High-Energy Physics and Reality

– Some Philosophical Aspects of a Science

Ph.D. Thesis
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1 Introduction

What I want to address in this thesis is the relationship between High-Energy Physics (HEP) and reality. First, however, I will have to discuss what can possibly be meant by these terms. The name HEP refers to the fact that studies of the microscopic structures of matter often consist in analyzing the reaction products of high energy particle collisons[1]. Questions about reality (e.g. what reality consists of), and their relations to the notions of truth and objectivity, have been discussed throughout intellectual history, and not least in connection with science. Consider for example the statement: "The goal of physics is to understand nature at the most fundamental level." In first approximation this amounts to saying that the goal of physics is to understand reality — simply by equating 'nature' with 'reality'. An impressive arsenal of philosophical arguments is raised against such an interpretation immediately. For instance: "Are you making claims of reality in itself or as it appears to you", "Is this distinction possible in the first place?", "Does 'understand' mean that the theories of physics are true, and if so, in what sense?", "How will physicists ever know if the goal has been obtained?", and "In what sense does physics deal with the most fundamental level?". These questions involve issues such as the conditions for obtaining knowledge, the possibility of reducing different branches of science to others and the meaning of concepts.

Traditionally, philosophy distinguishes between epistemological and ontological, or metaphysical, questions. Epistemology addresses the nature of acquisition and justification of knowledge whereas ontology is about what lies behind our experiences of the world — what the world is really like. Sometimes ontology, or metaphysics, has been ridiculed as unnecessary and speculative, but the ontological aspects of questions such as "is it possible to reduce biology to physics or is there a genuine division between the physical and the mental?", or "what are the causes or explanations for the phenomena we see" continue to attract interest from philosophers and scientists alike. Though the distinction between epistemology and ontology is useful, it is not always sharp. For instance, if one embraces the epistemological idea that scientific knowledge ultimately corresponds one-to-one with reality, then some ontological questions about what really exists are naturally answered.

An investigation of the philosophical aspects of HEP must necessarily, within the scope of this work, be selective regarding aspects of the HEP-reality relationship. A hint of such selectiveness, or focus, is provided by the philosophy of science. Specifically, the two topics 'theory-ladenness of experiments' and 'underdetermination of theories by observations' serve well to illuminate some epistemic aspects of HEP with ontological consequences. Very crudely, the content of the first is that experiments are always seen in a certain theoretical light, so that data are never pure. Underdetermination means that there is, in principle, always more than one theory capable of explaining the same set of observations.

The ideas of theory-ladenness and underdetermination will provide the starting point for the philosophical discussions of HEP to follow. But first I will review and comment upon some of the recent approaches to the philosophical understanding

[1]Sometimes this branch of science is referred to as elementary particle physics or simply particle physics. In a chapter 3, I will discuss the structure of HEP in more detail.

of science in general. In addition, I will address a more semantic aspect of science which suggests certain constraints on the epistemology of science. Indeed, Niels Bohr is often quoted as saying that science aims not to understand nature but what we can meaningfully say about it. This has lead others to analyze the language we use for a scientific description and the conditions which govern this activity. Such a project has been pursued by the philosopher Peter Zinkernagel with whom I have been discussing physics, epistemology and his theories on these matters for many years.

Despite my efforts to narrow the scope of study, I initially cut across a large number of issues related to the philosophy of science tradition. While this necessarily leads to some loss of details, it is my hope that the broad range of philosophical issues mentioned will put specific discussions of HEP in a proper context.

1.1 The method

Coming from a physics background, the method in this work has primarily been learning and applying. More specifically, I have used my background to analyze parts of HEP in light of the philosophical discussions and positions which I have continually studied. At the same time, I have attempted to keep a critical eye on the philosophical underpinnings of the conclusions about the relationship between science and reality which have been given in the literature.

The emphasis in this project is both on original case-studies of HEP and case-studies done by other scholars. It is well known that strict generalizations from single events, or even classes of events, are impossible. This is the philosophical problem of induction. On the other hand, if one is to understand science, or at least obtain some kind of overview, it is necessary to draw lessons from case-studies. By pointing to some of the problems raised by case-studies, one can illuminate the HEP-reality relationship from different perspectives. Except for many stimulating discussions with physicists and philosophers alike, the main input to the original case-studies presented in this work has come from the published literature.

1.2 Structure of the thesis

Although the discussion at times will be technical, I have tried to keep this thesis readable for all who have a general knowledge of science and philosophy. While the following can be read on its own, I will make explicit reference to the appendices which contain a substantial part of the actual research I was involved in during the course of my studies (see chapter 7 for an overview).

I begin in chapter 2 with a discussion of various philosophical positions on the relations between science and reality. Short introductions will be given to the ideas of incommensurable paradigms, theory-ladenness, and the underdetermination of theories by observations. Recently, these philosophical ideas have provided fuel for the social constructivists who have argued for a strong context-ladenness of science. I give a brief introduction to this branch of science studies and discuss how it is related to epistemology and ontology. Finally, by examining some ideas of Niels

Bohr and Peter Zinkernagel, I discuss how the objectivity concept might be more robust and general than some philosophers of science seem to hold.

Chapter 3 is a short introduction to HEP, its relations to other branches of physics, and the current state of the art — the Standard Model. In chapter 4, I briefly review a contemporary HEP experiment in order to indicate where theory is likely to be built into the analysis of experimental data. I then turn to the discovery of the the so-called weak neutral current which has been used to argue for at least three different attitudes toward scientific theories and facts. This chapter also includes a summary of a case-study dealing with the theoretical input to a series of experiments which have been carried out over the last 50 years. The result of these experiments, on the anomalous magnetic moment of the electron, provides the most accurate test of Quantum Electrodynamics — one of the corner-stone theories in HEP.

In chapter 5 I turn to the question of whether there are different theories which can explain the same experimental results in HEP. As a specific example, I summarize a case-study about the so-called 'Casimir effect' which shows how experiments can underdetermine choices between theories. Moreover, I briefly discuss criteria for theory choice in HEP. Finally, in chapter 6, I sum up some conclusions on the relationship between HEP and reality.

2 Reflections on Science

Before turning to the philosophical aspects of HEP, I will review some insights from the philosophy of science in general[2]. A common argument against this activity from scientists is that philosophers know too little of the technicalities of the scientific enterprise to say something relevant. On the other hand, when scientists deal with philosophical aspects of their work, they are often — by the philosophers — held to be too ignorant of philosophy. Although many philosophers and many scientists may be ignorant about the practice of the other activity, the separation is probably more a question of different interests. "How can we trust that the scientific method reveals facts about this world?" is not necessarily an important question for the scientist to take with her into the laboratory or into the theory division (although it may serve as a motivation). On the other hand, the philosopher of science need not know all the details of an experiment, or the specific calculations in a theory, to discuss questions such as the one just posed. Nevertheless, philosophical issues can be hard to separate from scientific ones. If science is about producing knowledge it is fair to ask how this knowledge is produced and what it is supposed to be about. In this chapter, where we primarily deal with the epistemology of science, it may be valuable to keep in mind Einstein's characterization of the relationship between these disciplines ([160] p.683):

> The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is – insofar as it is thinkable at all – primitive and muddled.

2.1 A spectrum view on realism

Mostly, the realism debate is framed in epistemological terms centered around the question of how the scientific product relates to the world. In order to get a grip on the various positions which have been defended on the relationship between science and reality, it is useful to present two positions which characterize the endpoints of a conceivable 'realism spectrum'[3]. These positions may be summarized by two theses:

<u>Thesis 1: Scientific theories are true descriptions of entities belonging to reality</u>

More specifically, there is a reality out there which exists independently of human cognition and which science has access to. Eventually the theories of science will be able to explain the phenomena in nature, including the ones studied in specific

[2]This cannot, of course, be a comprehensive introduction to the philosophy of science. For a good general introduction, see Hacking's *Representing and Intervening* [80]. Reflections on science have not been reserved for philosophers of science — historians and sociologists of science have taken their turn also. However, I shall not draw any sharp distinctions between these 'meta'-disciplines in the following.

[3]The spectrum view approach to the question of realism presented here has much in common with that of Redhead in his Tarner Lectures [150] p.11 ff.

scientific settings such as laboratories. This outcome is guaranteed by the scientific method of observing phenomena, formulating theories about these, and testing and revising theories in light of experiments until these agree. By this method scientists discover more and more truths about the world.

Within HEP this is, to some extent, the idea of the business: By probing more and more deeply into the micro world, HEP gradually uncovers the true structure of nature. A good account of this view can be found in some popular writings of physicists, for instance by Steven Weinberg [188] (see also e.g. [88]). Nevertheless, to lump all physicists into the category which is often dubbed 'scientific realism' would be a mistake. As we shall see, some physicists subscribe to other philosophical view points.

Thesis 2: Scientific 'truths' are determined solely by social consensus

The scientific product bears no resemblance to an independent existing reality but, at most, to a 'reality' created by the scientists themselves. It makes no sense to talk about objective truth reflecting how reality is in itself. Moreover, the idea of a fixed rational scientific method is renounced. In a strong form, the thesis implies that science should be regarded on equal footing (epistemologically) with other 'knowledge societies' such as religion, i.e. science is not 'better' or 'more true' than religion.

This view of science has, in recent times, been advanced by some philosophers of science, for instance Feyerabend [59], and some sociologists of science. One version of this, relativist, position is called social constructivism where it is held that facts and hence the knowledge of science are constructed through negotiations, accidental events and interpretations. Below, social constructivism will be discussed in more detail.

2.1.1 In between

As formulated above, either of the two positions on the realism issue is difficult to sustain. As we shall see, scientific realism has great difficulties in facing up to the philosophical counterarguments which have forcefully been put forward. Relativism, especially in its strong form, is confronted with nagging reflexivity arguments. For instance, if the relativist argues that "scientific truths are just an artifact of social consensus" can this then itself be a truth outside social consensus? Or, if no epistemic positions are better than others, then why should the relativists position be better?

I now turn to a few of the alternatives which have been defended in between scientific realism and relativism. First, we may place the position expressed by many members of the 'logical positivism' or 'logical empiricism' movement around the middle of the spectrum. Positivism takes the observed phenomena to be real. But while the theories may help to organize phenomena, they should not be seen as truths about reality, and the theoretical, unobservable, entities do not literally exist. However, this sharp distinction between the observational and theoretical terms, the latter being just calculational devices for connecting observational input with observational output, becomes a problem for positivism as a philosophical position. One reason for the problem is the idea of theory-ladenness (see below) which implies that theory is needed before one can even discuss observed phenomena.

A related position is called *instrumentalism* which, in short, holds that science may or may not extract facets of reality, but what counts is that science is useful. For instance, the aim of physics is to produce reliable predictions for experiments, not to provide true explanations of the phenomena. For physicists, endorsement of instrumentalism may arise either from a genuine reluctance to engage in metaphysics, or it may simply stem from a disinterestedness in philosophical questions such as 'what really is'. Within philosophy of science quite elaborate accounts of this position has been given, the most well-known example being that of van Fraassen [63][4]. For van Fraassen the failings of positivism are corrected by admitting that there is no sharp distinction between observational and theoretical terms. Van Fraassen maintains, however, that only what is in principle observable should be taken as real. Whether instrumentalism therefore escapes the positivistic problem of a sharp distinction between theoretical and observational terms is controversial (see e.g. [125]).

So far it might seem a good idea to draw a 'realism' spectrum, for instance with scientific realism to the far left, then instrumentalism and positivism near the middle, and finally relativism to the far right. One should, however, keep in mind that while the spectrum provides a good starting point, it is not adequate to frame all aspects of the realism debate. The epistemic spectrum view is about to what extent we should believe in the scientific product but not so much about what the world is really like and how it influences scientific beliefs. To the scientific realist the ontological component is quite clear: if we have direct access to an independently existing world then our knowledge reflects the ontology of the world. Thus, the causes of the phenomena is the world as it really is. However, once we start moving towards the relativism side of the spectrum the relation between knowledge and reality becomes more complicated. Few would deny that there *is* a reality independent of our cognition. But the instrumentalists and positivists insist that science cannot yield explanations of the causes behind the observed phenomena reflecting how the world really is. As we shall see, the constructivist version of relativism holds that there are explanations behind the observed phenomena in science, but that these relate to a 'social' reality — not a 'natural' reality.

We now turn more specifically to two of the arguments which have been used to argue against a simple realist attitude towards scientific theories.

2.2 Theory-ladenness of data

Boldly stated, theory-ladenness of data or observations implies that one cannot test theories in an objective manner — the data can be interpreted only in the light of theory[5]. With Kuhn and Feyerabend's ideas of incommensurable paradigms the notion of theory-ladenness became particularly relevant for understanding science. The paradigm provides scientists with a specific way of looking at data and observations.

[4]Van Fraassen calls himself a constructive empiricist, equating positivism with instrumentalism [63] p.10. For more on the differences and similarities between these terms see [80] p.28.

[5]Hacking ([80] p.171) quotes N.R. Hanson [83] as the first who coined the term theory-loaded.

But old paradigms are replaced by new ones in which old data and observations are either reinterpreted or cease to be significant. A central point in paradigm shifts, or major theoretical changes, is that they are not supposed to happen through a unique rational scientific process building on, for instance, firm experimental evidence. Instead, Kuhn compared the shifts to Gestalt switches like those found in psychology, and to religious conversions which need not be guided by any rationality. Moreover, since no paradigm is more true than its predecessor, there is no cumulative build-up of true knowledge of the world.

The evidence for these paradigm shifts is taken from the history of science. Kuhn argued that history shows a number of such shifts, illustrating "the scientific revolution as a displacement of the conceptual network through which scientists view the world" ([104] p.102). To some extent, Kuhn claimed that scientists embracing different paradigms live in different worlds and see different things. Thus, the paradigms are incommensurable: the theory or theories belonging to different paradigms cannot be compared in an objective manner, nor be translated into each other[6].

Scientific terms, be they theoretical or observational, can only be understood in the context of a paradigm or theory. Thus, the positivist, or instrumentalist, distinction between theoretical and observational terms is hard to maintain. The scientific realist might be tempted to use this as a basis for holding that all terms must be interpreted realistically. But it does not solve the problem if successive paradigms employ different theoretical entities and hence different ontologies. Thus, theory-ladenness provides immediate difficulty for a traditional view of science aiming to establish true theories by advancing hypotheses or theories, and testing them independently in experimental set-ups.

The influence of theory on experiment has been discussed extensively in the literature of history and philosophy of science. As I indicated above, the modern debate started out with Kuhn, Feyerabend, and Hanson. They focused primarily on aspects of the relationship between theory and experiment which may be called *philosophy of theoretical science*, that is, arguments derived from reasoning about scientific theoretical practices rather than their experimental counterparts. In recent years, however, a new approach to the history of science has emerged which may be called *philosophy of experiments*. Here the focus has been on the evolution of experimental practices instead of the evolution of theories and paradigms. The argument goes that Kuhn, Feyerabend, and their critics, underestimated the role in science of experiments as compared to theories (see e.g. [65] p.1,4 and [80] p.149,155). This underestimation, the argument continues, is important since experiments have a life of their own: When theory changes, experimental techniques do not necessarily change with them. Conversely, when standards of experimental demonstration change, it can be without theoretical reorientations (see e.g. [71]). This insight speaks against the view that experimental results can only be seen in

[6]Kuhn notes that the shift from Newtonian mechanics to special relativity has this character: Though one can derive something which looks like Newton's laws from special relativity, it is altogether different from Newton's laws since e.g. mass has changed its meaning from being conserved to be convertible with energy ([104] p.102). The extent to which examples like this support incommensurability between different paradigms is controversial, see e.g. [65] p.110. We shall later discuss the meaning of objectivity in more detail.

a certain theoretical light: the results may remain unaltered from one theory to another.

Hacking discusses two versions of the interplay between theory and experiment ([80] p.153): "The *weak* version says only that you must have some ideas about nature and your apparatus before you conduct an experiment". As Hacking contends, everybody would probably agree on this version. By contrast, the strong version "...says that your experiment is significant only if you are testing a theory about the phenomena under scrutiny" which Hacking argues that, for instance, Popper maintained[7]. By pointing to various examples from the history of science, Hacking argues that this strong version is simply not true. In the history of science one may find examples of many kinds of interplay between theory and experiment: Sometimes theory precedes experiments, sometimes not (see [80] p.157 for examples)[8]. The turn to practice in the history and philosophy of science shows, at least, just how difficult it is to make generalizations valid for all of science, for instance about theory-ladenness.

Nevertheless, it seems clear that theory-ladenness of experiments can work on a number of different levels. Theory may shape the experimental questions which scientists ask in the first place and theory may shape the way data are analysed. But one could also substitute 'cultural context' for theory thus arriving at 'the cultural context-ladenness of experiments'. And once this is done, one could substitute science for experiments, arriving at 'the cultural context-ladenness of science'. Perhaps Kuhn was already hinting at this move with his notion of paradigms. In any case, this move has been made in the post-Kuhnian era within the sociology of science. Since one strand of sociology of science seems to uphold a strong relativism with respect to scientific facts — for instance those of HEP — we shall explore the move in a little more detail.

2.3 The social constructivist turn

On a general level, social constructivism is a version of sociology of science which attempts to break down the distinction between the internal (or intellectual) and external (or social) level of science. The social constructivist holds that the social level influences or determines the intellectual achievements of science. Consequently, within social constructivism, it is more appropriate to talk about a context-ladenness of scientific facts rather than merely theory-ladenness[9].

The central point in constructivism is that scientific facts are not given directly from experiments or observations but constructed, for instance, through negotiations

[7] Popper, of course, rejected Kuhn's claim that there was no sharp distinction between theory and experiment (or observations), see e.g. [80] p.5.

[8] When we turn our attention to HEP in the following chapter, we shall see that experiments within this discipline are often strongly influenced by theory but also that it is sometimes possible to separate out the theory under test from the theory of the apparatus.

[9] This section is based on my article "Sociology of Science – Should Scientist Care?" [206] (reprinted in appendix A). The article is about the work on HEP of Karin Knorr-Cetina, a sociologist of science who has had a significant impact on constructivist ideas, see e.g. [22], [174] and [137]. The relation between constructivism and HEP will be dealt with in the following chapter.

among scientists and interpretations of the data at hand[10]. Moreover, scientific reality itself is constructed by selective and contextual scientific laboratory practices. When one focuses on how the social and political environment contribute to the construction of facts, the programme may be labelled 'social constructivism' (the term 'constructivism' is usually linked to microsociological studies only, for instance studies of laboratory practices). Acknowledgement of this position is often followed by some remarks about relativism and realism which can illuminate how 'strong' the consequences of the constructivist program are, e.g.:

> We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist. Our point is that "out-there-ness" is the *consequence* of scientific work rather than its *cause*. ([113] p.180)

It is somewhat unclear exactly what is stated here — it appears not to be a simple relativist position and the last sentence seems to be of anti-realist origin even though reality is not denied in the quote. In any case, it is obvious that the position expressed in the quote stands in sharp contrast to any scientific realist's intuition where an independent reality or out-there-ness is the ultimate cause for scientific findings and facts. In connection with the quote, an obvious question is: if 'out-there-ness' is not the cause of scientific facts — then what is? We shall see an answer to this question in a moment. But there might be a methodological version of relativism which avoids taking a definite stand on traditional philosophical questions. For instance, the sociologist may simply have a different interest from that of the scientist, namely to describe *why* the scientific community at a specific historical time trusted in a certain result, instead of *whether or not* the result was true. This form of relativism seems acceptable also from a realist's view of science since no realist would disagree that some results have turned out to be correct despite the fact that they were originally considered wrong or vice versa. Methodological relativism is incorporated in the constructivist's frame of reference:

> The constructivist program has extended this idea by claiming that the information produced by science is first and foremost *the product of scientific work*, and what we should do is try to describe how scientific work produces scientific information, rather than locating the focus of the analysis between the finished scientific product and the world." (Knorr Cetina [22])

But even this 'mild' form of relativism is not without assumptions. To shift the problem area of constructivism away from the knowledge-reality relation — and hence remain agnostic towards the scientific realist's epistemology and ontology — implies itself epistemology and ontology: Namely that there is a social world and that sociologists have access to this world. This situation has, for instance, been discussed and defended with recourse to Wittgenstein who has sometimes been taken to be

[10] A more detailed account of constructivism can be found in Sismondo's "Some Social Constructions" [174]. Constructivism has been used in various contexts and with different meanings in the literature but Sismondo attempts to point out these differences.

on the social constructivist side in the realist-relativist debate. Listen for instance to Collins and Yearley in Pickering's book *Science as Practice and Culture* [38]:

> In the absence of decisive epistemological arguments, how do we choose our epistemological stance? The answer is not to ask for the meaning but for the use. [footnote:] We mean this in the Wittgensteinian sense. We mean that the endlessly agonizing search for essential meanings is senseless, since the meaning of something equals its use in a form of life. Meaning and use are but two sides of the same coin. [...end of footnote]. Natural scientists, working at the bench, should be naive realists – that is what will get the work done. Sociologists, historians, scientists away from the bench, and the rest of the general public should be social realists. Social realists must experience the social world in a naive way, as the day-to-day foundation of reality (as natural scientists naively experience the natural world).

Collins and Yearley have another article in Pickering's volume [39] where they specify the methodological consequences of relativism which are formulated somewhat more strongly than Knorr Cetina did above:

> The methodological prescription that emerges from relativism is that explanations should be developed within the assumption that the real world does not affect what the scientist believes about it, however much the opposite view is an inevitable part of doing science. This means that when the scientist says "scallops" we see only scientists saying scallops. We never see scallops scalloping, nor do we see scallops controlling what scientists say about them.

