mass-points; and, second, these mass-points must be definitely localizable in space at any given time and with any given momentum. Quantum mechanics shows, however, that it is impossible to determine the exact location of an electron if we determine its momentum at the same time with an accuracy required by the classical laws. The impossibility here involved is not merely of a practical nature but is one in principle. We must therefore conclude that the individuality of an electron can no longer be defined or determined as that of a "thing" in space and time. If we continue to speak about in-

> dividual electrons we can do so only indirectly. That is, we can speak of their "individuality," not as something "given," but only as constituting specific focal points of relations, "intersections" within a system of effects. All formulations of quantum mechanics are systemic formulations concerning functional dependencies, not statements concerning individual "things" called "electrons." (Werkmeister, 1949, p. 773-776)

### 3.3.2 The Individual and the Cosmos in Renaissance Philosophy

Cassirer expositing Cusanus' philosophy. Platonic division/duality (can't get to ideal form from empirical, but appearances point to form). Sensible/intelligible.

> A physical circle or sphere never corresponds to the pure concept of either but rather must necessarily remain far behind it. We can refer the physical to the ideal; we can determine that a given, visible being corresponds with greater or less exactitude to the concept of the sphere or of the circle which is invisible *per se*. But the essential difference between archetype and copy can never be overcome. For what characterizes the pure truth of the archetype is precisely that it can never be 'more' or 'less'. To take away or to think away even the tiniest part of it is to destroy it in its essence. The sensible, on the other hand, not only suffers indeterminateness, but possesses therein its very nature. Insofar as being is attributable to it, the sensible 'is' only within this limitlessness of becoming, in this oscillating between being this-way and being that-way. (Cassirer, 1963 (1927), p. 21)

Cusanus talks about finite/infinite and Maximum.
participation(!)

> The division that separates the sensible from the intelligible, sense experience and logic from metaphysics, does not cut through the vital nerve of experience itself; indeed, precisely this division guarantees the validity of experience. With the same decisiveness and acuteness that he applied to the idea of 'separation', Cusanus now works out the idea of 'participation'. Far from excluding each other, separation and participation, [GREEK GOES HERE], can only be thought of *through* and *in relation* to each other. In the definition of empirical knowledge, *both* elements are necessarily posited and connected with each other. For no empirical knowledge is possible that is not related to an ideal being and to an ideal being-thus. But empirical knowledge does not simply contain the truth of the ideal, nor does it comprehend that truth as one of its constituent elements. The character of empirical knowledge is, as we have seen, its limitless determinability; the character of the ideal is its delimitation, its necessary and unequivocal *determinateness* which gives to determinability a definite form and

> direction. Thus, everything conditioned and finite aims at the unconditioned, without ever being able to attain it. That is the second basic thought in the *docta ignorantia*. In relation to theology, this concept affirms the idea of knowing ignorance; with relation to experience and empirical knowledge, it affirms the idea of ignorant *knowledge*. Experience contains genuine knowledge; but certainly this knowledge must recognize that, although it may go far, it can only reach a relative aim and end, never an absolute. And it must further recognize that in this realm of the relative there can be no exactness, no *praecisio*: rather, every pronouncement and every measurement, be it ever so precise, can and will be superseded by another, more precise. In this sense, all of our empirical knowledge remains a 'probability', an attempt, a hypothesis which, from the very beginning, is reconciled to being superseded by better, more exact attempts. Through this concept of probability, of *conjecture*, the notion of the eternal 'otherness' of idea and appearance is joined with the notion of the participation of the appearance in the idea. Only this union renders possible Cusanus' definition of empirical knowledge: 'conjectura est positiva assertio in alteritate veritatem uti est participans.' (Cassirer, 1963 (1927), p. 22-23)

- Me: non-local hidden variable theory (god) interpretation of some of Cusanus cosmology and individual relationship to god comments. From Cusanus *De visione Dei* and example of portrait with all-angle looking eyes, and a discussion of what might be considered similar to theory-laden observation or an "observer effect"

> At the beginning of the work, he recalls the self-portrait of Rogier van der Weyden, which he had seen in the town hall at Brussels. The portrait possessed a peculiar property: it seemed to look directly at the viewer, no matter where he stood. Imagine a portrait of this kind in the sacristy of a cloister, hung perhaps on the north wall, and the monks standing in a semicircle around it. Each of them will believe that the eye in the picture is looking directly at him. Not only must we attribute to the picture the ability simultaneously to face south, west, and east, but also a triple simultaneous movement; for while it is still for the stationary observer, it follows the moving one with its glance, so that if one of the brothers walks from east to west and another from west to east, it participates in both these opposite movements. We see, therefore, that this one and the same immobile face can move in such a way towards the east, that it simultaneously moves towards the west, and similarly, moves to the north and simultaneously to the south; a face that, standing fixed in one place, is simultaneously in all other places and which, while it is involved in one movement, is simultaneously involved in all the others.
>
> This represents for us, in a sensible parable, the nature of the basic relationship between God, the all-encompassing being,

and the being of the finite, the ultimate particular. Each particular and individual being has an immediate relationship to God; it stands, as it were, face to face with Him. But the true sense of the divine first discloses itself when the mind no longer remains standing at *one* of these relationships, nor even at their simple total, but rather collects them all in the unity of a vision, a *visio intellectualis.* Then we can understand that it is absurd for us even to want to think the absolute in itself without such a determination through an individual point of view. But we also understand that none of these points of view has any priority, because only the concrete totality of them can mediate a true picture of the Whole for us. In this whole every single viewpoint is included and recognized both in its accidentality and its necessity. And of course, every view of God is as much conditioned by the nature of the 'object' as by the nature of the 'subject'; every view includes the *thing seen* as well as the manner and the direction of the *seeing.* Each man can see himself only in God; just as he can see God only in himself. This pure *interpenetration* cannot be adequately characterized by any quantitative expression, or by any expression that is tied to the opposition of the 'part' to the 'whole'. 'Your true face is free of all limitations. It is neither of this size, nor of this shape, neither spatial nor temporal, for it is itself the absolute form, the face of faces. And when I consider how this face is the truth and the most adequate measure of all faces, I am astounded. For this face, that is the truth of all faces, is not this or that large, has no "more" or "less", nor is it like any other; as the absolute, it transcends all measure. So I see, oh Lord, that your face precedes every visible face, that it is the truth and the model of all faces. Therefore, any face that looks into yours sees nothing different from itself, because it sees its own truth. When I look at this picture from the east, it seems to me that I am not looking at it but it at me; the same is true when I look at it from the south or the west. Likewise, your face is turned to all who look at you. Whoever looks at you with love feels you looking lovingly at him—and the more he tries to look upon you with love, the more lovingly does your face look back at him. Whoever looks at you in anger finds your look angry; whoever looks at you joyfully finds you joyful. For as everything appears red to the physical eye when it looks through a red glass, so the spiritual eye, in its limitedness, sees you, the goal and object of the mind's observation, according to the nature of its own limitation. Man is capable only of human judgement. ... If the lion attributed a face to you, he would attribute that of a lion, the ox that of an ox, and the eagle that of an eagle. Ah, God, how wonderful is your face: the youth, if he would conceive of it, must imagine it as young, the man as male, the old man as old. In all faces the face of faces appears, veiled, as in an enigma—but it cannot be seen uncovered unless it be when we go beyond all faces to that

> secret, dark silence, wherein nothing remains of the knowledge and the concept of face.' [NOTE: TRANSLATED CASSIRER EARLY, THEN TRANSLATED CUSANUS] (Cassirer, 1963 (1927), p. 31-33)

We could say that each individual "particle" in a NLHVT has an immediate relationship to a motion from a tangent space (e.g. vibrating fluid bath example, driven from speaker "outside" of bath). Non-local to the droplet, the speaker is imparting and participating in the sustaining energy and tension and other properties of the fluid which characterize the trajectories and correlations and behaviors of the individual (or many) droplets.

> Cusanus borrows the *terminology* of the old 'division of the world', the *divisio naturae* made by Johannes Scotus Erigena. Scotus Erigena distinguished the nature which creates and is not created from (a) that which is created and creates, (b) that which is created and does not create, and finally, (c) that which neither creates nor is created. But this terminology now has an essentially new meaning. When Erigena speaks of the created and at the same time creating being, he is referring to the non-temporal emergence of things from their Ideas, i.e., from their eternal prototypes and archetypes. But Cusanus does not consider ideas to be creative forces in the Neo-Platonic sense. Instead, he requires a *concrete* subject as the central point and as the point of departure for all truly creative activity. And this subject, according to him, can exist nowhere but in the mind of man. The first and foremost result of this point of view is a new version of the *theory of knowledge*. Genuine and true knowledge is not merely directed towards a simple reproduction of reality; rather, it always represents a specific direction of intellectual activity. The necessity we recognize in science, and especially inmathematics, is due to this free activity. The mind attains genuine insight not when it reproduces external existence, but only when it 'explicates' itself and its own nature. Within itself, the mind finds the simple concept and 'principle' of the point; and from this, after continuously repeated movements, it produces the line, the surface, and the entire world of extension. Within itself, the mind finds the simple idea of 'now', out of which unfolds the infinity of temporal series. And just as the basic forms of intuition—space and time—are in this sense 'implied' by the mind, so too are the concepts of number and of size, as well as all logical and mathematical categories. In the development of these categories the mind creates arithmetic, geometry, music, and astronomy. In fact, everything logical—the ten predicates, the five universals, etc.—is included in this basic power of the mind. It is the necessary prerequisite to all 'discretion', i.e., all categorization of multiplicity according to species and classes; and it is the necessary prerequisite to the possibility of tracing the empirical-mutable back to strictly defined laws. In this foundation of the sciences the creative power

> of the rational soul reveals itself in both its basic aspects; by virtue of this power the human mind enters into time and yet remains above time understood as mere succession. For, as the origin and creator of science, the human mind is not so much in time, as time is in it. The mind itself, by virtue of its power of discretion, creates definite time intervals and time divisions and draws the boundaries between the hours, months, and years. Just as all essential differences come from God, so all conceptual differences emerge from the human intellect. And thus it is the primary source of that harmony which is always the resolution of *opposites*. In Ptolemy, the human intellect created the astrolabe; in Orpheus, the lyre. Invention does not come from without; it is simply the material and sensible realization of the concept.
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> Time is to the soul as the eye is to vision. Time is the organ the soul uses to fulfil its basic function, which is to order and sift the multiplicity, i.e., that which is variously dispersed. With this idealist conception, Cusanus lays the groundwork for the modern concept of time in mathematics and physics—a concept that will later emerge in the works of Kepler and in Leibniz. Furthermore, Cusanus initiates therewith a new view and a new evaluation of *history*. The interpretation of historical existence will now also be subjected to the fundamental antithesis of *complicatio* and *explicatio*. This existence, too, is no mere external 'happening', but rather represents the *activity* most appropriate to man. Only in history can man prove himself to be truly creative and free. Here it becomes clear that, despite the course of fortuitous events, and despite the force of external circumstances, man still remains the 'created God'. Man is completely enclosed in time; indeed, he is completely entrapped in the particularity of any given moment and completely enmeshed in the conditions of that moment; and yet, despite all this, man proves to be a *deus occasionatus*. He remains enclosed within his own being, never transgressing the limits of his own specifically human nature. But inasmuch as he develops and expresses every facet of his nature, man represents the divine in the form and within the limits of the human. Like every being, man has the right to fulfil and to realize *his* form. He may, he even must affirm this form, this limitation of his; for only by doing that can he honour and love God within it and give evidence of the purity of his own origin.
>
> (Cassirer, 1963 (1927), p. 40-43)

-Latter sounds like Hegel?

- Humans as determiners of value and religious turn (from inherent worthlessness/original sin)

> And although man's being is completely derived from God, there is nevertheless a sphere in which he functions as a free creator and in which he reigns autonomously. This is the sphere

of *value*. Without human nature there would be no such thing as value, i.e., there would be no principle for evaluating things according to their greater or lesser perfection. Imagine human nature removed from the world; with it will disappear every value-preference of one thing above another. God is, of course, the Master who strikes the coins; but the human mind determines how much they are worth. 'For although the human intellect does not give being to the value, there would nevertheless be no distinctions in value without it. Thus, if one leaves the intellect aside, one cannot know whether value exists. Without the power of judgment and of comparison, every evaluation ceases to exist, and with it value would also cease. Wherewith we see how precious is the mind, for without it, everything in creation would be without value. When God wanted to give value to his work, he had to create, besides the other things, the intellectual nature.' [CUSANUS QUOTE]

These sentences completely express the religious humanism and the religious optimism of Cusanus. How could that which is the principle and the source of value be worthless? How could it be lost to corruption and to sin? Just as nature was earlier raised up to God through the mediation of humanity, so now human culture has found its true theodicy. Culture confirms the freedom of the human spirit, which is the seal of its divinity. The spirit of asceticism is overcome; mistrust of the world disappears. The mind can come to know itself and to measure its own powers only by devoting itself completely and unconditionally to the world. Even sensible nature and sense-knowledge are no longer merely base things, because, in fact, they provide the first impulse and stimulus for all intellectual activity. The mind is the living illustration of eternal and infinite wisdom; but until it is stimulated to movement by that admiration which arises from contemplation of the sensible, it is, so to speak, asleep within us. This movement, which begins and ends in the mind itself, must pass through the world of the senses. The mind always 'assimilates' itself to the sensible world; mind becomes sight when presented with colour, hearing when presented with sounds. This descent into the world of perception is now no longer considered a decadence, a kind of sinful fall of knowledge; instead, it accomplishes the ascent of the sense-world itself which now raises itself from multiplicity to unity, from limitations to generality, from confusion to clarity.

Cusanus summarizes all of these thoughts in a pregnant metaphor. The human mind, he writes, is a divine seed that comprehends in its simple essence the totality of everything knowable; but in order for this seed to blossom and to bear fruit, it must be planted in the soil of the sensible world. The basic character of that 'copulative theology' sought by Cusanus lies in this reconciliation of mind and nature, of intellect and sense. With

> complete consciousness of method, he now opposes this to all theology that is merely 'disjunctive', negating, and divisive. (Cassirer, 1963 (1927), p. 44-45)

- Phenomenological turn?

## 3.4 Ryckman Reign of Relativity

- Everyone thought GR confirmed their own metaphysics... Logical empiricist interpretation of GR as refuting Kantianism and synthetic apriori. Schlick (Ph.D. under Planck, supported by Carnap and Reichenbach and Neurath) as authority.

> It will be seen that, however rhetorically useful, the claim that general relativity sounded the death knell of "the Kantian position" follows only if, as Schlick did, one ignored important neo-Kantian developments of Kant's thought as well as many of the most significant developments in relativity theory in the period 1915-1925. Schlick's judgment was narrowly based and by no means universally shared. To sample but one countering opinion, the Nobel prize winner and fellow Planck student Max von Laue stated, in the first actual textbook on general relativity in 1921, that Kantian epistemology was confirmed by the new theory, although "not every sentence of *The Critique of Pure Reason*" could be regarded as sacrosanct. Yet as pious children of this world, to borrow an expression of Hermann Weyl's, we know that if an assertion is repeated sufficiently often, while remaining unchallenged in the forum of debate, it commonly enters into currency as accepted background knowledge. Certainly the claim that general relativity decisively refuted transcendental idealism *tout à coup* is strewn through the literature of logical empiricism, percolating beyond to its prodigal progeny. Nor was it explicitly challenged in philosophical circles by anyone having the *gravitas* of authority possessed by Schlick, and then by Reichenbach, who would take over the mantle of authority on relativity theory within logical empiricism, as Schlick fell under the influence of Wittgenstein and turned away from philosophical investigations of physics. As a result, the allegation has remained unimpeached amidst the triple assault that proved fatal to the rest of logical empiricism: Quine's attack on the analytic-synthetic distinction, Hanson's and Toulmin's on the observational-theoretic distinction, and Kuhn's critique of logical empiricism's inductivism and its method of rational reconstruction. So it was that, when scientific realism began again to stir in late 1950s and early 1960s, as it always will, against the thin gruel of positivism and instrumentalism, there were scarcely any parties to the conflict who grasped the possibility of an alternative to *both* realism *and* instrumentalism or, beginning in the 1970s, to realism and the resuscitated bogey of "relativism".

> That alternative already existed, and it assumed several different, but related forms, in the "reign of relativity" from 1915 to 1925 through the efforts of Ernst Cassirer, Hermann Weyl, and Arthur Stanley Eddington. It is a philosophy that exists only in various incomplete realizations having at most a "family resemblance" among themselves. In this book it is called *transcendental idealism*, and although Kant is the paramount figure historically, its development by no means ended with Kant, as Cassirer, Husserl, Weyl, and others have shown. I will therefore use the term "transcendental idealism" far more broadly than is customary in most philosophical discussions. But for present purposes, the core constituent of the doctrine concerns the "transcendental constitution of objectivity" in fundamental physical theory, according to a "transcendental postulate", in broad generality affirming that "[a] nature is not thinkable apart from the coexistent subjects capable of experiencing that nature". The details of the various and differing conceptions of "transcendental constitution" in general relativity are described in detail below in discussions of Cassirer, Weyl, and Eddington.
>
> (Ryckman, 2005, p. 5-6)

- Cassirer neo-Kantianism not well known/translated

> Yet the Marburg tradition of neo-Kantianism, within which Cassirer had been educated, long before rejected the original Kantian distinction between the mental faculties of sensibility and understanding, and on this ground Cassirer could reinterpret the doctrine of pure intuition in *conceptual terms* as pertaining only to "the order in general of coexistence and succession". In his 1921 book of "epistemological reflections" on Einstein's theory of relativity, as discussed in chapter 2, he was in a position to grasp what is arguably the most philosophically significant aspect of general relativity, the principle of general covariance, as a "regulative principle" and constituent part of an ideal of physical objectivity from which all traces of "anthropomorphic" subjectivity have been removed. In an enlightened understanding (which is fully in the spirit of Cassirer's discussion), this is the requirement that dynamical laws must be formulated without a "background" space and time, a constitutive requirement of general relativity, but utterly violated in the standard operator formalism of quantum field theory.
>
> (Ryckman, 2005, p. 7)

- Weyl formulating advances towards GR in Husserlian transcendental phenomenology (contributing to lack of translations/understanding/wide awareness), debating foundations of math with Hilbert and Brouwer, Lie groups

- EPISTEMOLOGY IN THE TANGENT SPACE

- Overview:

In the spring of 1918, Weyl had proposed a geometric unification of gravitation and electromagnetism, a further step along the road of general relativity. The basis of the unification was a "pure infinitesimal geometry" permitting neither direct comparisons "at a distance" of direction nor, unlike the Riemannian geometry of Einstein's theory, of magnitude. Within such a geometry, Weyl recast Einstein's theory together with electromagnetism on the privileged epistemological basis of fundamental differential geometric notions having immediate validity only in the tangent space attached to each manifold point $P$, corresponding to a localized space of intuition. In opposition to the scientific realism of his day, and in a characteristically distinctive fashion combining Husserlian "essential analysis" of space and time as "forms of intuition" with mathematical construction, Weyl sought in this way to provide a transcendental-phenomenological account of the constitution of the *sense* of the objective world of relativity theory, the sense of a "being *for* consciousness". However, Weyl's epistemological motivations were expressed in the obscure language of Husserl, and his theory, thus misunderstood, was critically rejected on both physical and general methodological grounds.
The ties of Weyl's theory to observation *are* indirect; and, if we accept Weyl's recognition of the existence of a "natural gauge" of the world, simply presupposed in Einstein's posit of rods and clocks, they are also present in general relativity. The values of the metric at a point can be determined through the use of freely falling neutral "test particles" and by observing the arrival of light at points in the immediate neighborhood of that point. However, neither of these hypotheses, of "freely falling" test particles or of the behavior of light in a gravitational field, is independent of gravitational theory. Both can be derived from the Einstein field equations for particular models of space-time. For this reason, as Weyl repeatedly stressed regarding Einstein's theory, only the theory as a whole, comprising physics, geometry, and mechanics, can be confronted with observation. If that is so, then, as Schlick put it, there is no place for an empiricism worthy of the name to gain a place to stand. A different epistemology of science would have to be found. For without such an empiricist Archimedean point for general relativity, allegedly endorsed by Einstein and therefore to be retained at all cost, there could be no room for subsequent logical empiricist methodology of science to thrive. So too for the fruits of its analysis of science: an empiricist semantics for theoretical terms and sentences, the empiricist criterion of cognitive meaning, and the positivist rhetoric that any nonempirical statement was either analytic or meaningless "metaphysics". When, a full generation later, these invidious doctrines finally faded from the scene under assault from different quarters, the lack of a clear alternative was perhaps noticeable only to those whose horizon stretched back to the

philosophically fecund first years of general relativity. In its absence came the inevitable backlash of scientific realism and its several antitheses.

As it happened, of course, such an epistemology of science was developed, in part in bits and pieces of Weyl's mathematical and physical oeuvre and, in broader generality, in his monograph on philosophy mathematics and natural science in 1926. By then, Weyl had returned for good, except for a brief excursion into the new quantum theory, to purely mathematical pursuits. This left the playing field of "scientific philosophy" open to Reichenbach's "constructive axiomatization" of the theory of relativity (1924), where the mechanism of "coordiative definitions" took over from Schlick's still-born "empiricism with constitutive principles", and in this guise the new empiricist analysis of scientific theories acquired its mature form. After the "linguistic turn" of the early 1930s, the discourse became one of two vocabularies, or languages, "theoretical" and "observational", and of defining the former in terms of the latter, eventually through "meaning postulates". Citing Einstein as a guiding spirit, the logical empiricists claimed the authority of philosophical expertise regarding relativity theory, a title they are still perceived in many circles to hold, as it were, from beyond the grave, and despite Einstein's later public disavowals of their core positions. Ironically, Einstein's own philosophical evolution after 1915 carried him further and further away from the empiricism Schlick viewed as present in general relativity and toward neo-Kantian conceptions and the mathematical speculative methodology for which he had once chastised Weyl.

One more figure played a central role in the possible alternative tradition to logical empiricism and its successors that may be loosely associated with "the Kantian position". If one were to name the grand masters of general relativity in the early 1920s, besides the names of Einstein, Weyl, Hilbert, the young Wolfgang Pauli, Jr., and on the mathematical side, Élie Cartan and George D Birkhoff, only that of Arthur Stanley Eddington remains. Eddington, Plumian Professor of Astronomy at Cambridge since 1914, was already an internationally known astronomer in 1915. He would become, in the assessment of S. Chandrasekhar, "the most distinguished astrophysicist of his time". He was also the first in Britain to have any detailed knowledge of Einstein's new theory during the first World War. With his mathematical skills, he was also a highly creative relativity theorist. In fact, he was so connected to the new theory, as exponent, expositor, and theoretician, that he became known in Britain as "the apostle of relativity", and we have it from no less a source than Paul Dirac that in the early 1920s, his name, not Einstein's, was most closely linked there with the new theory.

Eddington was also heretical enough to accept Weyl's generalization of Einstein's theory and to generalize it further, for epistemological reasons essentially similar to Weyl's. Weyl had reconstructed the objective world of relativity physics within a "purely infinitesimal geometry", corresponding to the phenomenological standpoint of methodological solipsism wherein only such linear relations as could be present to an infinitesimally bounded spatio-temporal intuition were immediately evident. Eddington sought the same goal of constituting the "real world of physics" by reconstructing relativity theory within a differential geometry capable of yielding only objects that are a "synthesis of all aspects" present to all conceivable observers. The external world of physics might be *defined* in this way as a world conceived "from the viewpoint of no one in particular", a standpoint both necessary and sufficient for *objective* representation in physics. The epistemological significance of relativity theory lay in showing that the attempt to portray the physical world from this impersonal perspective resulted in its geometrization. In turn, the physical knowledge captured in such a portrayal is knowledge only of the world's structure. Physics *could be* about *no* other world than that expressly incorporating all viewpoints at once, an "absolute world" as opposed to the "relative" world of each individual perspective, that is, any "conceivable observer". The relation between the relative and the absolute is mathematically captured by the tensor calculus and physical knowledge accordingly must be represented in the form of tensor identities through a method Eddington called "world building".

As we shall see in chapters 7 and 8, Eddington was adamantly convinced that Weyl's "epistemological principle of the relativity of magnitude" (the origin of the modern "gauge principle") was an essential addition to the outlook of relativity of continually incorporating additional "points of view" into physics. But, in the intricacies of Weyl's transcendental-phenomenological framework of constitution, Eddington judged, Weyl had erred. For Weyl had not made clear that his geometry was *ideal* and purely mathematical, a geometrical skeleton for the "graphical representation" of existing physics from "the point of view of no one in particular". Eddington's idea, therefore, was to develop such an ideal geometry independently of physics, basing it on a purely local and *nonmetrical* relation of comparison, a symmetric linear connection. In a geometry based on such an "affine connection", rather than a metric, a more general kind of invariant than tensors can be "built up"; nonetheless, only one of these is mathematically identical to the metric tensor of Einstein's theory. Setting the two equivalent, one can proceed to "graphically represent" the tensorial quantities of existing physical theory, gravitation, and electromagnetism. The ideal geometry of Eddington's affine field theory then shows that Einstein's geometry, not Weyl's, is exact, but this is a demonstration from the most general "the point of view of no one in particular" available to a continuum theory in 1921. Eddington's theory is not a physical hypothesis but an explicit attempt to cast light on the origin and significance of the great field laws of gravitation and electromagnetism. Within the epistemological reconstruction of "world building", the differential geometric invariants appearing in these laws are structures selected from a vast number of other possible invariant structures derivable from given axioms of "primitive relation structure". Mind is the principle of selection; in particular, it is mind's interest in "permanence" that identifies the Einstein curvature tensor, regarded in "world building" as a purely geometrical quantity, with the physical energy-momentum tensor of matter. Hence, Einstein's law of gravitation for "matter" sources is simply a world geometric definition of matter. In the absence of matter, Einstein's law of gravitation for empty space (as amended with the cosmological constant) is a statement that the world is "self-gauging", that rods and clocks, apparatus of course part of the world (and explicitly so, in "world building"), are used in measuring the world. As Eddington pointed out later on, there [are] similarities between his view of physical knowledge and those of Kant. One difference, certainly, is that Eddington's account of the constitution of physical objectivity simply assumes relativity theory, where Kant had assumed Newton. I shall show that the similarities are considerably more noticeable when set in the context of transcendental idealism, more broadly conceived.

In the pages that follow it will be seen that the emergence of logical empiricism in the 1930s as the apotheosis of "scientific philosophy" (a reputation still widely upheld) had little to do with its purported expertise regarding relativity theory but was achieved largely through rhetoric and successful propaganda rather than through philosophical argument. Its most (and still) alluring appeal lay in a self-styled contrast of enlightenment versus reaction, and in its identification of science as the primary instrument of human advance from the dreary annals of superstition, dogma, and fanaticism that permeate human history. Its great myths even today have hardly been questioned: that relativity theory had overthrown any form of "Kantianism"; that "empiricism" stood opposed only to an antiscientific and dogmatic "rationalism"; that logical empiricism, itself modeled on the methodology of relativity theory, was *d'accord* with modern physics (relativity theory and quantum mechanics). The doctrinal triumph of logical empiricist philosophy of science itself, of course, was not lasting. Its employment of a new favorite tool, symbolic logic, as the *organon* of philosophy of science, an *ersatz* for actual knowledge of science, still succeeds to some extent in reviving the desiccated corpse of logical empiricism through the boom-and-bust cottage industry of mainstream philosophy of science. But even symbolic logic could not save "the received view" from the inevitable cognitive discord induced by a glaring awareness of the enormous gap between its rational reconstructive portrait of science and that of a new history of science, reinvigorated by Koyré and, above all, Kuhn, as was recognized by Hempel in his last writings. Rather, these myths live on institutionally, subconsciously continuing in the sclerotic distinction between "analytic" and "continental" philosophy. Surmounting that artificial distinction, the family resemblance among the "transcendental idealisms" of Cassirer, Weyl and Eddington contains the seeds of promise for an actual philosophical understanding of the *non plus ultra* role of abstract mathematics in fundamental physical theory.

In 1931, P.A.M. Dirac prefaced his celebrated paper on magnetic monopoles with several remarks that announce a sea change in the methodology of theoretical physics. Stating that drastic revision of fundamental concepts may be required to address the current problems of theoretical physics, Dirac nonetheless cautioned that such a transformation in outlook is likely to be beyond the power of human intelligence to directly grasp the required new ideas without the assistance of mathematical speculation. In the face of these cognitive limitations, a more indirect approach is suggested, wherein "the most powerful method of advance" would be

> to perfect and generalize the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities (by a process like Eddington's Principle of Identification).