Thus, in the social world of the sociologists, one finds scientists to study. In the natural world of the scientists, one finds scallops to study[11]. Obviously, scallops do not control what scientists say about them, but it seems odd for the sociologists to hold that the real world, containing something which marine biologists or fishermen have decided to call "scallops", does not affect what scientists believe. Perhaps, as Wittgenstein would have it, the meaning of "scallops" can be understood only by examining how the word is used. But Wittgenstein's philosophy of meaning does not seem to imply that use cannot be constrained by a non-social reality. It is therefore not at all obvious that Collins and Yearley's methodological relativism can be grounded in Wittgenstein's dictum.

Note that although Collins and Yearley in the first quote speak about epistemology, their 'naive social realist' view implies a reality of causes in just the same way as it does for naive scientific realists: The causes for their beliefs are found in the real world which they take to be merely social[12]. Collins and Yearley's distinction between a social world experienced by the sociologist as a 'day-by-day' foundation

[11] Though it is tempting to ask if the scallops could not also be part of the social world — for instance at a dinner table.

[12] The quotes of Collins and Yearley are taken from articles which are part of an exchange between two schools within sociology of science (or science studies): Sociology of Scientific Knowledge

of reality and the naive realism of the scientists may be linked to a general attempt at separating scientific knowledge from common sense facts about daily life. A similar attempt is indicated in a quote by Knorr Cetina where she comments on the pre-existence of subatomic particles i.e. in what sense they exist before they are 'discovered' in experiments [100]:

> Preexistence itself is a historically variable phenomenon; what objects are thought to have preexisted changes with these cultural practices and with scientific belief. Thus specific scientific entities like subatomic particles begin to 'preexist' precisely when science has made up its mind about them and succeeds in bringing them forth in the laboratory.

If this is the case, then scientific realism receives a serious blow since, obviously, if the particles did not exist before experiments are conducted then the assertion of fact construction is unavoidable. But how do we determine which entities are 'specific scientific' — how do we identify the borderline between scientific and social entities? If tables and chairs exist as material objects then how about things which are seen only through a microscope? Or inferred by means of a particle detector? In any case, scallops are probably easier to swallow as real for the social realist than sub-atomic particles[13]. But the assertion that knowledge of specific scientific entities is shaped by the social environment does not imply that they are determined by this environment. Indeed, constructivist authors can be less restrictive in granting a role for a non-social reality. Thus, while Knorr Cetina in [101] states that *"constructionism holds reality not to be given but constructed..."*, she also acknowledges some sort of resistance from a non-social reality [101][14]:

> Constructionist studies have recognized that the material world offers resistances; that facts are not made by pronouncing them to facts but by being intricately constructed against the resistances of the natural (and social) order.

But, if it is the case that the material world somehow constrains the facts which science produces then the thesis of context- or theory-ladenness of observations, experiments, and science, becomes less devastating for the scientific realist.

2.4 Underdetermination of theories by observations

Another problem for the scientific realist is that of underdetermination, which is sometimes formulated in terms of experiments (underdetermination of theories by

represented by Collins and Yearley and Actor Network Theory represented by Callon and Latour. In response to Collins and Yearley, Callon and Latour argue for a dissolution of the one-dimensional dichotomy between natural and social ontology — by introducing a dimension of stability, see [24]. To discuss this move here would take us too far astray but as Collins and Yearley suggest [39], the introduction of new dimensions to secure epistemic positions does not seem very helpful as there are no clear reasons to stop there — i.e. why not introduce yet another dimension and so forth.

[13] In the following chapter we shall discuss the strategies in physics for obtaining information about the sub-atomic world in more detail.

[14] Some constructivists have used the word 'constructionism' instead of constructivism, see e.g. [100]

experiments) rather than just observations. But in both cases it boils down to what has become known as the *Duhem-Quine thesis*. Duhem contended that one cannot test hypotheses in isolation (chapter VI of [50]): What is under investigation in an experiment designed to test a theoretical prediction is always a *set* of hypotheses or theories. Thus, if a particular experimental result is in conflict with a theoretical prediction, we can choose to blame the apparatus or other features of the theory rather than the theoretical result or hypothesis we try to test. As an example, assume that we have a theoretical prediction for a physical quantity x. We then go and measure x in some experiment and find that the experimental result is in disagreement with the theory, i.e. we get y where $y \neq x$. Is the theory then proved false? Surely not, because we might have made some mistakes in setting up or conducting the experiment. Or the theory may allow for an adjustment which accounts for the discrepancy. Quine pushed the point when he remarked "Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system" and, conversely, "no statement is immune to revision" ([147] p.43)[15].

To see the implication of Quine's remark for scientific theories, we can let the theory and the experimental result play the role of the 'system', whereas the 'statement' is the specific theoretical prediction. In our example from above, a changed 'system' could then, say, imply that the experimental result was actually a determination not of x itself but rather x plus something else which accounts for the difference[16]. If this 'something else' is not predicted by the theory in question then the observed value of y can, of course, not refute the theory. Such adjustments to a theory can be accomplished by auxiliary or 'ad hoc' hypotheses. The idea of letting experiments carry the burden of refuting theories is a cornerstone in Karl Popper's falsificationism. In his scheme, such ad hoc hypotheses should be avoided — if they do not add to the predictive power of the theory, but only serve to save it. The problem is, however, that it is not always easy to see whether auxiliary hypotheses yield new predictions. Moreover, when theories are in conflict with experimental results, it is certainly a more economic mode of reasoning to try auxiliary adjustments before throwing the whole theory into the trash can[17].

The Duhem-Quine thesis has also provided fuel to the social constructivists. For instance, Pickering writes in the introduction to his *Constructing Quarks* ([143]):

> It is always possible to invent an unlimited set of theories, each one capable of explaining a given set of facts. Of course, many of these theories may seem implausible, but to speak of plausibility is to point to a

[15] A collection of essays dealing with the Duhem-Quine thesis can be found in [82].

[16] This is sometimes referred to as systematic corrections, see section 4.3.1 and appendix E.

[17] For a short response from Popper to the Duhem-Quine thesis, see [82] p.114. Here Popper grants that it can be hard to locate what part of a theory went wrong, but that this is not a problem since falsificationism does not require that theories can be verified. However, even Popper's idea that theories can be well-corroborated if they pass a great number of experimental tests is threatened since the Duhem-Quine argument entails that one cannot know whether a specific theory has been falsified or not. It therefore seems more relevant when Popper points out that there are "quite a few" cases where one can actually separate out the hypothesis which is responsible for the refutation of a particular theory.

role for scientific *judgment*: the relative plausibility of competing theories cannot be seen as residing in data which are equally well described by all of them.

Thus, even when an experimental result coincides with a theoretical prediction it does not necessarily lend support to the theory since there might be other theories capable of explaining the same result. Hence, for the scientific realist who struggles towards true theories, the thesis of underdetermination works on two levels: First, an experimental result cannot support or falsify any given theory since the match or mismatch between theoretical prediction and experimental result might be 'explained away'. Second, a particular experimental result may be explainable by more than one theory, hence making choices between different theories difficult. Thus, if there are more theories predicting the same experimental result, it is not obvious which one the realist towards theories should choose as true. This dilemma, however, assumes that experimental results are the only ingredient in choices between theories. We shall later return to other criteria for choosing among theories which makes the realist's worry about underdetermination of theories by data more bearable.

2.5 Other positions on the realism issue

I will now briefly review two other realist positions which have both, to some extent, been formulated in response to the problems for the scientific realist reviewed above:

Entity realism has been advocated by Hacking [80] and Cartwright [31], though the starting points have been slightly different (see [80] p. vii). This position holds that entities are real insofar as they can be manipulated to produce observable effects. In this sense there is no gap between theoretical terms and observational terms. The theories themselves are not taken to be true descriptions of an independent reality, but nevertheless theoretical, unobservable, entities can be real. For instance, although the theories of electrons have changed over the years, there are still electrons even if no theory captures all the things which could be said about them. In this sense, entity realism attempts to circumvent theory-ladenness as an obstacle for obtaining objective knowledge about reality. For Hacking, the starting point is to accept that electrons must be real if one can manipulate them, that is, if they can be used to study other things through causes and effects that we understand. A similar point obtains for Cartwright since she holds that we should believe in e.g. electrons if our experimental evidence points to the effects which the entities are supposed to cause ([31] p.8). The point of entity realism is thus that one should not focus too much on the explanatory aspects which theories or fundamental laws may supply[18]. But because of the causal implications of (some) theoretical entities,

[18] In fact, Cartwright puts the point more strongly since she argues in favor of 'phenomenological' laws, but against high-level theory and laws of nature. According to Cartwright, phenomenological laws are about things that can in principle be observed directly, whereas theoretical laws can be known only by indirect inference ([31] p.1). For instance, Newton's theoretical law of gravitation is false in the sense that it does not describe the real motion of bodies except when, for instance, electrical forces can be neglected [31] p.58. It is not clear, however, whether this is an argument for more than the fact that one, in order to use physical laws, must specify their field of application

they can nevertheless be real. To some extent entity realism is an outcome of the, already mentioned, recent emphasis within the philosophy and history of science on the role of experiments and practice in science.

But theories have not been forgotten in this era of experiments and practice. There have been other attempts to meet the Kuhnian challenge of the changing ontology between different paradigms. One of these is Cao's *structural* realism which holds a view of theories grounded in conceptual continuity rather than radical changes between succeeding paradigms or theories. The point is that certain structures of succeeding theories are real and persistent. These structures can, for instance, be symmetry relations expressed through mathematical laws e.g. the so-called Lorentz symmetry expressing a central feature of the special theory of relativity, and they survive when theories change[19]. This structural realism is based on studies of field theories in the 20th century (which includes HEP, see next chapter). Nevertheless, structural realism aims to cast doubt on the incommensurability thesis in general, so it appears to have a wider scope as a realist framework for understanding scientific development ([28] p.367). In this view, structures have a more solid ontological status than entities (for instance electrons), though the reality of the latter may be gradually constituted by succeeding theories. In this sense, structural realism stands in contrast to entity realism, even though both operate with an ontological continuity across changes in theory (as opposed to Kuhn).

We thus see that an essential problem with the simple question "where do you stand on the realism/relativist issue?" is its generality. Should one be realist or anti-realist/relativist about esoteric features of quantum mechanics in the same way as tables and chairs? Biological entities, such as DNA, in the same way as quarks? Theories in the same way as entities? We have already seen social constructivists argue for different attitudes on the realism question depending on whether is was towards science or daily life. Moreover, various studies have pointed out that different fields of science employ different methods and different styles of reasoning (see e.g. [72]). And even within a realist framework of a specific scientific discipline, the ontological status of various theoretical terms and theories might not be the same[20].

2.5.1 A break for the scientist

Given the complicated state of affairs within philosophy of science, which we have only briefly reviewed, it would not be surprising if a contemporary scientist is reluctant to take a definite philosophical stand. If so, he may find support in a remark of Einstein's on philosophy and science, where Einstein argues in favor of the opportunist scientist. This scientist is a philosophical chameleon ([160] p.683):

> ...he appears as *realist* insofar as he seeks to describe a world independent of the acts of perception; as *idealist* insofar as he looks upon the concepts and theories as the free inventions of the human spirits; as *positivist* insofar as he considers his concepts and theories justified *only* to the extent

and appropriate boundary conditions.

[19] For more examples of ontological structures, see [28] p.361

[20] Thus, Hacking's 'realism by spraying' appears anti-realist towards quarks which cannot be manipulated to investigate properties of other particles.

> to which they furnish a logical representation of relations among sensory experiences. He may even appear as *Platonist* or *Pythagorean* insofar as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research.

An example of a contemporary scientist who seems to follow Einstein, at least in a philosophical sense, is Hawking, who defines his position as follows ([85] p.44):

> I would say that I am a realist in the sense that I think there is a universe out there waiting to be investigated and understood. ...But we cannot distinguish what is real about the universe without a theory. I therefore take the view, which has been described as simpleminded or naive, that a theory of physics is just a mathematical model that we use to describe the results of observations. A theory is a good theory if it is an elegant model, if it describes a wide class of observations, and if it predicts the results of new observations[21].

In this quote one finds points of intersection with both Kuhn and van Fraassen. But the first phrase about understanding has, as Hawking remarks, a definite realist flavour. Is understanding tied to theory? And if so, can our theoretical framework change over time thus making understanding temporary? In that case, Hawking's — and other physicists' — quest for a complete or final theory appear paradoxical[22]. Hawking, or Einstein for that matter, may not care whether their philosophical stands are coherent. But there is no clear argument why consistency should be sought only within scientific theories and not in epistemology[23]. In order to look for such epistemic 'common ground' for philosophy and history of science scholars, we now turn back to Kuhn.

2.6 A historicist view on science

Many recent philosophical and historical accounts of science, e.g. [28, 70, 143], have sought either to elaborate or discredit Kuhn's idea from *The Structure of Scientific Revolutions* that the evolution of science produces ever new theories about the world based on different ontologies. But there seems to be one lesson of Kuhn's that most of them share: theories, and criteria for choosing among them, are not independent of the particular historical and cultural context.

Upon reflecting on some of his critics, Kuhn remarked that there is a set of shared criteria for theory choice that works on a trans-paradigmatic or trans-theoretical basis. These criteria include accuracy, consistency, scope, simplicity and fruitfulness

[21] Hawking remarks in the same essay that "In theoretical physics, the search for logical self-consistency has always been more important in making advances than experimental results".

[22] See e.g. the final remarks in Hawking's *A Brief History of Time* where he speculates about knowing the mind of God [84], or [188], and [88] for speculations and philosophical reflection on a 'final' theory.

[23] Note that different epistemological stands towards different branches of science, e.g. microbiology and HEP, does not seem to be the target of either Einstein's or Hawking's remarks.

([105] p.322)[24]. Nevertheless, Kuhn added, all of these criteria are individually too vague to determine which theory should be chosen in a particular situation. As an example, Kuhn mentions that the oxygen theory of combustion could explain different things than the phlogiston theory, and hence that fit to experience, or accuracy, in itself was inadequate to choose between them ([105] p.323). Moreover, when the criteria are used together they may be in individual conflict. For instance, one theory may appear more simple than its rival but less consistent with other areas of established knowledge. Kuhn furthermore points out that even when scientists are committed to the same criteria for theory choice they may not pick the same, since the interpretation and relative weight of each of the criteria may vary from scientist to scientist and from one context to another. For this reason, Kuhn speaks about the criteria as values rather than rules. However, whether the criteria for theory choice are called rules or values their status is unclear. For instance, Kuhn notes that there are other 'creative disciplines', for instance philosophy, which have other sets of values but he does not discuss whether those specific for science lead to a particular trustworthy kind of knowledge. Moreover, though Kuhn takes the criteria to be permanent attributes of science, if their specification is left vague, their application and relative weight change with time and context ([105] p.335).

Many recent authors appear to agree with Kuhn on these points. One example is found in the following quote of Cao and Schweber ([27] p.79):

> The actual appraisal of a scientic theory is a complicated and intricate business that takes place on several levels simultaneously. Weakness on one level may be compensated for or neutralized by strengths at another level, and no single criterion can be taken as the universally valid determinant in the appraisal of scientific theories. In the appraisal of a particular theory by a scientific community, the determination of which criterion should act as the determinant and which criteria should recede into the background is more than a matter of logic and depends on the concrete situation, the historical background, and the cultural context.

Return now to Kuhn's essay referred to above [105]. Kuhn labels the set of shared criteria 'objective' as opposed to the individual reasons a scientist may have to apply them in a particular situation. This distinction is not taken to imply that a scientist's reasons for attributing particular weight on e.g. simplicity is merely a matter of taste. Rather, Kuhn suggests, the objective values (or shared criteria) can be put to work only when they have been specified further, and this specification depends on individual judgment[25]. The ambiguity of the objective factors associated with theory choice indicates that there is no clear separation between objective and subjective criteria. If the latter are contingent and needed to specify the implications

[24] Consistency is taken to be both at the internal level and in relation to other relevant theories. Scope means that the theory should say more than what was put into it, for instance, as regards to new empirical predictions.

[25] Insofar as a social constructivist would follow Kuhn on these points, he might add that this individual judgment will be determined by the social community. In the section on underdetermination above we saw Pickering argue for the role of judgment in theory choice.

of the former then choices among theories are contingent, that is, open to historical revision. At the end of the essay, Kuhn suggests that his analysis of these matters may point to the meaning of, or some limitations for, the concept of objectivity. Apparently, Kuhn imagines that objective criteria for theory choice function like a shell which acquires a definite content only through subjective factors. If that is the case then we seem left with a historicist position: We are always in a historical situation and things may change later. Consequently, we cannot rule out that what is established as a true theory about the world today may be seen as false tomorrow.

In the following section I will discuss a kind of response, inspired by Niels Bohr, to this Kuhnian historicism. More specifically, I will investigate the suggestion, by Peter Zinkernagel, that objectivity is partly determined by classical mechanics when this theory is viewed as a refinement of daily language. According to the historicist view, a scientific theory is deprived of a permanent objective status. But, if a scientific theory is needed in order to understand objectivity, then there are limitations to what questions can be posed as concerns the theory's objective status.

2.7 Objectivity

The need for a robust meaning of objectivity is common to all realist attitudes towards science: Science aims to establish knowledge which is, as far as possible, independent of subjective factors, hence objective. If the distinction between what is subjective and what is objective becomes too blurred, the realist ambition appears out of reach.

We have just seen Kuhn suggesting that his discussion of criteria for theory choice may point to some limitations for the meaning of objectivity. In fact, the historicist perspective has since been applied to the objectivity concept itself. As a particular aspect of the concept of objectivity, Daston and Galison have focused on the distinction between objectivity and subjectivity in connection with visual representations of scientific results [44][26]. Through examples, Daston and Galison have demonstrated that the concept of objectivity employed in connection with visual representations has changed significantly during the last two centuries from a 'truth-to-nature' approach (allowing for artistic intervention in the representations), via 'mechanical objectivity' (having the machine as the ideal and excluding fallibility of the senses), to 'judgment-to-the-audience' (acknowledging a need for audience-judgment of the representations) [44].

An 'image of objectivity' connected to representations of scientific results (such as atlases, the key example for Daston and Galison) need not be connected with a more general notion of objectivity, e.g. in association with questions like "are the social sciences objective?". One can, however, speculate whether a contingency in representational objectivity implies some sort of contingency for the concept as a whole[27]. Indeed, Daston and Galison acknowledge the possibility of a generalization of representational objectivity: "...we address the history of only one component of

[26]This section is based on my article [207] (reprinted in appendix B).

[27]The assumption seems plausible since the concept of objectivity has indeed changed historically. The common sense understanding of objective as being independent of individual factors dates back only to Kant.

objectivity, but we believe that this component reveals a common pattern, namely the negative character of all forms of objectivity" ([44] p.82). The 'negative character' of a form of objectivity is explained as the struggle against some sort of subjectivity, for instance, personal idiosyncrasies, mistrust of the senses or biases from theory.

Despite the suggested contingency of at least one form of objectivity, the concept as a whole has, as Daston and Galison describe, been identified with 'a view from nowhere' in the twentieth century (Thomas Nagel [136]). Daston and Galison warn against such a one dimensional aperspectival view, removed as far as possible from subjective perspectives, since objectivity has been used in many different modes and therefore there can be no single definition of the concept. Nevertheless, Daston and Galison's emphasis on the general 'negative' character of all forms or modes of objectivity seems to coincide with Nagel's aperspectival view[28]. If Daston and Galison are right in their characterization of objectivity as a historically contingent concept, a natural question is whether or not it is at all possible to have a 'transcendental' notion of objectivity (e.g. a notion which is not contingent upon shifting conceptions of 'objective science'). Presumably, Nagel would not reject a historical development of the concept of objectivity. Despite this, his characterization of objectivity as the view from nowhere, and as a method to reach that view (by gradually detaching the subjective in an enquiry) seems to transcend any historical issues. If the notion of objectivity, however, is truly contingent, any discussion of a 'right' definition or content of the term will likewise be contingent.

It is clear that Daston and Galison — like anyone who does science — have made judgments on what evidence to present, what notions to use, how exactly to formulate the argument etc. Nevertheless, they argue for a particular view, and the examples presented are meant as evidence in favor of such a view. But if there is no aperspectival notion of objectivity (in a general sense), to what extent then can Daston and Galison's argument be anything but their own subjective statement? A response from the authors could be that anybody is free to check their references and thus arrive at the same conclusion, but this merely strengthens the point that the argument has some sort of objective validity. Is the argument valid also tomorrow? next year? next century? My guess would be, that Daston and Galison would retreat from their position (that the notion of, at least a particular mode of, objectivity is historically contingent), or at least be willing to question its general validity, only by the appearance of new studies which illustrated that other influential texts or atlases from the different periods discussed did in fact employ similar notions of objectivity. In this sense, their own argument concerning objectivity is historically contingent. Besides, I assume that the authors consider their argument valid in an aperspectival sense. If that is the case, it is reasonable to question whether Daston and Galison are coherent when they write ([44] p.82):

> As historians of objectivity, we will not be concerned with recent controversies over whether objectivity exists and, if so, which disciplines have

[28]Moreover, Nagel does distinguish between various forms of objectivity. For instance, in an argument against reducing all phenomena to the physical realm ([136] p.27), he refers to both a 'physical' and a 'mental' objectivity.