Now this principle, as Eddington himself made clear, was directly inspired by Weyl's mathematical identification of the vector and tensor structures of his purely infinitesimal world geometry with those of gravitation and electromagnetism. That being the case, Weyl's 1918 theory can be justly regarded as the locus of the modern revival of the method of *a priori* mathematical conjecture in fundamental physical theory. How such a method can ever be fruitful in constructing well-confirmed fundamental physical theories has long appeared a mystery, for which extreme solutions (such as Platonism) have been seriously proposed. The argument of this book suggests that less desperate measures may have been overlooked. The work of Cassirer, Weyl, and Eddington on general relativity provides a needed "Copernican about-face" on the question, by demonstrating how and why *a priori* constraints of reasonableness can be imposed on nature without proudly (but naively) presuming them to be inherent in nature itself. They did not leave us a fully worked out presentation of an alternative epistemology of science, each going on to other endeavors that

> effectively removed their work from the sphere of the familiar that so bounds human understanding, even in philosophy. In all likelihood, such a completed account doesn't, or shouldn't, exist except as an ideal guiding inquiry. What they did leave has been allowed here to "speak for itself", a presentation that comes at times at the cost of effusive length, but that appeared necessary in the light of the unfamiliarity, and even inaccessibility, of many of their core writings. Perhaps any further development, any "future music", to quote Weyl again, might be well advised to at least consider what they once had to say. (Ryckman, 2005, p. 8-12)

- ch. 2 general covariance, philosophy of science followed Reichenbach's neo-Kantian amendments instead of Cassirer's. Following Schlick, Reichenbach's coordination principles/definitions between math and empirical observations

> Following Schlick's lead, Reichenbach came to see coordination principles as playing so "thin" a "constitutive" role as to be indistinguishable from stipulations that certain empirically accessible objects and processes are (approximately) described by core relations of the mathematical framework of a physical theory. Yet the most significant aspect of this reevaluation is that it consolidated a fundamental shift of epistemological discussion initiated by Schlick's influential *definition* of cognition as a "univocal coordination" in 1918. Subsequently, philosophers of science would have progressively less and less understanding of the relevance of any account of "physical objectivity" in accordance with conformity to presupposed "conditions of possible experience". Instead, the relevant epistemological issues of interest concerned the applicability of an uninterpreted mathematical formalism to an empirically given concrete subject matter or, in terms more redolent of mature logical empiricism, the semantical rules through which a mathematical framework acquires empirical content, and so "cognitive meaning" in physics.
>
> It is my contention that Cassirer's different articulation of a role for the relativized *a priori* has been rather amply confirmed in the subsequent development of physical theory. Namely, Cassirer expressly pinpointed the specific "meta-empirical" standing of invariance principles in physical theory, in particular, emphasizing that the principle of general covariance significantly transformed the concept of "objectivity" in physics. In this role, principles of invariance have *both* a "constitutive" *and* an ideal "regulative" *a priori* significance. To be sure, in Kant's account of "constitution of the object of knowledge" from the two independent contributions of sensibility and understanding, "regulative" principles, systematic ideals of unity of the "higher faculty" of pure reason, can play no direct "constitutive" role. Hence, one salient division between Cassirer's and Reichenbach's epistemological analysis of relativity theory

> pertains to just where modification or amplification of original Kantian doctrine is required in order to retain a constitutive but nonconventional meaning of the "relativized *a priori*". Ultimately, the different determinations reduce to opposing answers to the question of *whether there are nonanalytic* a priori *elements in physical theory*. As shown in chapter 3, due in large measure to Schlick's considerable authority regarding the new theory as well as his rhetorical ability to pose the issue in his own terms, this was a very short debate that, in the eyes of "scientific philosophy", Cassirer lost.
>
> In this chapter, and in those subsequent to it, I venture to challenge this received wisdom on grounds internal to various epistemological analyses of the theory of general relativity, all carried out within a broader genus of "transcendental idealism" than is to be explicitly found in Kant. Here, attention is directed toward how, constrained by the resources afforded by their competing revisions of Kantian doctrine, Reichenbach and Cassirer respectively assessed the implications of the principle of general covariance for the "epistemology of physical science". Of course, the matter of precisely what, if any, physical significance this principle may have, has long been perhaps the most controversial issue in the foundations of general relativity. Einstein's considered judgment, that the principle is not physically vacuous but has "considerable heuristic force" in the construction of physical theories, was explicitly recognized by Cassirer, but also has been adopted (or insisted upon) by several leading members of the current generation of theorists of quantum gravity. Following Cassirer's lead, I argue that, in epistemological terms, the "heuristic force" of general covariance is located in the principle's "constitutive" significance in constraining the concept of possible object in field theory to objects that are "background independent". This is to be understood as the expression of an "ideal of reason", namely, that "the objects of which the world is made do not live over a stage and do not live on space-time; they live, so to say, over each other's shoulders"[Rovelli quote, 2001, p. 108], a goal not yet attained in the present state of fundamental theory.
>
> (Ryckman, 2005, p. 14-15)

- hole argument, Einstein's general covariance with reference frames was noted by Kretschmann not to actually say something about physics but about mathematical restriction

> General covariance, if mere coordinate generality is intended, is merely a formal constraint on the theory's mathematical form, having *per se* nothing to do with a "principle of general relativity" or with the theory of gravitation. In a purely formal sense, an equation is generally covariant just in case it preserves its form (is "covariant") under arbitrary transformation from one coordinate chart to another, $\bar{x} \Rightarrow \bar{x}'$. Moreover,

> Kretschmann claimed that since, as Einstein affirmed, the totality of physical experience must ultimately refer to coincidences, any physical theory that preserves the lawful connections among coincidences can be written in generally covariant form, subject only to the introduction of additional variables. If it had been Einstein's intent to lend physical significance to general covariance, he had not succeeded in distinguishing that meaning from this purely formal constraint. In a response the next year, Einstein admitted the correctness of Kretschmann's objection, nonetheless maintaining that the "relativity principle", according to which laws of nature find "their sole natural expression in generally covariant equations", has a "significant heuristic force". By contrast, he argued, if one were to write down the equations of Newtonian gravitational mechanics in (four-dimensional) generally covariant form, the result would be seen to be so unnatural as to be readily excluded from theoretical consideration. One can only speculate about Einstein's criteria of theoretical naturalness, but in any case, his reply has been widely judged inadequate, or at least as an essential backpedaling, from his earlier claims on behalf of general covariance.
>
> (Ryckman, 2005, p. 17)

- hole argument, active general covariance idea (diffeomorphism moving points) vs. passive general covariance (passive being just like a symmetry property of a tensor, relabeling w/o physical implications)

> Clearly, Einstein's difficulty pertained to the interpretation of the diffeomorphic point transformations rather than to any trivial confusion regarding coordinate transformations, as has been frequently alleged. In late 1915, Einstein realized the faulty presupposition required for the discordant conclusion that these distinct solutions correspond to different *physical* situations. The answer lay in seeing that the coordinates $x^\sigma$ have no metrical or other physical meaning but serve as essentially arbitrary labels for space-time points, required for the operations of the differential calculus on a manifold. In other words, the points of the space-time manifold (and so also those within the "hole") *do not inherit their individuality, hence physical existence, from the underlying differential-topological structure* of the manifold. To the contrary, their *physically* distinguishing properties and relations derive not from coordinate labels, but from the fields assigned to them by the generally covariant equations of physical theory; in general relativity these include *at least* the metric field functions $g_{\mu\nu}$. In turn, in a generally covariant space-time theory of fields, only the thus-designated events (possible "point-coincidences") and the relations between them are the "true observables", a (topological) structure preserved under one-to-one continuous diffeomorphic pooint transformations. In the current parlance stemming from Hawking and Ellis (1973), general relativistic

> space-times are regarded as equivalent if they have isomorphic models $\{\langle M, g_{\mu\nu}, T_{\mu\nu}\rangle \langle M, D^* g_{\mu\nu}, D^* T_{\mu\nu}\rangle\}$, physically indistinguishable under a manifold diffeomorphism $D$. The physical equivalence of these models express the principle of general covariance, understood actively as diffeomorphism invariance. Having now recognized his mistake, Einstein in 1916 sought to underscore this new understanding by adopting a programmatic characterization of what is physically observable as, in principle, reducible to the broad category of "point-coincidences" (or intersections of world lines). This ensures that the conclusion of the hole argument can no longer go through, since only a physical process—the metric field (and possibly other physical fields)—can accord physical existence to the events that make up the space-time manifold. For according to this criterion there is truly no "empty space", no space-time points bereft of *at least* the metric field and so no (merely) "topological space". In holding that space has existence "only as a structural quality of the field", Einstein is underscoring his heuristic postulate that "spatio-temporal individuation of the points of the manifold in a general-relativistic model is possible only after the specification of a particular metric field, that is, only after the field equations of the theory (which constitutes its dynamical problem) have been solved". [Stachel 1989a quote] Then the striking statement situating physical reality in "point-coincidences" represents an attempt to distinguish clearly what is required for certain mathematical structures of the theory to have physical significance. It is not the positivist credo that, since the *in-principle observable* is found in the coincidence of points (intersections of world lines), only such coincidences as are actually observed are real. Alas, this was the message received in Machian circles and welcomed as a confirmation of Mach's positivist philosophy. But once the largely hidden context of the hold argument is restored, it is clear that in locating the "physically real" in "point-coincidences", Einstein gave rhetorical force to the *fact* that, in general relativity unlike special relativity, space-time coordinates alone can have no immediate physical—that is, no chronogeometrical—meaning.
>
> (Ryckman, 2005, p. 21-22)

- general covariance vs. general invariance (different groups) and GR general covariance should be general invariance

> To Einstein, the "most essential thing" lay in removing from phyiscal theory, once and for all, the idea of an inertial system, of any notion of background space-time structures that act (in the explanation of the inertial motion) but which in turn are not acted upon. *A fortiori* this holds for a unified field theory, or any "consistent field theory", where "representing reality by everywhere continuous, indeed even analytic functions" means that the very notion of a "particle" does not exist in

> "the strict sense of the word". [Einstein to Besso] In such a theory of the "total field", the dualism of matter and field is fully resolved in favor of the latter. "Particles" are everywhere to be described as "singularity free solutions of the completed field equations", representing localized large concentrations of electromagnetic, gravitational, and perhaps other forms of energy. Moreover, the law of motion of such "particles" must be derivable from the field equations governing the fundamental field variables, a requirement that is only met by a nonlinear theory and, Bergmann has argued, a direct consequence of the theory's generally covariant field equations. In point of fact, if the field equations are nonlinear (as are the Einstein field equations), there is no unambiguous way to separate the total field into the self-field of the particle (notoriously infinite, before "renormalization", in the usual nonalgebraic formulation of quantum field theory), and the finite external "incident" field immediately surrounding the particle's spatially local extended volume, primarily responsible for its instantaneous state of motion. Successful derivation of an equation of motion within such a theory thus entails the nonexistence of causal influences on the particle not mediated through the immediately adjacent field. In any theory of this kind, the very possibility of such "objects of experience" as "particles" with determinate properties, presupposes some criterion for the individuation of distinct physical systems that nowhere relies upon *a priori* structures of a "background" space and time.
>
> (Ryckman, 2005, p. 23-24)

- general covariance as a priori constitutive guiding requirement from Einstein and Cassirer, meta-level principles

- Reichenbach/Schlick coordination between concepts and reality, objects of latter unchanged and as they are; given vs posed, gegeben vs aufgegeben: Marburg (Cassirer) interpreting Kant as latter. But Schlick disagreed with realist way Reichenbach was going with coordinating reality as given.

- Cassirer general notion of invariance in scientific methodology, alternative "cognition" of physics compared to Reichenbach and Schlick

> Resisting the pull of mentalism, Cassirer refrained from characterizing anything but the logical form of this "intellectual *coordination*" (*gedankliche* ***Zuordnung***) through which diverse elements are connected into a systematic unity. The object of knowledge does not arise from the mere application of formal concepts to sensible experience but is "an expression for the form and mode of conceiving itself". Hence, on the functional theory of the concept, only a relative distinction can be made between the "form" and "content" of cognition. These are not completely independent realms of existence, but only reciprocal "moments", as concept and as intuition, of a basic process of cognitive synthesis that determines the concept

> of object. "Content" *is* only as determined through the serial relations of space and time, and the forms of magnitude and number. In physics, the epistemological high point of this (pre-relativistic) development had been attained by Hertz and especially Duhem, who stressed that concepts are pure symbols for relations and functional connections, not in any sense copies, or images, of the real.
>
> In this genealogy of the doctrine of the concept, no particular principle of form or order characteristic even of the present state of science can be taken as immutable or having *apodictic* validity. What remains unchanged through the successive changes in scientific knowledge is merely the "objectifying function" itself, the "supreme law of objectification". A fundamental axiom is Kant's claim that "objective validity and necessary universality (for everyone) are interchangeable concepts". Cassirer's guiding analogy is Felix Klein's *Erlanger Programm* where a geometry is characterized by the group of transformations under which given relations between points of the space are invariant. Similarly, the method of "transcendental philosophy" is to be a "*general invariant theory of experience" (*eine "**allgemeine Invariantentheorie der Erfahrung**"—original emphasis), isolating and investigating the most general elements of form that persist through all change in the material content of experience. Among these are the "categories" ("*Kategorien*") of space and time, of magnitude and functional dependence between magnitudes, presupposed in any empirical judgment or system of judgments. The aim of critical philosophy is to provide a complete inventory of the ultimate *logical invariants* (*de letzten* ***logischen Invarianten***) common to all possible forms of scientific experience, persisting from theory to theory as necessary and constitutive factors of any theory. That this is a goal neither completely attained nor attainable at any stage of knowledge is readily admitted. Rather, the significance of this aim is that it is a "*demand*" that a fixed direction is prescribed to "the continuous unfolding and development of systems of experience".
>
> (Ryckman, 2005, p. 40-41)

- general covariance a principle of objectifying unity, bringing knower/epistemologist closer to ideal form of objective object of knowledge [p. 44], towards relational and structural view of the objects of knowledge of scientific methods, satisfying maxim for methodological investigation of nature [Cassirer relating to Kant, Ryckman doesn't mention here but I'm thinking of universalizable maxim—i.e. if everyone follows it physics is composed of ideally intersubjective structures] (Cassirer)

> The interpretation of general covariance as a further development of the methodological principle of "objectifying unity" is the central theme in the remainder of Cassirer's essay. Where experience had unexpectedly failed to find the preferred reference frame posited by Galilean-Newtonian mechanics for the

motion of the solar system or the motion of the earth in Michelson's experiment, the theory of general relativity made a virtue out of necessity by requiring that there *cannot* and *must* not be such a preferred system. The general theory of relativity thus adopts the principle (*Prinzip*) "that for the physical description of the processes of nature [*Naturvörgange*] no particular reference body should be distinguished above all the others". The requirement of general covariance ("that all Gaussian coordinate systems are of equal value for the formulation of the general laws of nature") is designated a "rule of the understanding" ("*Regel des Verstandes*") adopted within physics not only as a formal requirement on mathematical representation, but as a "principle that the understanding uses hypothetically, as a norm of investigation, in the interpretation of experience". The sole meaning and justification of such a principle rests upon the fact that, through its application, it will be possible to attain the "synthetic unity of phenomena in their temporal relations" ("*synthetische Einheit der Erscheinungen nach Zeitverhältnissen*"), that is, lawful explanation of all observed phenomena. The guiding norm itself is unconditioned, and so ideal: it is just the "idea of unity of nature, of univocal determination itself". Nonetheless, with the requirement of general covariance, the general theory of relativity has given a new meaning to the Kantian idea of unity of nature as a "unity of determinate functional relations", assimilating under arbitrary transformations of the coordinates, all measurement results obtainable in particular reference systems. The concept of object of physics has become the concept of what remains invariant under such arbitrary transformation, and dynamics is more and more resolved into geometry (*reine Metrik*), a tendency, Cassirer observed, most clearly evident in Weyl's treatment of general relativity.

(Ryckman, 2005, p. 43)

Cassirer thus represented the principle of general covariance as a qualitatively new stage in the continual development of the conception of physical objectivity stretching back to the birth of modern science. In that process can be documented a progressive "movement of thought" (*Denkbewegung*), an unmistakable trend of the replacement of "substance" or "thing" concepts, uncritical "anthropomorphic" modes of representation, by functional and relational concepts. A yet further step, and a decisively higher stage of "de-anthropomorphization", has been taken with general relativity, for in its wake, the concept of "physical objectivity" incorporates the *methodological norm* of general covariance: that the laws of nature find their only natural expression in generally covariant equations. Although "objects of experience" require the choice of a suitable coordinate system (through the concrete calculation of a result to be compared with experimental data), there can be no

> general *preferred* set of coordinates (reference frames, or foliations of the space-time manifold). Singling out any reference frame for such distinction violates the spirit, and the letter, of general covariance, according to which *any* adopted reference object is itself a dynamical, not an absolute, object. As so "relativized", the fundamental concept of "object of nature" is not a picturable but a "pure structure" entity identifiable only in relation to other structures of the field. Deprived of the anthropomorphic stage of a background space-time that is always presupposed picturable or visualizable "thing-concepts", such a dynamical object is completely resolved into the pure measure relations (*reine Maßbeziehungen*) of a fully relational dynamics.
>
> (Ryckman, 2005, p. 45)

- Reichenbach's (neo)Kantian interpretation towards realism, Cassirer's towards idealism

> Stemming from a fundamental difference in the interpretation of Kantian epistemology, the 1920 monographs of Cassirer and Reichenbach on the theory of relativity point in diametrically different directions for subsequent philosophy of science. Each proposed a conception of the "relativized *a priori*" as meta-level constitutive principles governing empirical laws. However, the "relative" standing of Reichenbach's principles of coordination is counterbalanced by two epistemological methods consonant only with the commitments of scientific realism, a fact evidenced in his later writings, even as Schlick retreated from realism to a Wittgenstein-inspired positivist distaste of all "metaphysics". To philosophers not independently persuaded of the virtues of realism or positivism, Cassirer, the "historical" philosopher, proposed a significantly richer appreciation of the epistemological innovation of the theory of general relativity. While lacking the language and mathematical tools of symmetry readily available today, Cassirer nevertheless succeeded in grasping that the revolutionary epistemological idea of general relativity lies in general covariance, the regulative idea of all fundamental physical objects interacting through dynamical laws completely without reference to a background space-time. Such a conception of general covariance as an "idea of reason" constraining fundamental physical theory is no longer constitutively *a priori* in Kant's sense. That *regulative* ideals can play a heuristic but still *constitutive* role in physical cognition is then not Kantian orthodoxy. But it is universally agreed that general relativity needs occasion some revision or clarification in those deep, and often murky, waters.
>
> (Ryckman, 2005, p. 46)

- Einstein read Schlick's book on epistemology *Allgemeine Erkenntnislehre* and thought Schlick was intelligent and should get a professorship (and perhaps helped him get Vienna position).

> This reading had a visible influence in Einstein's widely read essay "Geometry and Experience" ("*Geometrie und Erfahrung*"), originating as a rare public lecture on 27 January 1921. There, Einstein not only commended the book's epistemological emphasis of the method of implicit definition, he also wrote of a "geometrical-physical theory" as "necessarily unintuitive, a bare system of concepts", the geometrical and some of the physical laws of which are conventions, whose relation to "experimental objects of reality (experiences)" is one of "coordination", all views found in Schlick's book. Perhaps having this reference in mind, the mathematician Hermann Weyl lamented to Edmund Husserl in March 1921 (see chapter 5, §5.2.1), that Schlick's epistemology book had a considerable resonance with "the leading theoretical physicists". The editors of the *Kant-Studien* could not possibly have regarded Schlick as neutral toward the Kantian or "critical" ("*kritizistische*") philosophy, for his epistemology book pointedly defended, in explicit opposition to all varieties of Kantianism (including Husserlian phenomenology) and Machian positivism, a form of scientific realism. Nonetheless, Schlick, as both philosopher and *Fachmann* regarding the theory of relativity, could be regarded as uniquely placed to assess what would be the most philosophically sophisticated attempt to link the general theory of relativity with the broad trend of Kantian thought.
>
> (Ryckman, 2005, p. 49)

- Schlicks assessment turning point against kantianism and towards logical empiricism, and was unfair towards Cassirer's neo-kantianism

> Third, and finally, on a careful reading, Schlick's argument bears not upon Cassirer's understanding of the "synthetic *a priori*" as regulative principles or "rules of the understanding" governing the development of concepts of physical objectivity, but upon a more traditional Kantian conception of apodictically certain and unrevisable principles. Perhaps to avoid tiresome discussions of Kant interpretation, perhaps because he considered it the identifying characteristic of all Kantian philosophy, perhaps for rhetorical purposes, Schlick located "the essence of the critical viewpoint" in the claim that the constitutive principles of physical knowledge
>
>> are to be *synthetic judgments a priori* in which to the concept of the *a priori* inseperably belongs the characteristic of *apodeicticity* (universal, necessary and inevitable validity). [1921 Critical or Empiricist Interpretation of Modern Physics]
>
> The "critical" (i.e. neo-Kantian) philosopher must maintain this understanding of the synthetic *a priori*, or else, in Schlick's lexicon, he is no longer a "critical" philosopher. Still more, continuing a reading of Kant given in his *Allgemeine Erkenntnislehre* (1918), Schlick refused to allow that the Kantian

> doctrine of "pure intuition" could ever be purged of its psychological trappings and so could not be revised or refined to be a "method of objectivification" in the manner Cassirer had adopted. The gauntlet thus laid down, Schlick had little trouble in dispatching such claims of Cassirer's book as could be represented in this fashion, misleadingly, since Schlick completely ignored the genetic character and historical evolution of the "regulative principles" and "rules of the understanding" that comprised the core of Cassirer's account of the development of the concept of physical objectivity culminating in general covariance. Accordingly, with this declaration the issue is no longer joined, for Cassirer had been denied any possibility of distinguishing his conception of constitutive *a priori* principles from an orthodoxy that Schlick could easily show was rendered obsolete by the new physics. In one recent assessment, Schlick's "challenge" to Cassirer to produce examples of such unrevisable synthetic *a priori* principles "represents a fundamental misconstrual of Cassirer's conception of the *a priori*". In any case, Schlick's traditional reading of the synthetic *a priori* was not a necessary one, as he himself already knew. The principal thesis of a 1920 monograph from the neo-Kantian perspective of Schlick's logical empiricist colleague-to-be, Hans Reichenbach, denied that apodictic certainty is inseparably attached to synthetic *a priori* principles. Schlick had reviewed Reichenbach's book and subsequently expended considerable effort, in correspondence with Reichenbach in late November, 1920, arguing that Reichenbach's theory-relative conception of synthetic *a priori* principles did not suffice to distinguish them from conventions in the sense of Poincaré. Showing some understandable sensitivity on this interpretive point, in his essay Schlick still insisted on this view of the *a priori*, while stating that his was "an inquiry directed to systematic rather than historical questions". Thus, he gave himself an easy target indeed.
> (Ryckman, 2005, p. 50-51)

> Based on a highly selective reading of these texts of Einstein, and of Helmholtz, Schlick's new empiricism is an almost "on-the-spot" improvisation, seeking to find the resources for an empiricist interpretation of the metric of spacetime in observable facts about measurement bodies and light rays whose fiduciary behavior has been fixed by conventional stipulation.
> (Ryckman, 2005, p. 52)

> Yet while celebrating Helmholtz as the Elijah of the new empiricism, Schlick also found it necessary to modify his previous unqualified endorsement of the holist conventionalism he had associated with Poincaré, a change that occasioned wider reaching ramifications within his general epistemology. Thus, in order to present the "consistent empiricism" he opposed to the neo-Kantians, Schlick eliminated the gray area recognized

> in the first edition of that work between "hypotheses" and "definitions". In the book's second edition (1925), the classification of types of judgment was revised to feature a sharp distinction between definitions and empirical judgments. This new discrimination was the prototype for the particular version of the analytic/synthetic distinction that became a defining characteristic of logical empiricism and the principal target, in an ironical turn of the wheel of fortune, of Quinean holism.
> (Ryckman, 2005, p. 52-53)

## 3.5 Weyl

Weyl here clearly seems to be indicating not a structural realist view, but an idealist view, which is not anti-realist. The symbols are something to satisfy the idealist agent, not the fundamental structures of reality, and not making transcendental reality bare to the mind. [IS THAT ACCURATE?]

> The stages through which research in the foundations of mathematics has passed in recent times correspond to the three basic possibilities of epistemological attitude. The set-theoretical approach is the stage of *naive realism* which is unaware of the transition from the given to the transcendent. Brouwer represents *idealism*, by demanding the reduction of all truth to the intuitively given. In axiomatic formalism, finally, consciousness makes the attempt to 'jump over its own shadow,' to leave behind the stuff of the given, to represent the *transcendent*—but, how could it be otherwise?, only through the *symbol*. Basically, the idealist viewpoint in epistemology has been adhered to by occidental philosophy since Descartes; nevertheless, it has searched again and again in metaphysics for an access to the realm of the absolute, and Kant, who meant to shoot the bolt once and for all, was yet followed by Fichte, Schelling, and Hegel. It cannot be denied that a theoretical desire, incomprehensible from the merely phenomenal point of view, is alive in us which urges toward totality. Mathematics shows that with particular clarity; but it also teaches us that that desire can be fulfilled on one condition only, namely, that we are satisfied with the symbol and renounce the mystical error of expecting the transcendent ever to fall within the lighted circle of our intuition. So far, only in mathematics and physics has symbolical-theoretical construction gained that solidity which makes it compelling for everyone whose mind is open to these sciences. Their philosophical interest is primarily based on this fact. (Weyl, 1949, p. 65-66)

Objectivity can only come from experience (context of geometry). Section 13 "The Problem of Relativity".

> Our knowledge stands under the norm of *objectivity*. He who believes in Euclidean geometry will say that all points in space are objectively alike, and that so are all possible directions.

> However, Newton seems to have thought that space has an absolute center. Epicurus certainly thought that the vertical is objectively distinguishable from all other directions. He gives as his reason that all bodies when left to themselves move in one and the same direction. Hence the statement that a line is vertical is elliptic or incomplete, the complete statement behind it being something like this: the line has the direction of gravity at the point $P$. Thus the gravitational field, which we know to depend on the material content of the world, enters into the complete proposition as a contingent factor, and also an individually exhibited point $P$ on which we lay our finger by a demonstrative act such as is expressed in words like 'I,' 'here,' 'now,' 'this.' Only if we are sure that the truth of the complete statement is not affected by free variation of the contingent factors and of those that are individually exhibited (here the gravitational field and the point $P$) have we a right to omit these factors from the statement and still to claim objective significance for it. Epicurus's belief is shattered as soon as it is realized that the direction of gravity is different in Princeton and in Calcutta, and that it can also be changed by a redistribution of matter. Without claiming to give a mechanically applicable criterion, our description bears out the essential fact that objectivity is an issue decidable on the ground of experience only. It also accounts for the two main sources of the error so often committed in the history of knowledge, that of mistaking a statement for objective that is not: (1) one overlooked certain relevant circumstantial factors on which the meaning of the statement depends although they are not mentioned explicitly in its elliptic form, (2) though these factors were recognized, one did not investigate carefully enough whether or not the truth of the statement is affected by their variation. It is no wonder then that at several phases in the course of the history of science the realm of that which is considered objective has shrunk. (Weyl, 1949, p. 71-72)

# 4 Introduction

If we are to load the truth values into our statements about the world, there will be a structure to our interactions with the world. In a technical sense, our observations will be augmented by measurement devices, and these measurement devices as part of the world will have dynamics under certain measurement conditions.

The dynamics of the total system can transition to a state that we distinguish as an informative signal, a meaningful event that we call a "measurement". A canonical formal representation of measurement events can be characterized by certain states that are invariant or symmetric under certain operations. The information of these states is more robust, and if we are carefully explicit about how representations of these states reflect the knowledge of an agent, robust states can be distinguished from other states due to information-theoretic properties.

Innovations in what a measurement might consist of, or the theoretical possibilities of what an agent could learn from measurement interactions, will adjust the information-theoretic characteristics that an engineer or decision maker could exploit. The structure of our knowledge of the world is dependent on our representation of measurement interactions in a theory of the world (which includes and is not separate from the measurement apparatus). This structure can, will, and probably should change in at least some accordance to a relevant domain.

It is not my intention to defend any particular historical strain of empiricism. Indeed, not everyone is convinced there is much of a consensus among empiricists beyond the basic assertion that at least a very important class of statements about reality find all or most of their meaning and justification through empirical inquiry and the experience of events observed in the world.

Rather, as other

## 4.1 The Descent in an Idealist Gradient

I hope to sketch in what follows the number of angles, hints, and potential philosophical burdens in the philosophy of physics which might lead one unknowingly down a path towards idealism. I am calling this, tentatively, the descent down an *idealist gradient*. For philosophers, the recognition of this gradient from a metaphysical point of view, from a birds eye view of the idea landscape, is crucially important. Historical views indicating in this direction, thought to be coherent by their proponents, cannot be scavenged piece-meal by modern philosophers without recognizing that the pieces originally pointed in a particular direction.

To this extent, for this proposal, I will be sketching the positions of a few prominent philosophers and philosophical positions, using *-isms* liberally to provide a coarse-grained view of the landscape. For example, the *-isms* of: structuralism, realism, empiricism, rationalism, intuitionism, and of course idealism. The central characters in the story, for now, are Poincare, Eddington, Cassirer, Weyl, Bohm,

The end goal of sketching this gradient, is to eventually evaluate certain foundational positions in the philosophy of physics with respect to this gradient. In particular, those positions of Bohm (and, to the extent it is related to Bohm's ideas in particular, to the proponents of Bohmian mechanics) and QBism.

If we can model the philosophical foundations of physics, specifically concerning the work of a number of prominent thinkers on quantum and relativistic foundations, as a surface or fabric that has a gradient, is there an attractor or minimum point in the surface? In other words, if we start from phenomenology, rationalism, structuralism, or empiricism, are we bound to a trajectory towards some area in the fabric?

In this proposal, I will suppose that the question is a worthwhile one to ask, and consider an affirmative answer. For a slogan underpinning the investigation to follow, I will suppose that there is an *elephant of idealism* that, when acknowledged, can both explain and explicate why we might see aspects of the gradient as pulling us towards an idealist attractor. The ultimate nature of this idealist attractor, and it's relation to e.g. Kant and Hegel and others, is still fuzzy and unclear.

However, at this preliminary stage, I hope to convincingly show that there is a general gradient towards idealism that has gone unnoticed which should be acknowledged and reckoned with by the foundations of physics community. Whether the anomalies are sufficient to pick up a potentially more parsimonious idealist solution, it is in my opinion far too early to tell. It does seem, however, than an idealist position should not be expected to fare worse than other highly speculative answers to major philosophical issues in the foundations of physics. This should be unsurprising, given the relative lack of attention in analytic philosophy departments (that do philosophy of science and in particular philosophy of physics) to e.g. the intricacies of "continental" philosophy like Hegelianism and synthetic metaphysics.[1]

- Eddington Quote - Clifford Quote - Ryckman on Weyl, Cassirer, and importantly Husserl - Dont necessarily need to find one particular idealist position but can explore cluster for hints of what might apply or be useful for understanding quantum foundations and measurement in a participatory view.

# 5 Quantum Bayesianism

The normatively compelling aspect of Bayesian probability theory and epistemology is usually summed up with reference to Dutch Book Arguments (DBA). Alternatively, there are reasons to prefer what are called **proper scoring rules**. I think it is reasonable to understand de Finetti considered proper scoring rules to be superior to DBA for the purposes of purity of elicitation and philosophical consistency avoiding game theoretic problems having to do with the information from published books of bets from the bookies. In a previous unpublished manuscript while at the MCMP I noted as much. [CITE OR BRING PASSAGES FROM THAT PAPER] See e.g. (de Finetti, 1981, 1990, 2008). I would be interested if historians have a definitive account of de Finetti's position, but for now I would like to take what I find to be a reasonable interpretation and build upon it.

For present purposes, it is important to note that, compared to objective or more physical interpretations of probability, these accounts treat probabilities as formal epistemological objects of a subjective agent first and foremost. Probabilities *are* subjective degrees of belief, and there are arguments that, for an agent, these numbers should behave according to certain rules. Otherwise there is trouble: sure losses, incoherence, irrationality...

If quantum probabilities might be of the same Bayesian character (subjective, epistemological, normatively compelling) it might be in part because they (the numbers) are generated by a more general procedure, in a sense inheriting the account from "classical" probabilities. However,

[1] Or, at least, this is the present author's experience, at the risk of generalizing. Perhaps there is a network of individuals and their work that I am wholly ignorant of!

there are important differences which make them seem much more *objective*. Surely, a quantum physicist or quantum information theorist should also respect some normative epistemological principles.

# 6 Theory-Laden Observation

Or just mention in intro?

# 7 Local vs. Non-Local

One lesson from quantum foundations, relevant also to the discussion over implications for what form empiricism can take, is that sometimes what is ruled out isn't so clear. For example, von Neumann was considered to have proven that *no* hidden variable amendments to quantum mechanics would be possible and consistent with observations. [SOURCE] This was the received view, until John Bell helped clarify exactly what assumptions, and for what kinds, of hidden variables von Neumann's theorem applied. It was *local* hidden variables that were ruled out, but certain *non-local* amendments like Bohmian mechanics are legitimately viable. [SOURCe]

THIS PARAGRAPH SEEMS LIKE NON-SEQUITUR WHAT WAS I THINKING? Likewise, we might take care to interpret the upheavals in positivism, logical empiricism, and any associated framework for loading up meaningful statements about the world from sense observations. What did objections *actually* show was untenable about empiricism? Certain amended forms of empiricism, like van Fraassen's *constructive* empiricism, or slight variations called *structural* empiricism, might be just fine. [CITE VF AND OTAVIO BUENO] Indeed, I will contribute to the arguments in favor of the general direction of these empiricist responses.

The point is that fundamentally, local interactions (if we grant that that even makes sense) are somehow structural windows in the interface that contain non-local information of "the world" regardless of what you think the world is.

# 8 The Pragmatic Ontologists

## 8.1 Carver Mead

If our purpose is to do theoretical physics or advance foundational understanding, and we don't stop at the phenomenological border, then we might wish to explore different ontologies behind the phenomenal and epistemic statistics.

The minimal statistical phenomenological and epistemological border, which admittedly has been extremely effective for most calculations and engineering purposes, might not be sufficient for philosophical understanding or potential theoretical unification efforts. [CITE Hossenfelder criticisms] But even if we look to the engineering scientist's understanding of quantum mechanics, there might be a pragmatic inference to the best explanation of entities existing in the world such that produce the appearances at the phenomenological border. One account in this vein might be arguably found in Carver Mead [CITE] who emphasizes the wave nature of matter, rejecting point particles, and the *conceptual understanding* which enables an engineer.

Indeed, macroscopic quantum states in superconductors are also emphasized by Mead to provide support for a conceptual understanding of the ontological reality of matter waves, as opposed to the statistical picture I have outlined at the phenomenological border. Likewise with lasers for the nature of light. Reality *has to be so*, according to this view, which flips the burden onto the epistemologist. In other words, if before the burden was on the realist or ontologist to provide sufficient grounds to go *beyond* the phenomenological or empiricist border, Mead either assumes (or is forced into) different (metaphysical) principles to account for very specific technical arrangements.

While I cannot vouch for the historical accuracy, Mead summarizes the historical development of the approach which emphasizes the statistical phenomenological border as contingent on the particular systems and experiments of the era:

> In 1913 Niels Bohr introduced his first approach to what is now called "the old quantum theory". The theory was not well defined, but was able to provide a classical conceptual basis for many of the observed properties of the emission spectral lines of excited gasses. Many physicists worked on improving and extending the theory, with some notable successes. A central conceptual aspect of the theory that has survived is that "quantum jumps" between energy states of atoms, give rise to emitted "photons". Efforts on the old quantum theory persisted well into the 1920's. In the mid-to-late 1920's, the work of de Broglie, Schrödinger, Heisenberg, Born, Pauli, and Dirac led from the old quantum theory to the quantum theory that is taught in schools today. Experiments during that formative period were still based on spectra from excited gasses, and hence were, by their very nature, only statistical in nature. The limitations of those experiments were built into the conceptual foundations of the new quantum mechanics, and remain there to this day. In fact, the limitation to statistical predictions was turned into a religion by Bohr and Heisenberg.

Mead also talks sympathetically of the transactional view, and the transactional interpretation of e.g. Cramer [SOURCE] Further detail on Mead's view in that direction are unnecessary for present purposes.

Also his approach, focused on the engineering of coherent populations of electrons (or photons), and transactions between coherent macroscopic states, leads one to expect in the future "more and more coherent forms of detection".[CITE] That is, if the world is such as to have persistent properties like phase of populations, which is physically important even though in measurement transactions it might be obliterated in a naive epistemology, we expect to be able to have 'weaker' forms of interactions that are useful.

> We are at the stage of detector evolution corresponding to the era of "crystal sets" in the evolution of radio receivers. We have already developed several forms of distributed coherent optical amplification. In the near future we will surely evolve more and more coherent forms of detection, which will then allow the kind of coherent systems at optical wavelengths that we routinely enjoy at radio wavelengths. The development of such coherent detectors has been seriously retarded by the Copenhagen view of laser beams made up of "photons"–little bullets carrying $h\nu$ of energy. Einstein got past that hangup, and we can as well. At that point the "Copenhagen photon" will take its proper place with all the other "noise" bequeathed us by the Copenhagen clan!

## 8.2 David Bohm

One might read Bohm and see a pragmatic side to introducing non-local hidden variables... I think that is what I was originally considering here. Not sure if this is in scope.