> it. We believe, however, that a history of scientific objectivity may clarify these debates by revealing both the diversity and contingency of the components that make up the current concept. Without knowing what we mean and why we mean it in asking such questions as "Is scientific knowledge objective?" it is hard to imagine what a sensible answer would look like.

An agnostic position towards the existence of objectivity in a general or transcendental sense seems to lend itself to the question of validity of scientific arguments. Leaving aside the fact that Daston and Galison are primarily concerned with representational objectivity, the implied possibility of non-existence of objectivity is a serious matter. It could be that an agnostic position towards objectivity is taken merely as a challenge to the adequacy of the one dimensional objectivity/subjectivity dichotomy (which would seem justified from the study of representational objectivity, since different kinds of subjective factors have been opposed to the objective at different times, [44] p.82). In any case, an argument aiming to clarify debates on objectivity must presumably rest on some more or less transparent conditions which makes it reasonable for others than the authors. That is, such an argument should be formulated as objectively as possible.

2.7.1 Conditions for description and objectivity

In the article [207], I discuss how the philosophy of Peter Zinkernagel implies that conditions for description include both formal and informal logic. The latter are rules, or conditions, for description that must be observed to make e.g. scientific and philosophical arguments meaningful. Peter Zinkernagel bases these conditions for description on analyses of ordinary or daily language. His point is that since all descriptive language, e.g. descriptions of physical experiences, is ultimately to be related to ordinary language, the rules of language are preconditions for any meaningful description in science. The significant role of ordinary language was also strongly emphasized by Bohr. In summarizing what may be called Bohr's conception of conditions for description, John Honner [90] p.14 writes:

> It is a (necessary) condition for the possibility of unambiguous communication that (suitably refined) everyday concepts be used, no matter how far the processes concerned transcend the range of ordinary experience.

Suitable refinements of everyday language mean that the concepts should be used in accordance with classical, i.e. non-quantum, physics. The argument for the necessity of ordinary language and the concepts of classical physics is, in Bohr's words, "...simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics." [11] p.39. Elsewhere, Bohr expresses the necessity of an ordinary language description of experiments even stronger by denoting it "a clear logical demand" [11] p.72. When Peter Zinkernagel states conditions for description, it is thus an attempt to make explicit what is to be understood by 'unambiguous language'. Moreover, the formulation of conditions for description is an attempt to give a precise formulation of Bohr's logical demand ([202] p.120).

The question is now to which extent conditions for description have any bearings on the notion of objectivity. To answer this question it is illustrative first to review Bohr's conception of objectivity. A clear indication of his view is found in [12] p.10, where Bohr talks about physics: "In this respect our task must be to account for such experience in a manner independent of individual subjective judgement and therefore objective in the sense that it can be communicated in the common human language."[29]. Bohr's account of objectivity fits into the 'negative' character described by Daston and Galison or Nagel's 'view from nowhere': objective knowledge is knowledge which is independent of subjective judgment. According to Peter Zinkernagel, by objective knowledge we "must understand knowledge which cannot be meaningfully denied..." ([202] p.1). Examples of objective knowledge are rules of language, including formal logic, insofar as they cannot be meaningfully denied if we are to use language in an unambiguous manner. This notion of objectivity is not in contradiction to Bohr or Nagel's concept aiming at an, as far as possible, elimination of subjective elements in a description. Rather it is a specification of Bohr's point of what is to be understood by unambiguous communication.

2.7.2 Classical physics and objectivity

Let us now turn to the question of what classical physics has to do with the contingency of objectivity. Peter Zinkernagel has suggested that classical physics, as a refinement of ordinary language, forms part of the conditions for description[30]. The suggestion is related to the Kantian response to Hume (one of the fathers of the positivist tradition) with respect to the causal laws: all effects have a cause. For Hume, the general validity of such a statement was just a habit of our experiences since it could not be derived from any formal logical considerations (see e.g. [94] p.44). Hume thus contended that we cannot know whether the causal laws will be valid also in the future: although we have seen billiard balls move according to causal laws many times in the past, we cannot be sure that they will do so also in the future. Kant, on the other hand, argued that the concepts of space and time, and, cause and effect have an *a priori* status[31]. Thus, if empirical knowledge about objects (billiard balls, for example) can be understood only if the objects are situated in space-time and subjected to causal laws, it does not make sense to question whether causal laws will be valid also for future experiences with objects.

[29]As Favrholdt shows in [58] p.69, Bohr's emphasis on the communication aspect of objective knowledge, does not imply that Bohr contrast personal conditions for experiences to those of a community: the criterions for making sensible descriptions on the personal level are the same as what may be communicated.

[30]The relation between physics in general and conditions for description has been elaborated in [204]. In that book, Peter Zinkernagel argues that laws of nature are necessary relations between different quantities, and discuss such laws within the theory of relativity and quantum mechanics: Within their field of application the laws of nature represent conditions for meaningful description.

[31]Space and time are forms of sensibility (i.e. preconditions for sensations of objects) whereas cause and effect are categories forming the necessary basis from which objects must be viewed to become objects of empirical knowledge. Both space-time and cause-effect, however, relate to objects in an *a priori* manner cf. [94] p.121

Such a conclusion can be strengthened by stating the laws of classical physics as necessary conditions for description or meaningful communication[32]. If classical physics states necessary conditions for description, it has important bearings on our knowledge of the future, that is, what we can meaningfully say about it. Assuming that the laws of classical physics are necessary conditions for meaningful communication the statement "we cannot be certain the sun will rise tomorrow" (or that we cannot be certain that the causal laws of classical physics are valid also tomorrow) becomes meaningless[33]. Perhaps the easiest way to see this is to note that classical physics provides us with the necessary conditions for understanding, for example, the concept 'tomorrow': we can only refer to 'tomorrow' in an unambiguous manner by referring to time in the sense of ordinary language. Since classical physics is a refinement of this language, a meaningful use of the word 'time' implies reference to some sort of mechanical system (a clock).

But has not the concept of time changed historically? Must we not say that the special theory of relativity from 1905 made time a concept relative to the frame of reference? And that the general theory of relativity from 1915 associated the reference frame not only to velocity but also to gravity? Surely, Newton's idea of absolute time was deemed wrong with the advent and acceptance of the theories of relativity. But if such historical hindsight is to be meaningful, it requires that we can refer to time, such as '1915', by the aid of a clock — for instance by counting the number of times the earth has revolved around the sun. This resembles Bohr's argument, given above, that it is necessary for our experiences to be formulated ultimately in classical terms[34]. To put the argument more paradoxically, it is not at all clear what it would mean to change the concept of time with time. If classical physics and time is connected in this way then there is a limit to how contingent the notion of objectivity can be: it would be meaningless to claim that the concept of objectivity might be understood altogether differently in, say, 50 years as we would not be able to state this claim without assuming that objectivity, at least partly, is the same thing as now.

In concluding this chapter, it should be noted that the concept of objectivity discussed in this section does not imply that criteria for theory choice are fixed once and for all. But it does suggest a limit to what can meaningfully be understood by historicity and thus how radical future changes in our knowledge about the world can be. In particular, the discussion suggests that it makes little sense to doubt the objective status of classical mechanics — despite its limited field of application as compared to the special theory of relativity and quantum mechanics. Apart from the brief remarks in this section, I have hardly discussed any philosophy of

[32]This is suggested in [202] p.223 and developed further in [204]. Classical physics is here taken to be the laws of Newtonian mechanics, that is, the laws of motion governing material objects. The discussion given here is a short version of that in [207]. There it is pointed out that the refinement of ordinary language, which classical physics provides, connects to our possibilities for speaking about material objects.

[33]Disregarding possibilities such as explosions in the core of the sun, which have, of course, nothing to do with the laws of classical physics within their field of application.

[34]That humans have become better to measure time with the advent of e.g. atomic clocks does not change the argument as also the usefulness of atomic clocks rests on the interaction between classical apparatus and radioactive decay of atoms.

language[35]. It should be pointed out, however, that discussions of realism, reality, etc. must take place in language. Consequently, if there are conditions for our use of language, conditions for meaningful communication, then there are constraints on what we can say about reality. This holds irrespective of the fact, which neither Bohr nor Peter Zinkernagel would deny, that reality exists independently of our language.

[35] For a discussion of the difficulties of fitting Bohr's views into contemporary realism debates in analytical philosophy, see e.g. [123].

3 The Structure of High-Energy Physics

In the last chapter, I discussed some general points about realism in philosophy of science. Before examining how these points relate to HEP, I will review some key elements of this discipline which will be relevant for the following chapters. This chapter serves as an overview of HEP and most of the physics details will be suppressed until specifically needed.

3.1 Theories leading to HEP

The theoretical framework of HEP is build on the basis of other theories within physics. This situation may be schematically represented as in fig. (3.1).

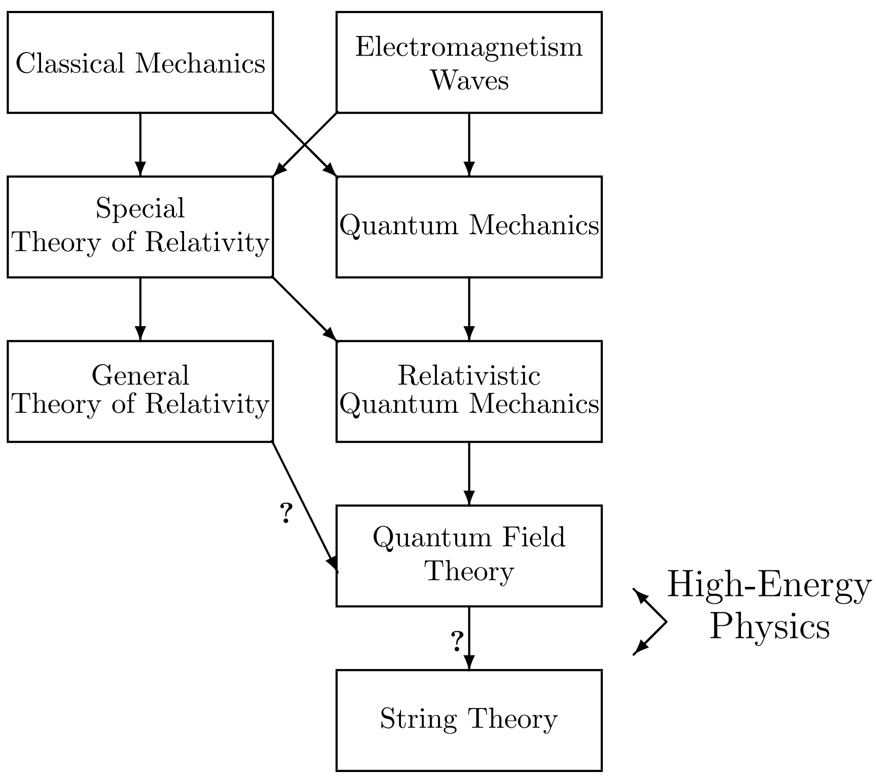

Figure 1: Levels of abstraction leading to High-Energy Physics. The question marks are explained in the text.

Fig. (3.1) makes it clear that a concept like theory-ladenness in HEP experiments, will have to be qualified with respect to which theories are supposed to influence the experiments. This point will be discussed further in the following chapter.

The figure may be used to illustrate two additional points[36]. First, it points to

[36] It should be noted, that Quantum Field Theory in recent years has been elaborated also from the standpoint of so-called critical phenomena in statistical mechanics (see e.g. [29]). This branch

the technical difficulties in understanding the framework of HEP[37]. Consider, for instance, the assumptions in a statement such as "string theory may be a representation of Nature at the most fundamental level". Besides the philosophical problems in this statement concerning 'representations of Nature', it is difficult to grasp the proposition unless one is familiar with the quantum field theories to which string theory should correspond in the energy regimes where experiments are possible[38]. Quantum field theories, in turn, are rooted in relativistic quantum mechanics which is a combination of the special theory of relativity and quantum mechanics. To examine the relation between these disciplines and 'Nature' it is, at least, helpful to be acquainted with classical mechanics which provides the limiting case of low velocities relative to that of light and length scales where Planck's constant is not important.

This description of string theory leads to another point of the figure, since it may be seen as an essential argument for the reductionism within physics. Indeed, the dream of string theory (see e.g. [188]) is in some sense to reverse all the arrows in figure 3.1, so that it becomes possible to derive all known physics from a few fundamental principles. However, in addition to the immense practical/calculational problems in such a reduction, there might be principal reasons for rejecting the physics reductionist picture. For instance, if classical mechanics indeed provides the necessary framework for understanding quantum mechanics as argued by Bohr, it becomes difficult to claim that classical mechanics can be derived as merely a limiting case for quantum mechanics[39]. Although reductionism will not be dealt with in any detail, I will in chapter 5 point to discussions in which the reductionist picture has been challenged also from within the framework of HEP itself.

3.2 The Standard Model

Central in the current structure of HEP stands the Standard Model — a collection of quantum field theories believed to capture essential features of the fundamental constituents of matter and their interactions. The Standard Model consists of the electroweak theory (which is a unified description of the electromagnetic and weak forces) and the theory for the strong interactions known as quantum chromodynam-

of physics is not included in the figure, as it will not be discussed further. But, strictly speaking, one could have included a third branch in the figure starting with thermodynamics (at the level of classical mechanics) leading to statistical mechanics leading, in turn, to QFT.

[37] This may also be indicated by the fact that, despite a growing body of philosophical and historical analyses, QFT has been a relatively rare subject in the philosophy of physics, as compared for instance to discussions on quantum mechanics.

[38] The current state of affairs in HEP does not include a quantum version of the theory of general relativity. Although attempts to formulate a four dimensional quantum field theory of gravity have failed so far, it is believed that gravity may also be described by a quantum field theory. Whence the arrow with the question mark in fig.(3.1) from general relativity to quantum field theory. Moreover, a quantum description of gravity serves as a major motivation for string theory which, however, has not yet shown to be of any experimental significance.

[39] Contemporary philosophical debates often question Bohr's interpretation of quantum mechanics but I here assume its correctness with respect to daily language as outlined in chapter 2 (see also e.g. [3] and discussions in [57]).

ics (QCD)[40]. Both the field theory for the electroweak forces and that of the strong force are modelled upon quantum electrodynamics (QED); the theory describing the interactions between electrons and photons, and the first quantum field theory (QFT) that proved relevant to experimental physics. According to the Standard Model, all matter consists of point-like particles which are either quarks or leptons (called fermions) whereas the forces between these particles are mediated through the exchange of so-called intermediate bosons, for instance the photon. Schematically, the matter particles and the force mediators in the Standard Model can be represented as follows:

Family	Lepton	Symbol (charge)	Quark	Symbol (charge)
1.	Electron	e^- (-1)	Down	d $(-\frac{1}{3})$
	Electron neutrino	ν_e (0)	Up	u $(+\frac{2}{3})$
2.	Muon	μ^- (-1)	Strange	s $(-\frac{1}{3})$
	Muon neutrino	ν_μ (0)	Charm	c $(+\frac{2}{3})$
3.	Tau	τ^- (-1)	Bottom	b $(-\frac{1}{3})$
	Tau neutrino	ν_τ (0)	Top	t $(+\frac{2}{3})$

Force	Boson	Charge
Electromagnetic force	γ (photon)	0
	W^+	+1
Weak force	W^-	-1
	Z^0	0
Strong force	g (gluon)	0

Out of the three families, or 'generations', only the particles of the first family are believed to exist commonly as stable particles in nature (although single quarks are not expected to exist as free particles but assumed to be bound within the so-called hadrons, for instance, protons). The leptons and quarks of the second and third family are only present in extreme energy situations for instance in HEP experiments, cosmic rays, or — according to the most popular cosmological theory — energies present right after the 'Big Bang'.

Each of the leptons and quarks has an anti-particle partner (which for neutrinos and quarks are denoted with a bar, e.g. an anti-electron neutrino is denoted $\bar{\nu}_e$) and all quarks come in three different 'colors'. Moreover, since there are eight

[40]The relation between 'models' and 'theories' has been discussed extensively in the philosophy of science literature but I will not go into any details in that discussion. It is sometimes stated by physicists that models are called so when they are believed to have somewhat less to do with reality than theories have (see e.g. 't Hooft in [88] p.257). In the case of the Standard Model, which is based on quantum field *theories*, such a meaning of the word 'model' seems less appropriate. For a discussion of the model concept in HEP see e.g. [42].

types of gluons (also associated with 'color'), the Standard Model operates with 48 matter particles and 12 force mediators[41]. All other particles are assumed to be combinations of these constituents. Thus, for instance, a proton is composed of two u quarks and one d quark.

While the Standard Model appears to have passed every experimental test to date, it is not believed to be fundamental. For instance, the Standard Model contains 20 *free* parameters. This means that the numerical values of these parameters cannot be explained by the model, but have to be determined experimentally. To a certain extent, the predictive power of the Standard Model decreases with the number of free parameters, and this provides an important motivation for physicists to seek for new theories. Suggestions for these include the so-called GUT's (Grand Unified Theories) which attempt to incorporate the electroweak and the strong coupling into one dynamical scheme, [154, 98] and string theories (see e.g. [98] p.53 and references therein).

The Standard Model, of course, has a history. Bits and pieces in the theoretical framework have been added and modified over the years — sometimes in response to conflicts with experiments and sometimes in accord with the theoretical strive for unification which has been with the physicists at least since Maxwell's days. Other factors in the development have undoubtedly played a role, but I shall not attempt to give any general summary on the history of the Standard Model in HEP[42] Nevertheless, the following chapter contains discussions on historical episodes in HEP which are well suited for philosophical reflection.

On the face of it, the world view of HEP thus describes a world composed of matter and force *particles*. But this is only partly true since the Standard Model is based on *field* theories rather than particle theories. According to QFT, it is the fields rather than the particles which carry the significant ontological status. This is illustrated by Weinberg's remark on 'essential reality' in QFT [186]:

> The inhabitants of the universe were conceived to be a set of fields – an electron field, a proton field, an electromagnetic field – and particles were reduced to mere epiphenomena. In its essentials, this point of view has survived to the present day, and forms the central dogma of quantum field theory: *the essential reality is a set of fields* subject to the rules of special relativity and quantum mechanics; all else is derived as a consequence of the quantum dynamics of these fields.

Nevertheless, the quantum duality between fields and particles (e.g. that electrons can be regarded either as particles or as waves) remains in the quantum field theoretical picture. This is so because the particles in QFT are regarded as quanta,

[41] In addition, at least, one so-called Higgs boson is needed for the unification of the electromagnetic and the weak force to make sense. For a good review of the Standard Model see e.g. [79].

[42] Brown and Hoddeson [20] set the birth of elementary particle physics (roughly equivalent to HEP) to around 1930 (where QED were initiated). For other historical accounts of HEP, see e.g. [143] and [28]. For a discussion of various commitments of physicists other than experimental puzzles and unification, see e.g. [70] p.246ff.

or excitations, of the fields[43]. The quantum dualism explains how it can be that the theory is formulated in terms of fields while the experiments aim to study the properties of particles (for a more detailed discussion on this point, see [153]).

[43]The ground state of a quantum field in which no quanta (particles) are present is called the vacuum state. In chapter 5 we shall discuss the vacuum concept and the fundamental ontology of QFT in more detail.

4 Experimental High-Energy Physics

The review of two key theses against realism in chapter 2 showed that it is complicated to determine what parts of the scientific product one should believe in. First, it is unclear in what sense experimental results can be understood independently of theory. Second, even if experiments are relatively independent of theory, they do not single out any one theory and therefore cannot bear the burden of determining which theories should be regarded as 'true'. In this chapter, I want to focus (primarily) on the first of these problems with respect to the experimental situation in HEP[44].

To begin with, it might be asked if HEP attracts any special attention from the philosophy of science point of view. Indeed, the concept of theory-ladenness of experiments (or underdetermination for that matter) has been discussed quite generally within philosophy of science. The justification for a particular examination of the philosophical aspects of HEP is twofold. First, regardless of any possible 'theory-ladenness' on direct sensing, it seems clear that the further experiments are moving away from direct sensing, the longer the chain of theories required to interpret the outcome, hence the more theory-laden the experimental results get. As we shall see below, the amount of data analysis needed in contemporary HEP experiments qualifies, in this simple sense, that HEP appears to be a 'limiting case' with regard to theory-ladenness. Second, and more generally, philosophical, historical, and sociological studies of science have shown that different branches of science are confronted with different methodological, epistemological, and ontological problems, implying different answers to reflexive or philosophical questions.

The outline of this chapter is as follows. After a short introduction to detector physics, I briefly review a contemporary experiment designed to analyze features of the quarks in order to point out some of the problems with which experimentalists are faced. I then review and comment on various accounts of the establishment of the weak neutral currents at a time of new theoretical ideas. After that I give a summary of a case-study of the theory-experiment interplay in a sequence of experiments conducted under a relatively fixed theoretical framework. Finally, I sum up the objectivity of HEP results in the light of the theory-experiment discussions.

4.1 Experimental evidence

We now turn to the question of how physicists extract knowledge about quarks or other sub-atomic particles from experiments. Experiments in HEP are typically conducted by accelerating beams of particles to very high energies, and then bringing them to collision in a detector. The point is to analyze the reaction products of such collisions in order to extract information about the constituents of the original particles and/or the identity of new particles formed in the collision. Depending on the type of detector, the outcome of a collision experiment can be photographs of particle tracks, where the particles have interacted in a so-called bubble chamber.

[44]Below, I will mention some examples of HEP experiments. While these will not be representative of all types of HEP experiments, they will provide some handles on the philosophy of HEP experiments.