# 9 Poincaré

> Instead of a summary condemnation we should examine with the utmost care the role of hypothesis; we shall then recognise not only that it is necessary, but that in most cases it is legitimate. We shall also see that there are several kinds of hypotheses; that some are verifiable, and when once confirmed by experiment become truths of great fertility; that others may be useful to us in fixing our ideas; and finally, that others are hypotheses only in appearance, and reduce to definitions or to conventions in disguise. The latter are to be met with especially in mathematics and in the sciences to which it is applied. From them, indeed, the sciences derive their rigour; such conventions are the result of the unrestricted activity of the mind, which in this domain recognises no obstacle. For here the mind may affirm because it lays down its own laws; but let us clearly understand that while these laws are imposed on *our* science, which otherwise could not exist, they are not imposed on Nature. Are they then arbitrary? No; for if they were, they would not be fertile. Experience leaves us our freedom of choice, but it guides us by helping us to discern the most convenient path to follow. Our laws are therefore like those of an absolute monarch, who is wise and consults his council of state. Some people have been struck by this characteristic of free convention which may be recognised in certain fundamental principles of the sciences. Some have set no limits to their generalisations, and at the same time they have forgotten that there is a difference between liberty and the purely arbitrary. So that they are compelled to end in what is called *nominalism*; they have

> asked if the *savant* is not the dupe of his own definitions, and if the world he thinks he has discovered is not simply the creation of his own caprice. Under these conditions science would retain its certainty, but would not attain its object, and would become powerless. Now, we daily see what science is doing for us. This could not be unless it taught us something about reality; the aim of science is not things themselves, as the dogmatists in their simplicity imagine, but the relations between things; outside those relations there is no reality knowable. (Poincaré, 1952 [1905], p. xxii-xxiv)

Poincaré discusses definitions and proofs in mathematics requiring mathematical induction (that is, the process of showing a property holds for $n = 1$, and that *if* it also holds for $n - 1$ *then* it also holds for $n$, and concluding that it is true for all $n$). He equates this to what he calls (in the translation) "recurrence" of essentially infinite syllogisms condensed, in mathematical reasoning *par excellence*, in a "cascade", one following the other. (Poincaré, 1952 [1905], p. 9)

> But however far we went we should never reach the general theorem applicable to all numbers, which alone is the object of science. To reach it we should require an infinite number of syllogisms, and we should have to cross an abyss which the patience of the analyst, restricted to the resources of formal logic, will never succeed in crossing. (Poincaré, 1952 [1905], p. 10-11)

Recurrence is "the only instrument which enables us to pass from the finite to the infinite." (Poincaré, 1952 [1905], p. 11) But what is his philosophical foundation or justification for this principle? Without going into too deep of philosophy of mathematics waters, it is in the vein of *intuitionism*. This seems like a vitally important point to follow up on, in the present context, since intuitionism has close relations to idealism. [DOES IT? WHERE?]

> We cannot therefore escape the conclusion that the rule of reasoning by recurrence is irreducible to the principle of contradiction. Nor can the rule come to us from experiment. Experiment may teach us that the rule is true for the first ten or the first hundred numbers, for instance; it will not bring us to the indefinite series of numbers, but only to a more or less long, but always limited, portion of the series.
>
> Now, if that were all that is in question, the principle of contradiction would be sufficient, it would always enable us to develop as many syllogisms as we wished. It is only when it is a question of a single formula to embrace an infinite number of syllogisms that this principle breaks down, and there, too, experiment is powerless to aid. This rule, inaccessible to analytical proof and to experiment, is the exact type of the *á priori* synthetic intuition. On the other hand, we cannot see in it a convention as in the case of the postulates of geometry.

> Why then is this view imposed upon us with such an irresistible weight of evidence? It is because it is only the affirmation of the power of the mind which knows it can conceive of the indefinite repetition of the same act, when the act is once possible. The mind has a direct intuition of this power, and experiment can only be for it an opportunity of using it, and thereby of becoming conscious of it.
>
> But it will be said, if the legitimacy of reasoning by recurrence cannot be established by experiment alone, is it so with experiment aided by induction? We see successively that a theorem is true of the number 1, of the number 2, of the number 3, and so on—the law is manifest, we say, and it is so on the same ground that every physical law is true which is based on a very large but limited number of observations.
>
> It cannot escape our notice that here is a striking analogy with the usual processes of induction. But an essential difference exists. Induction applied to the physical sciences is always uncertain, because it is based on the belief in a general order of the universe, an order which is external to us. Mathematical induction–i.e., proof by recurrence—is, on the contrary, necessarily imposed on us, because it is only the affirmation of a property of the mind itself.(Poincaré, 1952 [1905], p. 12-13)

The latter comments here, I take it, idealists of some schools would disagree with in the sense that they can *solve by metaphysical assumptions* the apparent differences in justification for mathematical and scientific induction.[2] That is, scientific induction is *also* the "affirmation of a property of the mind itself". They might also qualify or contrast a particular scientist's "mind" with that of a universal organizing principle as "Mind" (of which mind is an inseparable and integrated sub-part looking-onto-itself). [3]

For such an idealist, the mind's *reasoning about* experiences of particular scientific observations (of Mind) in scientific principle of induction, are *in* Mind. Likewise, the mind's *reasoning about* experiences of particular syllogisms (of Mind) in mathematical induction are *in* Mind. The distinction fades for such an idealist, who doesn't stop where Poincaré seems to.

> Mathematics may, therefore, like the other sciences, proceed from the particular to the general. This is a fact which might otherwise have appeared incomprehensible to us at the beginning of this study, but which has no longer anything mysterious about it, since we have ascertained the analogies between proof by recurrence and ordinary induction.
>
> No doubt mathematical recurrent reasoning and physical inductive reasoning are based on different foundations, but they

[2] Discussions on Hegel's idealism in particular with Ryan Mullaly have been helpful for my understanding on nuanced aspects of idealism.

[3] I will capitalize the 'M' as a convention the potentially more sophisticated notion, and as I'm not an expert on e.g. German idealism, I will imagine and construct for the moment an idealist position.

> move in parallel lines and in the same direction—namely, from the particular to the general.
>
> (Poincaré, 1952 [1905], p. 14)

But, is stopping and *not* solving the problem in an idealist framework the most coherent philosophical position? An idealist wouldn't see just a surface-level analogy between the two forms of induction, with different background justifications, but rather *homologous* rational processes in Mind (and understood by apparently distinct mind's of agents). Is there an epistemological impetus to *descend the idealist gradient*?

To rejoin the earlier discussion: If we are going to pursue a structural view of the phenomenological border, and identify the structures (of quantum theory) with those experienced by an agent,

A structuralist which takes an idealist position, going a bit further than Poincaré, would consider relations (characterized mathematically) of the world to be *absolute* in the sense that they are not subjective perspectives on an external world, but are *characteristic of the world as a unified existence of Mind*. Thus, any group structures wouldn't make sense as being of a phenomenological border *with* the world.

They are structures *of* the world, and the world is constituted as such that e.g. mathematical and scientific induction are both rational processes of Mind.

It seems to me that, while perhaps consistent, many of us will want to resist such a position. It seems that it assumes a common cause, if not outright circularity, just to solve the problem. But, of course, the picture isn't that simple, as there are independent arguments for forms of idealism that are not contingent on solving the problem of induction.

Proponents would say that their metaphysics *explains* or solves many apparent paradoxes.

Poincaré's structuralism of scientific objects is also obviously related to his thoughts on topology and what he calls *analysis situs*. The point being that *analysis situs* is the study of (relational) structures (like shapes) which persist across a variety of deformations. Topology is thus a natural field of mathematics for the structuralist to appeal to, and the group operations about topological structures.

## 9.1 Poincaré on Correlative Movement (relation to Cybernetics)

OUT OF SCOPE?

> Suppose a solid body to occupy successively the positions $\alpha$ and $\beta$; in the first position it will give us an aggregate of impressions $A$, and in the second position the aggregate of impressions $B$. Now let there be a second solid body, of qualities entirely different from the first—of different colour, for instance. Assume it to pass from the position $\alpha$, where it gives us the aggregate of impressions $A'$ to the position $\beta$, where it gives the aggregate of impressions $B'$. In general, the aggregate $A$ will have nothing in common with the aggregate $A'$, nor will the aggregate $B$

have anything in common with the aggregate $B'$. The transition from the aggregate $A$ to the aggregate $B$, and that of the aggregate $A'$ to the aggregate $B'$, are therefore two changes which *in themselves* have in general nothing in common. Yet we consider both these changes as displacements; and, further, we can consider them the *same* displacement. How can this be? It is simply because they may be both corrected by the *same* correlative movement of our body. "Correlative movement," therefore, constitutes the *sole connection* between two phenomena which otherwise we should never have dreamed of connecting.

On the other hand, our body, thanks to the number of its articulations and muscles, may have a multitude of different movements, but all are not capable of "correcting" a modification of external objects; those alone are capable of it in which our whole body, or at least all those in which the organs of our senses enter into play are displaced *en bloc*—*i.e.*, without any variation of their relative positions, as in the case of a solid body.

To sum up:

1. In the first place, we distinguish two categories of phenomena:—The first involuntary, unaccompanied by muscular sensations, and attributed to external objects—they are external changes; the second, of opposite character and attributed to the movements of our own body, are internal changes.

2. We notice that certain changes of each in these categories may be corrected by a correlative change of the other category.

3. We distinguish among external changes those that have a correlative in the other category—which we call displacements; and in the same way we distinguish among the internal changes those which have a correlative in the first category.

Thus by means of this reciprocity is defined a particular class of phenomena called displacements. *The laws of these phenomena are the object of geometry.*

*Law of Homogeneity.*—The first of these laws is the law of homogeneity. Suppose that by an external change we pass from the aggregate of impressions $A$ to the aggregate $B$, and that then this change $\alpha$ is corrected by a correlative voluntary movement $\beta$, so that we are brought back to the aggregate $A$. Suppose now that another external change $\alpha'$ brings us again from the aggregate $A$ to the aggregate $B$. Experiment then shows us that this change $\alpha'$, like the change $\alpha$, may be corrected by a voluntary correlative movement $\beta'$, and that this movement $\beta'$ corresponds to the same muscular sensations as the movement $\beta$ which corrected $\alpha$.

This fact is usually enunciated as follows:—*Space is homogeneous and isotropic.* We may also say that a movement which is once produced may be repeated a second and a third time, and so on, without any variation of its properties. In the first

> chapter, in which we discussed the nature of mathematical reasoning, we saw the importance that should be attached to the possibility of repeating the same operation indefinitely. The virtue of mathematical reasoning is due to this repetition; by means of the law of homogeneity geometrical facts are apprehended. To be complete, to the law of homogeneity must be added a multitude of other laws, into the details of which I do not propose to enter, but which mathematicians sum up by saying that these displacements form a "group."
> (Poincaré, 1952 [1905], p. 61-64)

In general Poincaré pursues the strategy of saying what geometry one *would* come to use to characterize transformations in experience (or equivalently, transformations in points of view) if an idealized subject were embedded in a different space or world. Ashby would perhaps be content with an effective list of state transitions, at least from the perspective of "the system", but if we are on a meta-level as scientists and philosophers, parsimonious summaries of these transitions, as found in one or another suitable system of geometry, are fine to construct. So long as at the systems-level of analysis, it is realized that the organism is not "comprehending" or "knowing" a geometry, rather it has learned an adaptive set of transitions (which can be summarized by meta-cognitive agents).

One can also pursue the idealized-agent-embedded-in-a-world idea to consider how e.g. self-similarity on a fractal surface (created by one of Poincaré's much appealed to recurrent processes, and characterized e.g. by a Hausdorff dimension $d_H$) would come to be reflected in the agent's epistemology. Not just the "cognitive geometry" but the *rules* that can be relied upon. These rules being, naturally, a deductive form of the rule of induction (both mathematical and scientific observation forms). On the "surface" of such a world, an agent would come to characterize the world by a certain property at each point in his experienced world, e.g. $d_H$. Much like how a property of curvature in Riemannian geometry is *embedded* in a fundamental way, and reflected by us as experiencers of a world that is summed up in general relativity.

> The sense of light, even with one eye, together with the muscular sensations relative to the movements of the eyeball, will suffice to enable us to conceive of space of three dimensions. The images of external objects are painted on the retina, which is a plane of two dimensions; these are *perspectives*. But as eye and objects are movable, we see in succession different perspectives of the same body taken from different points of view. We find at the same time that the transition from one perspective to another is often acccompanied by muscular sensations. If the transition from the perspective $A$ to the perspective $B$, and that of the perspective $A'$ to the perspective $B'$ are accompanied by the same muscular sensations, we connect them as we do other operations of the same nature. Then when we study the laws according to which these operations are combined, we see that they form a group, which has the same structure as that of the movements of invariable solids. Now,

> we have seen that it is from the properties of this group that we derive the idea of geometrical space and that of three dimensions. We thus understand how these perspectives gave rise to the conception of three dimensions, although each perspective is of only two dimensions,—because *they succeed each other according to certain laws.* Well, in the same way that we draw the perspective of a three-dimensional figure on a plane, so we can draw that of a four-dimensional figure on a canvas of three (or two) dimensions. To a geometer this is but child's play. We can even draw several perspectives of the same figure from several different points of view. We can easily represent to ourselves these perspectives, since they are of only three dimensions. Imagine that the different perspectives of one and the same object to occur in succession, and that the transition from one to the other is accompanied by muscular sensations. It is understood that we shall consider two of these transitions as two operations of the same nature when they are associated with the same muscular sensations. There is nothing, then, to prevent us from imagining that these operations are combined according to any law we choose—for instance, by forming a group with the same structure as that of the movements of an invariable four-dimensional solid. In this there is nothing that we cannot represent to ourselves, and, moreover, these sensations are those which a being would experience who has a retina of two dimensions, and who may be displaced in space of four dimensions. In this sense we may say that we can represent to ourselves the fourth dimension.
> (Poincaré, 1952 [1905], p. 68-70)

> The object of geometry is the study of a particular "group"; but the general concept of group pre-exists in our minds, at least potentially. It is imposed on us not as a form of our sensitiveness, but as a form of our understanding; only, from among all possible groups, we must choose one that will be the *standard*, so to speak, to which we shall refer natural phenomena.
>
> Experiment guides us in this choice, which it does not impose on us. It tells us not what is the truest, but what is the most convenient geometry. It will be noticed that my description of these fantastic worlds has required no language other than that of ordinary geometry. Then, were we transported to those worlds, there would be no need to change that language. Beings educated there would no doubt find it more convenient to create a geometry different from ours, and better adapted to their impressions; but as for us, in the presence of the same impressions, it is certain that we should not find it more convenient to make a change.
> (Poincaré, 1952 [1905], p. 70-71)

Is he saying that we shouldn't change our internal agentic geometric conceptions from Euclidean to non-Euclidean? How does this compare to a cybernetics/Ashby perspective?

-Interior Angles of Triangles in non-Euclidean geometries

## 9.2 Pragmatic Poincaré

-Geometrical systems as convention

-Measurement/Experiment not capable of being a crucial experiment to decide one geometrical system over another

-Chapter 5 point 6... isn't this wrong?

> 6. Experiments only teach us the relations of bodies to one another. They do not and cannot give us the relations of bodies and space, nor the mutual relations of the different parts of space. "Yes!" you reply, "a single experiment is not enough, because it only gives us one equation with several unknowns; but when I have made enough experiments I shall have enough equations to calculate all my unknowns." If I know the height of the main-mast, that is not sufficient to enable me to calculate the age of the captain. When you have measured every fragment of wood in a ship you will have many equations, but you will be no nearer knowing the captain's age. All your measurements bearing on your fragments of wood can tell you only what concerns those fragments; and similarly, your experiments, however numerous they may be, referring only to the relations of bodies with one another, will tell you nothing about the mutual relations of the different parts of space.
> (Poincaré, 1952 [1905], p. 79-80)

Contrast this with e.g. what Harris says is implied by quantum and relativity.

-p. 84 "Hence, *experiments have reference not to space but to bodies.*

- Reread section 8 in chapter 5, more relationship to Ashby?

> [Kirchoff] wanted a definition of a force, and he took the first that came handy; but we do not require a definition of force; the idea of force is primitive, irreducible, indefinable; we all know what it is; of it we have direct intuition. This direct intuition arises from the idea of effort which is familiar to us from childhood. But in the first place, even if this direct intuition made known to us the real nature of force in itself, it would prove to be an insufficient basis for mechanics; it would, moreover, be quite useless. The important thing is not to know what force is, but how to measure it. Everything which does not teach us how to measure it is as useless to the mechanician as, for instance, the subjective idea of heat and cold to the student of heat. (Poincaré, 1952 [1905], p. 105-106)

Intuitionism (about physical concepts) into pragmatics about measurement.

- Contrary hypothesis to relativity (principle of relative motion) would be "repugnant to the mind." (Poincaré, 1952 [1905], p. 111, 113)

- Physical laws, not just geometry, as expressions of convenience to satisfy the mind. (e.g. p. 117)

- Energetics (conservation of energy kinetic + potential energy, and Hamilton's principle of least action) wiki

> Every change that the bodies of nature can undergo is regulated by two experimental laws. First, the sum of the kinetic and potential energies is constant. This is the principle of the conservation of energy. Second, if a system of bodies is at $A$ at the time $t_0$, and at $B$ at the time $t_1$, it always passes from the first position to the second by such a path that the *mean* value of the difference between the two kinds of energy in the interval of time which separates the two epochs $t_0$ and $t_1$ is a minimum. This is Hamilton's principle, and is one of the forms of the principle of least action.
>
> The energetic theory has the following advantages over the classical. First, it is less incomplete—that is to say, the principles of the conservation of energy and of Hamilton teach us more than the fundamental principles of the classical theory, and exclude certain motions which do not occur in nature and which would be compatible with the classical theory. Second, it frees us from the hypothesis of atoms, which it was almost impossible to avoid with the classical theory. (Poincaré, 1952 [1905], p. 123-124)

> If $T + U + Q$ were of the particular form that I have suggested above, no ambiguity would ensue. Among the functions $\phi(T + U + Q)$ which remain constant, there is only one that would be of this particular form, namely the one which I would agree to call energy. But I have said this is not rigorously the case. Among the functions that remain constant there is not one which can rigorously be placed in this particular form. How then can we choose from among them that which should be called energy? We have no longer any guide in our choice.
>
> Of the principle of conservation of energy there is nothing left then but an enunciation:—*There is something which remains constant.* In this form it, in its turn, is outside the bounds of experiment and reduced to a kind of tautology. It is clear that if the world is governed by laws there will be quantities which remain constant. Like Newton's laws, and for an analogous reason, the principle of the conservation of energy being based on experiment, can no longer be invalidated by it. (Poincaré, 1952 [1905], p. 127-128)

- law of errors reference: wiki

> An eminent physicist said to me one day, *ápropos* of the law of errors:—every one stoutly believes it, because mathematicians imagine that it is an effect of observation, and observers imagine that it is a mathematical theorem. And this was for a long time the case with the principle of the conservation of energy. It is no longer the same now. There is no one who does not know that it is an experimental fact. But then who gives us the

> right of attributing to the principle itself more generality and more precision than to the experiments which have served to demonstrate it? This is asking, if it is legitimate to generalise, as we do every day, empiric data, and I shall not be so foolhardy as to discuss this question, after so many philosophers have vainly tried to solve it. (Poincaré, 1952 [1905], p. 129)

- Is he referring to e.g. Hume? Yes I think so given subsequent paragraphs.
- Mayer's law (thermodynamic conservation energy) wiki

> If the future state of the system is not entirely determined by its present state, it is because it further depends on the state of bodies external to the system. But then, is it likely that there exist among the parameters $x$ which define the state of the system of equations independent of this state of the external bodies? and if in certain cases we think we can find them, is it not only because of our ignorance, and because the influence of these bodies is too weak for our experiment to be able to detect it? If the system is not regarded as completely isolated, it is probable that the rigorously exact expression of its internal energy will depend upon the state of the external bodies. Again, I have supposed above that the sum of all the external work is zero, and if we wish to be free from this rather artificial restriction the enunciation becomes still more difficult. To formulate Mayer's principle by giving it an absolute meaning, we must extend it to the whole universe, and then we find ourselves face to face with the very difficulty we have endeavoured to avoid. To sum up, and to use ordinary language, the law of the conservation of energy can have only one significance, because there is in it a property common to all possible properties; but in the determinist hypothesis there is only one possible, and then the law has no meaning. In the indeterminist hypothesis, on the other hand, it would have a meaning even if we wished to regard it in an absolute sense. It would appear as a limitation imposed on freedom. (Poincaré, 1952 [1905], p. 133-134)

- Clausius's principle wiki

> Almost everything that I have just said applies to the principle of Clausius. What distinguishes it is, that it is expressed by an inequality. It will be said perhaps that it is the same with all physical laws, since their precision is always limited by errors of observation. But they at least claim to be first approximations, and we hope to replace them little by little by more exact laws. If, on the other hand, the principle of Clausius reduces to an inequality, this is not caused by the imperfection of our means of observation, but by the very nature of the question. (Poincaré, 1952 [1905], p. 135)

- measurement/observation inequalities like uncertainty principle/fourier transform

In summarizing section III of the book and conventionalism:

> The principles of mechanics are therefore presented to us under two different aspects. On the one hand, there are truths founded on experiment, and verified approximately as far as almost isolated systems are concerned; on the other hand, there are postulates applicable to the whole of the universe and regarded as rigorously true. If these postulates possess a generality and a certainty which falsify the experimental truths from which they were deduced, it is because they reduce in final analysis to a simple convention that we have a right to make, because we are certain beforehand that no experiment can contradict it. This convention, however, is not absolutely arbitrary; it is not the child of our caprice. We admit it because certain experiments have shown us that it will be convenient, and thus is explained how experiment has built up the principles of mechanics, and why, moreover, it cannot reverse them. Take a comparison with geometry. The fundamental propositions of geometry, for instance, Euclid's postulate, are only conventions, and it is quite as unreasonable to ask if they are true or false as to ask if the metric system is true or false. Only, these conventions are convenient, and there are certain experiments which prove it to us. At the first glance, the analogy is complete, the role of experiment seems the same. We shall therefore be tempted to say, either mechanics must be looked upon as experimental science and then it should be the same with geometry; or, on the contrary, geometry is a deductive science, and then we can say the same of mechanics. Such a conclusion would be illegitimate. The experiments which have led us to adopt as more convenient the fundamental conventions of geometry refer to bodies which have nothing in common with those that are studied by geometry. They refer to the properties of solid bodies and to the propagation of light in a straight line. These are mechanical, optical experiments.
>
> In no way can they be regarded as geometrical experiments. And even the probable reason why our geometry seems convenient to us is, that our bodies, our hands, and our limbs enjoy the properties of solid bodies. Our fundamental experiments are pre-eminently physiological experiments which refer, not to the space which is the object that geometry must study, but to our body—that is to say, to the instrument which we use for that study. On the other hand, the fundamental conventions of mechanics and the experiments which prove to us that they are convenient, certainly refer to the same objects or to analogous objects. Conventional and general principles are the natural and direct generalisations of experimental and particular principles. (Poincaré, 1952 [1905], p. 135-137)
>
> We now understand why the teaching of mechanics should remain experimental. Thus only can we be made to understand the genesis of the science, and that is indispensable for a com-

> plete knowledge of the science itself. Besides, if we study mechanics, it is in order to apply it; and we can only apply it if it remains objective. Now, as we have seen, when principles gain in generality and certainty they lose in objectivity. It is therefore especially with the objective side of principles that we must be early familiarised, and thhis can only be by passing from the particular to the general, instead of from the general to the particular.
>
> Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value. Some philosophers have generalised far too much. They have thought that the principles were the whole of science, and therefore that the whole of science was conventional. This paradoxical doctrine, which is called Nominalism, cannot stand examination. How can a law become a principle? It expressed a relation between two real terms, $A$ and $B$; but it was not rigorously true, it was only approximate. We introduce arbitrarily an intermediate term, $C$, more or less imaginary, and $C$ is *by definition* that which has with $A$ *exactly* the relation expressed by the law. So our law is decomposed into an absolute and rigorous principle which expresses the relation of $A$ to $C$, and an approximate experimental and revisable law which expresses the relation of $C$ to $B$. But it is clear that however far this decomposition may be carried, laws will always remain. We shall now enter into the domain of laws properly so called. (Poincaré, 1952 [1905], p. 138-139)

- conventionalism about geometry but not mechanics, and if fundamental mechanical laws are strictly false it is in a justified way different from the justifications for choosing a particular geometrical view applied to reality

-similar to Cartwright's anti-realism about fundamental laws (because they contradict the augmented-with-assumption phenomenological laws)?

> What, then, is a good experiment? It is that which teaches us something more than an isolated fact. It is that which enables us to predict, and to generalise. Without generalisation, prediction is impossible. The circumstances under which one has operated will never again be reproduced simultaneously. The fact observed will never be repeated. All that can be affirmed is that under analogous circumstances an analogous fact will be produced. To predict it, we must therefore invoke the aid of analogy—that is to say, even at this stage, we must generalise. However timid we may be, there must be interpolation. Experiment only gives us a certain number of isolated points. They must be connected by a continuous line, and this is a true generalisation. But more is done. The curve thus traced will pass between and near the points observed; it will not pass through the points themselves. Thus we are not restricted to

> generalising our experiment, we correct it; and the physicist who would abstain from these corrections, and really content himself with experiment pure and simple, would be compelled to enunciate very extraordinary laws indeed. Detached facts cannot therefore satisfy us, and that is why our science must be ordered, or, better still, generalised.
>
> It is often said that experiments should be made without preconceived ideas. That is impossible. Not only would it make every experiment fruitless, but even if we wished to do so, it could not be done. Every man has his own conception of the world, and this he cannot so easily lay aside. (Poincaré, 1952 [1905], p. 142-143)

"It is far better to predict without certainty, than never to have predicted at all." (Poincaré, 1952 [1905], p. 144)

- mathematical physics directs generalisation (catalogue), analogy with library planning/expenditures

- unity and simplicity. simplicity/complexity process loop

> No doubt, if our means of investigation became more and more penetrating, we should discover the simple beneath the complex, and then the complex from the simple, and then again the simple beneath the complex, and so on, without ever being able to predict what the last term will be. We must stop somewhere, and for science to be possible we must stop where we have found simplicity. That is the only ground on which we can erect the edifice of our generalisations. But, this simplicity being only apparent, will the ground be solid enough? That is what we have now to discover. (Poincaré, 1952 [1905], p. 148-149)

-role of hypothesis, should be ASAP submitted to experimental verification, unconscious hypotheses dangerous, D/Q-like thesis and underdetermination

> Let us also notice that it is important not to multiply hypotheses indefinitely. If we construct a theory based upon multiple hypotheses, and if experiment condemns it, which of the premisses must be changed? It is impossible to tell. Conversely, if the experiment succeeds, must we suppose that it has verified all these hypotheses at once? Can several unknowns be determined from a single equation? (Poincaré, 1952 [1905], p. 151-152)

- also makes points similar to batterman? 153-157
- optics

> The mind has to run ahead of the experiment, and if it has done so with success, it is because it has allowed itself to be guided by the instinct of simplicity. The knowledge of the elementary fact enables us to state the problem in the form of an equation. It only remains to deduce from it by combination

> the observable and verifiable complex fact. That is what we call *integration*, and it is the province of the mathematician. It might be asked, why in physical science generalisation so readily takes the mathematical form. The reason is now easy to see. It is not only because we have to express numerical laws; it is because the observable phenomenon is due to the superposition of a large number of elementary phenomena which are *all similar to each other*; and in this way differential equations are quite naturally introduced. It is not enough that each elementary phenomenon should obey simple laws: all those that we have to combine must obey the same law; then only is the intervention of mathematics of any use. Mathematics teaches us, in fact, to combine like with like. Its object is to divine the result of a combination without having to reconstruct that combination element by element. If we have to repeat the same operation several times, mathematics enables us to avoid this repetition by telling the result beforehand by a kind of induction. This I have explained before in the chapter on mathematical reasoning. But for that purpose all these operations must be similar; in the contrary case we must evidently make up our minds to working them out in full one after the other, and mathematics will be useless. It is therefore, thanks to the approximate homogeneity of the matter studied by physicists, that mathematical physics came into existence. In the natural sciences the following conditions are no longer to be found:—homogeneity, relative independence of remote parts, simplicity of the elementary fact; and that is why the student of natural science is compelled to have recourse to other modes of generalisation. (Poincaré, 1952 [1905], p. 158-159)

- associationist structure building?
- ch. X The Theories of Modern Physics goes through examples where relations hold but objects we picture the relations holding change. E.g. Fresnel, Coloumb, Carnot [structuralism]

> No theory seemed established on firmer ground than Fresnel's, which attributed light to the movements of the ether. Then if Maxwell's theory is today preferred, does that mean that Fresnel's work was in vain? No; for Fresnel's object was not to know whether there really is an ether, if it is or is not formed of atoms, if these atoms really move in this way or that; his object was to predict optical phenomena.
>
> This Fresnel's theory enables us to do today as well as it did before Maxwell's time. The differential equations are always true, they may be always integrated by the same methods, and the results of this integration still preserve their value. It cannot be said that this is reducing physical theories to simple practical recipes; these equations express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us now, as they did then, that there is such and such a relation between this thing and that; only, the

> something which we then called *motion*, we now call *electric current*. But these are merely names of the images we substituted for the real objects which Nature will hide for ever from our eyes. The true relations between these real objects are the only reality we can attain, and the sole condition is that the same relations shall exist between these objects as between the images we are forced to put in their place. (Poincaré, 1952 [1905], p. 160-161)

-when contradiction between two theories "Let us not be troubled, but let us hold fast to the two ends of the chain, lest we lose the intermediate links."

> In case of contradiction one of them at least should be considered false. But this is no longer the case if we only seek in them what should be sought. It is quite possible that they both express true relations, and that the contradictions only exist in the images we have formed to ourselves of reality. (Poincaré, 1952 [1905], p. 163)

- satisfying the mind; structuralism accounts for when old theories get brought back to scientific attention (Coulomb fluid example)

> Hypotheses of this kind have therefore only a metaphorical sense. The scientist should no more banish them than a poet banishes metaphor; but he ought to know what they are worth. They may be useful to give satisfaction to the mind, and they will do no harm as long as they are only indifferent hypotheses.
>
> These considerations explain to us why certain theories, that were thought to be abandoned and definitively condemned by experiment, are suddenly revived from their ashes and begin a new life. It is because they expressed true relations, and had not ceased to do so when for some reason or other we felt it necessary to enunciate the same relations in another language. Their life had been latent, as it were.
>
> Barely fifteen years ago, was there anything more ridiculous, more quaintly old-fashioned, than the fluids of Coulomb? And yet, here they are re-appearing under the name of *electrons*. In what do these permanently electrified molecules differ from the electric molecules of Coulomb? It is true that in the electrons the electricity is supported by a little, a very little matter; in other words, they have mass. Yet Coulomb did not deny mass to his fluids, or if he did, it was with reluctance. It would be rash to affirm that the belief in electrons will not also undergo an eclipse, but it was none the less curious to note this unexpected renaissance.
>
> (Poincaré, 1952 [1905], p. 164-165)

- Carnot second law example
- pragmatic usefulness criteria
- mechanisms, Hertz

Every time that the principles of least action and energy are satisfied, we shall see that not only is there always a mechanical explanation possible, but that there is an unlimited number of such explanations. By means of a well-known theorem due to Königs, it may be shown that we can explain everything in an unlimited number of ways, by connections after the manner of Hertz, or, again, by central forces. (Poincaré, 1952 [1905], p. 167-168)

- ether

We may conceive of ordinary matter as either composed of atoms, whose internal movements escape us, our senses being able to estimate only the displacement of the whole; or we may imagine one of those subtle fluids, which under the name of *ether* or other names, have from all time played so important a role in physical theories. Often we go further, and regard the ether as the only primitive, or even as the only true matter. The more moderate consider ordinary matter to be condensed ether, and there is nothing startling in this conception; but others only reduce its importance still further, and see in matter nothing more than the geometrical locus of singularities in the ether.

Lord Kelvin, for instance, holds what we call matter to be only the locus of those points at which the ether is animated by vortex motions. Riemann believes it to be locus of those points at which ether is constantly destroyed; to Wiechert or Larmor, it is the locus of the points at which the ether has undergone a kind of torsion of a very particular kind. Taking any one of these points of view, I ask by what right do we apply to the ether the mechanical properties observed in ordinary matter, which is but false matter? The ancient fluids, caloric, electricity, etc., were abandoned when it was seen that heat is not indestructible. But they were also laid aside for another reason. In materialising them, their individuality was, so to speak, emphasised—gaps were opened between them; and these gaps had to be filled in when the sentiment of the unity of Nature became stronger, and when the intimate relations which connect all the parts were perceived. In multiplying the fluids, not only did the ancient physicists create unnecessary entities, but they destroyed real ties. It is not enough for a theory not to affirm false relations; it must not conceal true relations.

Does our ether actually exist? We know the origin of our belief in the ether. If light takes several years to reach us from a distant star, it is no longer on the star, nor is it on the earth. It must be somewhere, and supported, so to speak, by some material agency.

The same idea may be expressed in a more mathematical and more abstract form. What we note are the changes undergone by the material molecules. We see, for instance, that the photographic plate experiences the consequences of a phenomenon of which the incandescent mass of a star was the scene several years before. Now, in ordinary mechanics, the state of the system under consideration depends only on its state at the moment immediately preceding; the system therefore satisfies certain differential equations. On the other hand, if we did not believe in the ether, the state of the material universe would depend not only on the state immediately preceding, but also on much older states; the system would satisfy equations of finite differences. The ether was invented to escape this breaking down of the laws of general mechanics.

Still, this would only compel us to fill the interplanetary space with ether, but not to make it penetrate into the midst of the material media. Fizeau's experiment goes further. By the interference of rays which have passed through the air or water in motion, it seems to show us two different media penetrating each other, and yet being displaced with respect to each other. The ether is all but in our grasp. Experiments can be conceived in which we come closer still to it. Assume that Newton's principle of the equality of action and re-action is not true if applied to matter *alone*, and that this can be proved. The geometrical sum of all the forces applied to all the molecules would no longer be zero. If we did not wish to change the whole of the science of mechanics, we should have to introduce the ether, in order that the action which matter apparently undergoes should be counterbalanced by the re-action of matter on something.

Or again, suppose we discover that optical and electrical phenomena are influenced by the motion of the earth. It would follow that those phenomena might reveal to us not only the relative motion of material bodies, but also what would seem to be their absolute motion. Again, it would be necessary to have an ether in order that these so-called absolute movements should not be their displacements with respect to empty space, but with respect to something concrete.

Will this ever be accomplished? I do not think so, and I shall explain why; and yet, it is not absurd, for others have entertained this view. For instance, if the theory of Lorentz, or which I shall speak in more detail in Chapter XIII., were true, Newton's principle would not apply to matter *alone*, and the difference would not be very far from being within reach of experiment. On the other hand, many experiments have been made on the influence of the motion of the earth. The results have always been negative. But if these experiments have been undertaken, it is because we have not been certain beforehand; and indeed, according to current theories, the compensation would be only approximate, and we might expect to find accurate methods giving positive results. I think that such a hope is illusory; it was none the less interesting to show that a success of this kind would, in a certain sense, open to us a new

> world.
>
> And now allow me to make a digression; I must explain why I do not believe, in spite of Lorentz, that more exact observations will ever make evident anything else but the relative displacements of material bodies. Experiments have been made that should have disclosed the terms of the first order; the results were nugatory. Could that have been by chance? No one has admitted this; a general explanation was sought, and Lorentz found it. He showed that the terms of the first order should cancel each other, but not the terms of the second order. Then more exact experiments were made, which were also negative; neither could this be the result of chance. An explanation was necessary, and was forthcoming; they always are; hypotheses are what we lack the least. But this is not enough. Who is there who does not think that this leaves to chance far too important a role? Would it not also be a chance that this singular concurrence should cause a certain circumstance to destroy the terms of the first order, and that a totally different but very opportune circumstance should cause those of the second order to vanish? No; the same explanation must be found for the two cases, and everything tends to show that this explanation would serve equally well for the terms of the higher order, and that the mutual destruction of these terms will be rigorous and absolute.
>
> (Poincaré, 1952 [1905], p. 168-172)

-unification of light, electricity, and magnetism. Lorentz ether theory/electrons, mechanical explanation, analogies between systems

> The most satisfactory theory is that of Lorentz; it is unquestionably the theory that best explains the known facts, the one that throws into relief the greatest number of known relations, the one in which we find most traces of definitive construction. That it still possesses a serious fault I have shown above. It is in contradiction with Newton's law that action and re-action are equal and opposite—or rather, this principle according to Lorentz cannot be applicable to matter alone; if it be true, it must take into account the action of the ether on matter, and the re-action of the matter on the ether. Now, in the new order, it is very likely that things do not happen in this way.
>
> However this may be, it is due to Lorentz that the results of Fizeau on the optics of moving bodies, the laws of normal and abnormal dispersion and of absorption are connected with each other and with the other properties of the ether, by bonds which no doubt will not be readily severed. Look at the ease with which the new Zeeman phenomenon found its place, and even aided the classification of Faraday's magnetic rotation, which had defied all Maxwell's efforts. This facility proves that Lorentz's theory is not a mere artificial combination which must eventually find its solvent. It will probably

have to be modified, but not destroyed. The only object of Lorentz was to include in a single whole all the optics and electro-dynamics of moving bodies; he did not claim to give a mechanical explanation. Larmor goes further; keeping the essential part of Lorentz's theory, he grafts upon it, so to speak, MacCullagh's ideas on the direction of the movement of the ether. MacCullagh held that the velocity of the ether is the same in magnitude and direction as the magnetic force. Ingenious as is this attempt, the fault in Lorentz's theory remains, and is even aggravated. According to Lorentz, we do not know what the movements of the ether are; and because we do not know this, we may suppose them to be movements compensating those of matter, and re-affirming that action and re-action are equal and opposite. According to Larmor we know the movements of the ether, and we can prove that the compensation does not take place.

If Larmor has failed, as in my opinion he has, does it necessarily follow that a mechanical explanation is impossible? Far from it. I said above that as long as a phenomenon obeys the two principles of energy and least action, so long it allows of an unlimited number of mechanical explanations. And so with the phenomomena of optics and electricity.

But this is not enough. For a mechanical explanation to be good it must be simple; to choose it from among all the explanations that are possible there must be other reasons than the necessity of making a choice. Well, we have no theory as yet which will satisfy this condition and consequently be of any use. Are we then to complain? That would be to forget the end we seek, which is not the mechanism; the true and only aim is unity.

We ought therefore to set some limits to our ambition. Let us not seek to formulate a mechanical explanation; let us be content to show that we can always find one if we wish. In this we have succeeded. The principle of the conservation of energy has always been confirmed, and now it has a fellow in the principle of least action, stated in the form appropriate to physics. This has also been verified, at least as far as concerns the reversible phenomena which obey Lagrange's equations—in other words, which obey the most general laws of physics. The irreversible phenomena are much more difficult to bring into line; but they, too, are being coordinated and tend to come into the unity. The light which illuminates them comes from Carnot's principle. For a long time thermodynamics was confined to the study of the dilatations of bodies and of their change of state. For some time past it has been growing bolder, and has considerably extended its domain. We owe to it the theories of the voltaic cell and of their thermo-electric phenomena; there is not a corner in physics which it has not explored, and it has even attacked chemistry itself. The same laws hold good;

> everywhere, disguised in some form or other, we find Carnot's principle; everywhere also appears that eminently abstract concept of entropy which is as universal as the concept of energy, and like it, seems to conceal a reality. It seemed that radiant heat must escape, but recently that, too, has been brought under the same laws.
>
> In this way fresh analogies are revealed which may be often pursued in detail; electric resistance resembles the viscosity of fluids; hysteresis would rather be like the friction of solids. In all cases friction appears to be the type most imitated by the most diverse irreversible phenomena, and this relationship is real and profound. (Poincaré, 1952 [1905], p. 175 - 178)

- physical chemistry and materials, unity though complex, satisfying mind again

> As we get to know the properties of matter better we see that continuity reigns. From the work of Andrews and Van der Waals, we see how the transition from the liquid to the gaseous state is made, and that it is not abrupt. Similarly, there is no gap between the liquid and solid states, and in the proceedings of a recent Congress we see memoirs on the rigidity of liquids side by side with papers on the flow of solids.
>
> With this tendency there is no doubt a loss of simplicity. Such and such an effect was represented by straight lines; it is now necessary to connect these lines by more or less complicated curves. On the other hand, unity is gained. Separate categories quieted but did not satisfy the mind.
>
> (Poincaré, 1952 [1905], p. 181-182)

- probability, subjective vs objective, gamblers fallacy, posterior and priors in inferring causes, base rate, curve fitting,
- a priori considerations for curves that are continuous and not sinuosities
- law of errors (Gauss) as 'transcendental curve', but for Poincare if it is transcendental it is because of convention and practicality that it is so.

> We must calculate the probable *a posteriori* value of each error, and therefore the probable value of the quantity to be measured. But, as I have just explained, we cannot undertake this calculation unless we admit *a priori–i.e.*, before any observations are made—that there is a law of the probability of errors. Is there a law of errors? The law to which all calculators assent is Gauss's law, that is represented by a certain transcendental curve known as the "bell".
>
> But it is first of all necessary to recall the classic distinction between systematic and accidental errors. If the metre with which we measure a length is too long, the number we get will be too small, and it will be no use to measure several times—that is a systematic error. If we measure with an accurate metre, we may make a mistake, and find the length sometimes

too large and sometimes too small, and when we take the mean of a large number of measurements, the error will tend to grow small. These are accidental errors.

It is clear that systematic errors do not satisfy Gauss's law, but do accidental errors satisfy it? Numerous proofs have been attempted, almost all of them crude paralogisms. But starting from the following hypotheses we may prove Gauss's law: the error is the result of a very large number of partial and independent errors; each partial error is very small and obeys any law of probability whatever, provided the probability of a positive error is the same as that of an equal negative error. It is clear that these conditions will be often, but not always, fulfilled, and we may reserve the name of accidental for errors which satisfy them.

We see that the method of least squares is not legitimate in every case; in general, physicists are more distrustful of it than astronomers. This is no doubt because the latter, apart from the systematic errors to which they and the physicists are subject alike, have to contend with an extremely important source of error which is entirely accidental—I mean atmospheric undulations. So it is very curious to hear a discussion between a physicist and an astronomer about a method of observation. The physicist, persuaded that one good measurement is worth more than many bad ones, is pre-eminently concerned with the elimination by means of every precaution of the final systematic errors; the astronomer retorts: "But you can only observe a small number of stars, and accidental errors will not disappear."

What conclusion must we draw? Must we continue to use the method of least squares? We must distinguish. We have eliminated all the systematic errors of which we have any suspicion; we are quite certain that there are others still, but we cannot detect them; and yet we must make up our minds and adopt a definitive value which will be regarded as the probable value; and for that purpose it is clear that the best thing we can do is to apply Gauss's law. We have only applied a practical rule referring to subjective probability. And there is no more to be said.

Yet we want to go farther and say that not only the probable value is so much, but that the probable error in the result is so much. *This is absolutely invalid:* it would be true only if we were sure that all the systematic errors were eliminated, and of that we know absolutely nothing. We have two series of observations; by applying the law of least squares we find that the probable error in the first series is twice as small as in the second. The second series may, however, be more accurate than the first, because the first is perhaps affected by a large systematic error. All that we can say is, that the first series is *probably* better than the second because its accidental error

> is smaller, and that we have no reason for affirming that the systematic error is greater for one of the series than for the other, our ignorance on this point being absolute.
> (Poincaré, 1952 [1905], p. 207-209)

- ch 12 Optics and electricity, Fresnel. THIS IS A VERY GOOD CHAPTER

> It is owing to Fresnel that the science of optics is more advanced than any other branch of physics. The theory called the theory of undulations forms a complete whole, which is satisfying to the mind; but we must not ask from it what it cannot give us. The object of mathematical theories is not to reveal to us the real nature of things; that would be an unreasonable claim. Their only object is to co-ordinate the physical laws with which physical experiment makes us acquainted, the enunciation of which, without the aid of mathematics, we should be unable to effect. Whether the ether exists or not matters little—let us leave that to the metaphysicians; what is essential for us is, that everything happens as if it existed, and that this hypothesis is found to be suitable for the explanation of phenomena. After all, have we any other reason for believing in the existence of material objects? That, too, is only a convenient hypothesis; only, it will never cease to be so, while some day, no doubt, the ether will be thrown aside as useless.
> (Poincaré, 1952 [1905], p. 211-212)

- Maxwell's theory

> The first time a French reader opens Maxwell's book, his admiration is tempered with a feeling of uneasiness, and often of distrust.
>
> It is only after prolonged study, and at the cost of much effort, that this feeling disappears. Some minds of high calibre never lose this feeling. Why is it so difficult for the ideas of this English scientist to become acclimatised among us? No doubt the education received by most enlightened Frenchmen predisposes them to appreciate precision and logic more than any other qualities. In this respect the old theories of mathematical physics gave us complete satisfaction. All our masters, from Laplace to Cauchy, proceeded along the same lines. Starting with clearly enunciated hypotheses, they deduced from them all their consequences with mathematical rigour, and then compared them with experiment. It seemed to be their aim to give to each of the branches of physics the same precision as to celestial mechanics.
>
> A mind accustomed to admire such models is not easily satisfied with a theory. Not only will it not tolerate the least appearance of contradiction, but it will expect the different parts to be logically connected with one another, and will require the number of hypotheses to be reduced to a minimum.

This is not all; there will be other demands which appear to me to be less reasonable. Behind the matter of which our senses are aware, and which is made known to us by experiment, such a thinker will expect to see another kind of matter—the only true matter in its opinion—which will no longer have anything but purely geometrical qualities, and the atoms of which will be mathematical points subject to the laws of dynamics alone. And yet he will try to represent to himself, by an unconscious contradiction, these invisible and colourless atoms, and therefore to bring them as close as possible to ordinary matter.

Then only will he be thoroughly satisfied, and he will then imagine that he has penetrated the secret of the universe. Even if the satisfaction is fallacious, it is none the less difficult to give it up. Thus, on opening the pages of Maxwell, a Frenchman expects to find a theoretical whole, as logical and as precise as the physical optics that is founded on the hypothesis of the ether. He is thus preparing for himself a disappointment which I should like the reader to avoid; so I will warn him at once of what he will find and what he will not find in Maxwell.

Maxwell does not give a mechanical explanation of electricity and magnetism; he confines himself to showing that such an explanation is possible. He shows that the phenomena of optics are only a particular case of electro-magnetic phenomena. From the whole theory of electricity a theory of light can be immediately deduced. (Poincaré, 1952 [1905], p. 213-216)

*The Mechanical Explanation of Physical Phenomena*

In every physical phenomenon there is a certain number of parameters which are reached directly by experiment, and which can be measured. I shall call them the parameters $q$. Observation next teaches us the laws of the variations of these parameters, and these laws can be generally stated in the form of differential equations which connect together the parameters $q$ and time. What can be done to give a mechanical interpretation to such a phenomenon? We may endeavor to explain it, either by the movements of ordinary matter, or by those of one or more hypothetical fluids. These fluids will be considered as formed of a very large number of isolated molecules $m$. When may we say that we have a complete mechanical explanation of the phenomenon? It will be, on the one hand, when we know the differential equations which are satisfied by the coordinates of these hypothetical molecules $m$, equations which must, in addition, conform to the laws of dynamics; and, on the other hand, when we know the relations which define the coordinates of the molecules $m$ as functions of the parameters $q$, attainable by experiment. These equations, as I have said, should conform to the principles of dynamics, and, in particular, to the principle of the conservation of energy, and to that of least action.

The first of these two principles teaches us that the total energy is constant, and may be divided into two parts:

(1) Kinetic energy, or *vis viva*, which depends on the masses of the hypothetical molecules $m$, and on their velocities. This I shall call $T$. (2) The potential energy which depends only on the coordinates of these molecules, and this I shall call $U$. It is the sum of the energies $T$ and $U$ that is constant.

Now what are we taught by the principle of least action? It teaches us that to pass from the initial position occupied at the instant $t_0$ to the final position occupied at the instant $t_1$, the system must describe such a path that in the interval of time between the instant $t_0$ and $t_1$, the mean value of the action—*i.e.*, the *difference* between the two energies $T$ and $U$, must be as small as possible. The first of these two principles is, moreover, a consequence of the second. If we know the functions $T$ and $U$, this second principle is sufficient to determine the equations of motion.

Among the paths which enable us to pass from one position to another, there is clearly one for which the mean value of the action is smaller than for all the others. In addition, there is only [one] such path; and it follows from this, that the principle of least action is sufficient to determine the path followed, and therefore the equations of motion. We thus obtain what are called the equations of Lagrange. In these equations the independent variables are the coordinates of the hypothetical molecules $m$; but I now assume that we take for the variables the parameters $q$, which are directly accessible to experiment.

The two parts of the energy should then be expressed as a function of the parameters $q$ and their derivatives; it is clear that it is under this form that they will appear to the experimenter. The latter will naturally endeavor to define kinetic and potential energy by the aid of quantities he can directly observe. If this be granted, the system will always proceed from one position to another by such a path that the mean value of the action is a minimum. It matters little that $T$ and $U$ are now expressed by the aid of the parameters $q$ and their derivatives; it matters little that it is also by the aid of these parameters that we define the initial and final positions; the principle of least action will always remain true.

Now here again, of the whole of the paths which lead from one position to another, there is one and only one for which the mean action is a minimum. The principle of least action is therefore sufficient for the determination of the differential equations which define the variations of the parameters $q$. The equations thus obtained are another form of Lagrange's equations.

To form these equations we need not know the relations which connect the parameters $q$ with the coordinates of the hypothetical molecules, nor the masses of the molecules, nor the expression of $U$ as a function of the coordinates of these molecules.

All we need know is the expression of $U$ as a function of the parameters $q$, and that of $T$ as a function of the parameters $q$ and their derivatives—*i.e.*, the expressions of the kinetic and potential energy in terms of experimental data.

One of two things must now happen. Either for a convenient choice of $T$ and $U$ the Lagrangian equations, constructed as we have indicated, will be identical with the differential equations deduced from experiment, or there will be no functions $T$ and $U$ for which this identity takes place. In the latter case it is clear that no mechanical explanation is possible. The *necessary* condition for a mechanical explanation to be possible is therefore this: that we may choose the functions $T$ and $U$ so as to satisfy the principle of least action, and of the conservation of energy. Besides, this condition is *sufficient*. Suppose, in fact, that we have found a function $U$ of the parameters $q$, which represents one of the parts of energy, and that the part of the energy which we represent by $T$ is a function of the parameters $q$ and their derivatives; that it is a polynomial of the second degree with respect to its derivatives, and finally that the Lagrangian equations formed by the aid of these two functions $T$ and $U$ are in conformity with the data of the experiment. How can we deduce from this a mechanical explanation? $U$ must be regarded as the potential energy of a system of which $T$ is the kinetic energy. There is no difficulty as far as $U$ is concerned, but can $T$ be regarded as the *vis viva* of a material system?

It is easily shown that this is always possible, and in an unlimited number of ways. I will be content with referring the reader to the pages of the preface of my *Électricité et Optique* for further details. Thus, if the principle of least action cannot be satisfied, no mechanical explanation is possible; if it can be satisfied, there is not only one explanation, but an unlimited number, whence it follows that since there is one there must be an unlimited number.

One more remark. Among the quantities that may be reached by experiment directly we shall consider some as the coordinates of our hypothetical molecules, some will be our parameters $q$, and the rest will be regarded as dependent not only on the coordinates but on the velocities—or what comes to the same thing, we look on them as derivatives of the parameters $q$, or as combinations of these parameters and their derivatives.

Here then a question occurs: among all these quantities measured experimentally which shall we choose to represent the parameters $q$? and which shall we prefer to regard as the derivatives of these parameters? This choice remains arbitrary to a large extent, but a mechanical explanation will be possible if it is done so as to satisfy the principle of least action.

Next, Maxwell asks: Can this choice and that of the two energies $T$ and $U$ be made so that electric phenomena will satisfy this principle? Experiment shows us that the energy of an electro-magnetic field decomposes into electro-static and electro-dynamic energy. Maxwell recognised that if we regard the former as the potential energy $U$, and the latter as the kinetic energy $T$, and that if on the other hand we take the electro-static charges of the conductiors as the parameters $q$—under these conditions, Maxwell has recognised that electric phenomena satisfies the principle of least action. He was then certain of a mechanical explanation. If he had expounded this theory at the beginning of his first volume, instead of relegating it to a corner of the second, it would not have escaped the attention of most readers. If therefore a phenomenon allows of a complete mechanical explanation, it allows of an unlimited number of others, which will equally take into account all the particulars revealed by experiment. And this is confirmed by the history of every branch of physics. In Optics, for instance, Fresnel believed vibration to be perpendicular to the plane of polarisation; Neumann holds that it is parallel to that plane. For a long time an *experimentum crucis* was sought for, which would enable us to decide between these two theories, but in vain. In the same way, without going out of the domain of electricity, we find that the theory of two fluids and the single fluid theory equally account in a satisfactory manner for all the laws of electro-statics. All these facts are easily explained, thanks to the properties of the Lagrange equations.

It is easy now to understand Maxwell's fundamental idea. To demonstrate the possibility of a mechanical explanation of electricity we need not trouble to find the explanation itself; we need only know the expression of the two functions $T$ and $U$, which are the two parts of energy, and to form with these two functions Lagrange's equations, and then to compare these equations with the experimental laws.

How shall we choose from all the possible explanations one in which the help of experiment will be wanting? The day will perhaps come when physicists will no longer concern themselves with questions which are inaccessible to positive methods, and will leave them to the metaphysicians. That day has not yet come; man does not so easily resign himself to remaining for ever ignorant of the causes of things. Our choice cannot be therefore any longer guided by considerations in which personal appreciation plays too large a part. There are, however, solutions which all will reject because of their fantastic nature, and others which all will prefer because of their simplicity. As far as magnetism and electricity are concerned, Maxwell abstained from making any choice. It is not that he has a systematic contempt for all that positive methods cannot reach, as may be seen from the time he has devoted to the kinetic theory of gases. I may add that if in his *magnum opus* he develops no complete explanation, he has attempted one in an article in the *Philosophical Magazine*. The strangeness and the

> complexity of the hypotheses he found himself compelled to make, led him afterwards to withdraw it.
>
> The same spirit is found throughout his whole work. He throws into relief the essential—*i.e.*, what is common to all theories; everything that suits only a particular theory is passed over almost in silence. The reader therefore finds himself in the presence of form nearly devoid of matter, which at first he is tempted to take as a fugitive and unassailable phantom. But the efforts he is thus compelled to make force him to think, and eventually he sees that there is often something rather artificial in the theoretical "aggregates" which he once admired.
>
> (Poincaré, 1952 [1905], p. 217-224)

- ch. 13 electrodynamics, ampere vs helmholtz regarding faraday unipolar induction experiment. a bit hard to follow

> III. *Difficulties raised by these Theories.*—Helmholtz's theory is an advance on that of Ampère; it is necessary, however, that every difficulty should be removed. In both, the name "magnetic field" has no meaning, or, if we give it one by a more or less artificial convention, the ordinary laws so familiar to electricians no longer applly; and it is thus that the electro-motive force induced in a wire is no longer measured by the number of lines of force met by that wire. And our objections do not proceed only from the fact that it is difficult to give up deeply rooted habits of language and thought. There is something more. If we do not believe in actions at a distance, electrodynamic phenomena must be explained by a modification of the medium. And this medium is precisely what we call "magnetic field," and then the electromagnetic effects should only depend on that field. All these difficulties arise from the hypothesis of open currents.
>
> IV. *Maxwell's Theory.*—Such were the difficulties raised by the current theories, when Maxwell with the stroke of a pen caused them to vanish. To his mind, in fact, all currents are closed currents. Maxwell admits that if in a dielectric, the electric field happens to vary, this dielectric becomes the seat of a particular phenomenon acting on the galvanometer like a current and called the *current of displacement.* If, then, two conductors bearing positive and negative charges are placed in connection by means of a wire, during the discharge there is an open current of conduction in that wire; but there are produced at the same time in the surrounding dielectric currents of conduction. We know that Maxwell's theory leads to the explanation of optical phenomena which would be due to extremely rapid electrical oscillations. At that period such a conception was only a daring hypothesis which could be supported by no experiment; but after twenty years Maxwell's ideas received the confirmation of experiment. Hertz succeeded in producing systems of electric oscillations which reproduce all the properties

of light, and only differ by the length of their wave—that is to say, as violet differs from red. In some measure he made a synthesis of light. It might be said that Hertz has not directly proved Maxwell's fundamental idea of the action of the current of displacement on the galvanometer. That is true in a sense. What he has shown directly is that electromagnetic induction is not instantaneously propagated, as was supposed, but its speed is the speed of light. Yet, to suppose there is no current of displacement, and that induction is with the speed of light; or, rather, to suppose that the currents of displacement produce inductive effects, and that the induction takes place instantaneously—*comes to the same thing.* This cannot be seen at the first glance, but it is proved by an analysis of which I must not even think of giving even a summary here. (Poincaré, 1952 [1905], p. 238-240)

- Rowland's experiment, Lorentz's theory

According to Lorentz's theory, currents of conduction would themselves be true convection currents. Electricity would remain indissolubly connected with certain material particles called *electrons.* The circulation of these electrons through bodies would produce voltaic currents, and what would distinguish conductors from insulators would be that the one could be traversed by these electrons, while the others would check the movement of the electrons. Lorentz's theory is very attractive. It gives a very simple explanation of certain phenomena, which the earlier theories—even Maxwell's in its primitive form—could only deal with in an unsatisfactory manner; for example, the aberration of light, the partial impulse of luminous waves, magnetic polarisation, and Zeeman's experiment.

A few objections still remained. The phenomena of an electric system seemed to depend on the absolute velocity of translation of the centre of gravity of this system, which is contrary to the idea that we have of the relativity of space. Supported by M. Crémieu, M. Lippman has presented this objection in a very striking form. Imagine two charged conductors with the same velocity of translation. They are relatively at rest. However, each of them being equivalent to a current of convection, they ought to attract one another, and by measuring this attraction we could measure their absolute velocity. "No!" replied the partisans of Lorentz. "What we could measure in that way is not their absolute velocity, but their relative velocity *with respect to the ether*, so that the principle of relativity is safe." Whatever there may be in these objections, the edifice of electrodynamics seemed, at any rate in its broad lines, definitively constructed. Everything was presented under the most satisfactory aspect. The theories of Ampère and Helmholtz, which were made for the open currents that no longer existed, seem to have no more than purely historic interest, and the inextricable complications to which these theories led have been almost

> forgotten. This quiescence has been recently disturbed by the experiments of M. Crémieu, which have contradicted, or at least have seemed to contradict, the results formerly obtained by Rowland. Numberous investigators have endeavoured to solve the question, and fresh experiments have been undertaken. What result will they give? I shall take care not to risk a prophecy which might be falsified between the day this book is ready for the press and the day on which it is placed before the public.
>
> THE END
>
> (Poincaré, 1952 [1905], p. 242-244)

# 10 Riezler Quotes

Subtitle of book: Lectures of Aristotle on Modern Physics at an International Congress of Science

Riezler is channeling or pretending for argumentative and dialectic purposes to be "Aristotle" in his address and critiques of modern science after the turn of the century.

> The declination of your psi functions in quantum theory refers to secondary not to primary, to compound not to simple movements. The psi functions report the chances of an observation to find a place $A$ for a charge $B$, or for a charge $X$ a place $Y$. They distribute the chances of a charge to places, the chances of a place to charges. The change of chance cannot be primary motion. Chance is chance for an event. Instead of the 'ether' the probability wave does the vibrating. But what vibrates? The chance for an event.
>
> Again, what is an event? Here you have no answer, except the answer of classical physics. But you cannot apply this unless you decide to lift yourselves up by your own bootstraps. The situation is curious. You proceed from discovery to discovery; that I do not deny. These discoveries are firmly rooted in shifting ground. They are rules for the coincidences of numbers signifying chances for events.
>
> In this situation you turn to philosophy to provide you with a theory of knowledge enabling you to get around any troublesome question. It is the old dodge: the real is the observable—at least for the physicist. You first define the physicist, setting his task. Then you limit the observable: the pointer readings of possible instruments. This is the 'reality' relative to your anonymous observer. I do not know whet11er this can be called theory of knowledge. Anyway it is not philosophy. It seems to me merely a definition of physics.
>
> But that we have already discussed. Applying this so-called theory to your new situation you argue: The only knowledge I shall and can have is knowledge of psi functions. I know all that 'is' when I know all I can. It may be and usually is the case

> that I do not know the psi function or know it insufficiently. Then the probability this psi function reports is but relative to my insufficient knowledge. However, if I have a 'maximum knowledge' of the psi function, this psi function is the condition of physics, the maximum knowledge is the thing itself.
>
> I understand why you assert this. You want to prevent naïve perceptions from interfering with the numbers of the anonymous observer. If an accurate location of an electron is not observable, then an accurate location should not be postulated. An 'accurate location' of an electron is for the physicist a senseless term, to which nothing real corresponds. I approve your intention. The anonymous observer should not mix into his numbers concepts he cannot legitimate. Neverthe1ess, you cannot stop here. Probabilities are probabilities of something. A probability of nothing is still more senseless than an accurate location of an electron.
>
> The physicists of past times have developed your preconceptions of tho measured and measurable quantities on the strength of occurrences on a large scale—without any insight into the microworld. Now you find that these quantities are measurable with but limited accuracy. Perhaps they are not really the right concepts. If they are not, then the task would be to discover the right concepts, the true building stones of Being. But this you do not attempt. Numbers, psi functions, are observable and therefore real, even though they are probabilities of nothing. No! my friends.
>
> (Riezler, 1940, p. 31-32)

## 11 Idealism and the Unity of Subject and Object

Adorno paper Hegel

Husserl moving from limitations in phenomenology towards Hegelian idealism

## 12 Transcendental Epistemology in the Tangent Space

A tangent space is an abstract mathematical space, characterized by

We can presume that the point on a manifold from which the tangent space is constructed is considered to be the object of realism and anti-realism debates. It is characterized e.g. by matter and natural dynamical laws. The tangent space, on the other hand, contains much more information. I argue that many fundamental epistemological and metaphysical debates actually occur in the tangent space, and it is helpful to realize this.

For example, Heisenberg talks about potentialities in quantum mechanics. Are these potentialities properties of the supposed real dynamics of the world, or are they things we can "see" in the tangent space? In other words, the potentialities are transcendental objects characterized by epistemology tangent to the phenomenological manifold.

The manifold points are the synthetic objects [WEYL, KANT, HEGEL?] comprised of the epistemological schema in the tangent space, and theory-laden yet phenomenologically adequate laws predicting measurement outcomes.

# 13 Structures as Synthetic Idealist Objects

Not objects of realism but objects of idealism.

## 13.1 Probabilities as Synthetic Objects

Grant that we are structuralists of some sort, dealing with the phenomenological or mathematical structures of experiences or the theory that may account for the phenomenological experiences in observation.

Say we assume group theory is a framework for characterizing either of these kinds of structures, whether they are mathematically explicit or presumed possible in some future state of epistemology (if philosophers can manage to improve upon our present situation).

Where are probabilities, particularly quantum probabilities, appropriately characterized in the structuralist landscape? Are they like rotations (and the group constituted by a set of rotations), rather more formal and mathematical? Or are they like conceptual or phenomenological structures, of a more epistemological character?

If a set of operations

## 13.2 It from Bit

Instead of going the participatory realism route, like Wheeler [FRENCH AND BITBOL TOO?] we can go with Weyl and Eddington and Cassirer towards rendering objects of fundamental physical theories as epistemological, and ideal.

This section needs Fuchs, French, Bitbol citations/discussion added

## 13.3 Kant and Geometry

A structural realist might say that what Euclidean geometry gets right (for understanding space and the physical world) is the approximate aspects of our reality, under certain limits, that are more or less invariant in non-Euclidean geometrical parts of modern physics. These invariant parts correspond, in Riemannian geometry, for example, to roughly the Minkowski parts of tangent spaces. So, in a space tangential to the Riemannian manifold, which we suppose has some better correspondence to measurable physics, under certain approximate conditions there is a structure which resembles that of a Euclidean structure.

## 14 Eddington on Quantum Probabilities

If we are interested in some aspects of potential consistecy between Eddington's views and PR or Qbism, it is also important to note before getting to some of the particular quantum probability statements, that Eddington does at least appear to adhere to one epistemological tenet about beliefs—that they can come in degrees. For many Bayesians, the probability calculus is about subjective degrees of belief. That is, the numbers are subjective or epistemological or ideal or synthetic objects.

> One believes with varying degrees of confidence. My belief that I know the exact number of protons and electrons in the universe does not rank among my strongest scientific convictions, but I should describe it as a fair average sort of belief. I am, however, strongly convinced that, if I have got the number wrong, it is just a silly mistake, which would speedily be corrected if there were more workers in this field. In short, to know the exact number of particles in the universe is a perfectly legitimate aspiration of the physicist. (Eddington, 1939, p. 171)

> It seems to me that the "enlarged" physics which is to include the objective as well as the subjective is just *science*; and the objective, which has no reason to conform to the pattern of systematisation that distinguishes present-day physics, is to be found in the non-physical part of science. We should look for it in the part of biology (if any) which is not covered by biophysics; in the part of psychology which is not covered by psychophysics; and perhaps in the part of theology which is not covered by theophysics. The purely objective sources of the objective element in our observational knowledge have already been named; they are *life, consciousness, spirit.*
>
> We reach then the position of idealist, as opposed to materialist, philosophy. The purely objective world is the spiritual world; and the material world is subjective in the sense of selective subjectivism.
>
> (Eddington, 1939, p. 68-69)

- Quoting the entire section VI of chapter V (Epistemology and Relativity Theory) as it expresses I think fairly clearly an intersubjective idealism about observational physics. Also note the comment about a subjective interpretation of probability.

> I have been continually emphasizing the subjectivity of the universe described in physical science. But, you may ask, was it not the boast of the theory of relativity that it penetrated beyond the relative (subjective) aspect of phenomena and dealt

with the absolute? For example, it showed that the usual separation of space and time is subjective, being dependent on the observer's motion, and it substituted a four-dimensional space-time independent of the observer. It may seem difficult to reconcile this view of Einstein's theory as lifting the veil of relativity which hides the absolute from us, with my present account of modern physics as acquiescing in, and making the bet of, a partially subjective universe.

It is necessary to remember that there has been thirty years' progress. Relativity began like a new broom, sweeping away all the subjectivity it found. But, as we have advanced, other influences of subjectivity have been detected which are not so easily eliminated. Probability, in particular, is frankly subjective, being relative to the knowledge which we happen to possess. Instead of being swept away, it has been exalted by wave mechanics into the main theme of physical law.

The subjectivity referred to in these lectures is that which arises from the sensory and intellectual equipment of the observer. Without varying this equipment, he can vary in position, velocity and acceleration. Such variations will produce subjective changes in the appearance of the universe to him; in particular the changes depending on his velocity and acceleration are more subtle than was realised in classical theory. Relativity theory allows us to remove (if we wish) the subjective effects of these *personal* characteristics of the observer; but it does not remove the subjective effects of *generic* characteristics common to all "good" observers—although it has helped to bring them to light.