Or it can be a set of 'reconstructed' particle tracks, where interactions between the particles and the detector medium are converted into electronic signals from which the reconstructions are made. Due to the high rates of data produced in present day detector experiments, these mostly use electronic detectors where, contrary to the bubble chamber, one can select what kind of data is interesting. In electronic detector experiments, raw data from the detector are filtered through a multi-step selection process which aims to cut away data coming from malfunctions of the detector or data irrelevant to the physics one wants to study[45].

Naturally, a lot of theory is involved in a HEP experiment. However, it is important to distinguish, as far as possible, between the theory under scrutiny (e.g. the theory predicting a specific process), and what may be called 'back-up' theories. The back-up theories in a HEP experiment includes, for instance, the theory of special relativity[46]. Indeed, in the design of the geometrical configuration of the accelerators and the detectors it is used that lifetimes of unstable particles are relative to the frame of reference. By virtue of the special relativity principles, the lifetimes of shortlived (as seen from the laboratory frame) particles are extended when these move with velocities close to the speed of light. Thus, the particles may have enough time to deposit energy in the surrounding media (for instance, in a bubble chamber) before disintegrating. In turn, the particles interactions with the detector medium result in bubble formation or the triggering of electronic devices. The use of back-up theories in the experiments corresponds more or less to what we earlier discussed as a weak relation between theory and experiment (section 2.2): One needs to have an idea about the working of the apparatus before conducting an experiment.

However, the relation between theory and experiment may be more intimate. The possible 'damaging' theory-ladenness can be found in two aspects of the data handling: First, selections are made in the data to single out tracks believed to originate in physical processes relevant to the experimental questions posed. Second, the analysis of the resulting tracks depends on theory. Unfortunately, to answer the question of whether the theories under test enter in the data extraction and analysis turns out to be a difficult task. For instance, the theory under test (e.g. QCD) influences how the initial cuts in the data are made, and may also be involved in the computer simulations which are necessary in the analysis of the data. It follows that the final experimental results can be limited by the theory used in the simulations. Even though experimental physicists claim that they are able to take this limitation into account, it remains unclear to what extent one can still talk of an 'objective' experimental test of an underlying physical theory.

The detectors which are used in HEP have sometimes been compared to microscopes which allow physicists to 'see' deep into the structure of matter. There are, however, at least two objections to the analogy between microscopes and detectors. The first is that some particles are thought to be created in the collision process prior to the detection, rather than liberated from existing matter (for instance, particles containing constituents from the second and third family referred

[45] For a non-technical review of various types of detectors and accelerators, see [143] p.23 ff. For a review of the data analysis process in contemporary HEP, see [21].

[46] Which is not surprising given the theoretical structure of HEP indicated in fig. (3.1).

to in the last chapter). This is clearly not the case in a microscope which cannot be said to create the phenomena to be observed (see also the discussion in [173]). The second point which can be raised against the analogy is this: The particles which are supposedly 'seen' in a detector are often not the ones which the experiment aims to study. Either because they are already decayed near the collision point (too short-lived) or because they are simply not believed to exist as free particles (quarks and gluons) which prevents a direct study. For the quarks and the gluons there is a special problem in this regard because the dynamics of the hadron formation from quarks (hadronization) cannot be calculated from the Standard Model. Therefore a 'phenomenological model' has to be invoked to describe the hadronization process[47]. In this sense, the chain of theories relating experimental results with physical phenomena is longest in the case of quarks, and I will therefore briefly review one type of quark experiments.

4.1.1 Studying the properties of quarks

Consider, as an example of a contemporary detector experiment, a measurement of the so-called s quark asymmetry [46]. The details of the experiment will not interest us here, but even a schematic presentation of the experiment will illustrate some of the difficulties in analyzing to what extent theory influence the data handling. The logic in the experiment can be summarized as follows: The Standard Model predicts a fermion asymmetry which means that the distribution of fermions (here s quarks) formed in e^+e^- collisions (colliding beams of electrons and positrons) is slightly asymmetric. That is, one expects that slightly more s quarks are produced in the forward direction as seen from the incoming electron. The experiment utilizes that one can 'tune' the beam energies to produce Z^0 bosons which subsequently decay, through intermediate states, to K mesons (among other things). Since K mesons are thought to contain s quarks, these mesons as final states serve as an indication that s quarks were present in the intermediate states. From the asymmetry of the K mesons, the asymmetry of the s quarks is then determined[48].

This summary, however, covers a highly complicated data handling process which involves reconstructing particle tracks from the electronic output of the detectors, and selecting the hadronic tracks associated with the K mesons. Specialized computer simulations, which among others aim to estimate the effect of theoretical presuppositions involved in the cuts and selection criteria, are involved in the track selection process[49]. These so-called Monte Carlo simulations mimic the processes believed to occur in the detector, when the beams collide[50]. Additional Monte Carlo

[47] There are a number of different phenomenological models available to physicists. These models are all inspired by QCD, in particular by the so-called confinement property of QCD (see below).

[48] The s quark asymmetry is used to a determination of one of the free parameters (the so-called Weinberg angle) in the Standard Model. In turn, this result can be compared with determinations of the Weinberg angle from other experiments.

[49] The simulations aim, for instance, to estimate the number of non-hadronic background events which are present in the data.

[50] The Monte Carlo programs generate random numbers which are used to simulate stochastic processes (e.g. the quantum interactions in the collision). For an account of the origin, and some philosophical aspects, of Monte Carlo simulations, see [73].

simulations are necessary to extract the s quark asymmetry from the K meson data. These simulations are based on phenomenological models which are needed to describe the processes by which hadrons are formed from quarks (a phenomenological model is part of a so-called event generator)[51]. Finally, Monte Carlo simulations are used to estimate the quantitative effect of the theoretical presuppositions (systematic errors) involved in the extraction of the s quark symmetry.

Although different methods of extracting the s quark asymmetry are used for checking the credibility of the K meson method, the question of whether the theoretical considerations involved in the computer simulations and data cuts entail a damaging relation between the theory and experiment, is far from trivial. In an article on data analysis in contemporary HEP, Butler and Quarrie mention, as one of the problems in the work with data reduction [21]:

> One builds some physics prejudices into the sorting and selecting that is required to achieve the data reduction. By using features of a model — such as the Standard Model of particle physics — to decide how to reduce the data, it is certainly possible to inadvertently eliminate events that are evidence of phenomena that lie outside the model or even contradict it. Although physicists are well aware of this problem and are constantly working to avoid it, there is always a certain discomfort.

Answering to a question about the possible influence of theory in the computer simulations used for the data analysis, Christopher LLewellyn Smith (Director General of CERN) answered[52]:

> [A] problem is that the analysis is entirely dependent on these huge Monte Carlo programs for the background... There is also the worry that these Monte Carlo programs, with their thousands of lines of codes written by many generation of graduate students, who remembers what was written into it? And there is a problem about correlation. A problem with the lab experiments that some of the parts of the MC [Monte Carlo] programs, the event generators, are common to the different experiments. As far as possible one has to try to keep the experiments independent, not just physically independent. But if they are sharing the same software there is a danger also that they look like independent confirmation of the same effect because you misestimated the background from the same program; it's a problem.

Naturally, the experimentalists do seek to vary the experimental conditions under which results are obtained[53]. Nevertheless, if physicists express worries about

[51] As indicated above, QCD cannot describe the hadronization process and so calculations from phenomenological models have to be used in reconstructing the original quark reaction. For more on the event generator JETSET, used in the asymmetry experiment, and its role in the data analysis, see [93].

[52] Interview with author August 25 1995, see appendix C

[53] In the s quark asymmetry experiment this variation consisted in using other particles than the K mesons to infer the s quarks. As LLewellyn Smith indicates, another strategy, not employed in the s quark experiment, is to use different event generators to see if a particular result is stable.

the role of theories in experiments, the complexity of the data analysis, and the possibilities for correlations, it does not seem unreasonable to be a bit sceptical towards the 'objectiveness' of quark experiments where Monte Carlo simulations are extensively used.

These remarks and the summary of the s quark experiment give an idea of how difficult it is to approach the question of theory-ladenness in modern HEP. In the following section we will try to get a little closer to the question of the relation between theory and experiment in HEP, by examining some case studies of a set of key experiments which led to the establishment of the Standard Model. The above discussion have focused on the epistemological question of how experiments are analyzed. Before trying to get a better handle on the epistemology of HEP, I will say a few words about the ontological status of quarks.

4.1.2 The reality of quarks

I noted in chapter 2 that physicists may not be interested in the philosophical aspects of their science. An example of a (theoretical) physicist who does take a stand on philosophy is Weinberg. In a popular book [188] Weinberg has a whole chapter entitled "Against philosophy", which is directed against various anti-realist positions, and in favor of the scientific realism view discussed earlier. Nevertheless, Weinberg acknowledges that he has no proof of his realism: "It certainly *feels* to me that we are discovering something real in physics, something that is what it is without any regard to the social or historical conditions that allowed us to discover it" [my emphasis] ([188] p.149). With respect to his attitude towards philosophy, Weinberg writes "The most dramatic abandonment of the principles of positivism has been in the development of our present theory of quarks" (p.144). That the quarks can have such an impact on philosophy is grounded in the *confinement* hypothesis which implies that quarks are never expected to be observable as individual particles (in contrast to a positivistic doctrine of only dealing with observables).

Part of Weinberg's critique of philosophy is directed against Pickering who in *Constructing Quarks* [143] argues that quarks are not entities in nature but rather socially constructed artifacts of experimental and theoretical practices. I will not go deeper into the controversy between Pickering and Weinberg on the quark issue, but a few remarks seem appropriate. First, Weinberg does not dispute Pickering's reconstruction of the historical events which led to the establishment of the quarks as real, but merely Pickering's conclusions ([188] p.149)[54]. Second, nobody would deny that the reality status of quarks was seen differently when the quark concept was suggested in 1964 (as merely a convenient way of organizing the hadrons, see [143] p.114.). Third, the conjecture of confinement was suggested *after* many fruitless searches for free quarks and has still not been theoretically justified.

Note that we are dealing with an epistemological problem which bears on ontology: Weinberg holds that the experimental success of QCD warrants belief in

[54]The experimental faith in the reality of quarks originates in the so-called deep inelastic scattering experiments of the late 1960s where electrons are fired into protons, apparently revealing substructure of the latter. These experiments, together with subsequent establishment of the c quark in 1974, provided much of the impetus for QCD as chronicled by Pickering [143].

entities like quarks[55]. Pickering, on the other hand, does not believe in the capability of experiments neither to confirm QCD nor to establish the reality of quarks. We will return to Pickering's anti-realist arguments in the following section.

Our brief review of an experiment on quark properties did not make matters more clear with respect to the epistemological status of quark experiments. The complicated data analysis, which may or may not involve theoretical assumptions of a damaging sort (i.e. could the answers to the experimental questions in some sense be build into the data analysis), makes it difficult to settle the outcome of quark experiments as objective facts. As we shall see below, it appears easier to gain confidence in experiments on other objects in the HEP world view.

4.2 Weak neutral currents — facts or artifacts?

The first experiments on weak neutral currents have been used in favor of opposing sides in the realism debate, and constitute therefore a good example of how the same historical episode can be used to underpin opposing philosophical views. I shall first indicate the problems which faced these experiments, and then comment on three different accounts of them — Pickering's sociological analysis [142], Galison's historical analysis [69], and Miller and Bullock's historio-physical analysis [126][56]. I will not go into the details of all the many facets of the weak neutral current discovery. Instead, I will try to summarize the important points with respect to the 'background analyses' (see below) illuminating the theory/experiment relations.

4.2.1 The discovery of the neutral currents

In the period from 1972 to 1974, the physics community came to believe that weak neutral currents had to exist. To say that neutral weak currents exist is to say that processes such as electron- (muonic) anti-neutrino scattering are possible:

$$\bar{\nu}_\mu + e^- \rightarrow \bar{\nu}_\mu + e^- \tag{1}$$

in which the force between these particles is mediated by the Z^0-boson[57].

According to the theoretical expectations in the 1960s, processes due to weak neutral currents were not supposed to happen, and had not been observed. Or rather, believed not to have been observed (see below). At the time, the theory for

[55]More precisely, Weinberg describes some of the theoretical developments which led to the confinement hypothesis. Nevertheless, since this hypothesis intends to justify the experimental situation of no free quarks, my reading of Weinberg seems fair.

[56]I will also use Pickering and Galison's book versions of this episode [70, 143] (in addition to their original texts) since the book versions put the neutral current episode in a broader philosophical and historical perspective. While I want to emphasize the differences between the conclusions of these authors it should be pointed out that Galison's analysis of the experiments is by far the most detailed. There has been a number of reviews on these experiments also in the physics literature, see for instance [35] and [171]. In addition Hones has given a philosophical-methodological analysis [89].

[57]The identification of the Z^0 boson as the force mediator was only made after the establishment of the WS theory, see below. In principle, the process could occur by a so-called four-fermi interaction, where the interaction between the fermions is direct. Still, this would entail the weak neutral current.

weak interactions was the so-called V–A (V minus A) theory, which only predicted *charged* weak processes in which W^+ and/or W^- bosons are mediated between the fermions[58]. By preparing beams of neutrinos (which are well suited to weak force studies, since they only experience this force) and sending them into a detector, charged processes had been detected on many occasions during the 1960s.

Although the possibility for neutral current processes existed in principle, the theoretical climate did not make the search for neutral currents central to the experiments with neutrino beams in the 1960s. The experiments, for instance the one at CERN using a bubble chamber called Gargamelle, were designed to examine aspects of the V–A theory as well as features of the quark model ([70] p.159 ff). But a theoretical result by 't Hooft from 1971, followed by intense dialogues between theorists and experimentalists, soon prompted the experimenters to change their focus and look for weak neutral currents. 't Hooft's result was that a certain class of the so-called gauge theories were renormalizable, which effectively means that these theories are able to produce experimental predictions. In particular, a gauge theory formulated independently by Weinberg (1967) and Salam (1968) was, with 't Hooft's result, able to give precise predictions relevant to the neutrino experiments. The theoretical attraction of the Weinberg-Salam (WS) theory was that it unified the electromagnetic and the weak forces in one description (resulting in the now commonplace name 'electroweak forces'). But in addition to weak charged currents, the renormalizable WS theory predicted the existence of weak neutral currents. Hence there was a conflict between existing experimental results and the WS theory.

The first published indication of the existence of the weak neutral current came from CERN in 1973, and was based on a 'golden' event: A bubble chamber picture apparently showing the outcome of the process (1); an electron (the event was called 'golden' since it was regarded as a particular clear example of a neutral current process, and since it was found in only one out of 100,000 pictures [126]). This prompted increased interest in another source of possible evidence for neutral currents: The neutrinos can also interact with hadrons so if the weak neutral currents exist then one expects processes like

$$\nu_\mu + N \to \nu_\mu + X \tag{2}$$

where N is either a neutron or proton, and X is any hadron or combination of hadrons.

Crucially to both these types of neutral current evidence ((1) and (2)) was the background analysis. Could the evidence stem from processes other than those associated with the weak neutral current? As a central point of disagreement between the analyses of Pickering, Galison, and Miller and Bullock is on the problem of background estimation in the hadronic case, I will restrict attention to this[59]. The

[58] V–A theory is a field theory and was formulated in the late 1950s as a descendent of Fermi's theory for weak processes. However, while the V–A theory enjoyed some phenomenological success, it was not renormalizable, yielding therefore nonsensical answers in higher orders of perturbation theory ([143] p.183) (see next chapter for more on perturbation theory and renormalization).

[59] Pickering is not very interested in the golden event, primarily because he holds that single events cannot convince physicists of the existence of new phenomena [142] p.93 (see below for a critique of this point). In our context it suffices to note that the golden event was not generally seen as conclusive evidence for the neutral current, see also [126] p.914.

process (2) was to be distinguished from another event involving instead a charged current (the exchange of a W^+ boson):

$$\nu_\mu + N \to \mu^- + X \tag{3}$$

Thus, in reactions involving hadrons in the final state, the appearance of a muon in the outcome (e.g. the bubble chamber picture) signifies that a charged process occurred in the detector. Trouble was that the muon in process (3) might escape detection. This could happen if the neutrino interacted according to (3) in the material surrounding the bubble chamber instead of inside it. In that case, the muon would most likely escape detection, and the X could very well be a neutron which could enter the chamber. Since neutrons cannot directly be distinguished from neutrinos in detectors, the charged process (3) would be indistinguishable from the neutral one (2) (the neutron n would interact according to $n + N \to X$)[60]. In the 1960s, there had indeed been seen muonless events but these were all interpreted as background: It was conjectured that the muon had, in fact, been present in the reaction but that it had escaped detection.

The important point in the background estimation was then to find out whether or not this 'neutron background' could account for all the neutral current candidates. The most sophisticated background estimate, but not the only one available, consisted in Monte Carlo simulations which (on the basis of the detector configuration, knowledge of the physics of neutrinos and neutrons, and some experimental results) were used to calculate whether the total number of neutral current candidates exceeded that expected from the background.

In 1973 and 1974 publications appeared from CERN which strengthened the conclusion of the existence of neutral currents based on hadronic neutral current events. Around the same time of these experiments in Europe, neutrino beam experiments were performed at the American National Laboratory[61]. The American experiment, which used electronic chambers to reconstruct particle tracks in contrast to the bubble chamber of the CERN experiment, reported evidence for the neutral currents in hadronic events in 1974. Although the Americans followed a different path to the neutral currents (see e.g. [70] p.198 ff), it suffices here to note that various Monte Carlo simulations also played a role in their estimates of the background from charged current events (see, however, the discussion below).

Enough have been said now to discuss the differences in Pickering, Galison, and Miller and Bullock's accounts as regards the question of an 'objective' discovery of weak neutral currents. I will review them in turn and we shall see that the different accounts of the same historical event makes clear that much depends on the philosophical starting point.

[60] Neutral particles such as neutrons and neutrinos do not leave any tracks in the detector. Their presence has to be inferred from the behavior of charged particles. Schematic representations of these processes and the detector configurations can be found e.g. in [70] p.171.

[61] The quest for neutral currents included an important element of competition between different experiments. The interaction between the groups in Europe and the US, as well as the internal dynamics in these groups, are described in [70], e.g. p.218.

4.2.2 Pickering and social constructivism

Pickering's analysis of the neutral current discovery is based on the methodological relativism discussed in chapter 2. This is witnessed in the title of his first paper "Against putting the phenomena first" [142] and summarized in the concluding chapter of his book ([143] p.404) where he comments on the historiographical objection to 'retrospective' realism, or 'the scientists' account' (that is, using the reality of neutral currents to explain the historical episode of their discovery):

> If one is interested in how a scientific world-view is constructed, reference to its finished form is circularly self-defeating; the explanation of a genuine decision cannot be found in a statement of what that decision was.

Pickering sees the neutral currents as an outcome of a symbiosis between theory and experiment, prompted effectively by the desire of both experimentalists and theorists to have new things to study. In his words ([142] p.87):

> I argue that the reality of the weak neutral current was the *upshot* of particle physicists' practices, and not the reverse. [emphasis in original] ...particle physicists accepted the existence of the neutral current because they could see how to ply their trade more profitably in a world in which the neutral current was real.

The first part of the quote is similar to that of Latour and Woolgar given in section 2.3, and represents the standpoint of the social constructivists: The social (here physicists practices) is given and the 'natural' (neutral currents) is constructed from that[62]. The second part of the quote adds an idea of 'construction by interest' in the social constructivists arsenal. Simply, it is smart for the physicists (both theorists and experimentalists) to invent the neutral currents, since it opens up new possibilities for research.

Pickering's strategy for supporting this point of view is to show that all the neutron background estimates were open to objections. His emphasis is on the shift in 'interpretative practices' that took place before and after the establishment of 't Hooft's result. First, he points out that the identification, made in the 1960s, of muon-less events with (background) charged current events could be criticized. The most detailed examination of the neutrino experiments in the 1960s showed that there were neutral event candidates in the data which could not be explained on the basis of the neutron background hypothesis, but this problem was left unattended at the time ([142] p.99). Second, Pickering lists a number of possible objections to the best Monte Carlo simulations in the 1970s ([142] p.95), all referring to the parameters which have to be fed into the simulation, for instance regarding idealizing assumptions of the detector geometry, and assumptions of the neutron formation from neutrino beams (related to the problem of hadronization discussed in the previous section). His conclusion is that the neutral current events were in the data both in the 1960s and the 1970s — that is, if physicists wanted them to be there.

[62]This label on Pickering is also worn by himself as his work is "intended as a contribution to the 'relativist-constructivist' programme in the sociology of science..." ([143] p. xi).

And this 'wish' was only present in the second period where, due to the advent of renormalizable theories, physicists "could see how to ply their trade more profitably in a world in which the neutral current was real".

As noted above, Pickering does not discuss the golden event in any detail, nor does he elaborate much on the data analysis of the American experiment, claiming that the pattern of changing interpretative practices was the same as in Europe. Even granting these choices of Pickering, however, his conclusion of the 'constructedness' of the neutral currents does not follow. That the data analyses, for instance the Monte Carlo simulations, are open to objections does not imply that they are arbitrary. Like the experimenters, Pickering cannot know with certainty whether the conclusion with respect to the neutral currents would change if all possible objections to the data analyses, in principle, were met.

Thus, Pickering's conclusion on the constructedness of the neutral currents is itself based on judgment — judgment informed by the emphasis on interests in social constructivism, and underpinned by a subtle combination of underdetermination and theory-ladenness: The data analyses are open to critique and hence data cannot conclusively choose between theories with and without neutral currents. Nevertheless, the weaknesses of the data analysis are utilized so as to support the theory which physicists want[63].

4.2.3 Galison and historicism

Galison's account is compatible with the methodic relativism approach which does not take the reality of the phenomena as an explanation of the historical episode. But although Galison, like Pickering, emphasizes the sociological aspects of the episode, this "does not force one to take a radically relativist stance toward experimental conclusions" ([70] p.277). The reason is partly what was spelled out above, namely that Pickering's conclusion, while being possible in principle, does not follow convincingly from his analysis.

Moreover, Galison is reluctant to grant theory the role of determining how experimenters reach their conclusions because experiments, in part, have a life of their own. Experimental practices, Galison argues, are not determined merely by theoretical fashions. As an example, Galison mentions that one of the physicists (D. Cline) involved in the American experiment was deeply committed to the non-existence of neutral currents but finally concluded his analysis by stating that he could not see how to "make this effect [of the neutral current] go away" [70] p.235. According to Galison, the American physicist reached this conclusion from data which he analysed in a simple way without appealing to Monte Carlo simulations. In this connection, Galison describes in detail how the members of the two collaborations reached their conclusion about neutral currents through different arguments. Some were committed to golden events, some to Monte Carlo simulations, and some to more crude estimates of the background. ([70] p.195):

> ...buried in the apparently simple antibackground arguments of the form

[63] Note that Pickering's argument is not that the theory of the phenomena under study (neutral currents) is directly involved e.g. in the Monte Carlo simulations. This was one of the possible source of theory-ladenness that we discussed in the previous section on quark experiments.

> *not A*, *not B*, and *not C* [referring to various processes potentially capable of simulating the neutral current processes] are many, partially autonomous paths of persuasion. But they were routes that, when taken together, led a heterogeneous group of physicists from Belgium, Britain, France, Germany, Italy, Switzerland, and the United States to stake their reputations on a new kind of physics.

Consequently, Galison argues against Pickering both on the interest level and on the predominant role of theories in experiments. From this, however, Galison does not conclude that neutral currents were discovered as a natural fact. Instead he emphasizes the various arguments that "...*experimentalists* use in their decision to class phenomena as real or artifactual" ([70] p.260). What, then, is Galison's own stand on the neutral currents? While no simple realism/anti-realism answer is given in Galison's account, he is sympathetic with Hacking ([70] p.261):

> Among philosophical views on experimentation, Hacking's seems to me the most congenial. Where I part company is when he elevates the criterion of *manipulation* above all others. ...there are other ways in which the experimentalist finds persuasive evidence...

(These "other ways" can, for instance, be stability of the result under variations of the Monte Carlo simulation input parameters). However, as outlined in chapter 2, the criterion of manipulation is for Hacking a criterion for entity realism. Galison, on the other hand, refers to this and additional criteria as ways of obtaining 'persuasive evidence'. The difference is almost at the rhetorical level only, but Galison's way of putting matters is consistent with the historicist view discussed in chapter 2: It is conceivable that ways of obtaining persuasive evidence for e.g. electrons are contingent (and thus that, in principle, the existing evidence may be cast in doubt in the future). By contrast, it is hardly imaginable that electrons, once manipulated, cannot be manipulated in the future[64]. In this sense 'persuasive evidence' appears to be a philosophically less committing phrase than 'entity realism': Persuasive evidence refers to epistemology only whereas entity realism implies both epistemology and ontology — electrons are part of the furniture of the world when they can be manipulated.

4.2.4 Miller and Bullock, and realism

The neutral current discovery has recently been re-examined by Miller and Bullock, partly in response to the what they see as the primarily experimental oriented accounts of Galison and Pickering. While Miller and Bullock find Galison's study invaluable, the emphasis on experiments as a mode of analysis "... can distort the complete historical scenario". Thus, Miller and Bullock find that it is now (1994)

[64]It may be objected here that this comparison between Hacking and Galison is a bit misleading since Hacking's emphasis on manipulation excludes neutral currents. Nevertheless, as we saw in the discussion of quark experiments, the Z^0 bosons, which in the WS theory are required by neutral currents, are in a certain sense manipulated to study quarks. I will return to this point below.

"...appropriate to return to this episode with a balanced exploration of the theme of unification [...] within an aesthetically pleasing theory" ([126] p.895).

One of Miller and Bullock's main points is an attempt to oppose Pickering's conclusions. They agree with Pickering that experimentalists somehow jumped on the theorists wagon, also, perhaps, for the possibility of career advancement. But, Miller and Bullock continue, the historical record shows that this did not compel the experimentalist to find the neutral currents[65] ([126] p.923). They also agree with Pickering on the complexity of the Monte Carlo simulations of the neutron background in the CERN experiment. But given that the best of these were at first doubted, and later, after refinements, accepted, the fragility of the Monte Carlo simulations cannot support Pickering's conclusion[66]. Finally, like Galison, Miller and Bullock point out that the golden event cannot easily be omitted, since such events can convince (some) physicists of the reality of a phenomenon.

While Galison appears reluctant to endorse Hacking's entity realism, Miller and Bullock find it insufficient. They uphold a realism for both the neutral currents as phenomena and for the WS theory:

> By realism philosophers mean that a scientific theory is true if it can explain data within its domain of applicability and so we believe that each of its statements is true *and* that the entities postulated be these statements actually exist whether or not they are directly measurable.

However, Miller and Bullock "...emphasize the word 'belief' because the side of logic is with the antirealists: you cannot beat their underdetermination argument" ([126] p.927). Nevertheless, they opt for a realism about theories and entities since it "is useful for exploring fundamental issues in science, and perhaps for scientific research as well" ([126] p.929). As we shall see in chapter 5, Miller and Bullock are not alone in this trade on usefulness as a guidance to epistemological and ontological issues.

4.2.5 The trust in neutral currents

Given the different accounts on the discovery of the neutral currents, one may ask if there is any coherent position to take on the realism question in this case? While neither Pickering or Miller and Bullock can logically prove their points, it seems to me that one can get a step further than Galison.

The first thing to note is that if one, like Pickering, wants to explore the logical loophole created by the combined forces of underdetermination and theory-ladenness, then a lot remains to be explained. The neutral current evidence was assembled by both visual and electronic detectors, both leptonic and hadronic processes, and both Monte Carlo simulations and more crude estimates of the background events. Even though the analyses of the neutral current discovery testify

[65]Precisely what they have in mind from the historical record is not clear, but it could very well be Galison's studies.

[66][126] p.925. This, however, hardly counts as an argument against Pickering since the first Monte Carlo simulations suggested that perhaps the neutron background *could* account for all the neutral current events, see [70] p.192ff.

that these standards of demonstration were not independent, one has to place high bets on the power of physicist's interests to explain this coherence.

Secondly, although Pickering's warning against retrospective realism should not be forgotten, it is hard not to take into account the current situation in HEP. Upon commenting on Pickering's lack of interest in the golden event, Miller and Bullock note that physicists at CERN in 1992 found 3000 neutral current events of the leptonic type (process (1) above). Moreover the Z^0 bosons are now routinely produced e.g. at CERN where they are used to examine the properties of quarks[67]. A claim that this current state of affairs in HEP is merely a consequence of an interpretative practice established 20 years ago, would have to be based on a social determinism which is doubtful, to say the least.

Therefore it is reasonable to hold that the *trust* in the experimental conclusion of the existence of neutral currents is prompted not only by the different ways in which the conclusion was established originally but also by the subsequent historical development. This suggests a philosophical generalization: The trust in experimental results increases, when these can be obtained through a variety of different experimental techniques — whether the variations refer to the background analysis (e.g. Monte Carlo simulations), the physical processes involved (e.g. leptonic or hadronic events), or the experimental apparatus (e.g visual or electronic detector). Moreover, later historical use and/or reconfirmation of the experimental conclusion adds further to its credibility[68].

We will come back to the implications of this trust concept shortly. But first we turn to a central sequence of experiments in QED — the 'parent' quantum field theory for the Standard Model — in order to discuss the combination of experimental variation and historical development in more detail. Since these experiments have led to the most precise comparison between theory and experiment in physics over a period of 50 years, they are well suited for a study of the philosophical aspects of experiments discussed above. We start out by reviewing the historical background for high precision tests of QED.

4.3 The electron g-factor

In Maxwell's electrodynamics there are no elementary magnetic charges. With the discovery of the electron in 1895, it was therefore assumed that the electron is the only charge carrier responsible for magnetism[69]. Moreover, since all electrons have the same charge and mass it follows that the density of charge should be proportional

[67] As Miller and Bullock point out, the relation between the Z^0 boson and the neutral current, assumes the WS theory [126] p.928. Therefore, if one wanted to defend entity realism with respect to neutral currents (by referring to the possibility of manipulating Z^0 bosons), it is doubtful whether at least some theory realism could be avoided. See also ([133] p.253) where it is argued that Cartwright's version of entity realism is problematic since one cannot easily distinguish between theory realism and entity realism.

[68] This suggestion, of course, is a variation of the well-known idea of reproducibility as a means for supporting belief in experimental results. For a good account on how reproducibility supports a certain kind of realism, see [148] and also my review of that book reprinted in appendix D.

[69] This section is a short summary of [115] written together with Benny Lautrup (reprinted in appendix E). Historical details of early electromagnetism can be found in [70].

to the density of mass for these charge carriers. This proportionality leads in turn to a relation between the magnetic moment of a current distribution, which is a measure of its magnetic field and the angular momentum of its mass distribution which is a measure of its state of rotation. They must in fact be proportional to each other with a constant of proportionality given by the ratio of the electron's charge to twice its mass ($e/2m$). If the electron were not the only charge carrier things would be different. Contamination from another charge carrier with different ratio between charge and mass would lead to a different constant of proportionality. In order to include this possibility a 'fudge-factor' g was introduced to take care of such deviations (so the ratio was written $g\,e/2m$). This g-factor or gyromagnetic ratio would accordingly be exactly 1 (i.e. $g = 1$) if the electron were the only charge carrier.

Experiments seeking to investigate this $g = 1$ prediction have been chronicled by Galison as another illustration of what constitute persuasive evidence for experimenters so as to *end* a series of experiments [70]. The curious history of the 'gyromagnetic' experiments illustrates the importance of theoretical ideas for the outcome of experiments. Around 1915 the prejudice of the theorists (among them Einstein) was strongly in favor of $g = 1$ and experimental results were duly found in this neighborhood at that time and as late as 1923 by Einstein's collaborator. Galison concludes in his analysis of this situation, that the theoretical prejudice would not by itself bias the experimental result, but could possibly have created a mindset in which experiments were terminated and the search for systematic errors given up when a result was found near the strongly expected one [70]. In 1928 Dirac published his relativistic theory of the electron. In this theory the electron has a built-in spin with an exact gyromagnetic factor of $g = 2$. For the next two decades this became a theoretical prejudice which agreed comfortably with experiments (see [163] p.211 ff).

In [115] we reflect on the development since 1947 of experiments on the magnetic moment of the electron, commonly referred to as $g-2$ [g minus 2] experiments, which provide the most precise comparison between theory and experiment in HEP. Our point of departure in [115] is the question of why experiments *continue* rather than how they end[70]. It is sometimes assumed that repetitions of experiments only take place in areas of controversy, for instance to test the stability of a new effect under variation of the experimental circumstances (see e.g. [36]). The $g - 2$ experiments have all been performed in a period under a fixed theoretical framework, QED. Nevertheless, the sequence of these experiments provides an interesting example of the interplay between theory and experiment which illustrates the trust in experimental results by reconfirmation and historical development.

[70] It should be noted that our account of the $g-2$ experiments is not a detailed historical analysis of the circumstances leading to the published articles on the magnetic properties of the electron. By this we do not mean to neglect the value of such studies. The importance of going behind the reconstructed logical ordering of the published papers has been emphasized by many recent scholars, see for instance [36] and [70] p.244.

4.3.1 $g-2$ experiments

A central focus in [115] is on the experimenters treatment of systematic errors which arise from experimental circumstances not taken into account and/or bias in the extraction of results from data. Theoretical considerations dictate what systematic errors can be expected in an experiment, and theory may be used to estimate their influence. In our account of the $g-2$ experiments we therefore pay particular attention to what kind of theoretical arguments were involved in the process of extracting a value for $g-2$ from the measurements.

The first suggestion that the g-factor of the electron might be different from 2 was made by Breit in 1947 [17], and was prompted by a disagreement between theory and precise measurements of the hyperfine structure of hydrogen obtained by Nafe, Nelson, and Rabi [135]. This prompted, also in 1947, the first actual measurement of $g-2$ by Kusch and Foley [106], *before* any QED calculation of $g-2$ had been made[71]. In turn, this began a series of experiments for determining the precise value of the difference between the actual g-factor of the electron and the Dirac value 2. One may roughly divide the modern development into three different phases that more or less follow each other sequentially in time: 1) atomic level experiments, 2) free electron spin precession experiments, and 3) free electron spin resonance experiments[72]. These titles refer to different experimental techniques which over the years lead to a remarkable increase in precision of the determination of $g-2$ (see table 1 in appendix E).

The $g-2$ experiments were, apart from the first by Kusch and Foley, pursued in the specific theoretical environment of QED. Without this theory, yielding more and more precise theoretical predictions, there would not have been much point in pushing the experiments to higher and higher precision. But, as we discuss in [115], the actual theoretical considerations employed in the various experimental techniques did not depend on QED.

The sequence of $g-2$ experiments is characterized by a continued refinement of the treatment of systematical errors limiting their precision. The historical development consists in a gradual stripping away of the electron's environment, with a corresponding elimination of systematic errors. In the atomic resonance experiments the electrons were found deep inside the atoms, making the extracted value dependent on complicated atomic-physics calculations. In the free electron spin precession experiments the electrons were removed from the atom and studied collectively in a magnetic field trap, but 'space charge' problems due to their collective charges ultimately set the limit to this kind of experiments. Finally, in the single electron spin resonance experiments the electrons in a so-called Penning trap could eventually be controlled so well as to eject all but one of them from the trap.

In the preceeding section we mentioned the, often cited, criterion for belief in experimental results when it is stable under variation of the experimental circumstances (see e.g. [148]). By this criterion the result of Kusch and Foley from 1947

[71] In 1948 Schwinger made such a QED calculation based on renormalization, see appendix E.

[72] The theoretical efforts after 1948 were all concerned with more precise calculations within QED (corresponding to higher orders in the 'perturbative' expansion, see next chapter). Apart from the most recent series of experiments (see [115]), the theoretical value for the electron g-factor was at all times in history known to higher precision than the experimental values.

has been amply confirmed by a number of different experimental methods. But the continued refinement of the experiments also illuminates Hacking's entity realism by the sublime manipulability of the electron. Our account in [115] does not prove that the data analysis in the $g-2$ experiments was not influenced by knowledge of the QED predictions. We find it however implausible that this should be the case due to the long sequence of $g-2$ experiments with their continuing stripping of the electron's environment. This stripping process was entirely based on theory that did not involve QED itself.

The trust in the results constitute a clear empirical success for QED. Whether this implies that QED is necessarily the correct framework for describing the electron is another story (see next chapter). In any case, a different theory would have to face up to the remarkable results for the anomalous magnetic moment of the electron.

4.4 The trust in, and interpretation of, experimental results

In this chapter I have discussed some of the relations between theory and experiments in HEP. A recurring, and of course well-known, theme has been that of experimental variation in order to establish an experimental conclusion. Based on this I submitted the argument that the trust in an experimental result as being sufficiently independent of theory (i.e. the outcome of the data analysis is not determined by the theory under scrutiny) is constituted by variation of the experimental circumstances and by subsequent reconfirmation of the result.

In the case of the weak neutral current, the reconfirmation involved a large number of identified neutral current events in leptonic reactions (similar to the 'golden' event from 1973). For the anomalous magnetic moment of the electron, the reconfirmation was constituted by refinements of experimental techniques leading to higher and higher precision (and every new determination of $g-2$ confirmed the digits found up to that point, see [115]). Of course, the contemporary experiments on quark properties may be reconfirmed later. But insofar as contemporary detector experiments, for instance aiming to investigate properties of quarks, are completely dependent on data analysis involving QCD assumptions (confinement and hadronization), such experiments seem more open to scepticism.

The idea of trust in experimental results is an epistemological issue. It is about ways of obtaining evidence, and about how confident we can be that the results are objective in the sense of being sufficiently independent of the theory predicting the result. The obvious question is how this is related to ontology. What conclusion does an objective experimental result allow? As we have seen, Hacking suggests that we can be realists about entities without being realists about theories. That is, entities are 'out there' — they really exist — if we are able to manipulate them to investigate something else. Theories, on the other hand, are not necessarily true (or candidates for truth).

For the experiments on electrons described in the last section this works fine. Electrons exist because we can manipulate them, and the development of the anomaly experiments indicates that experimenters have become better and better at this task (manipulating *one* electron in a Penning trap). Clearly, these experiments constitute a tremendous phenomenological success for QED but whether QED is a correct description of reality or true in all its details is not implied (in the next chapter we will come back to the issue of QED as 'true').

The neutral current result were obtained in the context of establishing whether or not neutral currents exist. Consequently, trust in the experimental result can hardly avoid to imply trust in the existence of neutral currents. As we saw above, this specifically means that certain processes occur, for instance specific interactions between neutrinos and electrons. But this is not exactly an existence claim of an entity. In fact, Hacking has a brief passage about neutral currents where he answers his own question of when weak neutral currents may become commonplace reality like electrons: "When we use them to investigate something else" ([80] p.272). As we indicated above, however, to manipulate neutral currents is to manipulate the Z^0 bosons. And it takes electroweak theory to make the association of neutral currents with Z^0 bosons. Consequently, if one wants to use the criterion of manipulation as an indicative for the existence of neutral currents, and hence as a reconfirmation of the neutral current experiments discussed above, electroweak theory must be 'true' to some extent (so theory sneaks in again, despite Hacking's claim).

Although we have not discussed the experiments leading to the conjecture of quarks, two things seem to be clear from the confinement hypothesis. First, the existence of quarks and the truth of QCD are hard to separate. Second, we are very far from experiments manipulating quarks to experiment on other phenomena (although there have been speculations of quark substructure, see e.g. [98] p.5). When we add to this the possible 'damaging' theory-ladenness on contemporary quark experiments, it therefore appears more reasonable to question the existence of quarks than that of neutral currents[73]. In any case, it seems clear that the separation of entities from HEP theory becomes increasingly difficult in the progression from electrons to neutral currents to quarks.

[73] I would like to emphasize that this suggestion is not based on the specific s quark experiment considered above, but on the apparent necessity of contemporary detector experiments to rely on not so clear data analysis.

5 Alternative Theories of High-Energy Physics?

We now turn to the consequences of the underdetermination thesis in its 'pure' form: Assuming that we have an experimental result which has been obtained relatively independent of an underlying theory (e.g. quantum field theory (QFT)) in what sense is this underlying theory then confirmed? And to what extent can the ontology or metaphysics of the underlying theory be held to be 'true' on the basis of experimental results? These are questions about realism on the theoretical level, and thus the experimental results are not problemized here as was the case in the last chapter. In this chapter, I will use the concept of vacuum in QFT as an illustration of the problem of theory underdetermination with respect to an experimental result.

The outline of this chapter is as follows. First, I give a brief historical introduction to the vacuum concept in order to set the stage for a case-study of how a "vacuum experiment" underdetermines the choice between QFT and a rival theory called source theory. Second, I introduce a bit of the QFT formalism which will also be necessary for the vacuum case-study. Finally, I discuss what can be learned from source theory, and other possible alternative theories of HEP, with respect to theory choice and reality.

5.1 A brief history of the vacuum

Before turning to the vacuum concept in modern HEP, it is in order to provide some historical background. The idea is not to present a rigorous historical or philosophical account of the development of the vacuum concept (which is closely connected to the development of the matter concept) but merely to indicate how the vacuum has been on the agenda from the beginning of man's inquiries about nature.

Aristotle was against the idea of the vacuum or void space. Not only did he argue that the concept was superfluous; it was also an absurdity since void space was identified with a three dimensional place and thus, to Aristotle, body. But this implied that two bodies (both the void and a 'material' body) could occupy the same place, an unthinkable situation at the time. On the other hand, the atomist claimed the ultimate structure of matter to be particles (atoms) with complete emptiness (vacuum) in between. The atomists, for instance Democritus, argued that the existence of the vacuum was necessary to explain, for instance, the separation of things to allow them to be distinguished. Thus, the scene was set for controversies up through history on the role and possibility of emptiness[74].

In more recent times, Faraday's wave theory of light and Maxwell's theory of electromagnetism prompted the need for a substantial vacuum; an ether. All forms of waves known up to that point had a medium through which they propagated,

[74] These historical notes on the vacuum concept are taken from Edward Grant's "Much Ado About Nothing" [78]. Up through the Middle Ages, the question of the vacuum was tied to a number of other issues, for instance the world's eternity and God's omnipresence. Thus, the seemingly simple dichotomy presented here — vacuum or not — is somewhat misleading (also because many different conceptions of the essence of the vacuum have been on the historical agenda [78] p.67 ff).

e.g. sea waves through water and sound waves through air. Thus, the ether as the medium for the electromagnetic waves seemed necessary (though the nature of this ether, not being matter yet still 'something', was problematic[75]).

The ether was abandoned with the special theory of relativity, in particular since this theory built on the insight that there could be no absolute reference frame (the ether was supposed to be the stationary medium through which electromagnetic waves propagated). For the discussion to follow, it is interesting to quote Einstein's conception of the relation between matter, fields and empty space (in 1946, [160] p.35):

> Matter – considered as atoms – is the only seat of electric charges; between the material particles there is empty space, the seat of the electromagnetic field, which is created by the position and velocity of the point charges which are located on the material particles.