Confining attention to the personal, as distinguished from the generic, subjectivity, let us see precisely what is meant by removing this subjectivity. There does not seem to be much difficulty in conceiving the universe as a three-dimensional structure viewed from no particular position; and I suppose we can, after a fashion, conceive it without any standard of rest or of non-acceleration. It is perhaps rather unfortunate that it is, or seems to be, so easy to conceive; because the conception is liable to be mischievous from the observational point of view. Since physical knowledge must in all cases be an assertion of the results of observation (actual or hypothetical), we cannot avoid setting up a dummy observer; and the observations which he is supposed to make are subjectively affected by his position, velocity and acceleration. The nearest we can get to a non-subjective, but nevertheless observational, view is to have before us the reports of all possible dummy observers, and pass in our minds so rapidly from one to another that we identify ourselves, as it were, with all the dummy observers at once. To achieve this we seem to need a revolving brain.

Nature not having endowed us with revolving brains, we appeal to the mathematician to help us. He has invented a transformation process which enables us to pass very quickly from one dummy observer's account to another's. The knowledge is expressed in terms of tensors which have a fixed system of interlocking assigned to them; so that when one tensor is altered all the other tensors are altered, each in a determinate way. By assigning each physical quantity to an appropriate class of tensor, we can arrange that, when one quantity is changed to correspond to the change from dummy observer $A$ to dummy observer $B$, all the other quantities change automatically and correctly. We have only to let one item of knowledge run through its changes—to turn one handle—to get in succession the complete observational knowledge of all the dummy observers.

The mathematician goes one step farther; he eliminates the turning of the handle. He conceives a tensor symbol as containing in itself all its possible changes; so that when he looks at a tensor equation, he sees all its terms changing in synchronised rotation. This is nothing out of the way for a mathematician; his symbols commonly stand for unknown quantities, and functions of unknown quantities; they are everything at once until he chooses to specify the unknown quantity. And so he writes down the expressions which are symbolically the knowledge of all the dummy observers at once—until he chooses to specify a particular dummy observer.

But, after all, this device is only a translation into symbolism of what we have called a revolving brain. A tensor may be said to symbolise absolute knowledge; but that is because it stands for the subjective knowledge of all possible subjects at once.

This applies to personal subjectivity. To remove the generic subjectivity, due say to our intellectual equipment, we should have similarly to symbolise the knowledge as it would be apprehended by all possible types of intellect at once. But this could scarcely be accomplished by a mathematical transformation theory. And what would be the result if it were accomplished? According to Chapter IV, if we remove all subjectivity we remove all the fundamental laws of nature and all the constants of nature. But, after all, these subjective laws and facts happen to be important to beings who are not equipped with revolving brains and variable intellects. And if the physicist does not take charge of them, no one else is qualified to do so. Even in relativity theory, which deals with the absolute (in a somewhat limited sense), we continually hark back to the relative to examine how our results will appear in the experience of an individual observer. We are not so eager now as we were twenty years ago to eliminate the observer from our world view. Sometimes it may be desirable to banish him and his subjective distortion of things for a time, but we are bound to bring him back in the end; for he stands for—ourselves. (Eddington, 1939, p. 85-88)

### 14.0.1 Chapter VI: Epistemology and Quantum Theory

> I must still keep hammering at the question, What do we really observe? Relativity theory has returned one answer—we only observe *relations*. Quantum theory returns another answer—we only observe *probabilities*. (Eddington, 1939, p. 89)

- Heisenberg uncertainty principle as consequence of different measurements as source for irreducible indeterminism in QM

> The suggestion is that in the new physics the so-called probabilities are actually the real entities—the elemental stuff of the physical universe. We have precise knowledge of *them*; and it would seem retrogressive to postulate other entities behind them of which our knowledge must always be uncertain.
>
> I think that this idea is at the back of a rather common suggestion that a proper reformulation of our elementary concepts would banish the present indeterminism from the system of physics. The idea is that the indeterminism revealed by the new physics is not intrinsic in the universe, but appears only in our attempt to connect it with the obsolete universe of classical physics. Probability would then be merely the funnel through which the new wine is poured into old bottles.
>
> But the suggestion overlooks the essential feature of the indeterminism of the present system of physics, namely that the quantities which it can predict only with uncertainty are quantities which, *when the time comes*, we shall be able to observe with high precision. The fault is therefore not in our having chosen concepts inappropriate to observational knowledge. For example, Heisenberg's principle tells us that the position and velocity of an electron at any moment can only be known with a mutually related uncertainty; and, taking the most favourable combination, the position of the electron one second later is uncertain to about 4 centimetres. This is the uncertainty of the prediction from the best possible knowledge we can have at the time. But one second later the position can be observed with an uncertainty of no more than a fraction of a millimetre. It has often been argued that the impossibility of knowing simultaneously the exact position and exact velocity only shows that position and velocity are unsuitable conceptions to use in expressing our knowledge. I have no special attachment to these conceptions; and I will grant, if you like, that our knowledge of the universe at the present moment can be regarded as perfectly determinate (the supposed indeterminacy being introduced in translating it into an inappropriate frame of conception). But that does not remove the "indeterminism" (which is distinct from the "indeterminacy"), namely that this knowledge, however expressed, is inadequate to predict quantities which, independently of our frame of conception, can be directly observed when the time comes.

> Returning to the more general aspect of the probability conception, we find that it cannot be got rid of by any transformation of outlook. It is not possible to transform the current system of physics, which by its equations links probabilities in the future with probabilities in the present, into a system which links ordinary physical quantities in the future with ordinary physical quantities in the present, without altering its observable content. The bar to such a transformation is that probability is not an "ordinary physical quantity". At first sight it appears to be one; we obtain knowledge of it from observation, or from a mixture of observation and deduction, as we obtain knowledge of other physical quantities. But it is differentiated from them by a peculiar irreversibility of its relation to observation. The result of an observation determines definitely a probability distribution of some quantity, or a modification of a previously existing probability distribution; but the connection is not reversible, and a probability distribution does not determine definitely the result of an observation. For an ordinary physical quantity there is no difference between making a new determination and verifying a predicted value; but for probability the procedures are distinct.
>
> Thus we may expand the answer of quantum theory that "we only observe probabilities" into the form: The synthesis of knowledge which constitutes theoretical physics is connected with observation by an *irreversible* relation of the formal type familiar to us in the concept of probability. (Eddington, 1939, p. 90-91)

- End of quote above talking about probability as structural invariant in some sense like Cassirer would general covariance (does Ryckman also notice this?)

- Irreversibility illustrated through bag w/ colored ball example, comparing to wave packet probability distributions

> According to wave mechanics, an observation determines or produces a concentrated wave packet in the probability distribution. This wave packet diffuses according to laws embodied in the equations of the theory; and we can calculate the form into which the wave packet will have spread one unit of time later. *But the theory does not assert that this is the form of wave packet which would be produced by an observation made one unit of time later.* On the other hand, if from the observationally determined form of a sound wave at one instant we calculate the form into which it will have spread one unit of time later, the whole point of the theory is that we obtain the form which would be determined by observations made one unit of time later. (Eddington, 1939, p. 93)

- "an observation" vs "an item of observational knowledge". Holistic view, Quine?

> More generally we must recognise that an item of observational knowledge involves, besides a primary pointer reading, secondary pointer readings identifying the circumstances in which the primary pointer reading occurred. It must be admitted that even an isolated pointer reading is an item of knowledge of a sort; but it is not with such items that the scientific method deals. For scientific knowledge the association with other pointer readings is an essential condition; and we may therefore describe physical knowledge as a knowledge of the associations of pointer readings. (Eddington, 1939, p. 100)

- Referring to other book, criticizing older position. Alluding to uncertainty principle: pointer reading interference. Wave mechanics(!) Scope of physical knowledge and defining physical universe.

> In *The Nature of the Physical World* it is emphasised that physical knowledge is concerned with the connection of pointer readings rather than with the pointer readings themselves; and it is concluded that the connectivity of pointer readings, as expressed by the laws of physics, supplies the common background which realistic problems always demand—the background described by the tertiary pointer readings which are not determined afresh for each individual item of knowledge. But, if I may venture to criticise the author of that book, he does not seem to have appreciated the difficulty which arises through the interference of pointer readings with one another when we contemplate such an unlimited multiplicity of pointer readings. It is true that the interference is negligible in molar physics (to which the discussion in *The Nature of the Physical World* was limited). But in a fundamental discussion of this kind it is not legitimate to separate molar physics from microscopic physics; for we have seen (p. 76) that neither branch is logically complete in itself.
>
> Our definition of the physical universe is that it is the world which physical knowledge is formulated to describe. The interference of observations creates a difficulty which must be met in one of two ways. Either we must take the complete description of the physical universe to embody more than the totality of our possible knowledge of it; so that, whichever of two interfering observations we choose to make, there will be a place for it in the description. Or we must adopt a *flexible* universe containing nothing which is not represented by our actual knowledge (or in theoretical discussions by the supposedly actual knowledge furnished as data of the problem considered). In the first alternative we cannot consistently suppose all the items of the complete description to be represented by actual pointer readings; and it is therefore not true to say that its structure is a connectivity of pointer readings. The second alternative is adopted in wave mechanics, which accepts as leading features of the physical universe the probability waves created by actual observation of the physical quantities with which they are

> associated. Clearly there is no more than a formal distinction between the study of a universe flexible according to the knowledge we happen to have of it and a direct study of the knowledge itself. Either alternative brings us back to the conclusion that the common background is required to connect one item of knowledge with the rest of knowledge, rather than one element of an external universe with the rest of the universe. (Eddington, 1939, p. 100-102)

- Eddington provides a summary (section V of ch. VI Epistemology and Quantum Theory) which will be useful to have (I feel like I'm typing the whole book...) Adapting very slightly the enumeration to be LaTeX friendly

> The following summary will recall the principal conclusions that we have so far reached:
>
> 1. Physical knowledge (by definition) includes only knowledge capable of observational test; an item of physical knowledge must therefore assert the result of a specified observational procedure.
> 2. The definitions of the terms used in expressing physical knowledge must be such as to secure that (1) is satisfied. In particular the definition of a physical quantity must specify unambiguously a method of measuring it.
> 3. Strict adherence to (2) involves a number of modifications of the conceptions and practice of classical physics; and indeed there still survive glaring violations of it in current quantum theory. The points (4) to (9) below arise when the definitions are scrutinised from this point of view.
> 4. The first definitions required are those of length and time-interval, since the definitions of other physical quantities presuppose these. The standards of length and time must be structures specified by pure numbers only (since no other quantitative terms are available at this early stage). This means that the standards must be reproducible from a quantum specification.
> 5. Only short standards, suitable for measuring infinitesimal displacements in space and time, are provided by such specifications; and it must not be assumed that the infinitesimal displacements so measured are integrable.
> 6. Owing to the interference of exact observations with one another, an attempt to define observationally the exact conditions under which the measurement of a physical quantity is intended to be carried out breaks down. It is therefore necessary to leave the minor details to chance.
> 7. In this way the probability conception is incorporated in the fundamental definitions. It introduces an irreversible relation between observation and formulated observational knowledge. This irreversibility makes the existing system

of physics indeterministic, considered as a system of prediction of what can be observed at a future time.

8. Certain quantities used in the formulation of physical knowledge in classical physics are found to have no definition satisfying (2). These are unobservables, e.g. absolute simultaneity at a distance.
9. Other quantities, conditionally observable, have been employed in conditions in which they are unobservable. For example, the definition of relative coordinates presupposes that the particles are distinguishable, but ordinary relative coordinates are still used erroneously in problems concerning indistinguishable particles.
10. The conclusions (4) to (9) are reached by considering the way in which physical knowledge is obtained and formulated. We refer to them as epistemological or *a priori* conclusions, to distinguish them from *a posteriori* conclusions derived from a study of the results of observations which have been obtained and formulated in this way.
11. Although epistemological conclusions are of the nature of truisms, they have far-reaching consequences in physics. Thus the unobservability of absolute simultaneity (8) leads to the special theory of relativity; the non-integrability of displacement (5) leads to Einstein's theory of gravitation; the introduction of the probability conception in a fundamental way (7) leads to the method of wave mechanics.
12. In the modified theories which result, epistemological principles play a part which was formerly taken by physical hypotheses, i.e. generalisations suggested by an *a posteriori* study of the results of observation.
13. Current relativity theory and quantum theory, as usually accepted, have not yet taken full advantage of this epistemological method. It appears that when the epistemological scrutiny of definitions is systematically applied, and its consequences are followed up mathematically, we are able to determine all the "fundamental" laws of nature (including purely numerical constants of nature) without any physical hypothesis.
14. This means that the fundamental laws and constants of physics are wholly subjective, being the mark of the observer's sensory and intellectual equipment on the knowledge obtained through such equipment; for we could not have this kind of *a priori* knowledge of laws governing an objective universe.
15. It is not suggested that the physical universe is wholly subjective. Physical knowledge comprises, besides "laws of nature", a vast amount of special information about the particular objects surrounding us. This information is doubtless partly objective as well as partly subjective.

16. The subjective laws are a consequence of the conceptual frame of thought into which our observational knowledge is forced by our method of formulating it, and can be discovered *a priori* by scrutinising the frame of thought as well as *a posteriori* by examining the actual knowledge which has been forced into it.
17. The characteristic form of the fundamental laws of physics is the stamp of subjectivity. If there are also laws of objective origin, they may be expected to be of a different type. It seems probable that wherever effects of objective governance have appeared they have been regarded as an indication that the subject is "outside physics", e.g. conscious volition, or possibly life.
18. Epistemological laws (if correctly deduced) are compulsory, universal, and exact. Since the fundamental laws of physics are epistemological, they have this character—contrary to the view usually advocated in scientific philosophy, which has assumed that they are merely empirical regularities.(Eddington, 1939, p. 102-105)

### 14.0.2 Later in the book

> Since then microscopic physics has made great progress, and its laws have turned out to be comprehensible to the mind; but, as I have endeavoured to show, it also turns out that they have been imposed by the mind—by our forms of thought—in the same way that the molar laws are imposed. (Eddington, 1939, p. 180)

- applicability of "laws of chance" vs. Heisenberg uncertainty limits and quantum probabilities??? !!!

> In current physical theory the undetermined element in the behaviour of a system is treated as a matter of chance. If there were serious deviations from the law of chance, observation and theory would not agree. We may therefore say that it is a hypothesis in physics, supported by observation, that there are no objective laws of governance—unless chance is described as a law.
>
> Nevertheless, if we take a wider view than that of physics, I think it would be misleading to regard chance as the characteristic feature of the objective world. The denial of objective laws of governance is not so much a hypothesis of physics as a limitation of its subject matter. Deviations from chance occur, but they are regarded as manifestations of something outside physics, namely consciousness or (more debatably) life. There is in a human being some portion of the brain, perhaps a mere speck of brain-matter, perhaps an extensive region, in which the physical effects of his volitions begin, and from which they are propagated to the nerves and muscles which translate the

volition into action. We will call this portion of the brain-matter "conscious matter". It must be exactly like inorganic matter in its obedience to the fundamental laws of physics which, being of epistemological origin, are compulsory for all matter; but it cannot be identical in all respects with inorganic matter, for that would reduce the body to an automaton acting independently of consciousness. The difference must necessarily lie in the undetermined part of the behaviour; the part of the behaviour which is undetermined by the fundamental laws of physics must in conscious matter be governed by objective law or direction instead of being wholly a field of chance.

The term "law of chance" tends to mislead, because it is applied to what is merely an absence of law in the usual sense of the term. It is clearer to describe the conditions by reference to correlation. The hypothesis of current physical theory, which is confirmed by observation of inorganic phenomena, is that there is no correlation of the undetermined behaviour of the individual particles.

Accordingly the distinction between ordinary matter and conscious matter is that in ordinary matter there is no correlation in the undetermined parts of the behaviours of the particles, whereas in conscious matter correlation may occur. Such correlation is looked upon as an interference with the ordinary course of nature, due to the association of consciousness with the matter; in other words, it is the physical aspect of a volition. This does not mean that, in order to execute a volition, consciousness must direct each individual particle in such a way that correlation occurs. The particles are merely a representation of our knowledge in the frame of thought corresponding to the concept of analysis and the atomic concept. When we apply the system of analysis which gives this representation, we cannot foresee whether the resulting particles will have correlated or uncorrelated behaviour; that depends entirely on the objective characteristics of whatever it is that we are analysing. When non-correlation is assumed, as is customary in physics, it is assumed as a hypothesis. But, without making any hypothesis, we can say that correlation and non-correlation are representations in our frame of thought of different objective characteristics; and since non-correlation admittedly represents the objective characteristic of systems to which the ordinary formulae of physics apply, correlation must represent another objective characteristic which—since it is not characteristic of systems to which the formulae of physics apply—is regarded by us as something "outside physics".

In the discussion of freewill provoked by the modern physical theories, it has, I think, generally been assumed that, since the ordinary laws of inorganic matter leave its behaviour undetermined within a certain narrow range, there can be no scientific objection to allowing a volition of consciousness to decide the exact behaviour within the limits of the aforesaid range. I will call this hypothesis *A*. For any system on a molar scale the permitted range is exceedingly small; and very far-fetched suppositions are necessary to enable volition, working in so small a range, to produce large muscular movements. To obtain a wider range we must admit correlation of the behaviour of the particles. This is the theory we have been discussing, and will be called hypothesis *B*. In former writings I have advocated hypothesis *B* mainly on the ground of the inadequacy of hypothesis *A*; but in the present mode of approach hypothesis *B* presents itself as the obvious and natural solution.

Although leading to the same conclusion, my earlier discussions[4] were marred by a failure to recognise that hypothesis *A* is nonsense; so that I was more apologetic than I need have been for going beyond it. There is no half-way house between random and correlated behaviour. Either the behaviour is wholly a matter of chance, in which case the precise behaviour within the Heisenberg limits of uncertainty depends on chance and not on volition. Or it is not wholly a matter of chance, in which case the Heisenberg limits, which are calculated on the assumption of non-correlation, are irrelevant. If we apply the law of chance to the tossing of a coin, the number of heads in 1000 throws is undetermined within the limits, say, 450 to 550. But if a coin-tossing machine is used which picks up and throws the coin not entirely at random, the non-chance element is not a factor deciding which number between 450 and 550 will turn up; a correlation, or systematic tendency in tossing, may produce any number of heads from 0 to 1000.

The fallacy of hypothesis *A* was that it assumed the behaviour to be restricted by the ordinary laws of physics including the hypothesis of non-correlation or "law of chance", and then to be further restricted (or decided) by a non-chance factor (volition). But we cannot suppose the behaviour to be restricted by chance and non-chance (non-correlation and correlation) simultaneously. The applicability of the law of chance is a hypothesis; the admission that the behaviour is not governed solely by chance denies the hypothesis. So if we admit volition at all, we must not forget first to remove the hypothesis of chance if we have been applying it; in particular we must drop the Heisenberg limits which apply only to non-correlated behaviour. If volition operates on the system, it does so without regard to the Heisenberg limits. Its only limits are those imposed by the fundamental epistemological laws.

Our volitions are not entirely unconsequential; so that there must be laws of some kind applying to them and connecting them with other constituents of consciousness, though such laws are not expected to be of the mathematically exact type characteristic of subjective law. Primarily the sphere of ob-

[4] *The Nature of the Physical World*, pp. 310-315. *New Pathways in Science*, p. 88.

> jective law is the interplay of thoughts, emotions, memories and volitions in consciousness. In controlling volitions objective law controls also the correlations which are the physical counterparts of volitions.
>
> Our philosophy has led to the view that in so far as we can separate the subjective and objective elements in our experience, the subjective is to be identified with the physical and the objective with the conscious and spiritual aspects of experience. To this we now add, as a helpful analogy provided it is not pressed too far, that conscious purpose is the "matter" and chance the "empty space" of the objective world. In the physical universe matter occupies only a small region compared with the empty space; but, rightly or wrongly, we look on it as the more significant part. In the same way we look on consciousness as the significant part of the objective universe, though it appears to occur only in isolated centres in a background of chaos. (Eddington, 1939, p. 180-184)

## 14.1 Probabilities as Structural Objects

EPISTEMOLOGICAL (Compare to modern Bayesian DOB, Qbism)
Intersubjective, communicable
Convergence?
Convention?
Poincare –¿ not arbitrary
persistent? –¿ uranoid

## 14.2 Uranoids

In some of the most interesting epistemologically sophisticated treatises on the foundations of physics, it is not uncommon to find authors consciously label an important concept for ease of reference as they build and explicate their thoughts. For context, I am thinking Bohm's notion of rheomode, for example.

Eddington (1939) does something similar with the concept of a uranoid. It is a word used as shorthand to refer to an important focus point of his structuralist views on the epistemology of physics. Specifically, it refers to the structure "that is contemplated as continually existing". (Eddington, 1939, p. 166)

## 14.3 Qbism, Selective Subjectivism, Participatory Realism

From de Finetti, Ramsey, and subsequently championed by others, the Bayesian interpretation of probabilities holds that probabilities are not frequencies [FISHER?], not propensities [POPPER?], not logically derived [KEYNES?]. Probabilities are *subjective degrees of belief.* Regardless of the manner in which an agent comes to justify their subjective degrees of belief, there are very good reasons why these subjective degrees of belief should, at a minimum, *not* violate the axioms of probability (non-negativity, finite additivity, sum to 1).

Probabilities, under this interpretation, can perhaps be taken as mental conventions that are apparently useful for epistemology, rationality, statistics, and science. While conventional, that doesn't mean any or all subjective degrees of belief *are* or *should be* arbitrary. A similar point was made by Poincaré in his conventionalist philosophy of mathematics about geometry in particular.

Quantum Bayesianism, or Qbism for short, tries to think similarly about quantum probabilities. This is not the place to argue all of the finer points, but suffice to say for present purposes that there are rules for quantum probabilities that seem like more than just conventions or subjective degrees of belief in the way that Bayesian probabilities can be used to characterize uncertainty in other scenarios. Don't quantum probabilities correspond to something more substantive or lawlike?

# 15 Comparing Selective Subjectivism and Participatory Realism

While perhaps agreeing on the fundamentals, it is striking that Eddington (who I'm using as a caricature for the moment) considers his position to be much more aligned with what he calls "idealism". Perhaps he was unaware of phenomenology or Husserl, but it brings up an important question that it seems to me should also be addressed with newer interpretations in quantum foundations, in particular participatory realism and any connections to the view of quantum probabilities as subjective degrees of belief (Qbism).

What is the proper philosophical context for understanding, is it phenomenology or idealism? Is it transcendental phenomenological idealism? Using Husserl as a reference, are we talking about his earlier views or later views where he may have distanced himself from his earlier phenomenology? I think Michel Bitbol will have some thoughts on this particular point.

# 16 Is Idealism Smuggling in Quantum Mysticism?

NO.

But the "observer effect" will need to be sorted out from participatory realism and selective subjectivism etc.

**This section is very important!**

- spectrum of Berkeley, Solipsism very different from sophisticated transcendental phenomenological idealism of Cassirer or Eddington

- very many reasons why forms of quantum mysticism are... not correct, and not to be confused with sophisticated philosophical idealism program. extremely important that we make that clear for a number of reasons, not least to make sure that a major strain in philosophy and philosophy of science is not disregarded and guilty by association

## 16.1 Formal Epistemology and the Structure of Physical Experience

THIS SEEMS OUT OF PLACE

Eddington treats the true or right things about the foundations of physics as epistemological principles, and not statements about an objective external world (realism). The structures that physicists talk about are more experiential and phenomenological, and *more* normatively compelling than if we supposed they corresponded to realist structures out there in the world.

Normatively compelling epistemological principles are perhaps the most interesting part of formal epistemology. For example, in Bayesian epistemology, the axioms of probability theory are normatively compelling for an idealized agent's subjective degrees of belief.

# 17 Weyl

## 17.1 On Open Systems

Hartmann and Cuffaro papers

Ch. 4 in Weyl collection

Comments in Weyl (1949) in the context of infinity and the continuum touches on aspects of process philosophy.

> Space is infinite not only in the sense that it never comes to an end; but at every place it is, so to speak, inwardly infinite, inasmuch as a point can only be fixed step-by-step by a process of subdivision which progresses *ad infinitum.* This is in contrast with the resting and complete existence that intuition ascribes to space. The 'open' character is communicated by the continuous space and the continuously graded qualities to the things of the external world. A real thing can never be given adequately, its 'inner horizon' is unfolded by an infinitely continued process of ever new and more exact experiences; it is, as emphasized by Husserl, a limiting idea in the Kantian sense. For this reason it is impossible to posit the real thing as existing, closed and complete in itself. The continuum problem thus drives one toward epistemological idealism. Leibniz, among others, testifies that it was the search for a way out of the "labyrinth of the continuum" which first suggested to him the conception of space and time as orders of the phenomena. "From the fact that a mathematical solid cannot be resolved into primal elements it follows immediately that it is nothing real but merely an ideal construct designating only a possibility of parts" (correspondence Leibniz-De Volder, Leibniz, *Philosophische Schriften*, II, p. 268).
> (Weyl, 1949, p. 41)

## 17.2 On Quantum Probabilities

Weyl seems to have a physicalist interpretation of probabilities, which I find odd given the intuitionist and idealist strains in his thought. See e.g. Appendix C in (Weyl, 1949).

But he does say that the formalist (Hilbert) is in line with idealist Kant (p. 64), and so maybe it would make sense to discuss whether it makes more sense to characterize quantum probabilities as idealist or intuitionist... Have to think about that some more.

In his section on Brouwer's intuitive foundation for mathematics, Weyl

> If I consider an insight a valuable treasure, then the propositional abstract is merely a document indicating the presence of a treasure without disclosing its location. Its only value may lie in the fact that it causes me to look for the treasure. It is a worthless piece of paper as long as it is not endorsed by a real proposition such as '2 is an even number.' Whenever nothing but the *possibility* of a construction is being asserted, we have no meaningful proposition; only by virtue of an effective construction, an executed proof, does an existential statement acquire meaning. In any of the numerous existential theorems in mathematics, what is valuable in each case is not the theorem as such but the construction carried out in its proof; without it the theorem is an empty shadow.
>
> (Weyl, 1949, p. 51)

Actually, now that I think about it, Weyl is probably indeed having the position I think he has. For *pure* mathematics, he will go with constructive intuitionism. For natural science where the symbols represent some epistemological relationship to an external reality, the symbols *must* be a synthetic combination of ideal and intuitionistic *and also* something transcendental in the external world (e.g. the quantum world).

If they are synthetic in this way, then indeed one would naturally have to have some kind of "participatory" relationship.

Which brings back to Eddington's selective subjectivism, but also Cassirer's analysis in e.g. Cusanus and renaissance philosophy.

Measurement is like construction in a proof, but it requires an interaction with the world. See p. 65 (Weyl, 1949, p. 65)

[IS THAT RIGHT? THINK ABOUT IT SOME MORE]

> The stages through which research in the foundations of mathematics has passed in recent times correspond to the three basic possibilities of epistemological attitude. The set-theoretical approach is the stage of *naive realism* which is unaware of the transition from the given to the transcendent. Brouwer represents *idealism*, by demanding the reduction of all truth to the intuitively given. In axiomatic formalism, finally, consciousness makes the attempt to 'jump over its own shadow,' to leave behind the stuff of the given, to represent the *transcendent*—but, how could it be otherwise?, only through the *symbol*. Basically, the idealist viewpoint in epistemology has been adhered to by occidental philosophy since Descartes; nevertheless, it

has searched again and again in metaphysics for an access to the realm of the absolute, and Kant, who meant to shoot the bolt once and for all, was yet followed by Fichte, Schelling, and Hegel. It cannot be denied that a theoretical desire, incomprehensible from the merely phenomenal point of view, is alive in us which urges toward totality. Mathematics shows that with particular clarity; but it also teaches us that that desire can be fulfilled on one condition only, namely, that we are satisfied with the symbol and renounce the mystical error of expecting the transcendent ever to fall within the lighted circle of our intuition. So far, only in mathematics and physics has symbolical-theoretical construction gained that solidity which makes it compelling for everyone whose mind is open to these sciences. Their philosophical interest is primarily based on this fact.

(Weyl, 1949, p. 65-66)

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# 18 Eddington's *The Philosophy of Physical Science* NOTES/QUOTES

## 18.1 Chapter I: Scientific Epistemology

- scientific epistemology more involved and inseperable in revolutions compared to past where

> To advance science and to philosophise on science were essentially distinct activities. In the new movement scientific epistemology is much more intimately associated with science. For developing the modern theories of matter and radiation a definite epistemological outlook has become a necessity; and it is the direct source of the most far-reaching scientific advances.
>
> We have discovered that *it is actually an aid in the search for knowledge to understand the nature of the knowledge which we seek.*
>
> By making practical application of our epistemological conclusions we subject them to the same kind of observational control as physical hypotheses. If our epistemology is at fault, it will lead to an *impasse* in the scientific developments proceeding from it; that warns us that our philosophical insight has not

> been deep enough, and we must cast about to find what has been overlooked. In this way scientific advances which result from epistemological insight have in turn educated our epistemological insight. Between science and scientific epistemology there has been a give and take by which both have greatly benefited.
>
> In the view of scientists at least, this observational control gives to modern scientific epistemology a security which philosophy has not usually been able to attain. It introduces also the same kind of progressive development which is characteristic of science, but not higherto of philosophy. We are not making a series of shots at ultimate truth, which may hit or miss. What we claim for the present system of scientific philosophy is that it is an advance on that which went before, and that it is a foundation for the advances which will come after it.
>
> (Eddington, 1939, p. 5)

- seems to be a nice connection to Bell's theorem and recent Nobel prize experimental work?

- positivism as scientific epistemology for physics specifically? counterfactuals, theory laden observation,

> For the truth of the conclusions of physical science, observation is the supreme Court of Appeal. It does not follow that every item which we confidently accept as physical knowledge has actually been certified by the Court; our confidence is that it would be certified by the Court if it were submitted. But it does follow that every item of physical knowledge is of a form which might be submitted to the Court. It must be such that we can specify (although it may be impracticable to carry out) an observational procedure which would decide whether it is true or not. Clearly a statement cannot be tested by observation unless it is an assertion about the results of observation. *Every item of physical knowledge must therefore be an assertion of what has been or would be the result of carrying out a specified observational procedure.*
>
> I do not think that anyone—least of all, those who are critical of the modern tendencies of physics—will disagree with the first axiom of scientific epistemology, namely that the knowledge obtained by the methods of physical science is limited to observational knowledge in the sense explained above. We do not deny that knowledge which is not of an observational nature may exist, e.g. the theory of numbers in pure mathematics; and non-committally we may allow the possibility of other forms of insight of the human mind into a world outside itself. But such knowledge is beyond the borders of physical science, and therefore does not enter into the description of the world introduced in the formulation of physical knowledge. To a wider synthesis of knowledge, of which physical knowledge is only a part, we may perhaps correlate a "world" of which the

physical universe is only a partial aspect. But at this stage of our inquiry we limit the discussion to physical knowledge, and therefore to a physical universe from which, by definition, all characteristics which are not the subject of physical knowledge are excluded.

A distinction is commonly made between observational and theoretical knowledge; but in practice the terms are used so loosely as to deprive the classification of all real significance. The whole development of physical science has been a process of combining theory and observation; and in general every item of physical knowledge—or at least every item to which attention is ordinarily directed—has a partly observational and partly theoretical basis.

(Eddington, 1939, p. 9-10)

Thus our axiom that all physical knowledge is of an observational nature is not to be understood as excluding theoretical knowledge. I *know* the position of Jupiter last night. That is knowledge of an observational nature; it is possible to detail the observational procedure which yields the quantities (right ascension and declination) which express my knowledge of the planet's position. As a matter of fact I did not follow this procedure, nor did I learn the position from anyone who had followed the procedure; I looked it up in the Nautical Almanac. That gave me the result of a computation according to planetary theory. Present-day physics accepts that theory and all its consequences; that is to say, it admits the calculated position as a foreknowledge of the results which would be obtained by carrying out the recognised observational procedure. Of my two pieces of knowledge, namely knowledge of the results of a mathematical computation and foreknowledge of the results of an observational procedure, it is the latter which I assert when I claim to know the position of Jupiter. If, on submission to the Court of Appeal, my foreknowledge of the result of the observational procedure proves to be incorrect, I shall have to admit that I was mistaken and did not know the position of Jupiter; it will be no use my urging that my knowledge of the result of the mathematical computation was correct.

It is the essence of acceptance of a theory that we agree to obliterate the distinction between knowledge derived from it and knowledge derived from actual observation. It may seem one-sided that the obliteration of the distinction should render all physical knowledge observational in nature. But not even the most extreme worshipper of theory has proposed the reverse—that in accepting the results of an observational research as trustworthy we elevate them to the status of theoretical conclusions. The one-sidedness is due to our acceptance of observation, not theory, as the supreme Court of Appeal.