The special theory of relativity made clear that no distinction between fields and the ether was necessary [159]. It is important to note that the electromagnetic field (the 'ether') is a *consequence* of material particles (charges). With the advent of quantum mechanics, and in particular with QFT, this ontological primacy of particles over fields has been challenged (as I indicated in the beginning of the last chapter). The origin of these challenges will be discussed in more detail when we turn to the vacuum case-study below. First, I introduce a bit of the QFT language which can be seen as a guide to the reading of the case-study where the technicalities of QFT can no longer be avoided[76].

5.2 QFT and the vacuum

A central issue in QFT is the calculation of the amplitudes or probabilities of various quantum processes. For instance, if an electron beam collides with a positron beam (e.g. in a particle accelerator) then the probability that a certain number of electrons will 'scatter' off positrons can be calculated from QFT. Quite generally, the transition probability for a certain process is calculated from the scattering- or S-matrix and given by:

$$| < out|S|in > |^2 \tag{4}$$

That is, given a certain initial state $|in>$ (e.g. an incoming electron and positron), the probability for some resulting final state $|out>$ (e.g. a state where the positron and electron have scattered off each other) can be derived from the S-matrix. In QFT, the S-matrix refers to all possible intermediate states in the process (for electron-positron scattering such an intermediate state could consist of a photon). If one restricts attention to relatively low scattering energies, the amplitude can be calculated within QED which primarily deals with electrons, positrons and photons. Nevertheless, scattering should be understood in general terms, and may involve new particles formed in the collision if enough energy is present (we saw an example

[75] For instance, the ether needed to be infinitely rigid to explain the fact that light waves are always transverse. See e.g. [159] for a review on the ether concept.

[76] More rigorous accounts on QFT can be found in any introductory book on QFT (e.g. [79]).

of this in the s quark experiment discussed in chapter 4, where electron-positron scattering led to Z^0 boson formation and subsequently s quarks).

The explicit calculation of the S-matrix elements is difficult, and can only be carried out with the aid of perturbative techniques. In the following I concentrate on QED, where the employment of perturbation techniques amounts to treating the interaction between the electron and photon (between the electron-positron field and the electromagnetic field) as a small perturbation to the collection of the 'free' fields. In the higher order calculations of the resulting perturbative expansion of the S-matrix, divergent (infinite) integrals, which involve intermediate states of arbitrarily high energies, are encountered. In standard QED, these divergencies are circumvented by redefining or 'renormalizing' the charge and the mass of the electron. By the renormalization procedure, all reference to the divergencies are absorbed into a set of infinite bare quantities. Although this procedure has made possible some of the most precise comparisons between theory and experiment (such as the $g-2$ determinations described in the last chapter) its logical consistency and mathematical justification remain a subject for controversies[77]. I will briefly review how the renormalization program is related to the vacuum concept in QED.

The vacuum is defined as the *ground state* or the lowest energy state of the fields. This means that the QED vacuum is the state where there are no photons and no electrons or positrons. However, as we shall see in next section, since the fields are represented by quantum mechanical operators, they do not vanish in the vacuum state but rather fluctuate. The representation of the fields by operators also leads to a vacuum energy (sometimes referred to as vacuum zero-point energy).

When interactions between the electromagnetic and the electron-positron field in the vacuum are taken into account, which amounts to consider higher order contributions to the S-matrix, the fluctuations in the energy of the fields lead to the formation of so-called virtual electron-positron pairs (since the field operators are capable of changing the number of field quanta (particles) in a system). It is evaluations of contributions like these to the S-matrix that lead to the divergencies mentioned above and prompt the need for renormalization in standard QED.

Thus, even though the vacuum state contains no stable particles, the vacuum in QED is believed to be the scene of wild activity with zero-point energy and particles/anti-particles pairs constantly popping out of the vacuum only to annihilate again immediately after. This picture might be seen as merely an artifact of a sophisticated mathematical theory but physicists tend to claim that some experimental verification of these features of the vacuum has been obtained. We will turn to these claims in the following section, which is based on the article [156] (reprinted in appendix F)[78]. Below, the QED vacuum concept is compared to that of an alternative theory, source theory. This comparison provides an excellent illustration of the underdetermination point with respect to the ontology associated with HEP[79].

[77]Some problems for renormalization and a possible new framework for dealing with QED has been discussed in my master thesis [205]. See also [27] (and below).

[78]The article [156], written together with Svend E. Rugh and Tian Y. Cao, also contains some notes on the development of the QFT vacuum concept since the advent of quantum mechanics.

[79]It should be noted that we have so far only addressed features of the QED vacuum. The situation is much more complicated in both electroweak theory, which demands the presence of a

5.3 The Casimir effect and the interpretation of the vacuum

The QED vacuum concept has sometimes been justified by appealing to the so-called Casimir effect which is the prediction of an attractive force between two electrically neutral and perfectly conducting parallel plates. The expression for this Casimir force is

$$\frac{\mathcal{F}}{\mathcal{A}} = \left(\begin{array}{c} \text{The force per unit} \\ \text{surface area between the} \\ \text{two parallel plates} \end{array} \right) = -\frac{\pi^2}{240}\frac{\hbar c}{d^4} \tag{5}$$

where $\hbar$ is the Planck constant, c is the finite velocity of electromagnetic propagation and d denotes the distance between the plates.

The Casimir effect is usually taken as important evidence for the physical reality of vacuum fluctuations and vacuum zero-point energy. Less well known is that the Casimir force can be derived from other points of view, some of which do not employ the concepts of vacuum fluctuations or vacuum zero-point energy. In [156], we briefly sketch and discuss some of these approaches and examine their implications for the current understanding of the vacuum.

An important reason for investigating the Casimir effect is its manifestation before interactions between the electromagnetic field and the electron/positron fields are taken into consideration. In the language of QED, this means that the Casimir effect appears already in the zeroth order of the perturbative expansion. In this sense the Casimir effect is the most evident feature of the vacuum. On the experimental side the Casimir effect has been tested on a number of occasions[80]. The main thrust of [156] is an examination of two essentially different ways of looking at the Casimir effect:

1. *The boundary plates modify an already existing QED vacuum.* I.e. the introduction of the boundaries (e.g. two electrically neutral, parallel plates) modify something (a medium of vacuum zero-point energy/vacuum fluctuations) which already existed prior to the introduction of the boundaries.

2. *The effect is due to interactions between the microscopic constituents in the boundary plates.* I.e the boundaries introduce something (the media) which give rise to the effect: the atomic or molecular constituents in the boundary plates act as (fluctuating) sources which generate the interactions between the constituents. The macroscopic effect (i.e. the macroscopic attractive force between the two plates) arises as a summed up (integrated) effect of the mutual interactions between the many microscopic constituents in these boundary plates.

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so-called Higgs boson even in the vacuum state, and in QCD where the coupling between the fields cannot be regarded as a small perturbation. For a semi-popular review of the vacuum situation in electroweak theory and in QCD, see [2].

[80] The first experimental support for the original suggestion by Casimir of the attraction between two neutral perfectly conducting plates were given by Sparnaay in 1958 [176]. The most precise experiment measuring the originally proposed Casimir force has been reported by Lamoreaux (1997) [108].

The first view refers explicitly to the omnipresent existence of a fluctuating QED vacuum or, at least, to a vacuum with non-trivial properties which would exist (with these properties) also in the absence of the modifying boundary plates. Depending on the origin of the forces between the individual constituents in the plates, the second view may or may not support or refer to the existence of a non-trivial vacuum. When the boundary plates are viewed as dielectrical materials, the plates are considered to be composed of atoms or molecules with fluctuating dipole moments (and, in principle, higher order multiple moments as well).

In [156] we illustrate the conceptual ambiguity of interpreting the very same quantitative force (5) by referring to four different interpretations of the Casimir effect[81]. The first interpretation is Casimir's original proposal in terms of vacuum zero-point energy of the electromagnetic field. Casimir's calculation (1948) is directly linked to the existence of a vacuum field between the plates. A second interpretation is Lifshitz's theory (1956, in English) where the Casimir effect is a limiting case (perfectly conducting plates) for macroscopic forces between dielectrica. Lifshitz theory employs a random fluctuating field in the plates whereas no explicit reference is given to an independent fluctuating QED vacuum field in between the plates. In fact, the electromagnetic field between the plates, which is generated by the fluctuations in the plates, is not treated as a quantum field, but as the solution to the classical Maxwell equations with sources generated in the plates. (However, as we discuss in [156], the argument has been put forward, that QED vacuum fluctuations are needed, indirectly, in order to sustain the fluctuating sources in the plates). As a third approach we mention, briefly, that a calculation of the Casimir effect can proceed perfectly well within standard QED in which a systematic normal ordering of the field operators has been carried out (see below). The fourth interpretation is based on Schwinger's source theory in which the vacuum is taken to be completely void (without fluctuating fields in the 'empty space'). For the purpose of this section is suffices to summarize the standard QED and the source theory interpretation of the Casimir effect.

5.3.1 The QED vacuum as a consequence of field quantization

In QED, the Casimir force is a consequence of the difference between the zero-point energy of the electromagnetic field with and without the plates. That a non-vanishing (fluctuating) electromagnetic field is present between the plates (and outside) is entailed by the standard quantum field theory procedure known as *canonical quantization* in which the field modes of the electromagnetic field are represented as a set of quantum harmonic oscillators. In this quantization procedure, the electromagnetic field is first confined in a 'quantization volume' V giving rise to a certain discrete set of mode vibrations (normal modes) of the field[82]. The field is then

[81]These four interpretations are representative for our purpose. In fact, there are more interpretations which are fully compatible with the quantitative predictions of the Casimir force, e.g interpretations in terms of 'stochastic electrodynamics' (see e.g. [128]) and in path integral formulations of QED [19].

[82]The utility of the harmonic oscillator picture rests on the *linearity* of the theory which is quantized and it finds application in the theory of electromagnetic interactions. By contrast, the theory of strong interactions, quantum chromodynamics (QCD), is a highly non-linear theory, and

Fourier expanded in terms of the normal modes and the coefficients (the amplitudes) in this Fourier expansion are replaced by operators, namely, annihilation ($\hat{a}_k$) and creation ($\hat{a}_k^\dagger$) operators, subject to a definite set of commutation relations. The *quantum state* of the field is specified by a set of integers $n_k = a_k^\dagger a_k$, one for each normal mode k (n_k is called the number operator and may be thought of as the number of field quanta in the mode k with frequency $\omega = ck$). The Hamiltonian, or energy operator, may be written

$$\hat{H} = \sum_k (\hat{n}_k + \frac{1}{2})\hbar\omega_k \equiv \sum_k (\hat{a}_k^\dagger \hat{a}_k + \frac{1}{2})\hbar\omega_k \tag{6}$$

where the sum is extended over all the possible normal modes compatible with the boundary conditions of the quantization volume. Contrary to its classical counterpart, each quantized harmonic oscillator has a zero-point energy ($\frac{1}{2}\hbar\omega_k$) and since the quantized electromagnetic field has an infinite number of modes, the resulting field energy is infinite.

The vacuum state of the theory is defined as the quantum state with lowest energy (i.e. the ground state of the theory). From eqn (6) we see that this is the state where there are no field quanta in any mode, i.e. $\hat{n}_k|0>= \hat{a}_k^\dagger \hat{a}_k|0>= 0$ (we shall return to the infinite value of the energy of this vacuum state shortly).

Moreover, when the field is quantized, neither the electric ($\boldsymbol{E}$) nor the magnetic field ($\boldsymbol{B}$) commute with the operator describing the number of photons in each field mode[83]. This means that when the number of photons has a given value (i.e. according to eqn (6), when the energy has a given, fixed value), the values of $\boldsymbol{E}$ and $\boldsymbol{B}$ necessarily fluctuate[84]. Equivalently, one may note that the commutation relations between the operators $\boldsymbol{E}$ and $\boldsymbol{B}$ precludes the possibility of having zero values for both the magnetic and the electric field in the vacuum state[85]. This is in sharp contrast to classical electromagnetism where $\boldsymbol{E} = 0, \boldsymbol{B} = 0$ is a valid solution to the Maxwell equations in the vacuum.

its quantum states, in particular its ground state, cannot with good approximation be expressed in terms of harmonic oscillators.

[83] See e.g. Heitler [86] p.64.

[84] In general, the meaning of 'fluctuations' in a quantum mechanical quantity $\hat{\xi}$ is that the mean value (expectation value) may be zero, $< \hat{\xi} >= 0$ but $< \hat{\xi}^2 >\neq 0$. Thus, fluctuations of $\boldsymbol{E}$ and $\boldsymbol{B}$ in the vacuum state refer to the situation where their mean values are zero, but the mean values of $\boldsymbol{E}^2$ and $\boldsymbol{B}^2$ are non-zero.

[85] The commutation relations between the field components of $\boldsymbol{E}$ and $\boldsymbol{B}$ may be inferred from the commutation relations for the creation and annihilation operators in terms of which the quantized electromagnetic field components are written (see e.g. [86] pp. 76-87). According to the commutation relations, field strengths at two points of space-time which cannot be connected by light signals (the two points are space-like separated) commute with each other. This means, that at a *given instant of time t*, field strengths in different space points commute. In a given space-time point the different field components does not commute, however, and the commutator is in fact formally infinite. In view of the commutation relations between field components of $\boldsymbol{E}$ and $\boldsymbol{B}$ Landau and Peirls (1931) and subsequently Bohr and Rosenfeld (1933) [13] set out to investigate the physical interpretation of these commutator relations. It is important to note that these considerations, e.g. by Bohr and Rosenfeld, are confined to an analysis not about the fluctuating QED vacuum (when left alone) but to what one may operationally measure (with the aid of a measurement apparatus, viz. various test charge distributions etc.).

It follows from this discussion that both the zero-point energy of the vacuum and the vacuum fluctuations are consequences of the quantization of the electromagnetic field. However, the zero-point energy has a more formal character and can be removed by reordering the operators in the Hamiltonian by a specific operational procedure called normal (or 'Wick') ordering [197] (placing creation operators to the left of annihilation operators). It should be emphasized that the ordering of operators in a quantum theory is quite arbitrary and is not fixed in the transition from the classical to the quantum mechanical description of, say, the electromagnetic field. The normal ordering amounts to a formal subtraction of the vacuum zero-point energy (a subtraction of a c-number) from the Hamiltonian. This leaves the dynamics of the theory and its physical properties, including the vacuum fluctuations, unchanged[86]. For example, the interaction between an atom and the electromagnetic field will remain the same irrespective of the choice of ordering of the operators. Nevertheless, the Casimir effect within standard QED is usually obtained by considering changes in the zero-point energy of the electromagnetic field, that is, before the normal ordering procedure has been carried out[87].

Besides canonical quantization, another standard quantization procedure is the *path integral quantization* in which the amplitude for a quantum mechanical process is written as a sum (an integral) over many 'histories' (paths). In several respects, the path integral quantization is equivalent to canonical quantization (for some differences between these approaches, see e.g. Weinberg [190], Sec.9). However, it should be noted in passing that the vacuum fluctuations in QED are entailed by a field ontology interpretation of the canonical formalism of QED. In the path-integral formalism of QED, it is possible to give a particle ontology interpretation, which, at least, will render the concept of the vacuum fluctuations ontologically unclear (see e.g. [29]). Conceptually, the path-integral formalism resembles Schwinger's source theory approach in the sense that its machinery for generating functionals (related to the Green's functions of the theory) utilizes the presence of sources[88].

5.3.2 Source theory approach by Schwinger

Schwinger's work on QED can be divided into two periods. In the first period (1947-67), he made important contributions to the renormalization program which was mainly operating within the framework of local operator field theory. In the second period (from around 1967) Schwinger, however, became more critical towards the operator field theory and the related renormalization program. According to Schwinger, the renormalization prescription involved extrapolations of the theory (QED) to domains of very high energy and momentum (or very small spacetime scales) which are far from the region accessible by experiments. Thus, in Schwinger's view, the renormalization program contained unjustified speculations about the in-

[86] The normal ordered Hamiltonian has expectation value equal to zero but $\boldsymbol{E}^2$ has a non-zero (in fact, infinite) expectation value implying that fluctuations are still present.

[87] Although, as described in [156], the Casimir effect can also result from a standard QED calculation in which normal ordering has been performed and where the constituents of the boundaries are taken into account. This calculation, however, is still based on the existence of a fluctuating vacuum field [156].

[88] See [156] for a review of the role of Green's functions in source theory.

ner structures of elementary particles and was conceptually unacceptable ([166], see also [27]). He was also very dismissive to an alternative to QFT, S-matrix theory, which was very popular in the 1960s, because it rejected any microscopic spacetime description[89]. His critique of the major research programs in the 1960s led to the construction of his own alternative theory, the source theory.

The source theory is "a theory intermediate in position between operator field theory and S-matrix theory..." ([166], p.37) restricting attention to experimentally relevant scales. Whereas the fields in QED can be expressed by operators capable of changing the number of particles in a system, this role is taken over by the sources in Schwinger's theory: "The function of K [a scalar source function] is to create particles and annihilate antiparticles, while K^* [the conjugate of K] creates antiparticles and annihilates particles." ([166], p.47). The calculational apparatus in source theory resembles closely that of standard QED (and even closer if QED is formulated in the path integral formulation) and thus can be used, for instance, to calculate the Casimir effect, which is to be expected if source theory is to obtain the same numerical success as QED has achieved.

The important conceptual difference between standard QED and source theory concerns the status of the field. In source theory, the sources are primary, they represent properties of particles in the particular experimental situation; and the fields are not independent of sources. Thus, conceptually, within the source theory framework, the vacuum must be empty: If there are no sources, then there are no fields. Hence there are no field fluctuations in the void[90].

Despite the emphasis on experimentally relevant scales, source theory has a vacuum concept that goes beyond mere experimental considerations. In Schwinger's words [168]:

> ...the vacuum is the state in which no particles exist. It carries no physical properties; it is structureless and uniform. I emphasize this by saying that the vacuum is not only the state of minimum energy, it is the state of *zero* energy, zero momentum, zero angular momentum, zero charge, zero whatever.

However, the usual interpretations of the Casimir effect posed a serious challenge to the source theory concept of the vacuum, and the major motivation for Schwinger's involvement in interpreting the Casimir effect was precisely, and explicitly, for defending his concept of the vacuum ([170] p.2):

> The Casimir effect is usually thought of as arising from zero-point fluctuations in the vacuum [reference to [16]]. This poses a challenge for source theory, where the vacuum is regarded as truly a state with all physical properties equal to zero.

In [156] it is reviewed how the Casimir effect can be derived within the framework of source theory, that is, without any reference to a fluctuating vacuum with zero-point energy. As indicated above, the difference between the conventional QED

[89] I return briefly to the S-matrix theory in section 5.5.

[90] In this sense the field concept in source theory is similar to Einstein's pre-QED conception (see last section) where the electromagnetic field is a consequence of charged matter. Thus, source theory rejects the 'dogma of QFT' (chapter 3) which holds that the fields are ontological primary.

approach and Schwinger's source theory approach is that in source theory the fields result from the sources and they are c-number fields (associating each point in space with a vector) rather than q-number (operator) fields.

It should be noted that Schwinger's dissatisfaction with renormalization and his emphasis on the empty vacuum are aspects of the same problem because, at least within standard quantum field theory, the conceptual basis for renormalization is the substantial conception of the vacuum. Since the focus here is on the Casimir effect, one can at first put renormalization aside because the Casimir effect is usually calculated only to the zeroth order of the perturbative expansion where no renormalization effect is to be taken into account. However, there are other effects, usually associated with the vacuum, which involve higher order calculations (such as the Lamb shift or the anomalous magnetic moment of the electron). On the one hand one may therefore say that, for instance, the Lamb shift is a 'deeper probe' of the QED vacuum structure because it involves aspects of the renormalization prescription (which accompanies higher order calculations in QED). On the other hand, it can be argued that the Casimir effect comes closest to the vacuum because only the oscillator ground state energy (a zeroth order effect) is involved.

In the framework of Schwinger's source theory, no renormalization procedure will be needed since the physical quantities, such as the electron mass, are fixed directly by experiments. This is conceptually different from the renormalization prescription in conventional QED in which the physical mass refers to the renormalized mass which is the sum of an infinite bare mass parameter and another infinite renormalization factor. Nevertheless, in order to account for higher order effects such as the Lamb shift and the anomalous magnetic moment, Schwinger does encounter infinities. Schwinger's treatment of these infinities is not fully transparent but he argues that they are removed by imposing certain physical requirements on the expressions[91]. Schwinger points out that the physical requirements are related to two features of source theory: First, the need of preserving the phenomenological description of initial and final particles in collisions as being without further interactions, i.e. the constraint that free particles do not have self-interactions ([167], p.20)[92]. Second, the theory is not extended to experimentally inaccessible regions and thus refrain from attributing physical significance to very large momentum values (or very small distance values) of the Green's functions ([167], p.41).

5.4 QFT vs. source theory

Insofar as source theory can explain the same 'vacuum' phenomena as standard QFT (at least those referring to QED, see below), the question naturally arises as to whether one should ascribe any reality to the QFT vacuum. Also it might be questioned if metaphysical speculations of the vacuum are necessary at all.

The first thing to note is that physicists stand on the vacuum concept may influence what problems in physics are considered relevant. Take for instance the

[91]These requirements are enforced on Schwinger's expressions by direct modifications of the divergent parts of the (higher order) Green's functions.