(Eddington, 1939, p. 10-11)

- counterfactual hypothetico-observational physical knowledge "means knowledge of the result of a hypothetical observation, not hypothetical interpretation of the result of an actual observation." -p. 12 footnote

> Although it would be true to assert that 240,000 miles is the result of an actual observational procedure of swinging pendulums, etc., that is not what we intend to assert when we say that the distance of the moon is 240,000 miles. By employing accepted theory we have been able to substitute for the actual observational procedure a hypothetical observational procedure which would yield the same result if it were carried out. The gain is that hypothetico-observational knowledge can be systematised and gathered into a coherent whole, whereas actual observational knowledge is sporadic and desultory.
>
> One cannot help feeling a misgiving that hypothetico-observational knowledge is not entirely satisfactory from a logical standpoint. What exactly is the status of conditional knowledge if the condition is not fulfilled? Can any sense at all be attributed to a statement that if something, which we know did not happen, had happened, then certain other things would have happened? Yet I cannot help prizing my knowledge that 240,000 x 1760 yard-sticks *would* reach from here to the moon, although there is no prospect that they ever *will* do so.
>
> (Eddington, 1939, p. 13)

- generalization of hypothetico-observational knowledge into laws vs systematisation

## 18.2 Chapter II: Selective Subjectivism

-ch 2, fish net ichthyologist analogy w/ two generalizations: all fish are greater than 2 inches in size, all fish have gills.

- generalizing data vs generalizing the epistemological and instrumental methods used to generate/gather data
- distinction between 'scientific philosophy' (logical empiricism?) and what is done in practice, where epistemic generalisations are more secure

> If the ichthyologist extends his investigations, making further catches, perhaps in different waters, he may any day bring up a sea-creature without gills and upset his second generalisation. If this happens, he will naturally begin to distrust the security of his first generalisation. His fear is needless; for the net can never bring up anything that it is not adapted to catch.
>
> *Generalisations that can be reached epistemologically have a security which is denied to those that can only be reached empirically.*
>
> It has been customary in scientific philosophy to insist that the laws of nature have no compulsory character; they are uniformities which have been found to occur hitherto in our limited experience, but we have no right to assert that they will occur invariably and universally. This was a very proper philosophy

> to adopt as regards empirical generalisations—it being understood, of course, that no one would be so foolish as to apply the philosophy in practice.
> (Eddington, 1939, p. 19)

> The situation is changed when we recognise that some laws of nature may have an epistemological origin. These are compulsory; and when their epistemological origin is established, we have a right to our expectation that they will be obeyed invariably and universally. The process of observing, of which they are a consequence, is independent of time or place.
>
> But, it may be objected, can we be sure that the process of observing[5] is unaffected by time or place? Strictly speaking, no. But if it is affected—if position in time and space or any other circumstance prevents the observational procedure from being carried out precisely according to the recognised specification—we can (and do) call the resulting observation a "bad observation". Those who resent the idea of compulsion in scientific law may perhaps be mollified by the concession that, although it can no longer be accepted as a principle of scientific philosophy that the laws of nature are uncompulsory, there is no compulsion that our actual observations shall satisfy them, for (unfortunately) there is no compulsion that our observations shall be *good* observations.
>
> What about the remaining laws of nature, not of an epistemological origin, and therefore, so far as we know, non-compulsory? Must they continue to mar the scheme as a source of indefensible expectations, which nevertheless are found to be fulfilled in practice? Before worrying about them, it will be well to wait till we see what is left of the system of natural law after the part which can be accounted for epistemologically has been removed. There may not be anything left to worry about.
>
> The introduction of epistemological analysis in modern physical theory has not only been a powerful source of scientific progress, but has given a new kind of security to its conclusions. Or, I should rather say, it has put a new kind of security within reach.
> (Eddington, 1939, p. 20-21)

- epistemologist observes observers and helps regulate by figuring out what scientific observers actually observe and what we can say about *good* observations

[5]The standard specification of the procedure of observing must be sufficiently detailed to secure a unique result of the observation. It is the duty of the observer to secure that all attendant circumstances which can affect the result, e.g., temperature, absence of magnetic field, etc., are in accordance with specification. Epistemological laws governing the results of the observation are such as are inferable solely from the fact that the procedure was as specified. The contingency referred to in this paragraph is exemplified by the fact that it is impossible to make a really "good" observation of length in a strong magnetic field, because the standard specification of the procedure of determining length requires us to eliminate magnetic fields (p. 80).

> The epistemologist accordingly does not study the observers as organisms whose activities must be ascertained empirically in the same way that a naturalist studies the habits of animals. He has to pick out the good observers—those whose activities follow a conventional plan of procedure. What the epistemologist must get at is this plan. Without it, he does not know which observers to study and which to ignore; with it, he need not actually watch the good observers who, he knows already, are merely following its instructions, since otherwise they would not be good.
>
> The plan must be sought for in the mind of the observer, or in the minds of those from whom he has derived his instructions. The epistemologist is an observer only in the sense that he observes what is in the mind. But that is a pedantic description of the way in which we discover a plan conceived in anyone's mind. We learn the observer's plan by listening to his own account of it and cross-questioning him.
>
> (Eddington, 1939, p. 23)

- epistemological knowledge is a priori in a legitimate sense due to the method of analysis, compared to the knowledge of observations which is a posteriori. doesn't mean no objective knowledge (of objective world), but there is a selective and subjective aspect which is more certain (and recognition of this is called by Eddington *selective subjectivism*, and mathematical structures can characterize this which is why he also considered calling his position *structuralism*)

> It seems appropriate to call the philosophical outlook that we have here reached *selective subjectivism*. "Selective" is to be interpreted broadly. I do not wish to assert that the influence of the procedure of observing on the knowledge obtained is confined to simple selection, like passing through a net. But the term will serve to remind us that the subjective and the objective can be combined in other ways than by mere addition. In mathematics a very general type of such combination is that of operator and operand, selective operators being a particular case.
>
> Selection implies something to select from. It seems permissible to conclude that the material on which the selection is performed is objective. The only way to satisfy ourselves of this is to examine carefully the ways in which subjectivity can creep into physical knowledge through the procedure of observing. So far as I can see, selection or operations mathematically akin to it cover the whole range of possibility; that is to say, the whole subjectivity is comprised in operations of a selective type. The subjectivity being confined to the operators, the ultimate operand must be free from subjectivity.
>
> I see no reason to doubt the foregoing argument, but it depends on a vigilance of scrutiny which I cannot guarantee as conclusive. "Objective" is essentially a negative characteristic

> (non-subjective) of knowledge, although we regard it as a positive characteristic of the thing to which the knowledge refers; and it is always more difficult to demonstrate a negative than a positive conclusion. I accept an objective element in phyiscal knowledge on, I think, reasonably strong grounds, but not with the same assurance as the subjective element which is easily demonstrable.
>
> Selective subjectivism, which is the modern scientific philosophy, has little affinity with Berkeleian subjectivism, which, if I understand rightly, denies all objectivity to the external world. In our view the physical universe is neither wholly subjective nor wholly objective—nor a simple mixture of subjective and objective entities or attributes.
>
> (Eddington, 1939, p. 26-27)

- he distinguishes from Berkeley, and also indicates that he thinks he is *reconstructing* modern scientific philosophy/epistemology, not necessarily to prescribe but describing to a certain degree.

## 18.3 Chapter III: Unobservables

- ch 3 Unobservables. micro vs molar physics.
  - formulating a principle of (general) relativity

> Perhaps the nearest approach to a formulation of the principle is the statement that we observe only *relations* between physical entities. (Eddington, 1939, p. 31)

  - epistemologist concerned with "What is it we really observe?"

> Dividing physicists into three classes—relativity physicists, quantum physicists, experimental physicists—the relativity physicist studies the hard facts of observation. The quantum physicist follows the same principle as far as he can; but owing to the more intricate and more remote nature of his subject, the aim of constructing a theory which shall embrace only the observable facts represents his ideal rather than his achievement. As for the experimental physicist, I will only say that because a man works in a laboratory it does not follow that he is not an incorrigible metaphysician.
>
> (Eddington, 1939, p. 32-33)

> Since aether is not matter, it cannot be assumed *a priori* that the usual attributes of matter—density, rigidity, momentum, etc.—are also attributes of the aether. Accordingly the hypothesis to be tested is that velocity, although a well-known attribute of matter, is not one of the attributes of the aether. Put in this way, it is not a truth that could have been foreseen *a priori*; it is a mildly surprising, but clearly possible, conclusion deduced *a posteriori* from the null result of experiments

> designed to detect effects which would be expected if there existed a luminiferous aether with the type of structure to which velocity could be attributed.
>
> This attitude is popular with those who dislike the epistemological inquiry associated with the new developments of physics. It is so easy to cut short an argument one does not want to understand by saying: "I am not interested in your reasons, but I am quite willing to try any conclusion you may have reached as a hypothesis to be tested by observation. Then, if it is confirmed, it will take rank with the other confirmed hypotheses of physics, and we shall not need your arguments." By this kind of short-circuiting, the more difficult considerations are cut out of the subject; and we can embark at once on the straightforward mathematical deduction of the consequences of the hypothesis with a view to observational test. Thus the new wine is put into the old bottles. It does not burst the bottles; but it loses most of its invigorating—my opponents would perhaps say, its intoxicating—qualities.
>
> (Eddington, 1939, p. 34-35)

- "unobservability is a matter of epistemological principle, not of physical hypothesis"

> As a further example, it was pointed out ten years ago that when we are dealing with particles, such as electrons, which are indistinguishable from one another observationally, the ordinary coordinate $\xi = x_2 - x_1$ of one particle relative to another is not an observable; the observable in this case is a type of quantity previously unfamiliar in analysis, namely a "signless coordinate" $\eta = \pm\xi$. Up to the present, quantum physicists have chosen to ignore this imposture; and the modern textbooks still adhere to the erroneous theory of a system of two such particles, which assumes the observable to be $\xi$. They have thereby missed the opening for a much needed advance.
>
> I have mentioned this last example because it is a clear case in which unobservability is a matter of epistemological principle, not of physical hypothesis. For simplicity, consider particles in one dimension only, say east and west. If we have a green ball and a red ball, we can observe that the green ball is, say, 5 inches west of the red ball. Accordingly, for purposes of description, we introduce an observable quantity $\xi$ which states the distance of the green ball from the red ball measured towards the west; a negative value of $\xi$ will indicate that the green ball is to the east. But suppose instead that we have two balls exactly alike in colour, and with no distinction at all that we can observe. In such a system there is no observable corresponding to $\xi$. We can observe that the balls are 5 inches apart in the east-west line, and we can introduce an observable $\eta$ which states the distance apart. But, unlike $\xi$, $\eta$ is a signless quantity.

> It is a natural mistake to apply the ordinary theory of the observable behavior of particles (*particle mechanics*, as we call it) to protons and electrons, overlooking that at an early stage in that theory, namely in introducing and defining a relative coordinate $\xi$, it was taken for granted that the particles could be distinguished observationally. This mechanics becomes inapplicable when $\xi$ is unobservable. For protons and electrons we have a modified mechanics with $\eta$ as the observable. This fundamental difference in the mechanics must be followed up mathematically; and although the problem is rather difficult, I think it is rigorously deducible that the difference is equivalent to a force between the particles which is actually the well-known Coulomb force. That is to say, the electrostatic (Coulomb) force between electrons and protons is not an "extra" arising we know not why, but is simply a term which had dropped out in the ordinary derivation of the equations through the oversight of taking $\xi$ instead of $\eta$ as the observable, and had therefore to be re-inserted empirically.
>
> Those unfamiliar with wave-mechanics may be astonished that there should be a difference between the mechanics of distinguishable particles and the mechanis of indistinguishable particles. But it ought not to surprise quantum physicists, since it is universally admitted that there is a difference in their statistics, which is no less mysterious. Indeed I have never been able to understand why those who are well aware of the important consequences of indistinguishability in large assemblies do not trouble to examine its precise consequences in smaller systems. Whether we consider the well-known effect on the statistics of large assemblies or the less well-known effect on the mechanics of a system of two particles, the conclusions appear incredible unless we bear in mind the subjectivity of the world described by physics and of all that it is said to contain. It is naturally objected that the particles cannot be affected by our inability to distinguish them, and it is absurd to suppose that they modify their behaviour on that account. That would be true if we were referring to wholly objective particles and wholly objective behaviour. But our generalizations about their behaviour—the laws of mechanics—describe properties imposed by our procedure of observation, as the generalisations about catchable fish were imposed by the structure of the net. The objective particles are unconcerned with our inability to distinguish them; but they are equally unconcerned with the behaviour which we attribute to them partly as a consequence of our failure to distinguish them. It is this observable behaviour, and not the objective behaviour, that *we* are concerned with.
>
> (Eddington, 1939, p. 35-37)

- progress/advance as reduction in number of fundamental hypotheses. three ways: abandonment of mechanical ideal of explanation (math instead of hyp. stuff), unification (e.g. light/optics with electromagnetism), and replacement by epistemological principles (exemplified by relativity)

> With the coming of relativity theory yet a third method of reducing the number of hypotheses crept in, namely the replacement of physical hypotheses by epistemological principles. We have already noticed the way in which an epistemological conclusion can play the same part as a physical hypothesis so far as observational consequences are concerned.
>
> We have seen (p. 20) that laws and properties which have an epistemological origin are compulsory and universal. It may be added that, in some cases at least, they are exact. For the unobservability of certain quantities—which is the most common form of statement of an epistemological principle—is traced to a logical contradiction in their definitions; and the consequences (in so far as they are reached by logical deduction alone, and not by combination with more or less uncertain and inexact hypotheses) are quite definite. The pervasion of fundamental physics by epistemology has therefore greatly changed its character, and brought exactitude within reach. So long as the methods were wholly *a posteriori*, there was no warrant for regarding the deduced laws of nature as better than approximations.
>
> To avoid misapprehension it is best to state here (prematurely) that although we now recognise laws which we can confidently assert are exact, the subject-matter of these exact laws is probability. There is therefore not a corresponding precision in the laws of observational phenomena (as distinguished from the laws of *probability* of the phenomena); and, notwithstanding its newly acquired exactness, the system of fundamental physical laws is indeterministic.
>
> (Eddington, 1939, p. 45-46)

- I think that he probably doesn't mean irreducibly indeterministic, only that the character of the laws as we represent in our minds for now/then says that that is the case.

## 18.4 Chapter IV: The Scope of Epistemological Method

- ch 4 The Scope of Epistemological Method

> According to the classical conception of microscopic physics, our task was to discover a system of equations which connects the positions, motions, etc. of the particles at one instant, with the positions, motions, etc. at a later instant. This problem has proved altogether baffling; we have no reason to believe that any determinate solution exists, and the search has been frankly abandoned. Modern quantum theory has substituted another task, namely to discover the equations which connect knowledge of the positions, motions, etc. at one instant with

the knowledge of the positions, motions, etc. at a later instant. The solution of this problem appears to be well within our power.

The mathematical symbolism describes our knowledge, and the mathematical equations trace the change of this knowledge with time. Our knowledge of physical quantities is always more or less inexact; but the theory of probability enables us to give an exact specification of inexact knowledge, including a specification of its inexactitude. The introduction of probability into physical theories emphasises the fact that it is knowledge that is being treated. For probability is an attribute of our knowledge of an event; it does not belong to the event itself, which must certainly occur or not occur.

Wave mechanics investigates the way in which probability redistributes itself as time elapses; it analyses it into waves and determines the laws of propagation of those waves. Generally the waves tend to diffuse; that is to say, our knowledge of the position (or of any other characteristic) of a system becomes vaguer the longer the time elapsed since an observation was made. A sudden accession to knowledge—our becoming aware of the result of a new observation—is a discontinuity in the "world" of probability-waves; the probability is reconcentrated, and the propagation starts again from the new distribution. There are exceptional forms of probability distribution of certain of the attributes of microscopic systems which do not diffuse, or diffuse very slowly; so that our knowledge of these attributes does not so rapidly grow out of date. Particular attention is lavished on these "steady states" and on the equations determining them, since they provide a basis for long-range predictions.

The statement often made, that in modern theory the electron is not a particle but a wave, is misleading. The "wave" represents our knowledge of the electron. The statement is, however, an inexact way of emphasising that the knowledge, not the entity itself, is the direct object of our study; and it may perhaps be excused by the fact that the terminology of quantum theory is now in such utter confusion that it is well-nigh impossible to make clear statements in it. The term "electron" has at least three different meanings[6] in common use in quantum theory, in addition to its loose application to the probability wave itself.

Wave mechanics shows us immediately why the distinction between observatables and unobservables is so essential. A "good" observation of a quantity, although it does not determine the quantity precisely, narrows down the range in which it is likely to lie. It creates a condensation in the probability distribution of the quantity or, as we usually say, forms a

[6]Namely, the particle represented by a Dirac wave-function, the particle introduced in second quantisation and the particle represented by the internal (relative) wave-function of a hydrogen atom.

> wave packet in it. The method of wave mechanics is to investigate the wave equations which govern the propagation of waves from such a source. But if the quantity is unobservable, these wave packets cannot be formed. A study of the propagation of waves which there is no means of producing can have no application to physics; and a theory which professes to deduce observationally verifiable results by such analysis is evidently vitiated by a mis-identification.
> (Eddington, 1939, p. 50-52)

- absolutely wild, very clear notion of epistemic wave function and arguably a form of qbism from 1939 when this was written

- reception of Einstein's theory, theory is not identical to math, what the role of a physicist actually is (not just applying math)

> When Einstein's theory arrived, which not only propounded a new epistemology but applied it to determine the law of gravitation and other practical consequences, physicists were puzzled how to classify it. Some argued that it was philosophy, *alias* metaphysics, and must be rejected out of hand. Others conceded that the formulae appeared to agree with observation and accomplished a valuable systematisation of knowledge, but believed that a "genuinely physical" interpretation of its meaning would in time supplant the epistemological jargon which at present envelops it. Fewer realised that the new epistemological outlook is the very heart of the theory, supplanting a fallacious system of thought which was barring progress. Even now we often find authors, who are by no means ignorant of the reasons for the change of thought, propounding theories for which they claim the advantage that they involve only Newtonian conceptions. As though it could be an advantage to incorporate a fallacious and obsolete view of the nature of observational knowledge!
> This vagueness and inconsistency of the attitude of most physicists is largely due to a tendency to treat the mathematical development of a theory as the only part which deserves serious attention. But in physics everything depends on the insight with which the ideas are handled before they reach the mathematical stage.
> The consequence of this tendency is that a theory is very commonly identified with its leading mathematical formulae. We continually find special relativity theory identified with the Lorentz transformation, general relativity with the transformation to generalised coordinates, quantum theory with the wave equation or the commutation relations. It cannot be too strongly urged that neither relativity theory nor quantum theory are summed up in fool-proof formulae for use on all occasions. A relativist is not a man who employs Lorentz-invariant formulae (which were introduced some years before the relativity theory appeared), but one who understands in what circumstances formulae ought to have Lorentz-invariance; nor is he a

> man who transforms equations into generalised coordinates (a practice at least a century old), but one who understands in what circumstances a special system of coordinates would be inapplicable. In quantum problems allowance must be made for the backward state of the theory; and the world is still awaiting a quantist who understands in what circumstances the standard wave equation and the commutation relations are applicable—as distinct from one who merely applies them and hopes for the best.
>
> (Eddington, 1939, p. 55-56)

- Eddington sees little reason, given relativity's clear dependence/import as being subjective/epistemological, to suppose that there are parts of fundamental theory which are subjective and parts that are objective... nor a reason to try when it seems probably better to just go full blown idealism. Pragmatic reasons? SLIDE DOWN IDEALIST GRADIENT

> I do not see how anyone who accepts the theory of relativity can dispute that there has been some replacement of physical hypotheses by epistemological principles; nor do I think that those who accept the theory with understanding will be inclined to dispute it. The more controversial question is, How far can this replacement extend? Here my conclusion, based on purely scientific investigation, is much more drastic than that of most of my colleagues. I believe that the whole system of fundamental hypotheses can be replaced by epistemological principles. Or, to put it equivalently, all the laws of nature that are usually classed as fundamental can be foreseen wholly from epistemological considerations. They correspond to *a priori* knowledge, and are therefore *wholly subjective*.
>
> I am sorry to have to put in the forefront what will generally be regarded as an individual scientific conclusion; but this cannot be avoided. I think I can see a clear philosophy emerging from the conclusion that the system of fundamental laws is wholly subjective. I cannot see any coherent philosophy emerging from the conclusion that some are subjective and some objective. Immediately I start on that line I am beset with objections and perplexities which I do not know how to meet. I do not condemn it on that account; perhaps with a great deal more thought a way of progress could be seen. But there is no inducement to spend my time trying to overcome the difficulties of a philosophy associated with scientific beliefs which I do not share. No one can contemplate entering on a difficult research based on premises which he has reason to believe erroneous. You will find plenty of philosophies of objective natural law; you will find here a philosophy of subjective natural law. If ever a philosophy of mixed subjective-objective natural law is developed, it will not be by me, for I am convinced that there is no scientific support for such a philosophy.
>
> (Eddington, 1939, p. 56-57)

> My conclusion is that not only the laws of nature but the constants of nature can be deduced from epistemological considerations, so that we can have *a priori* knowledge of them.
>
> Treating the scheme of natural law as a whole, as it is set out in the fundamental equations of physics, four constants of nature which are pure numbers[7] are involved. These I find to be predictable *a priori*. I focus attention on these, because it is a more stringent test of the power of the epistemological method to provide a number (verifiable in some cases to about 1 part in 1000) than to provide forms of law. I think that the classical physicist had an inner feeling that the inverse square law was a natural form of weakening of an effect by distance, which might be expected *a priori* to apply to gravitation—though it would, of course, be contrary to his principles to acknowledge any *a priori* expectation.
>
> (Eddington, 1939, p. 58-59)

- one constant in particular: cosmical number, i.e. number of particles in the universe (which in a later chapter he gives an exact number...!)

- Eddington openly struggling with probability in subjective view of natural laws... perhaps a Bayesian view would merge well?! [WORK ON]

> The "law of chance" is not usually counted as a fundamental law of physics, and I do not include it among the laws that can be foreseen wholly from epistemological considerations. But according to the modern system of physics all our predictions of phenomena are predictions of what will probably happen, and are based on an assumption of non-correlation of the behaviour of individual particles which is derived from the law of chance. *Without an appeal to the law of chance physics is unable to make any prediction of the future.* The law of chance might therefore be claimed to be the most fundamental and indispensable of all physical laws. The reason why it is omitted is that, from the ordinary point of view, randomness is a negation of law; and it seems unnecessary to lay down a law saying that there is no law. But the ordinary view takes it for granted that the physical universe, and the particles into which we analyse it, are wholly objective; and the status of the law of chance (or non-correlation) requires reconsideration when applied to a partly subjective universe. It is impossible to treat this point fully until a late stage of the discussion. The view finally adopted will be found on pp. 180, 218. If in the meantime the reader finds my argument tending apparently to a more and more incredible conclusion, he may await a later twist that will soften it into something which will, I think, not too grossly affront his commonsense.
>
> (Eddington, 1939, p. 61)

[7] Formed by eliminating our three arbitrary units (centimetre, gram and second) from the seven constants of nature ordinarily recognised. (*New Pathways in Science*, p. 232)

> Without further apology I shall now assume the reader's assent to the proposition that all the fundamental laws and constants of physics can be deduced unambiguously from *a priori* considerations, and are therefore wholly subjective.
>
> (Eddington, 1939, p. 62)

- shifts burden of proof to anyone who thinks the laws are objective, fishing net analogy again

> But if our purpose is to determine laws of objective origin coming through to us in a form modified by subjective selection, I do not think the best way is to suppress all theories about the objective world—at any rate as working hypotheses. But at first sight the progress of physics seems to contradict this; for it was just when hypothesis about the objective world was abandoned, and we turned to a direct study of physical knowledge, that progress became astonishingly rapid.
>
> The explanation is simple. All this progress relates to subjective law. It all relates to uniformities imposed on the results of observation by the procedure of observation.
>
> (Eddington, 1939, p. 62)

- laws of nature (dynamical/differential equations) vs special facts (initial conditions), Laplace determinism in classical physics, not applicable in new physics. New physics, being epistemological, does not demarcate the same way between these, and what we might think as a special fact he claims to show is also epistemological ("subjective").

> But this mode of distinction is possible only in a deterministic universe. In the current indeterministic system of physics, there is no corresponding demarcation between the laws and the special facts of nature. The present system of fundamental laws does not furnish a complete set of rules for the calculation of the future. It is not even part of such a set, for it is concerned only with the calculation of probabilities; and if ever the search for a scheme of definite prediction is renewed, it will be necessary to start again from the beginning on different lines. The part played by the special facts is also altered. The special facts, which distinguish the actual universe from all other possible universes obeying the same laws, are not given once for all at some past epoch, but are being born continually as the universe follows its unpredictable course. Moreover, in the differential equations of quantum theory the boundary conditions are not the objective facts but the knowledge we happen to possess about them.
>
> The simple demarcation in classical theory between fundamental laws of nature and special facts is associated with determinism and cannot be carried over into the modern theory. But, approaching the question from the point of view of subjectivity, a new line of demarcation appears. We have found that the supposedly fundamental laws are wholly subjective. It is

> only reasonable that the part of our knowledge which is wholly subjective should be of a recognisably different type from that which involves the objective characteristics of the universe. It appears that this difference was not overlooked by the earlier physicists; and we find the region to be annexed to pure subjectivity already marked out under another name, viz. "fundamental".
>
> The special facts, on the other hand, cannot be inferred from epistemological considerations and are not wholly subjective. It is the essence of our conception of a special fact that it might quite well have been otherwise—that there is no *a priori* reason why it should be what it is. It is true that many have held the view that the laws of nature might quite well have been otherwise; but they would scarcely assert that this is an inseperable part of the conception of a law of nature. Everyone recognises that it is in some sense taking a greater liberty with the universe to imagine the laws of nature to have been different than to imagine the special facts to have been different.
>
> Results deducible by the *a priori* epistemological method are compulsory, and it is therefore impossible that the method should be extended to predict the special facts, which "might quite well have been otherwise". I am afraid that before I finish I shall have persuaded the docile reader to believe so many "impossible" things that the world will make little impression on him, and he will not jib at impossibility when I want him to. Let me then put the point rather differently. If by an advance of epistemological theory we succeed in predicting one of the so-called special facts in a wholly *a priori* way, we shall at once amend the classification: "Clearly we were mistaken in supposing that it was a special fact. Now that we see more clearly into its origin, we realise that there is a law of nature which compels it to be so."
>
> The cosmical number affords a good example of such a change of view. Regarded as the number of particles in the universe, it has generally been looked upon as a special fact. A universe, it is held, could be made with any number of particles; and, so far as physics is concerned, we must just accept the number allotted to our universe as an accident or as a whim of the Creator. But the epistemological investigation changes our idea of its nature. A universe cannot be made with a different number of elementary particles—consistently with the scheme of definitions by which the "number of particles" is assigned to a system in wave-mechanics. We must therefore no longer look on it as a special fact about the universe, but as a parameter occurring in the laws of nature, and, as such, part of the laws of nature.
>
> (Eddington, 1939, p. 63-65)

- special facts somehow changing and evidenced by uncertainty principle

> Within the limits of the uncertainty principle they are ever-changing as the moments pass by.
>
> The special facts are partly subjective and partly objective, depending partly on our procedure in obtaining observational knowledge and partly on what there is to observe. To separate the subjective or objective elements completely, we must consider laws; since a law or regularity may originate wholly in our procedure of observation or wholly in the objective world. It may be questioned whether we could ever isolate an objective law as completely as a subjective law, since it would have to be presented to us *via* our subjective forms of thought; but at least we could detect a regularity and recognise that is origin was objective, even if we could only describe it in subjective terms.
>
> (Eddington, 1939, p. 66)

- law of Nature (capital) vs law of nature, former about nature herself and latter about observational regularity. physics enlarged in scope because of subjective and objective parts, what to call it? science? Idealism.

> It seems to me that the "enlarged" physics which is to include the objective as well as the subjective is just *science*; and the objective, which has no reason to conform to the pattern of systematisation that distinguishes present-day physics, is to be found in the non-physical part of science. We should look for it in the part of biology (if any) which is not covered by biophysics; in the part of psychology which is not covered by psychophysics; and perhaps in the part of theology which is not covered by theophysics. The purely objective sources of the objective element in our observational knowledge have already been named; they are *life, consciousness, spirit.*
>
> We reach then the position of idealist, as opposed to materialist, philosophy. The purely objective world is the spiritual world; and the material world is subjective in the sense of selective subjectivism.
>
> (Eddington, 1939, p. 68-69)

## 18.5 Chapter V: Epistemology and Relativity Theory

- ch. 5 Epistemology and Relativity Theory. quantity and measurement definitions (instrumental)

> It has come to be the accepted practice in introducing new physical quantities that they shall be regarded as *defined* by the series of measuring operations and calculations of which they are the result. Those who associate with the result a mental picture of some entity disporting itself in a metaphysical realm of existence do so at their own risk; physics can accept no responsibility for this embellishment.
>
> (Eddington, 1939, p. 71)

- Poincare on geometry (S&H), theorist and experimenter must agree to metrologist definitions, instructions for a procedure of measurement. fear of experiment,

> The definition of length or distance and the corresponding definition of time-extension are particularly important, because in general the definitions of other physical quantities presuppose that length and time-extension have been defined, and any ambiguity of their meaning would spread through the whole superstructure. If, instead of length being defined observationally, its definition were left to the pure mathematician, all the other physical quantities would be infected with the virus of pure mathematics.
>
> Practical physicists have long been occupied with the accurate determination of lengths, and the principles which they strive to follow were settled before the theory of relativity arose. This branch of practical physics is called metrology. When therefore it became necessary to adopt formally an observational definition of length, there could be no question of setting up a rival procedure. The definition must give instructions as to a procedure of measurement of lengths. To the metrologist these instructions amounted simply to "Carry on".
>
> (Eddington, 1939, p. 73)

> Accordingly, by length in relativity theory we mean what the metrologist means, not what the pure geometer means. In accepting relativity principles, the physicist puts aside his paramour pure mathematics, dismisses their go-between metaphysics, and enters into honourable marriage with metrology. I am afraid those who represent the bride are inclined to suspect that he is not entirely off with his first love. Some writings on relativity look a bit mathematical. Since I am not entirely convinced of the innocence of some of my colleagues, I must on this point answer only for myself. I declare that the suspicions are groundless. If I sometimes employ pure mathematics, it is only as a drudge; my devotion is fixed on the physical thought which lies behind the mathematics. Mathematics is a useful vehicle for expression and manipulation; but the heart of the theory is elsewhere:
>
> Euphelia serves to grace my measure
> But Chloe is my real flame.
>
> The crucial part of the definition of length is the specification of a standard which shall be available for comparison at any place and at any time.
>
> (Eddington, 1939, p. 74)

- reproducible physical structure standard of length like calcite crystal, quantum structure of matter providing standard of lenght for relativity theory?

> Molar physics always has the last word in observation, for the observer himself is molar.
>
> The secret of the union of molar and microscopic physics—of relativity theory and quantum theory—is "the full circle". They are not so much branches forking from one root as semicircles joined at both ends. Generally we enter on the circle at the junction now under discussion, where relativity theory takes its standard of length from quantum theory. But relativity theory, which has made greater progress along its arc than quantum theory along its arc, is already exploring the other junction, where the cosmical constant and matters of that kind are involved.
>
> (Eddington, 1939, p. 77)

- at least relativity reliance on quantum means they share length standard; constants changing?

> It is often suggested that some of the constants of nature, e.g. the velocity of light or the gravitational constant, vary with time. Unless the standards of length and time-extension have been carefully defined, such discussions are meaningless; and much that has been written on the subject is discounted by the fact that the writers are evidently unaware of the nature of the definition of these standards. Anyone who suggests variation of a fundamental constant has before him a heavy task of reconstruction of theory and reinterpretation of observational measurements before he can reach any observational confirmation or contradiction of his suggestion. Meanwhile I think that progress of the epistemological method has assured us that the constants of nature (apart from our arbitrary units) are numbers introduced by our subjective outlook, whose values can be calculated *a priori* and stand for all time. For this reason my personal conclusion is that there is no more danger that the velocity of light or the constant of gravitation will change with time than that the circumference-diameter ratio $\pi$ will change with time.
>
> Let us examine more closely what is implied in the suggestion that the velocity of light *in vacuo* changes with the time. An immediate consequence is that the ratio of the wavelength $\lambda$ to the period $T$ of any spectral line, say a hydrogen line, changes with time. Now for all epochs the standard of time is a time-period in some quantum-specified structure, and the standard of length is a space-extension in some quantum-specified structure. We may take this structure to be a hydrogen atom in the quantum-specified state in which it emits the line considered. It follows that either the ratio of the period of the emitted light to the time-period intrinsic in the emitting atom varies with time, or the ratio of the length of the emitted waves to the spatial scale of structure of the emitting atom varies with time. I do not think those who propose the variability of the velocity

> of light realise that, if their words have any meaning, they imply that the period of the light has no constant relation to—is therefore not determined by—any corresponding periodicity in its source; or alternatively, that the wavelength of the light has no constant connection with the linear scale of its source. If this were true, it would involve a conception of atomic structure so far removed from that of present-day quantum theory that scarcely anything in our present knowledge would survive. (Eddington, 1939, p. 77-79)

- extreme accuracy, quantum standard of length in strong $E$ or $B$, conventional math theories that approach standard in limit as $E, B$ go to zero

- Section V, does he talk next about something like renormalization? Talking about a short metric for length (as opposed to long where strain/distortions are greater, shorter is proportionately less strain?)