[92]This criterion has also been used in the formulation of an alternative to renormalization theory, see below.

problem of the 'cosmological constant' which may in some sense provide an 'experimental test' on aspects of the vacuum concept [156] (see also e.g. [187, 189]). The cosmological constant (Λ) refers to the term $\Lambda g_{\mu\nu}$ in the Einstein equations (which relates matter with the curvature of space-time),

$$R_{\mu\nu} - \frac{1}{2} g_{\mu\nu} R - \Lambda g_{\mu\nu} = \frac{8\pi G}{c^4} T_{\mu\nu} \tag{7}$$

and a non-vanishing value of Λ will have measurable consequences for astrophysics and cosmology (see also [30]). According to standard QFT, the cosmological constant (Λ) is equivalent to the summed vacuum energy density from any known (and unknown) quantum field. The essence of the cosmological constant problem is that any reasonable theoretical estimate of Λ lies at least $\sim 50 - 100$ orders of magnitude above any observed value (which is either zero or close to zero). In turn, this discrepancy can only be circumvented by assuming that the various contributions to Λ cancel each other to an extreme accuracy, which at present understanding of QFT seems absurd[93].

Clearly, the cosmological constant problem in this form is only relevant if the vacuum energy and fluctuations are ascribed any ontological significance, and thus 'empty space' is a scene of wild activity. This is illustrated by a quote of Weinberg who appears to be clear on the ontological consequences of the Casimir effect [187]:

> Perhaps surprisingly, it was a long time before particle physicists began seriously to worry about this problem [of the cosmological constant], despite the demonstration in the Casimir effect of the reality of zero-point energies [in the vacuum].

Taking the QFT vacuum structure seriously has also been used to suggest that the universe itself might be the result of vacuum fluctuations (see e.g. [2]). Moreover, some physicists have worried whether the universe may somehow be in a 'false' vacuum state and that some day a transition to the 'true' vacuum state could be realized. According to this argument that situation would be a disaster since the universe, as we know it, would cease to exist (see e.g. [149] p.16-24). Such grandiose cosmological speculations and worries are clearly also relevant only if the vacuum of QFT is interpreted realistically.

5.4.1 Ontology of QFT

While source theory suggests that QED's conceptual basis of operator fields leading to a non-trivial vacuum is not the only alternative available, the situation is less transparent when the attention is shifted to the whole QFT program which also includes weak and strong interactions.

[93]The cosmological constant problem does not, however, concern the "QED vacuum" in isolation. Other phenomena in modern quantum field theory, such as the process of spontaneous symmetry breaking (e.g. associated with the Higgs field) also contribute in building up an effective cosmological constant. The connection between the vanishing of the cosmological constant, which has been called a veritable crisis for high-energy physics [187], and the vacuum concept was a main focus in [155].

A full comparison between source theory and QFT (including also weak and strong forces) cannot be conducted as the source theory 'program' was never completed. For instance, Schwinger never gave an account of the strong interactions although the lack of a satisfactory theory for these interactions provided one of the main motivations for source theory [168][94].

But was this because a source theory account of the strong interactions is not possible in principle? Or because research into source theory was not given enough resources and/or interest? While it is not my task to attempt an answer to such questions it should be pointed out that source theory was launched around the same time as the proof by 't Hooft in 1971 of gauge theories being renormalizable (mentioned in chapter 4) which revived interest in field theories of weak and strong interactions[95].

Nevertheless, the insights of source theory have been important for some of the recent developments in HEP (see also [131]). For instance, the idea of effective field theories have been inspired by source theory ([27] p.68). Effective field theories are restricted to considerations in a relatively low energy regime and can therefore include non-renormalizable interactions (since no considerations of arbitrary high energy internal states are invoked). On the effective field theory view the Standard Model can be seen as merely a low energy approximation to some underlying theory. It is thus admitted to source theory that the renormalization procedure (involving considerations of internal states with arbitrarily high energy) might be an unreasonable move[96]. The effective field theory view has also been used to argue against the possibility of an underlying theory, and in favor of a 'pluralism' in ontology. According to this view every new energy scale probed by experiments will reveal new substructure of particles, and there are no reasons to believe that this will end, for instance, with strings being the ultimate structure (for more on the philosophy and motivation of effective field theory, see [27]). In this sense, the effective field theory view is just as phenomenological as source theory which restricts attention to experimentally accessible scales.

But effective field theories are still about fields so one should expect that the 'dogma of field theory' about the ontological primacy of fields over particles are retained in the 'effective' view. As mentioned earlier however, the path integral formalism, which somehow resembles source theory and is widely used in contemporary HEP, may be interpreted in terms of particles rather than fields[97]. Thus, the reality

[94] Nevertheless, Schwinger discussed, among other things, the deep inelastic scattering experiments from the viewpoint of source theory rather than the quark model ([61] p.393) (for more on the merits of source theory see [131]). It should be noted that there are no quarks in source theory: "He [Schwinger] could accept the electroweak synthesis (to which he contributed so much) but not quarks and QCD" [131].

[95] Moreover, the now accepted theory of the strong interactions (QCD) was given major boosts on both the theoretical and experimental side in the early seventies.

[96] Recall that high energies correspond to small distances so renormalization assumes complete knowledge of the internal structures of particles (or at least that this internal structure is not important to the renormalized results).

[97] Whereas both the path integral method and the source theory approach originate in a so-called quantum action principle, Schwinger, however, did not accept the path integral solution of the action principle as a satisfactory starting point of a theory. In Schwinger's view, sources and numerical fields provided this starting point (see [168] p.424).

of a substantial vacuum, connected with operator fields, and the speculation in some ultimate structures of matter may still be be open to source theory challenges also when the attention is on the QFT framework in its totality.

If one accepts this point of underdetermination of theoretical ontology on the basis of experimental evidence, a natural question is what other criteria are involved in theory choices. Before I turn to this question which couples to the discussion in chapter 2, I briefly mention two other candidates for alternatives to QFT.

5.5 Other alternatives to QFT?

The best known example of an alternative to QFT from the recent history of HEP is probably the S-matrix theory which was briefly mentioned above. S-matrix theory was initiated by Heisenberg in 1943 as a response to the crisis prompted by the divergencies in QED ([42] p.46). Heisenberg's hope was to free the S-matrix from its quantum field theoretical basis, and thus not to let it refer to the intermediate states as in the 'normal' QED S-matrix for scattering. For Heisenberg the S-matrix was to be used as a operational device which transformed initial states into final states with no physical interpretation associated to the processes in between. Nevertheless, the object was still to calculate transition probabilities from some general assumptions of the S-matrix[98].

After the advent of renormalization theory in the late 1940s, S-matrix theory fell into the background but it was resurrected in the 1950s, not least because of the non-renormalizability of the weak interactions and QFT's inability to give quantitative predictions for strong interactions. Although S-matrix theory, advocated principally by Chew, in the early 1960s had some experimental success in connection with the strong interactions, it soon lacked ability to supply new empirical predictions (partly because of calculational difficulties, see [42] p.54). For our purposes here it is sufficient to note that the S-matrix theory of strong interactions suggested a nuclear democracy in which no hadron was to be regarded as fundamental (in direct contrast to the atomistic picture offered by the quark model) and that no operator fields were employed ([42] p.56). Moreover, S-matrix theory in no way proved inconsistent (contrary to what QFT has been claimed to be due to the infinities) nor was it in conflict with any data, and it could also be argued to be conceptually simpler than QFT ([42] p.75).

Another candidate for an alternative to QFT, which also rejects renormalization theory has been formulated by Peter Zinkernagel (PZ theory) [205, 203]. In PZ theory, the removal of divergent contributions from the perturbative expansion bears some resemblance to the source theory approach since free particles are not allowed to have self-interactions [205]. However, PZ theory is formulated independently of source theory, and does not reject the operator formalism as such. Rather, PZ theory involves a reconceptualization of the space-time description of the intermediate states in scattering processes where the virtual states, in contrast to free states, modify themselves. Moreover, a criterion of simplicity is used explicitly in a

[98]These assumptions were analyticity (the constraint that the S-matrix elements are analytic functions of their arguments), unitarity (the constraint of conservation of probability) and Lorentz invariance (the constraint of special relativity) (see [42] p.47).

constructive manner to impose well-defined physical limits on otherwise divergent integrals. PZ theory has so far been used only to calculate the anomalous magnetic moment of the electron up to second order in the perturbative expansion (resulting in complete agreement with the standard QED calculation).

While I found in [205] that PZ theory is a viable, and perhaps even preferred, alternative to renormalization theory, its ultimate fate is unclear. For the purposes of this section it suffices to note that no serious consideration has been paid to the PZ theory in connection with presentations at various physics institutes. Reasons for this lack of interest probably include the fact that renormalization in itself is no longer considered controversial, and that PZ theory, like source theory, do not promise any experimental deviations from the highly successful standard QED[99].

5.6 Theory selection and reality

I mentioned earlier that the notion of objectivity is central to all realist attitudes to science in which science is assumed to establish knowledge about a mind-independent reality. Clearly, theory choice, and thus which theories are somehow supposed to be better representations of reality, should not depend too strongly on subjective judgment. As we saw in chapter 2, Kuhn suggested that the objective criteria in theory choice of accuracy, consistency, scope, simplicity, and fruitfulness cannot dictate a specific choice: the relative strength and concrete formulation of these criteria depend completely on subjective judgment which is historical contingent. I argued in chapter 2 that classical mechanics is in some sense immune to Kuhn's historicism. But what is the status of Kuhn's claim with respect to the above discussion of theories in HEP?

Schwinger was convinced that source theory was conceptually and computationally simpler than QFT ([168] p.413). We saw above that the same point, with respect to conceptual simplicity, has been argued for S-matrix theory, and it can also be argued for the PZ theory of quantum electrodynamics [203, 205]. Moreover, as we have seen in connection with the Casimir effect, the explanatory power with respect to experimental results in HEP (connected to the accuracy criterion) can hardly be the guide in theory choice. Of course, when accuracy is paired with scope — leading to a kind of fruitfulness — QFT is superior as it has produced far more results than any of the alternatives. But as indicated above this *could* have been an instance of sociological and historical factors dictating where the resources went, rather than the sign of QFT being a better theory. Schwinger, for one, was probably aware of this point when he stated his hope that source theory would be the way future students would learn QED ([168] p.413).

As we indicated in the last chapter this argument of fruitfulness as the guide in theory choice is exactly what Pickering pursues in [143]. Pickering describes how the emphasis of the physics community in the 1970s was shifted from the S-matrix theory to QFT where both theories and experimentalists could see how to "ply their trade better" by jumping the QFT wagon. Thus, while the physicists motivation may

[99] I would like to add that, while I do see minor internal problems for PZ theory (see [205]), this non-existing interest appears unjustified. The very possibility of alternative theories to standard QED served as a major motivation for the present project.

be metaphysical in spirit, it seems on the basis of the discussion above to be more instrumental in practice: Move on with whatever works (and Pickering smiles in the background). But a caveat is in order. Just because some theoretical frameworks do open up for new possibilities of experimental and theoretical investigations it does not follow that any theoretical framework can be made to be fruitful. Both the instrumentalist and the social constructivist are left with the question of *why* a particular theoretical framework is fruitful (even if other frameworks might also be so). Only some kind of realism seems capable of explaining this coincidence, that is, if there is no relation between the contents of a theory and reality, it becomes somewhat mysterious why it works.

However, it remains the case that just because a theoretical framework is fruitful it does not follow that each of its statements must be true (as Miller and Bullock wanted in the last chapter). Listen for instance to Cushing who, after reviewing the methodologies of S-matrix theory and QFT (in 1982), remarked ([42] p.78):

> When one looks at the succession of blatantly *ad hoc* moves made in QFT (negative-energy sea of electrons, discarding of infinite self energies and vacuum polarization, local gauge invariance, forcing renormalization in gauge theories, spontaneous symmetry breaking, permanently confined quarks, color, just as examples) and of the picture which emerges of the "vacuum" (aether?), as seething with particle-antiparticle pairs of every description and as responsible for breaking symmetries initially present, one can ask whether or not nature is *seriously* supposed to be like that.

The discussion in this chapter indeed suggests that the QFT picture of the vacuum, with profound cosmological consequences, may not be exactly what 'nature had in mind'. Moreover it may well be that HEP at the theoretical level has reached a degree of abstraction where the difference between realism and instrumentalism is not so clear cut. Nevertheless, the 'why theories work' question is still pressing. For this reason, studies of which parts of a theoretical formalism are understood realistically and which parts are more conceptual devices seem well worth pursuing.

In this chapter I have emphasized the differences among various theories of HEP but it should be kept in mind that there are also similarities. For instance, all of the theories discussed above employ Lorentz invariance, the fundamental symmetry of special relativity, and all are committed to the principles of quantum mechanics (for instance, one only calculates *probabilities* for microscopic processes). In this (admittedly weak) sense, the combination of the principles of special relativity and those of quantum mechanics appear to be 'conditions for descriptions' of HEP[100]. The situation also speaks in favor of the idea that Lorentz invariance and quantizability are structural properties which survives even when theories change, and might therefore be seen as real features of the physical world[101].

[100]Building on Bohr's insights, it was suggested in chapter 2 that classical mechanics forms part of the conditions for descriptions of quantum mechanics and the special theory of relativity. See also [204, 203].

[101]This is suggested by Cao [28] p.5 and is based on a study of the conceptual developments of 20th century field theories. Quantizability is a "...structural property of a continuous plenum, which is connected with a mechanism by which the discrete can be created from, or annihilated into, the continuous" [28] p.361. (for more examples of structural properties, see same page)

6 Conclusions

In the preceeding chapters I have touched upon a number of topics in HEP and philosophy of science. I will here give a brief summary of the discussion.

We started out by reviewing parts of the philosophy of science complex. In this connection, I suggested that the notion of objectivity, based on daily language extended with classical mechanics, is robust with respect to future changes in scientific theories[102]. The reason for making this point was that the historicism emerging from Kuhn has been claimed also to apply to objectivity itself. As already indicated, a stable notion of objectivity is required if we are at all to engage in epistemological discussions of science.

We then turned to the question of how some central points from philosophy of science influence the experimental and theoretical situation in HEP. Two things were pointed out with respect to the theory-ladenness of experiments. First, the experiments *do* depend on the theory under scrutiny. In the detector experiments, we saw that physicists must have some idea of what to look for in order to extract data. For the experiments on the anomalous magnetic moment the point was that these would not have been performed without the interplay with ever more refined QED calculations. In this connection, I re-iterated the standard argument — supported by both scientists and many philosophers of science — of increasing trust in an experimental result by reconfirmation, for instance, through variation of the experimental method. Nevertheless, we saw that the data analysis in detector experiments *might* be so intimately tied to the theory under scrutiny that an objective test of the theory is excluded. In particular, since contemporary detector experiments must rely on complex Monte Carlo simulations containing QCD assumptions, theory-ladenness becomes a greater concern here.

Second, with respect to the existence of the entities under study in HEP experiments, we investigated the criterion of manipulation — which might also help to free the experiment from the theory under study. To this end we noted that electrons can be manipulated in a sublime manner without reference to QED, whereas it takes (electroweak) theory to associate neutral currents with Z^0 bosons before one can speak of manipulating neutral currents, e.g. to examine quarks. In turn, due to the strong link between QCD and quarks (confinement), the latter cannot be manipulated. Consequently, it appears that the trust in the HEP-theory independent existence of these entities decreases from electrons to neutral currents to quarks[103].

When we turned to the underdetermination thesis, it became clear that there is more to worry about than the existence of entities. For instance, although the experiments on the anomalous magnetic moment makes the 'parent quantum field theory' QED the most precisely tested theory in physics, this does not imply that QED gives a true, or even approximately true, description of reality: Either the vacuum is a scene of wild activity as QED claims, or the vacuum is stone dead as

[102]In addition, this provided support for Bohr's conception of the necessity for retaining the framework of classical mechanics in our description of other physical theories, as more than a mere limiting case of quantum mechanics and the special theory of relativity.

[103]It should be noted that entities beyond the Standard Model, such as strings, are excluded in this comparison due to the lack of experimental indications for their existence.

held by source theory. Any speculations of an 'approximative empty' vacuum make little sense here. While this suggests instrumentalism as a viable compromise with respect to the vacuum, the question of *why* QED, electroweak theory, and QCD have been so empirically successful (theory-ladenness or not) is still in need of an answer.

Thus, HEP theories and entities must have *something* to do with reality. The discussion in this thesis has demonstrated that this 'something' is not a simple one-to-one correspondence between the statements of HEP and reality. Rather, I would argue for a *balanced* realism based on the important insights in both entity realism, structural realism, and a 'modified' version of constructivism. With the latter, a certain amount of constructedness in the scientific product is granted while it is nevertheless insisted that the knowledge production is constrained by a reality which is bigger than us[104]. Despite the concern for objectivity in complex detector experiments, theoretical presuppositions still do not guarantee the expected answers. For instance, the Higgs boson needed in electroweak theory has not been 'found' in experiments although most physicists expect it to exist. Moreover, it is reasonable to grant some ontological status to entities which can be manipulated (think of the lonely electron in the Penning trap). Finally, it seems hard to avoid the structural realism assertion that, at least, the combined principles of quantum mechanics and special relativity must somehow reflect properties of reality (in the light of the 'why QFT works' question).

In conclusion I would like one more time to address the question "Why worry about the philosophical aspects of a science which seems to do well on its own?". Aside from possible technological implications, HEP is often legitimized by appealing to the search for knowledge about the most fundamental parts of nature and the origin of the universe. In my view, it is both reasonable and challenging to ask to what extent we can expect sensible answers from a science committing itself to such grand metaphysical issues.

[104] Compare this to Radder's referential realism, see [148] and appendix D.

7 List of Works

The following is an overview of my work (reprinted in the appendices) on which the thesis, to a large extent, has been based:

- **A** 'Sociology of Science – Should Scientists Care?'. The article was written at an early stage of the project as a kind of answer to social constructivism. Printed in *EASST Review* vol.15, 3 (1996)

- **B** 'Conditions for Objectivity'. The article was written in connection with a course on 'Scientific Objectivity' given by Hilary Putnam and Peter Galison (Harvard University 1996). Subsequent discussions with Peter Galison on the subject are reflected in the discussion in section 2.7. To be submitted.

- **C** 'An Interview with C. LLewellyn Smith'. This was part of an original plan to conduct a number of interviews with various physicists and to deal more extensively with science policy issues.

- **D** 'Referential Realism and Appropriate Technology'. An invited review of Hans Radder's book *In an About the World*. Published in *EASST Review*, vol. 16, 3 (1997).

- **E** '$g-2$ and the trust in experimental results'. Written together with Benny Lautrup. Submitted to *Studies in the History and Philosophy of Modern Physics*, February 1998.

- **F** 'The Casimir effect and the interpretation of the vacuum'. Written together with Svend E. Rugh and Tian Y. Cao, Submitted to *Studies in the History and Philosophy of Modern Physics*, February 1998.

In addition, I have been involved in the following articles (not included here):

- 'Grænser for videnskab' [Limitations for Science] (written together with Benny Lautrup). The article sketches some limits to science posed by political and economic interests outside science proper and their relation to internal problems (theory-ladenness and complexity of the problems studied). Examples include the role of science in a major Danish bridge construction project, the magnetic moment of the electron, and contemporary HEP experiments. Printed in Aksel Wiin-Nielsen (ed.) *Prometeus vender tilbage* [Return of Prometeus], Teknisk forlag (1997). (The article was translated into the Swedish and reprinted in *Arkhimedes* [Finnish journal for physics and mathmatics], vol. 5 (1997)

- 'Højenergifysikkens verdensbillede – fup eller fakta?' [The worldview of High Energy Physics – Right or Wrong?]. This article reviews aspects of contemporary HEP and cosmology. It is pointed out that the philosophical aspects of these diciplines are rarely on the agenda in communications from science to the public. To appear in book on "Naturfilosofi" [Philosophy of Nature].

- ‘On the Cosmological Constant Problem and the Reality of Vacuum Fluctuations’ (written together with Svend Erik Rugh). A preliminary study of the origin of the cosmological constant problem and the experimental evidence for vacuum fluctuations. Contribution to the *Conference on Relativistic Astrophysics in Honour of Prof. Igor Novikov’s 60th Birthday*, Copenhagen, January 1996. Available upon request.

A Sociology of Science – Should Scientists Care?

— on the work on high energy physics of Karin Knorr Cetina[105]

Introduction

At least since the 1976 formulation of the strong programme by D. Bloor (Bloor 1976)[106] there has been an ongoing debate concerning a possible regress when accounts of science and scientific activity are given from the point of view of relativism[107]. In the sociology of science the question of regress is particulary relevant when the sociologist is labelled a 'constructivist' or a 'social constructivist', since relativism is often associated with these positions. This has to do with the enhanced role of 'judging' natural science which enters sociology of science when constructivist positions are acknowledged i.e. when the sociologist is arguing about truth or fact making in natural science - what 'truth' status should one assign then, to the argument itself. The intension and interests of sociologists of science may not include judging natural science or evaluating its epistemic credibility, but the claim of fact-construction (implying that facts could have been otherwise) - at least - challenges the picture of natural science as dealing with indisputable facts of nature. These aspects of sociology of science has led to heated discussions between sociologists - or rather STS people in general - and scientists (the situation is sometimes referred to as 'Science Wars'). Examples of these discussions include the exchange which took place in August 1994 in the British Association for the Advancement of Science between Harry Collins and Lewis Wolpert (reviewed in THES (Collins 1994)) and the debate following "Higher Superstition" (Gross 1994), e.g. on the electronic mailing list 'sci-tech-studies'.