> We cannot always remove the bodies that are causing the strain. If we are measuring up the solar system, we cannot begin the proceedings by clearing away the sun. Thus, in general, we have to be content with short standards which are proportionately less affected by strain. With the short standard we can only measure short distances directly. To a first approximation we can determine large distances by measuring them in short sections, and summing or integrating the results;[8] but to a higher approximation this method also leads to ambiguous results. This ambiguity is known as the *non-integrability of displacement*. (Eddington, 1939, p. 82)

- non-integrability of displacement as fundamental to GR

> The failure to define long distances observationally, or in mathematical language the non-integrability of displacement, is the foundation of Einstein's theory of gravitation. According to the usual outlook gravitation is the cause of the trouble; gravitation produces the strains which render long standards useless. But Einstein's outlook is more nearly that the "trouble"—the non-integrability of displacement—is the cause of gravitation. I mean that in Einstein's theory the ordinary manifestations of gravitation are deduced as mathematical consequences of the non-integrability of displacement. I cannot enter here into the details, which require a large treatise; but the gist of it is that Einstein showed how to specify the non-integrability quantitatively, and used the numbers thus introduced—the famous $g_{\mu\nu}$—as a measure of the influence which disturbs the ideal conditions in which displacements would be integrable. "Gravitational field" is the name which we have given to this influence. As might be expected, this systematic specification

[8] That is to say, we *define* a large distance as the result of integrating short distances (provided that the result is unambiguous) instead of defining it as the result of a comparison with a long standard.

of the gravitational field has been found to be more precise than the casual specification of it by one of its effects which happened to strike Newton's attention when he sat under an apple-tree.

Einstein's specification is more accurate than Newton's; but that the two refer to the same thing is seen when we recall that it was the strain, produced by the two ends of the long standard trying to fall with different accelerations towards the sun or moon, which vitiated it as a standard and frustrated our effort to measure directly an integrated length. We need therefore not be surprised that from Einstein's specification the more ordinary manifestations of gravitation in falling bodies can be deduced.

This is a particularly good example of the way in which epistemological study has brought about a great advance in science; and it is worth while to recall the principal steps. If physics is to describe what we really observe, we must overhaul the definitions of the terms employed in it so that they explicitly refer to observational facts and not to metaphysical conjectures. Length and time interval in particular need to be carefully defined, since they are the basis of nearly all other physical definitions. To avoid circular definitions it is essential that the standards of length and time interval should be the extensions of structures completely specified by pure numbers. With such structures as standard we obtain a definition of infinitesimal intervals (in the absence of an electromagnetic field), but we do not obtain an exact definition of long intervals. Thus, in order that physics may express purely observational knowledge, it is necessary to develop a system of description of the location of events based wholly on infinitesimal distances and time intervals; we thereby avoid reference to long intervals which have no exact observational definition. This system of location, depending on infinitesimal intervals, is the foundation of general relativity theory. In relativity theory a long distance is in general an approximate conception only; it is incapable of exact definition.[9]

As soon as we realise that the definition of length does not cover long distances and so does not imply integrability of displacement, integrability becomes a special hypothesis which requires defending. One does not accept hypotheses gratuitously. Proceeding from this rational basis of spacetime measurement we find that the phenomenon of gravitation appears automatically—unless we deliberately introduce a hypothesis of integrability to exclude it—and in this way we are led immediately to Einstein's theory of gravitation.(Eddington, 1939, p. 83-85)

[9]The breakdown of the ordinary definition leaves the term at the disposal of investigators, and various technical definitions of long distances have been proposed. But these technical uses of the term are irrelevant here.

- Quoting the entire section VI of chapter V (Epistemology and Relativity Theory) as it expresses I think fairly clearly an intersubjective idealism about observational physics. Also note the comment about a subjective interpretation of probability.

> I have been continually emphasizing the subjectivity of the universe described in physical science. But, you may ask, was it not the boast of the theory of relativity that it penetrated beyond the relative (subjective) aspect of phenomena and dealt with the absolute? For example, it showed that the usual separation of space and time is subjective, being dependent on the observer's motion, and it substituted a four-dimensional space-time independent of the observer. It may seem difficult to reconcile this view of Einstein's theory as lifting the veil of relativity which hides the absolute from us, with my present account of modern physics as acquiescing in, and making the bet of, a partially subjective universe.
>
> It is necessary to remember that there has been thirty years' progress. Relativity began like a new broom, sweeping away all the subjectivity it found. But, as we have advanced, other influences of subjectivity have been detected which are not so easily eliminated. Probability, in particular, is frankly subjective, being relative to the knowledge which we happen to possess. Instead of being swept away, it has been exalted by wave mechanics into the main theme of physical law.
>
> The subjectivity referred to in these lectures is that which arises from the sensory and intellectual equipment of the observer. Without varying this equipment, he can vary in position, velocity and acceleration. Such variations will produce subjective changes in the appearance of the universe to him; in particular the changes depending on his velocity and acceleration are more subtle than was realised in classical theory. Relativity theory allows us to remove (if we wish) the subjective effects of these *personal* characteristics of the observer; but it does not remove the subjective effects of *generic* characteristics common to all "good" observers—although it has helped to bring them to light.
>
> Confining attention to the personal, as distinguished from the generic, subjectivity, let us see precisely what is meant by removing this subjectivity. There does not seem to be much difficulty in conceiving the universe as a three-dimensional structure viewed from no particular position; and I suppose we can, after a fashion, conceive it without any standard of rest or of non-acceleration. It is perhaps rather unfortunate that it is, or seems to be, so easy to conceive; because the conception is liable to be mischievous from the observational point of view. Since physical knowledge must in all cases be an assertion of the results of observation (actual or hypothetical), we cannot avoid setting up a dummy observer; and the observations which he is supposed to make are subjectively affected by his posi-

tion, velocity and acceleration. The nearest we can get to a non-subjective, but nevertheless observational, view is to have before us the reports of all possible dummy observers, and pass in our minds so rapidly from one to another that we identify ourselves, as it were, with all the dummy observers at once. To achieve this we seem to need a revolving brain.

Nature not having endowed us with revolving brains, we appeal to the mathematician to help us. He has invented a transformation process which enables us to pass very quickly from one dummy observer's account to another's. The knowledge is expressed in terms of tensors which have a fixed system of interlocking assigned to them; so that when one tensor is altered all the other tensors are altered, each in a determinate way. By assigning each physical quantity to an appropriate class of tensor, we can arrange that, when one quantity is changed to correspond to the change from dummy observer $A$ to dummy observer $B$, all the other quantities change automatically and correctly. We have only to let one item of knowledge run through its changes—to turn one handle—to get in succession the complete observational knowledge of all the dummy observers.

The mathematician goes one step farther; he eliminates the turning of the handle. He conceives a tensor symbol as containing in itself all its possible changes; so that when he looks at a tensor equation, he sees all its terms changing in synchronised rotation. This is nothing out of the way for a mathematician; his symbols commonly stand for unknown quantities, and functions of unknown quantities; they are everything at once until he chooses to specify the unknown quantity. And so he writes down the expressions which are symbolically the knowledge of all the dummy observers at once—until he chooses to specify a particular dummy observer.

But, after all, this device is only a translation into symbolism of what we have called a revolving brain. A tensor may be said to symbolise absolute knowledge; but that is because it stands for the subjective knowledge of all possible subjects at once.

This applies to personal subjectivity. To remove the generic subjectivity, due say to our intellectual equipment, we should have similarly to symbolise the knowledge as it would be apprehended by all possible types of intellect at once. But this could scarcely be accomplished by a mathematical transformation theory. And what would be the result if it were accomplished? According to Chapter IV, if we remove all subjectivity we remove all the fundamental laws of nature and all the constants of nature. But, after all, these subjective laws and facts happen to be important to beings who are not equipped with revolving brains and variable intellects. And if the physicist does not take charge of them, no one else is qualified to do so. Even in relativity theory, which deals with the absolute (in a

> somewhat limited sense), we continually hark back to the relative to examine how our results will appear in the experience of an individual observer. We are not so eager now as we were twenty years ago to eliminate the observer from our world view. Sometimes it may be desirable to banish him and his subjective distortion of things for a time, but we are bound to bring him back in the end; for he stands for—ourselves. (Eddington, 1939, p. 85-88)

## 18.6 Chapter VI: Epistemology and Quantum Theory

- ch 6, Epistemology and Quantum Theory

> I must still keep hammering at the question, What do we really observe? Relativity theory has returned one answer—we only observe *relations*. Quantum theory returns another answer—we only observe *probabilities*. (Eddington, 1939, p. 89)

- Heisenberg uncertainty principle as consequence of different measurements as source for irreducible indeterminism in QM

> The suggestion is that in the new physics the so-called probabilities are actually the real entities—the elemental stuff of the physical universe. We have precise knowledge of *them*; and it would seem retrogressive to postulate other entities behind them of which our knowledge must always be uncertain.
>
> I think that this idea is at the back of a rather common suggestion that a proper reformulation of our elementary concepts would banish the present indeterminism from the system of physics. The idea is that the indeterminism revealed by the new physics is not intrinsic in the universe, but appears only in our attempt to connect it with the obsolete universe of classical physics. Probability would then be merely the funnel through which the new wine is poured into old bottles.
>
> But the suggestion overlooks the essential feature of the indeterminism of the present system of physics, namely that the quantities which it can predict only with uncertainty are quantities which, *when the time comes*, we shall be able to observe with high precision. The fault is therefore not in our having chosen concepts inappropriate to observational knowledge. For example, Heisenberg's principle tells us that the position and velocity of an electron at any moment can only be known with a mutually related uncertainty; and, taking the most favourable combination, the position of the electron one second later is uncertain to about 4 centimetres. This is the uncertainty of the prediction from the best possible knowledge we can have at the time. But one second later the position can be observed with an uncertainty of no more than a fraction of a millimetre. It has often been argued that the impossibility of knowing simultaneously the exact position and exact velocity only shows

> that position and velocity are unsuitable conceptions to use in expressing our knowledge. I have no special attachment to these conceptions; and I will grant, if you like, that our knowledge of the universe at the present moment can be regarded as perfectly determinate (the supposed indeterminacy being introduced in translating it into an inappropriate frame of conception). But that does not remove the "indeterminism" (which is distinct from the "indeterminacy"), namely that this knowledge, however expressed, is inadequate to predict quantities which, independently of our frame of conception, can be directly observed when the time comes.
>
> Returning to the more general aspect of the probability conception, we find that it cannot be got rid of by any transformation of outlook. It is not possible to transform the current system of physics, which by its equations links probabilities in the future with probabilities in the present, into a system which links ordinary physical quantities in the future with ordinary physical quantities in the present, without altering its observable content. The bar to such a transformation is that probability is not an "ordinary physical quantity". At first sight it appears to be one; we obtain knowledge of it from observation, or from a mixture of observation and deduction, as we obtain knowledge of other physical quantities. But it is differentiated from them by a peculiar irreversibility of its relation to observation. The result of an observation determines definitely a probability distribution of some quantity, or a modification of a previously existing probability distribution; but the connection is not reversible, and a probability distribution does not determine definitely the result of an observation. For an ordinary physical quantity there is no difference between making a new determination and verifying a predicted value; but for probability the procedures are distinct.
>
> Thus we may expand the answer of quantum theory that "we only observe probabilities" into the form: The synthesis of knowledge which constitutes theoretical physics is connected with observation by an *irreversible* relation of the formal type familiar to us in the concept of probability. (Eddington, 1939, p. 90-91)

- End of quote above talking about probability as structural invariant in some sense like Cassirer would general covariance (does Ryckman also notice this?)
- Irreversibility illustrated through bag w/ colored ball example, comparing to wave packet probability distributions

> According to wave mechanics, an observation determines or produces a concentrated wave packet in the probability distribution. This wave packet diffuses according to laws embodied in the equations of the theory; and we can calculate the form into which the wave packet will have spread one unit of time

> later. *But the theory does not assert that this is the form of wave packet which would be produced by an observation made one unit of time later.* On the other hand, if from the observationally determined form of a sound wave at one instant we calculate the form into which it will have spread one unit of time later, the whole point of the theory is that we obtain the form which would be determined by observations made one unit of time later. (Eddington, 1939, p. 93)

- "an observation" vs "an item of observational knowledge". Holistic view, Quine?

> More generally we must recognise that an item of observational knowledge involves, besides a primary pointer reading, secondary pointer readings identifying the circumstances in which the primary pointer reading occurred. It must be admitted that even an isolated pointer reading is an item of knowledge of a sort; but it is not with such items that the scientific method deals. For scientific knowledge the association with other pointer readings is an essential condition; and we may therefore describe physical knowledge as a knowledge of the associations of pointer readings. (Eddington, 1939, p. 100)

- Referring to other book, criticizing older position. Alluding to uncertainty principle: pointer reading interference. Wave mechanics(!) Scope of physical knowledge and defining physical universe.

> In *The Nature of the Physical World* it is emphasised that physical knowledge is concerned with the connection of pointer readings rather than with the pointer readings themselves; and it is concluded that the connectivity of pointer readings, as expressed by the laws of physics, supplies the common background which realistic problems always demand—the background described by the tertiary pointer readings which are not determined afresh for each individual item of knowledge. But, if I may venture to criticise the author of that book, he does not seem to have appreciated the difficulty which arises through the interference of pointer readings with one another when we contemplate such an unlimited multiplicity of pointer readings. It is true that the interference is negligible in molar physics (to which the discussion in *The Nature of the Physical World* was limited). But in a fundamental discussion of this kind it is not legitimate to separate molar physics from microscopic physics; for we have seen (p. 76) that neither branch is logically complete in itself.
>
> Our definition of the physical universe is that it is the world which physical knowledge is formulated to describe. The interference of observations creates a difficulty which must be met in one of two ways. Either we must take the complete description of the physical universe to embody more than the totality of our possible knowledge of it; so that, whichever of two interfering observations we choose to make, there will be a place for it in

> the description. Or we must adopt a *flexible* universe containing nothing which is not represented by our actual knowledge (or in theoretical discussions by the supposedly actual knowledge furnished as data of the problem considered). In the first alternative we cannot consistently suppose all the items of the complete description to be represented by actual pointer readings; and it is therefore not true to say that its structure is a connectivity of pointer readings. The second alternative is adopted in wave mechanics, which accepts as leading features of the physical universe the probability waves created by actual observation of the physical quantities with which they are associated. Clearly there is no more than a formal distinction between the study of a universe flexible according to the knowledge we happen to have of it and a direct study of the knowledge itself. Either alternative brings us back to the conclusion that the common background is required to connect one item of knowledge with the rest of knowledge, rather than one element of an external universe with the rest of the universe. (Eddington, 1939, p. 100-102)

- Eddington provides a summary (section V of ch. VI Epistemology and Quantum Theory) which will be useful to have (I feel like I'm typing the whole book...) Adapting very slightly the enumeration to be LaTeX friendly

> The following summary will recall the principal conclusions that we have so far reached:
>
> 1. Physical knowledge (by definition) includes only knowledge capable of observational test; an item of physical knowledge must therefore assert the result of a specified observational procedure.
> 2. The definitions of the terms used in expressing physical knowledge must be such as to secure that (1) is satisfied. In particular the definition of a physical quantity must specify unambiguously a method of measuring it.
> 3. Strict adherence to (2) involves a number of modifications of the conceptions and practice of classical physics; and indeed there still survive glaring violations of it in current quantum theory. The points (4) to (9) below arise when the definitions are scrutinised from this point of view.
> 4. The first definitions required are those of length and time-interval, since the definitions of other physical quantities presuppose these. The standards of length and time must be structures specified by pure numbers only (since no other quantitative terms are available at this early stage). This means that the standards must be reproducible from a quantum specification.
> 5. Only short standards, suitable for measuring infinitesimal displacements in space and time, are provided by such

specifications; and it must not be assumed that the infinitesimal displacements so measured are integrable.

6. Owing to the interference of exact observations with one another, an attempt to define observationally the exact conditions under which the measurement of a physical quantity is intended to be carried out breaks down. It is therefore necessary to leave the minor details to chance.
7. In this way the probability conception is incorporated in the fundamental definitions. It introduces an irreversible relation between observation and formulated observational knowledge. This irreversibility makes the existing system of physics indeterministic, considered as a system of prediction of what can be observed at a future time.
8. Certain quantities used in the formulation of physical knowledge in classical physics are found to have no definition satisfying (2). These are unobservables, e.g. absolute simultaneity at a distance.
9. Other quantities, conditionally observable, have been employed in conditions in which they are unobservable. For example, the definition of relative coordinates presupposes that the particles are distinguishable, but ordinary relative coordinates are still used erroneously in problems concerning indistinguishable particles.
10. The conclusions (4) to (9) are reached by considering the way in which physical knowledge is obtained and formulated. We refer to them as epistemological or *a priori* conclusions, to distinguish them from *a posteriori* conclusions derived from a study of the results of observations which have been obtained and formulated in this way.
11. Although epistemological conclusions are of the nature of truisms, they have far-reaching consequences in physics. Thus the unobservability of absolute simultaneity (8) leads to the special theory of relativity; the non-integrability of displacement (5) leads to Einstein's theory of gravitation; the introduction of the probability conception in a fundamental way (7) leads to the method of wave mechanics.
12. In the modified theories which result, epistemological principles play a part which was formerly taken by physical hypotheses, i.e. generalisations suggested by an *a posteriori* study of the results of observation.
13. Current relativity theory and quantum theory, as usually accepted, have not yet taken full advantage of this epistemological method. It appears that when the epistemological scrutiny of definitions is systematically applied, and its consequences are followed up mathematically, we are able to determine all the "fundamental" laws of nature (including purely numerical constants of nature) without any physical hypothesis.

14. This means that the fundamental laws and constants of physics are wholly subjective, being the mark of the observer's sensory and intellectual equipment on the knowledge obtained through such equipment; for we could not have this kind of *a priori* knowledge of laws governing an objective universe.
15. It is not suggested that the physical universe is wholly subjective. Physical knowledge comprises, besides "laws of nature", a vast amount of special information about the particular objects surrounding us. This information is doubtless partly objective as well as partly subjective.
16. The subjective laws are a consequence of the conceptual frame of thought into which our observational knowledge is forced by our method of formulating it, and can be discovered *a priori* by scrutinising the frame of thought as well as *a posteriori* by examining the actual knowledge which has been forced into it.
17. The characteristic form of the fundamental laws of physics is the stamp of subjectivity. If there are also laws of objective origin, they may be expected to be of a different type. It seems probable that wherever effects of objective governance have appeared they have been regarded as an indication that the subject is "outside physics", e.g. conscious volition, or possibly life.
18. Epistemological laws (if correctly deduced) are compulsory, universal, and exact. Since the fundamental laws of physics are epistemological, they have this character—contrary to the view usually advocated in scientific philosophy, which has assumed that they are merely empirical regularities.(Eddington, 1939, p. 102-105)

## 18.7 Chapter VII: Discovery or Manufacture?

- Ch. VII Discovery or Manufacture? About limits of measurement. Fourier/spectroscopic analysis of (white) light. Also discussed Rutherford experiment (!), which of course is one of the topics of Larson's Case Against the Nuclear Atom.

> The realisation that natural white light is a quite irregular disturbance, into which regularity is introduced by our method of spectroscopic examination of it, was the first sign of an uneasiness among physicists as to whether in our experiments we may not interfere so much as to destroy what we were seeking to investigate. The uneasiness has become more acute in modern atomic physics, since we have no tool fine enough to probe an atom without grossly disturbing it.
>
> The question I am going to raise is—how much do we discover and how much do we manufacture by our experiments? When the late Lord Rutherford showed us the atomic nucleus, did he *find* it or did he *make* it? It will not affect our admiration

> of his achievement either way—only we should rather like to know which he did. The question is one that scarcely admits of a definite answer. It turns on a matter of expression, like the question whether the spectroscope finds or whether it makes the green colour which it shows us. But since most people are probably under the impression that Rutherford found the atomic nucleus, I will make myself advocate for the view that he made it.
>
> (Eddington, 1939, p. 108-109)

- WAVES WAVES WAVES. Form. Advocating for the "make" view. Continuing on observer effect (physical interaction) but also how subjective selection makes. Procrustes. Observer effect stuff. Questioning neutrinos.

> The tendency of writers on quantum theory has been perhaps to go farther than I do in emphasising the *physical* interference of our experiments with the objects which we study. It is said that the experiment puts the atoms or the radiation into the state whose characteristics we measure. I shall call this Procrustean treatment. Procrustes, you will remember, stretched or chopped down his guests to fit the bed he had constructed. But perhaps you have not heard the rest of the story. He measured them up before they left next morning, and wrote a learned paper "On the Uniformity of Stature of Travellers" for the Anthropological Society of Attica.
>
> The physical violence, however, is not really the essential point. Ideally the experimenter might wait until the conditions of his experiment happened naturally, as those engaged in the observational sciences are forced to do. We grossly interfere with the irregularity of white sunlight by passing it through a spectroscope; but sunlight may occasionally fall through a crevice on to a natural crystal and form a spectrum without our help. The standard conditions, which turn aimless measurement into a good measurement of a definite physical quantity useful for scientific induction, may sometimes occur without human interference. But, so far as physical theory is concerned, it makes no difference whether we *create* or whether we *select* the conditions which we study. Whether the interference of the observer is physical or selective, it is none the less marked in the resulting conclusions. The kind of observation on which physical theory is based is not a casual taking notice of things around us, nor a general running round with a measuring rod. Under cover of the term "good" observation the bed of Procrustes is artfully concealed.
>
> To what length can this interference be carried? I do not think that any limit can be set *a priori*. It is pertinent to remember that the concept of substance has disappeared from fundamental physics; what we ultimately come down to is *form*. Waves! Waves!! Waves!!! Or for a change—if we turn to relativity

theory—curvature! Energy which, since it is conserved, might be looked upon as the modern successor of substance, is in relativity theory a curvature of space-time, and in quantum theory a periodicity of waves. I do not suggest that either the curvature or the waves are to be taken in a literal objective sense; but the two great theories, in their efforts to reduce what is known about energy to a comprehensible picture, both find what they require in a conception of "form".

Substance (if it had been possible to retain it as a physical conception) might have offered some resistance to the observer's interference; but form plays into his hands. Suppose an artist puts forward the fantastic theory that the form of a human head exists in a rough-shaped block of marble. All our rational instince is roused against such an anthropomorphic speculation. It is inconceivable that Nature should have placed such a form inside the block. But the artist proceeds to verify his theory experimentally—with quite rudimentary apparatus too. Merely using a chisel to separate the form for our inspection, he triumphantly proves his theory. Was it in this way that Rutherford rendered concrete the nucleus which his scientific imagination had created?

Do not be misled by thinking of the nucleus as a sort of billiard ball. Think of it rather as a system of waves. It is true that the term "nucleus" is not strictly applicable to the waves (cf. the electron, p. 51): but it is equally unrigorous to speak of the nucleus as having been "discovered". The discovery does not go beyond the waves which represent the knowledge we have of the nucleus.

Does the sculptor's procedure differ in any essential way from that of the physicist? The latter has a conception of a harmonic wave form which he sees in the most unlikely places—in irregular white light, for example. With a grating instead of a chisel, he separates it from the rest of the white light and presents it for our inspection. Just as the sculptor separates the rough block of marble into a bust and a heap of chips, so the physicist separates the irregular wave disturbance into a simple harmonic green wave and a scrap-heap of other components. In Fourier and other recognised methods of analysis, physics allows and practises the splitting of form into components. It allows us to select a form which *we ourselves* have prescribed, and treat the rest as contamination which we can remove, if we can devise the necessary apparatus, so as to exhibit our selected form by itself. In every physical laboratory we see ingeniously devised tools for executing the work of sculpture, according to the designs of the theoretical physicist. Sometimes the tool slips and carves off an odd-shaped form which we had not expected. Then we have a new experimental discovery.

It is difficult to see where, if at all, a line can be drawn. The question does not merely concern light waves, since in modern physics form, particularly wave form, is at the root of everything. If no line can be drawn, we have the alarming thought that the physical analyst is an artist in disguise, weaving his imagination into everything—and unfortunately not wholly devoid of the technical skill to realise his imagination in concrete form.

An illustration may show that a serious practical question is raised. Just now nuclear physicists are writing a great deal about hypothetical particles called *neutrinos* supposed to account for certain peculiar facts observed in $\beta$-ray disintegration. We can perhaps best describe the neutrinos as little bits of spin-energy that have got detached. I am not much impressed by the neutrino theory. In an ordinary way I might say that I do not believe in neutrinos.[10] But I have to reflect that a physicist may be an artist, and you never know where you are with artists. My old-fashioned kind of disbelief in neutrinos is scarcely enough. Dare I say that experimental physicists will not have sufficient ingenuity to *make* neutrinos? Whatever I may think, I am not going to be lured into a wager against the skill of experimenters under the impression that it is a wager against the truth of a theory. If they succeed in making neutrinos, perhaps even in developing industrial applications of them, I suppose I shall have to believe—though I may feel that they have not been playing quite fair.

The question is raised whether the experimenter really provides such an effective control on the imagination of the theorist as is usually supposed. Certainly he is an incorruptible watch-dog who will not allow anything to pass which is not observationally true. But there are two ways of doing that—as Procrustes realised. One is to expose the falsity of an assertion. The other is to alter things a bit so as to make the assertion true. And it is admitted that our experiments *do* alter things.

I have been acting as advocate for an extreme view, presuming that your natural prejudices are all the other way. I must now try to recover the poise of a judge. I do not think that *as yet* the analytical imagination of the mathematical physicist has developed into the unfettered imagination of the artist. He plays the game according to certain rules which, arbitrary as they may seem at first sight, express an epistemological principle that goes deep into the roots of human thought. This we shall discuss presently. But have we a guarantee that the rules are for all time? The boy who outrageously breaks the rules of a game may be suitably punished by his companions, or he may be commemorated as the founder of Rugby football.

[10] Doubtless until a truer understanding of the spin problem is reached, it is better to make shift with neutrinos than to ignore the difficulty which they are intended to meet. I have no objection to neutrinos as a temporary expedient, but I would not expect them to survive—except that, as suggested in this paragraph, survival may not be wholly a question of intrinsic merit.

> The man who makes neutrinos will not be punished if he has overstepped the rules; he will be acclaimed for freeing physics from an obstruction to its useful development.
> (Eddington, 1939, p. 109-113)

## 18.8 Chapter VIII: The Concept of Analysis

- ch. VIII Concept of Analysis. Intellectual activity. Objects of knowledge filtered through frame of thought. Parts require set of parts.

> To explain why we have to start from the notion of a complete set of parts rather than from the apparently simpler notion of a single part, I must ask a question. Is the bung-hole of a barrel part of the barrel? Think well before you answer; because the whole structure of theoretical physics is trembling in the balance. (Eddington, 1939, p. 119)

> Next suppose that we adhere to Euclid's axiom and decide that the bung-hole of a barrel is not part of the barrel. The objection to this is that it has long ceased to be the form of thought employed in physics. It is, I think, really a compound association of two concepts, the concept of analysis and the concept of substance. The concept of substance introduces a clear distinction of positive and negative; so that we can have a limited form ofthe concept of analysis, which we may call substance-analysis, in which the systems of analysis are restricted to those which furnish a complete set of *positive* parts. When the analysis is not associated with substance (or with a structurally equivalent concept), when for example it is associated with wave form, the restriction cannot be imposed. In optics darkness is considered to be constituted of two interfering light waves; light may be a "part" of darkness. In Fourier analysis the components partially cancel one another in the manner of positive and negative quantities. Thus, although there may be cases in physics in which analysis is applied to entities which by definition are essentially positive and the restriction of substance-analysis applies, we now look on it as an incidental restriction in a particular application and not as part of the fundamental concept of analysis.
>
> That the general form of the concept of analysis is the form accepted in physical science is shown conclusively by the example of the positron. A positron is a hole from which an electron has been removed; it is a bung-hole which would be evened up with its surroundings if an electron were inserted. But it would be out of the question nowadays to define "part" in such a way that electrons are parts of a physical system but positrons are not.
>
> You will see that the physicist allows himself even greater liberty than the sculptor (p. 111). The sculptor removes material to obtain the form he desires. The physicist goes further and

> adds material if necessary—an operation which he describes as removing negative material. He fills up a bung-hole, saying that he is removing a positron. But he still claims that he is only revealing—sorting out—something that was already there. (Eddington, 1939, p. 120-121)

> Our purpose is to expose, not necessarily to justify, the frame of thought underlying the expression of our physical knowledge. Partially at least we emancipate ourselves from a frame of thought as soon as we realise that it is only a frame of thought and not an objective truth we are accepting. Any power for mischief it may have is sterilised so long as it is kept exposed. I would not like to say that the concept of analysis is a necessity of thought. But, whether it is a necessary form or not, it has dominated the development of present-day physics, and we have to follow up its influence on the scheme of description of phenomena which has resulted.
> (Eddington, 1939, p. 121)

- analysis (reduction?) breeding sameness, inevitability of structurally same units, concept of same unit of structure, impress of form of thought, electron vs. protons, quantum

> I will call this specialisation of the concept of analysis the *atomic concept*, or for greater precision the *concept of identical structural units*.
>
> The new conception is, not merely that the whole is analysable into a complete set of parts, but that it is analysable into parts which resemble one another. It is at the opposite pole from the analysis, say, of a human being into soul and body, in which the two parts belong to altogether different categories of entities. I will go farther, and say that the aim of the analysis employed in physics is to resolve the universe into structural units which are *precisely* like one another.
>
> It may be objected that the structural units recognised in present-day physics, though resembling one another to a certain extent, are not precisely alike. The Fourier components of white light, though all simple harmonic trains of waves, differ in wave-length—a difference which we observe as difference of color. But this difference is not intrinsic. It depends on the relation of the observer to the structural unit; if he recedes from the source of light, green light turns to red. Intrinsically the constituents of light—the wave trains or the photons—are all precisely alike; it is only in their relations to the observer, or to external objects generally, that they differ. That is the essence of relativity theory. All the variety in the world, all that is observable, comes from the variety of relations between entities. Therefore when we reach the consideration of the intrinsic nature or structure of the entities that are related, there is nothing left but sameness—in so far as that nature or struc-

ture comes within the scope of physical knowledge and is part of the universe which physical knowledge describes.

Granting that the elementary units found in our analysis of the universe are precisely alike intrinsically, the question remains whether this is because we have to do with an objective universe built of such units, or whether it is because our form of thought is such as to recognise only systems of analysis which shall yield parts precisely like one another. Our previous discussion has committed us to the latter as the true explanation. We have claimed to be able to determine by *a priori* reasoning the properties of the elementary particles recognised in physics—properties confirmed by observation. This would be impossible if they were objective units. Accordingly we account for this *a priori* knowledge as purely subjective, revealing only the impress of the equipment through which we obtain knowledge of the universe and deducible from a study of the equipment. We now say more explicitly that it is the impress of our frame of thought on the knowledge forced into the frame.

We have just seen that the concept of identical structural units is implicit in the relativity outlook, which attributes variety to relations and not to intrinsic differences in the relata; but I suppose it would be too much to claim that the relativity outlook is engrained in us—that our minds are so constituted that we cannot help moulding our thoughts in the Einsteinian way. I want to show therefore that the concept of identical structural units expresses a very elementary and instinctive habit of thought, which has unconsciously directed the course of scientific development. Briefly, it is the habit of thought which regards variety always as a challenge to further analysis; so that the *ultimate* end-product of analysis can only be sameness. We keep on modifying our system of analysis until it is such as to yield the sameness which we insist on, rejecting earlier attempts (earlier physical theories) as insufficiently profound. The sameness of the ultimate entities of the physical universe is a foreseeable consequence of forcing our knowledge into this form of thought. That it is really engrained in us can be seen from the following example.

Analysis of matter, as usually presented in present-day theory, reaches a considerable degree of homogeneity of the ultimate parts, but does not quite attain the ideal. We find protons exactly like one another; we also find electrons, like one another but differing from protons. Thus the physicist recognises two varieties of elementary units; and nowadays it is difficult to restrain him from adding several others. Why does a proton differ from an electron? The answer suggested by relativity theory is that they are actually similar units of structure, and the difference arises in their relations to the general distribution of matter which forms the universe. The one is related right-handedly and the other left-handedly. This accounts for the difference of charge; and the difference of mass is also (in a more complicated way) a difference of relation to the external matter without which there would be no means of determining mass observationally. There is no reasonable doubt that this answer is correct; but what interests us here is not the scientific answer resulting from the application of relativity theory, but the way in which we instinctively try to account for the difference. We cannot allow ourselves to think of the difference between a proton and an electron as an irreducible dualism—like the difference between soul and body. (I use the best comparison I can find; but the form of thought, which insists on getting behind—on explaining—variety, is so universal that even the dualism of soul and body is challenged by it.) No sooner do we discover a difference between protons and electrons than we begin to wonder what makes them different. When this question arises, we always fall back on structure. We try to explain the difference as a difference of structure, the structure of the proton being presumably the more complicated. But if protons and electrons possess structure, they cannot be the ultimate units of which structure is built. Therefore the present variety of the end-products of physical analysis is an indication that we have not yet touched bottom; and we must push our investigations farther, till we reach identical units which will not challenge us to farther analysis. The inference, as it happens, is fallacious, because the difference between protons and electrons is in the external relations and is not intrinsic. But a fallacious inference is informative as to our background of thought; and the thought which insists on intruding is that things which differ do so because they have different structure. The difference resides in the structure and not in the units out of which structure is built.

I conclude therefore that our engrained form of thought is such that we shall not rest satisfied until we are able to represent all physical phenomena as an interplay of a vast number of structural units intrinsically alike. All the diversity of phenomena will then be seen to correspond to different forms of relatedness of these units or, as we should usually say, different configurations. There is nothing in the external world which dictates this analysis into similar units, just as there is nothing in the irregular vibrations of white light which dictates our analysis of it into monochromatic wave trains. The dictation comes from our own way of thought which will not accept as final any other form of solution of the problem presented by sensory experience.

In current quantum theory the analysis approaches, but has not yet reached, this ideal. For that reason quantum physicists are still unsatisfied that they have got to the bottom of the relationships of the various kinds of particle that they recognise, and of the connection between gravitation, electromagnetism and quantisation.

(Eddington, 1939, p. 122-126)

- he then refers to p. 162-169
- section IV, self sufficiency, interaction

The conception of permanently self-sufficient parts of the physical universe is self-contradictory; for such parts are necessarily outside observational knowledge, and therefore not part of the universe which observational knowledge is formulated to describe.

The model structure of an atom is incomplete unless it contains some provision by which *we* can become aware of what is happening in the atom. In short, physics having taken the world to pieces, has the job of cementing it together again. The cement is called *interaction*.

(Eddington, 1939, p. 127)

## 18.9 Chapter IX: The Concept of Structure

Properly to realise the conception of group-structure, we must think of the pattern of interweaving as abstracted altogether from the particular entities and relations that furnish the pattern. In particular, we can give an exact mathematical description of the pattern, although mathematics may be quite inappropriate to describe what we know of the nature of the entities and operations concerned in it. In this was mathematics gets a footing in knowledge which intrinsically is not of a kind suggesting mathematical conceptions. Its function is to elucidate the group-structure of the elements of that knowledge. It dismisses the individual elements by assigning to them symbols, leaving it to non-mathematical thought to express the knowledge, if any, that we may have of what the symbols stand for.

We shall refer to this abstraction as the mathematical concept of structure, or briefly as the *concept of structure*. Since the structure, abstracted from whatever possesses the structure, can be exactly specified by mathematical formulae, our knowledge of structure is communicable, whereas much of our knowledge is incommunicable. I cannot convey to you the vivid knowledge which I have of my own sensations and emotions. There is no way of comparing my sensation of the taste of mutton with your sensation of the taste of mutton; I can only know what it tastes like to me, and you can only know what it tastes like to you. But if we are both looking at a landscape, although there is no way of comparing our visual sensations as such, we can compare the *structures* of our respective visual impressions of the landscape. It is possible for a group of sensations in my mind to have the same structure as a group of sensations in your mind. It is possible also that a group of entities which are not sensations in anyone's mind, associated together by relations of which we can form no conception, may have this same structure. We can therefore have structural knowledge of that which is outside everyone's mind. This knowledge will consist of the same kind of assertions as those which are made about the physical universe in the modern theories of mathematical physics. For strict expression of physical knowledge a mathematical form is essential, because that is the only way in which we can confine its assertions to structural knowledge. Every path to knowledge of what lies beneath the structure is then blocked by an impenetrable mathematical symbol.

Physical science consists of purely structural knowledge, so that we know only the structure of the universe which it describes. This is not a conjecture as to the nature of physical knowledge; it is precisely what physical knowledge as formulated in present-day theory states itself to be. In fundamental investigations the conception of group-structure appears quite explicitly as the starting point; and nowhere in the subsequent development do we admit material not derived from group-structure.

The fact that structural knowledge can be detached from knowledge of the entities forming the structure, gets over the difficulty of understanding how it is possible to conceive a knowledge of anything which is not part of our own minds. So long as the knowledge is confined to assertions of structure, it is not tied down to any particular realm of content. It will be remembered that we have separated the question of the nature of knowledge from the question of assurance of its truth. We are not here considering how it is possible to be assured of the truth of knowledge relating to something outside our minds; we are occupied with the prior question how it is possible to make any kind of assertion about things outside our minds, which (whether true or false) has a definable meaning.

(Eddington, 1939, p. 141-143)

What sort of thing is it that I know? The answer is *structure*. To be quite precise, it is structure of the kind defined and investigated in the mathematical theory of groups.

(Eddington, 1939, p. 147)

The bewilderment of the philosophers evidently arises from a belief that, if we start from zero, any knowledge of the external world must begin with the assumption that a sensation makes us aware of something in the external world—something differing from the sensation itself because it is non-mental. But knowledge of the physical universe does not begin in that way. One sensation (divorced from knowledge already obtained by other sensations) tells us nothing; it does not even hint at anything outside the consciousness in which it occurs. The starting

point[11] of physical science is knowledge of *the group-structure of a set of sensations* in a consciousness. When these fragments of structure, contributed at various times and by various individuals, have been collated and represented according to the forms of thought that we have discussed, and when the gaps have been filled by an inferred structure depending on the regularities discovered in the directly known portions, we obtain the structure known as the physical universe.
(Eddington, 1939, p. 147-148)

The recognition that physical knowledge is structural knowledge abolishes all dualism of consciousness and matter. Dualism depends on the belief that we find in the external world something of a nature incommensurable with what we find in consciousness; but all that physical science reveals to us in the external world is group-structure, and group-structure is also to be found in consciousness. When we take a structure of sensations in a particular consciousness and describe it in physical terms as part of the structure of an external world, it is still a structure of sensations.
(Eddington, 1939, p. 150)

Let us denote by $X$ the entity of which the physical universe is the structure,[12] and distinguish the small part $X_s$ known to be of sensory nature from the remainder $X_u$ of which we have no direct awareness. It may be suggested that there remains a dualism of $X_s$ and $X_u$ equivalent to the old dualism of consciousness and matter; but this is, I think, a logical confusion, involving a switch over from the epistemological view of the universe as the theme of knowledge to an existential view of the universe as something of which we have to obtain knowledge. Structurally $X_u$ is no different from $X_s$, and to give meaning to the supposed dualism we have to imagine a supplementary non-structural knowledge of $X_u$ revealing its unlikeness to $X_s$. We have to suppose that a direct awareness of $X_u$, if we could possess it, would show that it was not of sensory nature. But the supposition is nonsense; for if we had the supposed direct awareness of $X_u$, it would *ipso facto* be a sensation in our consciousness. Thus we cannot give meaning to the dualism without making a supposition which eliminates the dualism.

Although the statement that the universe is of the nature of a "thought or sensation in a universal Mind" is open to criticism, it does at least avoid this logical confusion. It is, I think, true in the sense that it is a logical consequence of the form of thought which formulates our knowledge as a description of a universe.

[11] I mean the logical starting point, not the historical starting point, of a subject which has grown out of crude beginnings

[12] I usually call $X$ the "external world", the "physical world" being limited to the structure of the external world.

> But it requires more guarded expression if it is to be accepted as a truth transcending forms of thought.
>
> To sum up. The physical universe is a structure. Of the $X$ of which it is the structure, we only know that $X$ includes sensations in consciousness. To the question: What is $X$ when it is not a sensation in any consciousness known to us? the right answer is probably that the question is a meaningless one—that a structure does not necessarily imply an $X$ of which it is the structure. In other words, the question takes us to a point where the form of thought in which it originates ceases to be useful. The form of thought can only be preserved by still attributing to $X$ a sensory nature—a sensation in a consciousness unknown to us. What interests us is not the positive conslusion, but the fact that in no circumstances are we required to contemplate an $X$ of non-sensory nature. (Eddington, 1939, p. 150-151)

- then Russell

## 18.10 Chapter X: The Concept of Existence

- disagreement over what exists, overdraft example
  - GO BACK HERE AND RE-READ AND GET QUOTES
  -idempotence
  uranoid

## 18.11 ChapterXI: The Physical Universe

> I believe there are 15,747,724,136,275,002,577,605,653,961,181,555,468,044,717,914,527,116,709,366,231,425,0 protons in the universe, and the same number of electrons. (Eddington, 1939, p. 170)

- omelette analogy, uncountable particles

> A nucleus is composed of protons and electrons in the same way that an omelette is composed of eggs; that is to say, when the omelette appears on the table, there are fewer eggs in the larder. The proton-electron composition assigned to the various nuclei is amply confirmed by transmutation experiments which apply directly the "omelette" criterion of composition. I think the craze for a metaphysical pronouncement, that eggs and electrons cease to exist when they are scrambled, has now died away; but it was in any case an irrelevancy. (Eddington, 1939, p. 170-171)

- degrees of belief, uncountable,

> One believes with varying degrees of confidence. My belief that I know the exact number of protons and electrons in the universe does not rank among my strongest scientific convictions, but I should describe it as a fair average sort of belief. I

> am, however, strongly convinced that, if I have got the number wrong, it is just a silly mistake, which would speedily be corrected if there were more workers in this field. In short, to know the exact number of particles in the universe is a perfectly legitimate aspiration of the physicist. (Eddington, 1939, p. 171)

> Let us see why protons and electrons are uncountable. It is not merely because there are so many of them. Quantum physicists tell us that an electron is not definitely in one place but is smeared over a probability distribution; also that electrons are indistinguishable from one another. That is not very promising material for counting. There is nothing to remember about the electron you last counted—neither its position nor any distinguishing mark. So how can you know whether the next you notice is a new one or one already counted? By the uncertainty principle, the more closely you pin down its position at one instant, the more uncertain you are of its velocity and where it will turn up next. When you retire to rest, as a variant to counting sheep in a green field, perhaps you may like to try counting electrons in a probability distribution. (Eddington, 1939, p. 171-172)

- debunking $N$ as an important/objective number (after giving calculation, he thinks it shows lack of objectivity). reminds me of Poincare: not objective, not arbitrary, but a convention imposed

> I have told you what I believe to be the true story of the cosmical number $N$. What should we conclude from it?
> To put it crudely, we have *debunked* $N$. It is not an enumeration of a crowd of discrete particles constituting the objective universe. Since it is merely a number foisted on us by quantum theory, being associated in an *a priori* way with its methods of analysis, is it any longer of interest? I do not think its scientific interest is at all affected. Intrinsically the number of particles in the universe, even if it were genuine, would be a matter of rather trivial curiosity. The number is scientifically iimportant because it keeps cropping up in more prosaic problems. It fixes the ratio of the electrical to the gravitational force between a proton and electron—a quantity which practical physicists have been at great pains to determine. (Eddington, 1939, p. 177)

> I have singled out $N$ for attention because, of all the knowledge comprised in fundamental physics, knowledge of the number of elementary particles had seemed least likely to be tainted with subjectivity. It was therefore particularly suitable for a test case. But the same subjectivity appears everywhere and is usually not so difficult to discern. The whole scheme of physical law is debunked, if you like to put it that way. But debunking the laws of optics will not put out the sun's light; debunking the law of gravitation will not prevent us from falling down

> stairs; debunking the laws of ballistics will not put a stop to war. Even if the mystery is torn from them, the laws of our semi-subjective universe are valid in that universe, and in the technical discoveries and inventions of science will continue to bear fruit for good or evil. (Eddington, 1939, p. 178)

> Since then microscopic physics has made great progress, and its laws have turned out to be comprehensible to the mind; but, as I have endeavoured to show, it also turns out that they have been imposed by the mind—by our forms of thought—in the same way that the molar laws are imposed. (Eddington, 1939, p. 180)

- applicability of "laws of chance" vs. Heisenberg uncertainty limits and quantum probabilities??? !!!

> In current physical theory the undetermined element in the behaviour of a system is treated as a matter of chance. If there were serious deviations from the law of chance, observation and theory would not agree. We may therefore say that it is a hypothesis in physics, supported by observation, that there are no objective laws of governance—unless chance is described as a law.
>
> Nevertheless, if we take a wider view than that of physics, I think it would be misleading to regard chance as the characteristic feature of the objective world. The denial of objective laws of governance is not so much a hypothesis of physics as a limitation of its subject matter. Deviations from chance occur, but they are regarded as manifestations of something outside physics, namely consciousness or (more debatably) life. There is in a human being some portion of the brain, perhaps a mere speck of brain-matter, perhaps an extensive region, in which the physical effects of his volitions begin, and from which they are propagated to the nerves and muscles which translate the volition into action. We will call this portion of the brain-matter "conscious matter". It must be exactly like inorganic matter in its obedience to the fundamental laws of physics which, being of epistemological origin, are compulsory for all matter; but it cannot be identical in all respects with inorganic matter, for that would reduce the body to an automaton acting independently of consciousness. The difference must necessarily lie in the undetermined part of the behaviour; the part of the behaviour which is undetermined by the fundamental laws of physics must in conscious matter be governed by objective law or direction instead of being wholly a field of chance.
>
> The term "law of chance" tends to mislead, because it is applied to what is merely an absence of law in the usual sense of the term. It is clearer to describe the conditions by reference to correlation. The hypothesis of current physical theory, which is confirmed by observation of inorganic phenomena, is that

there is no correlation of the undetermined behaviour of the individual particles.

Accordingly the distinction between ordinary matter and conscious matter is that in ordinary matter there is no correlation in the undetermined parts of the behaviours of the particles, whereas in conscious matter correlation may occur. Such correlation is looked upon as an interference with the ordinary course of nature, due to the association of consciousness with the matter; in other words, it is the physical aspect of a volition. This does not mean that, in order to execute a volition, consciousness must direct each individual particle in such a way that correlation occurs. The particles are merely a representation of our knowledge in the frame of thought corresponding to the concept of analysis and the atomic concept. When we apply the system of analysis which gives this representation, we cannot foresee whether the resulting particles will have correlated or uncorrelated behaviour; that depends entirely on the objective characteristics of whatever it is that we are analysing. When non-correlation is assumed, as is customary in physics, it is assumed as a hypothesis. But, without making any hypothesis, we can say that correlation and non-correlation are representations in our frame of thought of different objective characteristics; and since non-correlation admittedly represents the objective characteristic of systems to which the ordinary formulae of physics apply, correlation must represent another objective characteristic which—since it is not characteristic of systems to which the formulae of physics apply—is regarded by us as something "outside physics".

In the discussion of freewill provoked by the modern physical theories, it has, I think, generally been assumed that, since the ordinary laws of inorganic matter leave its behaviour undetermined within a certain narrow range, there can be no scientific objection to allowing a volition of consciousness to decide the exact behaviour within the limits of the aforesaid range. I will call this hypothesis $A$. For any system on a molar scale the permitted range is exceedingly small; and very far-fetched suppositions are necessary to enable volition, working in so small a range, to produce large muscular movements. To obtain a wider range we must admit correlation of the behaviour of the particles. This is the theory we have been discussing, and will be called hypothesis $B$. In former writings I have advocated hypothesis $B$ mainly on the ground of the inadequacy of hypothesis $A$; but in the present mode of approach hypothesis $B$ presents itself as the obvious and natural solution.

Although leading to the same conclusion, my earlier discussions[13] were marred by a failure to recognise that hypothesis $A$ is nonsense; so that I was more apologetic than I need have been for going beyond it. There is no half-way house be-

[13] *The Nature of the Physical World*, pp. 310-315. *New Pathways in Science*, p. 88.

tween random and correlated behaviour. Either the behaviour is wholly a matter of chance, in which case the precise behaviour within the Heisenberg limits of uncertainty depends on chance and not on volition. Or it is not wholly a matter of chance, in which case the Heisenberg limits, which are calculated on the assumption of non-correlation, are irrelevant. If we apply the law of chance to the tossing of a coin, the number of heads in 1000 throws is undetermined within the limits, say, 450 to 550. But if a coin-tossing machine is used which picks up and throws the coin not entirely at random, the non-chance element is not a factor deciding which number between 450 and 550 will turn up; a correlation, or systematic tendency in tossing, may produce any number of heads from 0 to 1000.

The fallacy of hypothesis $A$ was that it assumed the behaviour to be restricted by the ordinary laws of physics including the hypothesis of non-correlation or "law of chance", and then to be further restricted (or decided) by a non-chance factor (volition). But we cannot suppose the behaviour to be restricted by chance and non-chance (non-correlation and correlation) simultaneously. The applicability of the law of chance is a hypothesis; the admission that the behaviour is not governed solely by chance denies the hypothesis. So if we admit volition at all, we must not forget first to remove the hypothesis of chance if we have been applying it; in particular we must drop the Heisenberg limits which apply only to non-correlated behaviour. If volition operates on the system, it does so without regard to the Heisenberg limits. Its only limits are those imposed by the fundamental epistemological laws.

Our volitions are not entirely unconsequential; so that there must be laws of some kind applying to them and connecting them with other constituents of consciousness, though such laws are not expected to be of the mathematically exact type characteristic of subjective law. Primarily the sphere of objective law is the interplay of thoughts, emotions, memories and volitions in consciousness. In controlling volitions objective law controls also the correlations which are the physical counterparts of volitions.

Our philosophy has led to the view that in so far as we can separate the subjective and objective elements in our experience, the subjective is to be identified with the physical and the objective with the conscious and spiritual aspects of experience. To this we now add, as a helpful analogy provided it is not pressed too far, that conscious purpose is the "matter" and chance the "empty space" of the objective world. In the physical universe matter occupies only a small region compared with the empty space; but, rightly or wrongly, we look on it as the more significant part. In the same way we look on consciousness as the significant part of the objective universe, though it appears to occur only in isolated centres in a background of

> chaos. (Eddington, 1939, p. 180-184)

- quoting section IV in total, "rational correlation of experience", orthodoxy seeming to pay lip service to [idealist position] without acknowledging or coming to grips with major implications [anti-realism], epistemological confusion, makes clear that subjective view applies in particular to physics
- maybe he would like epistemic psi?

> I am about to turn from the scientific to the philosophical setting of scientific epistemology. This is accordingly a suitable place at which to make a comparison with the most commonly accepted view of scientific philosophy. The following statement is fairly typical:
>
> > That science is concerned with the rational correlation of experience rather than with the discovery of fragments of absolute truth about an external world is a view which is now widely accepted.[14]
>
> I think that the average physicist, in so far as he holds any philosophical view at all about his science, would assent. The phrase "rational correlation of experience" has a savour of orthodoxy which makes it a safe gambit for applause. The repudiation of more adventurous aims gives a comfortable feeling of modesty—all the more agreeable if we fancy that someone else is being told off. For my own part I accept the statement, provided that "science" is understood to mean "physics". It has taken me nearly twenty years to accept it; but by steady mastication during that period I have managed to swallow it all down bit by bit. Consequently I am rather flabbergasted by the light-hearted way in which this pronouncement, carrying the most profound implications both for philosophy and for physics, is commonly made and accepted.
>
> I have no serious quarrel with the average physicist over his philosophical creed—except that he forgets all about it in practice. My puzzle is why a belief that physics is concerned with the correlation of experience and not with absolute truth about the external world should usually be accompanied by a steady refusal to treat theoretical physics as a description of correlations of experience and an insistence on treating it as a description of the contents of an absolute objective world. If I am in any way heterodox, it is because it seems to me a consequence of accepting the belief, that we shall get nearer to whatever truth is to be found in physics by seeking and employing conceptions suitable for the expression of correlations of experience instead of conceptions suitable for the description of an absolute world.
>
> The statement evidently means that the methods of physics are incapable of discovering fragments of absolute truth about

[14] Unsigned review, *Phil. Mag.*, vol. 25, p. 814, 1938

> an external world; for we should have no right to withhold from mankind the absolute truth about the external world if it were within our reach. If the laboratories, built and endowed at great expense, could assist in the discovery of absolute truth about the external world, it would be reprehensible to discourage their use for this purpose. But the assertion that the methods of physics cannot reveal absolute (objective) truth or even fragments of absolute truth, concedes my main point that the knowledge obtained by them is wholly subjective. Indeed it concedes it far too readily; for the assertion is one that ought only to be made after prolonged investigation. As I have pointed out, sciences other than physics and chemistry are not so limited in their scope. The discovery of unmistakable signs of intelligent life on another planet would be hailed as an epoch-making astronomical achievement; it can scarcely be denied that it would be the discovery of a fragment of absolute truth about the world external to us.
>
> Keeping to physics, the commonly accepted scientific philosophy is that it is not concerned with the discovery of absolute truth about the external world, and its laws are not fragments of absolute truth about the external world, or, as I have put it, they are not laws of the objective world. What then are they, and how is it that we find them in our correlations of experience? Until we can see, by an examination of the procedure of correlation of our observational experience, how these highly complex laws can have got into it subjectively, it seems premature to accept a philosophy which cuts us off from all other possible explanations of their origin. This is the examination that we have been conducting.
>
> The end of our journey is rather a bathos after so much toil. Instead of struggling up to a lonely peak, we have reached an encampment of believers, who tell us "That is what we have been asserting for years". Presumably they will welcome with open arms the toilworn travellers who have at last found a resting place in the true faith. All the same I am a bit dubious about that welcome. Perhaps the assertion, like many a religious creed, was intended only to be recited and applauded. Anyone who *believes* it is a bit of a heretic. (Eddington, 1939, p. 184-186)

- last comment reminds me of J.S. Mill talking about how it isn't enough to coincidentally believe the truth (living truth vs. dead dogma). Perhaps he was aware of On Liberty or had to read Mill.

## 18.12 ChapterXII: The Beginnings of Knowledge

- FINALLY HE MENTIONS KANT, VERY RESTRICTED verification principle/instrumentalism of logical positivism

> It is, I think, inadvisable to try to describe a scientifically grounded philosophy by the labels of the older philosophical

> systems. To accept such a label would make the scientist a party to controversies in which he has no interest, even if he does not condemn them as altogether meaningless. But if it were necessary to choose a leader from among the older philosophers, there can be no doubt that our choice would be Kant. We do not accept the Kantian label; but, as a matter of acknowledgement, it is right to say that Kant anticipated to a remarkable extent the ideas to which we are now being impelled by the modern developments of physics.
>
> Reference may also be made to another general philosophical system, namely *logical positivism*. Our insistence that physical quantities are to be defined in such a way that the assertions of physics admit of observational verification, may suggest an affinity with logical positivism. The meaning of a scientific statement is to be ascertained by reference to the steps which would be taken to verify it. This will be recognised as a tenet of logical positivism—only it is there extended to all statements. When it is limited, as here, to items of physical knowledge, it is in no sense a philosophical tenet; it is only a bringing into line of the language of theoretical and of experimental physics, so that we may not claim the support of observation for assertions which have no observational foundation. If it were a general characteristic of knowledge, it would not be so useful to us in discriminating physical knowledge from other kinds of knowledge. We are therefore not particularly predisposed to favour the more general assertion of logical positivism that the meaning of all non-tautological statements is to be ascertained in the same way, namely by reference to the procedure of verifying them. (Eddington, 1939, p. 188-189)

- leans into phil mind and phenomenology in section II, subjective or sympathetic knowledge of a subject (as opposed to structural k.), basically knowledge/theory of other minds in social complex, refs Wells's Country of the Blind and justifiably rejecting others sense knowledge, talks about solipsism and consciousness,

> Without the sympathetic faculty which enables me to recognise myself, not as an individual *mei generis*, but as an element of a social complex, the conception of "human knowledge" could not arise; and it would therefore seem illogical to reject this faculty in defining the extent of human knowledge.
>
> No one believes in solipsism, and very few even assert that they do. Those who are obsessed by the word "existence" come somehow to the conclusion that other consciousnesses besides their own exist; that is to say, other consciousnesses can be the subject of that mysterious sentence which they never finish. Those who adopt the epistemological approach take for their subject matter a knowledge which embodies the experiences of other individuals on the same footing as their own experience. Formally this is non-committal; it is not necessary to assign

> reasons for choosing a particular theme of study. But undoubtedly the choice is determined by a conviction, akin to religious conviction, that this co-operative knowledge is the most worth while. This conviction is inconsistent with a solipsistic outlook.
>
> It would be meaningless to attribute consciousness to another man without knowing at all what we are attributing to him. But consciousness is not a structural concept describable by purely structural knowledge; nor is the consciousness that we attribute to another man anything of which we have direct awareness, since it is not our own consciousness. It follows that, if our recognition of conscious beings other than ourselves has any meaning at all, their consciousness must be something of which we have a knowledge which is neither structural knowledge nor direct awareness; and any description of it must be expressed in terms of the third kind of knowledge which we have called sympathetic understanding. (Eddington, 1939, p. 193-194)

- then he basically talks about qualia and how the physicist "has a domain independent of sympathetic knowledge", he notes that he wants to avoid "the dilemma of either (a) denying that there is any knowledge other than physical knowledge or (b) relapsing into the solipsism which we repudiate at the very beginning of physical science." (p. 195)

- sensations and feelings,

> The recognition that certain memories are to be treated as a knowledge of past sensations is essential for physical science; because, as we shall see later, the first step towards structural knowledge is a comparison of sensations in one consciousness. The datum of physical science is not awareness of a sensation, but awareness that a sensation is like, or different from, a sensation which we formerly had. Granting this, the sensations of one person alone provide sufficient material for structural analysis; and it would be possible to develop from it a scientific theory which, except that it is presented in an egocentric frame of thought, would agree with ordinary physical theory. But since the analysis would never take us outside a single consciousness, it would give no indication of a world external to that consciousness. The externality of the physical world results from the fact that it is made up of structures found in different consciousnesses.
>
> Thus the recognition of sensations other than our own, though not required until a rather later stage of the discussion, is essential to the derivation of an *external* physical universe. Our direct awareness of certain aural and visual sensations (words heard and read) is postulated to be an indirect knowledge of quite different sensations (described by the words heard and read) occurring elsewhere than in our own consciousness. Solipsism would deny this; and it is by accepting this postulate that physics declares itself anti-solipsistic. (Eddington, 1939, p. 198-199)

- awareness

## 18.13 ChapterXIII: The Synthesis of Knowledge

- pointing, phil. lang., inferences from data not linguistic pointers of data
  - "I know that" is idempotent (?)
  - holism about consc.

> To sum up: "I" is first a label or pointer-word attached to a particular consciousness, and consequentially to the sensations, emotions, etc. into which the consciousness is divided by the concept of analysis; and secondly, as associated with self-consciousness, it is part of a verbal form "I am aware of 'I"' used to point to a residuum of awareness which eludes the concept of analysis. The phrase points to the datum (of which we have immediate knowledge) that our whole awareness is not fully represented by the parts into which we customarily divide it; in other words, it is a unity and not an assemblage of parts. It appears to be no more than linguistic custom that "I" is made in the first case the subject and in the second case the object of the verb "to be aware". When we try to get behind the wording, we find nothing to support the view that awareness is a subject-object relation or even a subject-intransitive relation. (Eddington, 1939, p. 206-207)

  - phil lang critique of realism

> And, so I suppose, realists will not insist that whenever we gesticulate something must be gesticulated, and that the something is unaffected by our gesticulation of it, being indeed precisely what it would be if it were not being gesticulated. Yet I sometimes wonder how a realist would regard the gesture known as "cocking a snook". It would seem clear that something must be cocked; and I fear the only logical conclusion is that there is a realm of existence containing uncocked snooks which are exactly what they would be if they were being cocked—but perhaps that is too dangerous a thought to pursue when philosophers are trying to express what they think of one another! (Eddington, 1939, p. 213-214)

> It would seem that the first time we perceive a new taste, our consciousness becomes modified in such a way that thereafter an imagining of the taste is possible. We ordinarily say that a memory of the taste is stored up in it. I do not see how this can be reconciled with the realist view that imagining and perceiving are independent relations of consciousness to a sensum outside consciousness.
>
> In the passage that I have quoted it is recognised that, if perceiving is purely a relation between the mind and an external object, the object is not modified by our perceiving of it. It is not clear whether it is also recognised that the mind is not modified. If the mind is modified by the act of perceiving, it is

incorrect to describe perceiving as a "relation"; and the argument based on the existence of more than one kind of relation falls to the ground. On the other hand, if neither the mind nor the sensum is modified by the act of perceiving, how is it that it is not until after the perception that a new kind of relation of the mind to the sensum becomes possible, namely remembering or imagining? (Eddington, 1939, p. 215)

It would be illogical to attribute the similarity of the structures in different consciousnesses to a common cause without allowing to the common cause a status fully as objective as the structures themselves. I therefore take it as axiomatic that the external world must have objective content. But according to our conclusions, the laws of physics are a property of the frame of thought in which we represent our knowledge of the objective content, and thus far physics has been unable to discover any laws applying to the objective content itself. This raises the question, How is it that we are able to make successful predictions of phenomena without knowing any law controlling the objective content of the universe and therefore without knowing how the objective content is going to behave?
Although it is rather the fashion for scientific writers to say that physics is not concerned with objective truth, it would be unsafe to take them at their word. Apparently the statement is intended to closure discussion, rather than to assert a principle whose far-reaching implications invite investigation. Our own conclusion is less sweepingly expressed; but it is meant seriously, and we must examine the difficulties to which it seems to lead.
Much of the difficulty disappears if we keep in mind that *pure* subjectivity is confined to the laws—the regularities—of the physical world. The variety of appearances around us is primarily an objective variety. That a subjective distortion is introduced in our apprehension of things is no more than physicists have been accustomed to admit. (Eddington, 1939, p. 217)

It is often pointed our that the primary difference of outlook between the scientist and the savage is that the savage attributes all that he finds mysterious in nature to the activity of demons or other spirits. For the savage any physical object may be possessed of demonic volition, and it is impossible to count on its behaviour except in so far as the directing demon may be amenable to prayer and propitiation. Physical science has made a place for itself by greatly limiting the sphere of demonic activity, so that there is an extensive realm of experience in which behaviour can be counted on and scientific prediction is possible. Great as may be the practical effects of this change, it is a matter of detail (special fact) rather than of principle. Demonic activity (volition) remains, though it is limited to certain centres in men and the higher animals.

> Prayer and propitiation may still influence the course of physical phenomena when directed to these centres. We now think it ludicrous to imagine that rocks, sea and sky are animated by volitions such as we are aware of in ourselves. It would be thought even more ludicrous to imagine that the volitionless behaviour of rocks, sea and sky extends also to ourselves, were it not that we have scarcely yet recovered from the repressions of 250 years of deterministic physics.
>
> Accordingly we do not regard the principle of non-correlation as one of the fundamental laws of physics. Non-correlation usually applies; but correlation occurs exceptionally, and the result is an unexpectedness of behaviour which is recognised by us as a physical manifestation of conscious volition. In saying that the behaviour is unexpected, we mean unexpected from the point of view of physics, which supplies the gap left by our ignorance of the springs of objective behaviour by assuming non-correlation. Actually the volitional behaviour may be fully expected—it may be an answer to our own request—but this expectation takes into account knowledge of the objective world not comprised in physical science and not reducible to the accepted pattern of physical law. In so far as the comparative rarity of correlation can be considered a law, it is a law of distribution of consciousness rather than a law of the physical world. (Eddington, 1939, p. 219-220)

# 19 Kilmister on Eddington

- ch. 1, mystery of Eddington (great astrophysicist) being more speculative and perhaps wrong in two later books in period after Dirac electron (1928)

- ch. 2, Einstein special relativity (Galilean to Lorentzian transformations, rejection of Newtonian absolute time) not liked by British Maxwellians

> Such is the real content of Einstein's paper but this is not quite how things looked when seen from Cambridge. The influence of Maxwell himself had been profound. He had inferred from his electromagnetic equations the existence of wave solutions travelling with the speed of light and he had identified light itself with such a solution. The problem for Maxwell, and many British physicists, was the medium of transmission of the waves. This supposed medium, the aether, was seen by Maxwell as a physically real diffuse medium filling the universe. The definitive account of this at the turn of the century was *Aether and Matter* (Larmor 1900). In it, aether theory had assumed a very subtle and elegant form. But if the aether was physical it should be possible to determine the motion of the earth through it by optical experiments. Maxwell suggested such an experiment, which was carried out in America by Michelson, first alone and then in conjunction with Morley.

In the Michelson-Morley experiment a beam of light is split in two by a half-silvered mirror. The two parts travel out and back along two directions at right-angles, being reflected by mirrors at the ends of two rigid arms. The relative time of the return rays is monitored by means of optical 'interference'. A patter of light and dark bands is obtained on a screen, because the two paths do not differ by an exact number of wavelengths. The apparatus is then rotated in the same horizontal plane through a right-angle. If one arm were, for instance, initially moving forward through the aether, so that the light speed would be different from that along the other transverse arm, then after rotation the situation will be reversed. The first arm will now be transverse to the 'aether wind' and the other arm will be suffering the effect of opposite movement. The interference fringes will therefore shift. No such effect was found, nor was this a coincidence resulting from the earth just happening to be at rest in the aether at the time. For a repetition six months later again gave a null result.

Einstein makes no reference to the Michelson-Morley experiment in his paper and at one time believed himself to have been ignorant of it, but later he admitted that it must have had some unconscious effect on his thinking. In the very much more aether-conscious atmosphere of British physics, the experiment was seen as an empirical demonstration that it was impossible to detect the motion of the earth through the aether. This led to a variety of responses. Few were yet happy to draw the obvious conclusion that, unfortunate as it might be for any intuitive need to have a medium in which light was travelling, no such aether existed. Some found Einstein's explanation of the Michelson-Morley experiment (that, as a simple consequence of the Lorentz transformation, the speed of light, unlike all other speeds, is unchanged by the motion of the observer) uncongenial and preferred to explain the result by hypothesising changes in the lengths of the arms of the apparatus. At the extreme of this party can, perhaps, be placed Rutherford who, as late as 1910 rejoined, to the suggestion that no Anglo-Saxon could understand relativity, 'No! they have too much sense'. Between the extremes were those who accepted Einstein's theory with greater or less enthusiasm, and saw as the next problem the rendering of it and aether theory as a consistent whole.

(Kilmister, 1994, p. 16-18)