This article will discuss an analysis of experimental High Energy Physics (HEP) carried out by a sociologist of science and some of the (philosophical) foundations on which constructivism is based. The discussion is from the physicists point of view - albeit not the view of an experimental high energy physicist but rather a theoretical physicist who is sympathetic towards the idea of 'construction' in experimental HEP. If there is indeed a war going on between STS people and scientists, I will attempt to remain in a quiet, reflexive, area of the battlefield.

A central question in this article is: Given that sociologists make claims about the nature of scientific activity, to what extent should natural science react to these claims? The article is divided into five sections which, apart from the introduction, include: First, a discussion of the frame of reference for constructivist science studies as represented in the works of Karin Knorr Cetina. Second, a brief account of my own views on experimental HEP. Third, a short review and discussion of the analysis on HEP given by Karin Knorr Cetina with respect both to my views and the claims

[105]Printed in EASST Review, Vol. 15, 1996

[106]All references in this appendix are given at the end of the appendix.

[107]This debate can be found in the literature on the history, philosophy, and sociology of science. Some recent examples are (Niiniluoto 1991) and (Collins and Yearley 1992).

of constructivism. Finally, I sum up and suggest an answer to the question posed in the title.

Social Constructivism or Constructivism

Karin Knorr Cetina is a sociologist of science who has had a major impact on constructivist ideas - a point which is illustrated by many references to her work and position (e.g. Callebaut 1993, Sismondo 1993 and Niiniluoto 1991). Reading her work on High Energy Physics presupposes some background knowledge in order to grasp what is contained by the notion of construction. Below, I will give a short review and some remarks on social constructivism and constructivism[108].

The central point in constructivism (which Knorr Cetina also labels 'constructionism' in Knorr Cetina 1993) is that scientific facts are constructed through negotiations, accidental events and interpretations. Scientific reality itself is also constructed by selective and contextual scientific laboratory practices. When one focuses on how the social and political environment contributes to the construction of facts, the programme may be labelled 'social constructivism' (the term 'constructivism' is usually linked to microsociological studies only, for instance laboratory studies). Acknowledgement of this position is often followed by some remarks about relativism and realism which can illuminate how 'strong' the consequences of the constructivist program are, e.g.:

"We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist. Our point is that "out-there-ness" is the *consequence* of scientific work rather than its *cause*." (Latour and Woolgar 1986, p.180)[109]

It is somewhat unclear exactly what is stated here - it is not a simple relativist position and the last sentence seems to be of anti-realist origin even though reality is not denied in the quote. The position may be conceived of as transcending the philosophical debate on realism vs. anti-realism by introducing 'irrealism', a version of anti-realism/relativism where scientists, through their work, create reality (which could have been different), see e.g. Hacking 1988. In any case it is obvious that the position expressed in the quote stands in sharp contrast to a traditional scientific intuition where an objective reality or out-there-ness is the ultimate cause for scientific findings and facts. A milder version of relativism is the 'methodological relativism' where the sociologist simply has a different interest than the scientist, namely to describe *why* the scientific community at a specific historical time trusted in a certain result instead of *whether or not* the result was true. Although these questions - context of discovery vs. truth - may be difficult to separate, this form of relativism is acceptable also from a traditional view of science since no scientist

[108]A more detailed account can be found in Sismondo 1993. Constructivism has been used in various contexts and with different meanings in the literature but Sismondo attempts to encompass these differences.

[109]In an interview with W. Callebaut (Callebaut 1993), Knorr Cetina takes the same position as Latour and Woolgar.

would disagree that some results have turned out to be correct despite the fact that they were originally considered wrong or vice versa. The relevance of methodological relativism may be granted also from a traditional point of view when scientists believe that truth will win in the long run. Existence of a final truth or a complete knowledge of how nature works is questionable even among natural scientists. A version of methodological relativism for laboratory studies is incorporated in the frame of reference of constructivism which can be seen from:

"The constructivist program has extended this idea by claiming that the information produced by science is first and foremost *the product of scientific work*, and what we should do is try to describe how scientific work produces scientific information, rather than locating the focus of the analysis between the finished scientific product and the world." (Knorr Cetina in Callebaut 1993)

In another quote, Knorr Cetina acknowledges also the stronger epistemic relativism which *"asserts that knowledge is rooted in a particular time and culture"* and *"...holds that knowledge does not just mimic nature and insofar as scientific realism wishes to make such claim, epistemic relativism is anti-realist"* (Knorr Cetina and Mulkay 1983). It is claimed here that knowledge does not *just* mimic nature. Now *if* scientific knowledge in part mimics nature, for instance with respect to knowledge reflecting the resistance of material objects (e.g. we cannot move through walls - except by using doors), then there are areas of knowledge which essentially cannot be otherwise. Construction of facts are therefore constrained by, not only social but also, natural factors or simply reality. Though Knorr Cetina states that *"constructionism holds reality not to be given but constructed..."* (Knorr Cetina 1995), she seems to acknowledge some sort of resistance from material reality:

"Constructionist studies have recognized that the material world offers resistances; that facts are not made by pronouncing them to facts but by being intricately constructed against the resistances of the natural (and social) order." (Knorr Cetina 1995)

Thus, when a 'resistance from material reality' is granted, the notion of fact-construction should not be so scary to scientists as is suggested when the word 'construction' is associated with hard relativism or an 'any theory goes'-position. Nevertheless, a quantitative question like "to what extent is nature mirrored by scientific knowledge?" remains important to the question of how scientists should react to the claims of constructivism.

Theory-dependent data analysis in HEP

I noted in the introduction that I am sympathetic to the idea of construction in experimental HEP. Let me briefly explain: Experiments in HEP are typically conducted by accelerating beams of particles to very high energies and then bringing them to collisions in a detector. The point is to analyze the reaction products of such collisions to determine the constituents of the original particles and the identity of new particles formed in the collision. From the collisions to 'sensible data', however, is a big step. Raw data from the detector are filtered through a multi-step selection process which aims to cut away data coming from malfunctions of the detector or data irrelevant to the physics one wants to study. Since theory dependent computer simulations play a central role in this data selection process, it follows that the final experimental results can be limited by the theory used in the simulations (which is often the theory one is testing in the experiments). Even though experimental physicists claim that they are able to take this limitation into account it remains unclear to what extent one can still talk of an 'objective' experimental test of the underlying physical theory. Note, that these concerns with construction in experimental HEP are aimed at the interpretational and technical aspects of the enterprise and do not focus on accidental events or social interactions in a laboratory such as negotiations among scientists[110]. Thus, the question posed here is: Does the technological set-up and the data handling processes in experimental HEP guarantee that physicists extract facets of reality or is it possible that the results are 'constructed' by adjusting the apparatus and data selection criteria, so as to fit a particular physical model?

From a constructivist point of view the experimental results are always dependent on both the theory used and specific microsociological events such as evaluations, negotiations, strategic alliances between scientists etc. However, whether one accepts a strong or weak form of realism, a necessary condition for natural science, if natural science is to be distinguished from pure religion, is the attempt or norm of objectivity. This holds irrespective of any sociological or historical constraints on natural science since what distinguishes religion from science is - at least - that the reality science operates within offers resistance. As argued above, scientific facts and theories are restricted by nature itself. Phrased differently, social constraints on scientific activity may be granted but these are not the only ones.

Experimental HEP according to Knorr Cetina

The following section is based primarily on chapter 3 in the book "Epistemic Cultures" (1994b) by Knorr Cetina titled "The Care of the Self: Particle Physics and its Negative Epistemics" and "The Disunity of Two Leading Sciences" (Knorr Cetina 1994a) appearing in "The Disunity of Science - Boundaries, Contexts and Power" by P. Galison and D. Stump. I will review some of Knorr Cetina's points in these texts, reflect on them in the light of my own views on experimental HEP and examine how Knorr Cetina's notions correspond to constructivist perspectives as outlined above.

The first point does not relate directly to HEP, but illustrates a difference in the underlying assumptions which can be found in sociology and physics texts. In "The Disunity of Two Leading Sciences" Knorr Cetina presents *"two stories about kinds of empiricism"*, and a comparison between the different cultures of experimental behaviour in HEP and Molecular Biology[111]. For a physicist, Knorr Cetina's use of

[110] Even though interpretations may depend on results of negotiations or social conditioned criteria for success.

[111] In this article I will concentrate on Knorr Cetina's discussion of HEP, leaving out aspects concerning Molecular Biology.

the word 'stories' may seem odd at first glance - is it not an attempt to give a correct description of scientific activity? The notion of a story could be a consequence of a relativist approach concerning the regress discussed in the introduction: The claim that facts are constructed and consequently could have been otherwise makes it difficult to offer conclusions on the basis of empirical investigations and even harder to make 'this is the way science really is' statements. Now, this may very well be besides the point of Knorr Cetina and perhaps it is also a trivial conclusion from constructivism that observations and facts in science studies could have been different. Nevertheless, I assume that the motivation, at least in part, for Knorr Cetina's 'stories' is to pass a message which may be relevant to other members of the community and that the message is based on empirical facts and their interpretations[112]. Texts can of course be relevant to someone without containing any facts, e.g. one may find a particular poem highly relevant, but facts are likely to belong to the characteristics of scientific language: Science - including social science - differs from poetry, for instance, in its use of logical and/or empirical arguments which include attempts to convince other scientists of a 'best' approach or, at least, that one's own position and/or studies are reasonable. Thus, scientific texts do contain facts regardless of their origin - that is - whether these are objective or a consequence of social practices (I use the term 'fact' in the sense of either an empirical or a logical fact). Constructivism is an approach to science studies where the interest is often on complexities and controversies in science. Studying complexities implies looking 'at' the complexities and in some sense the attempt to 'look through' the complexities seeking an understanding of them (for instance by deconstruction). Thus, the choice of constructivism as the theoretical framework for science studies seems to be an attempt to provide knowledge of scientific activity - knowledge which is supported by the arguments and/or empirical studies of the sociologist. In this sense, 'stories' seem inadequate as labels on constructivist studies.

HEP as a closed system involved with 'negative' epistemics

In The Disunity of Two Leading Sciences (1994a) Knorr Cetina motivates her work by emphasizing the differences between previous laboratory studies (by herself, Latour and Woolgar, Lynch and Traweek) and the recent ones carried out by herself. According to Knorr Cetina, the old studies assumed that different empirical sciences followed similar procedures in obtaining results (<u>the</u> scientific method). In addition, although they discussed the construction of scientific knowledge with respect to negotiations, accidental events and interpretations, they did not capture the construction of scientific knowledge through the empirical machinery. Knorr Cetina's recent studies aim to investigate what the old ones missed (as I have argued earlier, the machinery in HEP is closely related to the interpretation aspects). Knorr Cetina might understand my concerns for experimental HEP although, from the constructivist point of view, these concerns are not linked to HEP in particular but are rather a common feature of science in general. Nevertheless, HEP has features different from other sciences and Knorr Cetina sets off by comparing experimental HEP with the brain, arguing that:

[112] Assuming that some distinction between facts and interpretations can be made.

"...like the brain, particle physics operates within a closed circuitry. In many ways, it operates in a world of objects separated off from the environment, a world entirely reconstructed from within the boundaries of a complicated multi-level technology of representation." (Knorr Cetina 1994a p.3)

Knorr Cetina describes experimental HEP as mostly concerned with investigations of itself by studying the apparatus (limitations and malfunctions), and the various 'anti-forces' which complicates the extraction of sensible physics from the data samples (anti-forces include noise, smearing of the signals, and data from physical processes other than the ones the experiment aims to analyze). Another area of experimental work is 'limit analyses' which aims to put limits on possible knowledge: If a search for a particular particle is conducted in an experiment where a certain energy was available and the particle is not found, then it is concluded that the particle in question is most likely heavier (i.e. one has put a lower limit on the mass of the particle by not finding it in the energy regime investigated[113]). It is interesting to note that since experiments are mostly self-analyses and perhaps limit analyses in the above sense, HEP is left with the rather strange situation where the 'good' experiment (which either supports or falsifys a given assumption) is ruled out:

"Measurements in HEP appear to be curiously immature beings, more defined by their imperfections and shortcomings than by anything they can do.", "...Purely experimental data, as physicists say, 'means nothing by itself.'" and "...Experimental data are wholly, utterly dependent upon a particular detector configuration and on the criteria applied in extracting information out of the detector. Another detector, another set of criteria, yields other measurements." (Knorr Cetina 1994b p. 111)

The experimental strategies and procedures of HEP are referred to as the 'negative epistemics' of HEP due to the complex relations between signs and objects, the emphasis on self or inward analysis and the key role of 'negative' knowledge - *"knowledge of errors and uncertainties, of objects not of primary interest in a piece of research, and of self-limitations"* (Knorr Cetina 1994b, p.101). In the conclusion of her analysis, Knorr Cetina praises HEP for its emphasis on reflexivity (in the concluding section of Knorr Cetina 1994a): According to Knorr Cetina, HEP has turned reflexivity into a necessary principle for doing science by devoting many resources to self-understanding, self-observation and self-description. By referring to the problems of reflexivity in 'science studies', Knorr Cetina indicates that this field might have a lesson to learn from HEP and asks the question: *"Perhaps it would be time to ask if we have to have foundations, whether we cannot build a theory of knowledge from circular foundations?"* (Knorr Cetina 1994a). It is not clear exactly what Knorr Cetina implies by circular foundations but, most likely, it could refer to the absence of generally accepted criteria and methodological standards for 'how science studies should be done'. As part of the reflexivity in HEP, Knorr Cetina comments on how physicists deal with the problem of having available different theories to describe the same data. According to Knorr Cetina, the physicists invoke the different

[113]In physics mass and energy are equivalent.

theories as a source of error, and she indicates (Knorr Cetina 1994a p.11) that other sciences, including science studies, might benefit from using this approach instead of splitting the field and scientists into different groupings. The 'different theories' referred to here, however, are in fact different phenomenological models which are not intended to provide any physical explanation of the collision processes but they do play a key role in the data analysis. Moreover, the phenomenological models all have initial assumptions which are connected to the same overall physical theory - the general accepted 'Standard Model' for HEP. Thus, to label the different phenomenological models as different <u>theories</u> is an exaggeration and it is difficult to see how other sciences could benefit from implementing error considerations of the above kind[114] . Most scientists would agree with Knorr Cetina when she notes that *"scientificity consists in considering all theories available, provided they are not completely outdated by recent measurements"* (Knorr Cetina 1994a p.11) but, as I have indicated, it is not obvious in which sense 'theories' (phenomenological models) can be completely outdated by measurements in HEP.

The reality of HEP

In a passage on the objects that physicists work with (namely elementary particles), Knorr Cetina comments that these objects are, in a very precise sense, 'unreal' since *"they are too small to be ever seen except indirectly through a detector, too fast to be captured and contained in a laboratory space, too dangerous to be handled directly"* (p.4 in Knorr Cetina 1994a). This can hardly be an argument for the non-reality of physicists' objects: Does a 'real' thing need to be visible (the air)? How fast are things allowed to travel to be 'real'? Can something 'unreal' be dangerous? When Knorr Cetina adds that elementary particles are often very short-lived and so 'always rather history than present' one might ask how long something has to exist in order to be considered real? Obviously, experimental HEP objects are not as easy to access and comprehend as cups and tables but to note them as 'unreal' on the basis of the above is too drastic an interpretation. As argued by Hacking (Hacking 1982), one has to be a realist with respect to scientific entities when these can be manipulated in order to experiment on other entities, that is, when one has to rely on the causal properties of the first entities (Hacking uses the example of the electron). This is precisely the case in HEP when a beam of particles is prepared to investigate properties of other particles. Hacking's argument, however, leaves it open to how we should think about particles which are not used to manipulate other particles, for instance because they are very shortlived. Particles in HEP are usually 'seen' as peaks in certain diagrams (illustrating the outcome of an experiment). The physics argument for the 'realness' of these particles relies on what may be conceived of as their causal properties: 'Something' caused the peak and this something is called a particle. The question then is how can one be sure that the peaks are not just artifacts of the machinery (see below)? One can also ask about the preexistence of subatomic particles i.e. did they exist before they were 'discovered' in the experiment? On this point Knorr Cetina writes:

[114] A defense for Knorr Cetina on this point could be that the notion of 'different theories' seems to be taken from a physicist (Knorr Cetina 1994b, p.126)

”Preexistence itself is a historically variable phenomenon; what objects are thought to have preexisted changes with these cultural practices and with scientific belief. Thus specific scientific entities like subatomic particles begin to ’preexist’ precisely when science has made up its mind about them and succeeds in bringing them forth in the laboratory.” (Knorr Cetina 1993)

This is probably as close to scientific anti-realism as it gets since, obviously, if the particles did not exist before experiments are conducted, then the assertion of construction is unavoidable. To counter this position one can, aside from again arguing with Hacking, question what is meant by ’specific scientific entities’. Imagine travelling to an island which has not yet been explored. Now, if you meet a new animal and assign it a name then the name may not have preexisted but what about the animal itself? This example may have little to do with physics but in the light of the above quote it poses the question of how to distinguish entities which are ’specific scientific’ from those which are not. It is difficult to see the possibility for a position which acknowledges a ’daily-life’ realism towards cups and tables but claim anti-realism with respect to scientific entities since the borderline is very hard to identify: If a chair exists as a material object then how about things which are seen only through a microscope? If, on the other hand, constructivism implies a general anti-realism thesis, then, besides the problems of material resistance mentioned earlier, it becomes impossible for Knorr Cetina to claim that constructivism is interested only in the actual scientific work and not the relation between scientific objects and the world. A general anti-realism thesis implies a particular point of view on this relation. Nevertheless, I do think Knorr Cetina has a point where it is appropriate to question what is meant by ’reality’. This has to do with the relation between signs (or symbols) and objects in HEP: Are the symbols (the message one gets that a tiny current has run through some part of the detector) pointing back to the fact that the object (an elementary particle) really was in the detector? This is indeed a central problem since - as Knorr Cetina describes - signs from interesting events are smeared and mixed with other signs perhaps from uninteresting physical events or perhaps from malfunctions in the detector. Since contemporary theory is an ingredient in sign processing and data analyses, the possibility exists that the reality of the objects is ’constructed’ in accordance with the theories physicists believe. Thus, the machinery in experimental HEP might not just be an enlarged microscope.

How are facts constructed in HEP?

The claim that nature is a result of constructed scientific facts is probably the main point of disagreement between scientists and constructivists. However, a pragmatic interpretation may be agreed upon from both sides, namely that scientists understand nature <u>through</u> scientific facts (i.e. when the existence of electrons, protons and neutrons are established scientists say that material entities are composed of these particles). Nevertheless, most scientists would turn back and insist that scientific facts, including facts in experimental physics, are found or discovered in the realm of nature which is the cause and not the consequence of these facts. Even when scientists concede an approach to the history of science where 'discoveries' are anomalies which are later reinterpreted as discoveries, the construction of facts is not implied. Like the positivist tradition which grants irrationality in the context of discovery, the scientist may grant that discoveries are not simply 'stumbling over new facets of nature'. When it comes to the context of justification or the question of validity, however, I assume that most scientists would shy away from the notion of construction, for instance, by referring to repeated experiments which either support or falsify a given discovery. As described earlier, the constructivist approach focuses on negotiations, accidental events, interpretations and in addition to this, Knorr Cetina takes into account the machinery of HEP (Knorr Cetina 1994a,b). It is not clear in Knorr Cetina's work exactly how all these factors enter the fact construction process in HEP but in any case she labels the experimental strategies and procedures as cultural preferences. In the end of Epistemic Cultures (Knorr Cetina 1994b) a physicist P comments on the text, and it seems that his main objection is exactly this 'cultural preference' point. Physicist P argues that experimental HEP does not have a choice as to which strategies and procedures one should use:

> "But it is clear to me that we cannot do our work in any other way. If we did not use these methods we would not generate any results. They (other sciences) can obviously do without this obsession with errors, for example. While we could not possibly. There is no cultural choice for us in this."

This comment of P can be read as a defense against constructivism: When P argues that physicists have no methodological choice, then the notion of construction is weakened since if the facts could not have been obtained in any other way, then the role of, for instance, negotiations becomes diminished. Moreover, if the way facts are obtained in HEP is the only possible way, then it looks like it becomes a matter of taste if these facts should be regarded as constructed or indisputable statements of nature since no one would be able to tell the difference. Nevertheless, the position of physicist P seems too simple, especially if P implies that the world of HEP with all its experiments, methods and machinery is completely predetermined. To deny the role of cultural choices in HEP, which may or may not be rational choices, seems radical: Negotiations concerning which aspects of HEP are most important to investigate and which methods and equipment are to be used do play a role. Meetings are held where decisions are made from among a number of possibilities. In any case the assumption - that given a certain amount of money and a certain number of physicists with such and such abilities at a given time in history, would definitely lead to a particular way of performing science - places very high bets on rational choices in science. In the light of P's remark, there is an important lesson to be learned from Knorr Cetina's study which experimental high energy physicists could most likely benefit from. This is her focus on the importance of the distinction between the design/construction of the detector (the apparatus) and the physics analysis. Knorr Cetina points out that this distinction penetrates all experimental work: Participants in the experiments, various steps in the project, and data itself, are always either 'detector related' or 'physics related'. This lack of distinction leads to a different way of thinking at the two levels of the experimental procedure according to Knorr Cetina. Even from a traditional realist account of science, this makes the transition from experiments or data to the support or falsification of some theory in HEP very difficult. In this regard, it is worth noting the very complex situation in experimental HEP where hardly anyone has the possibility of understanding all the details in an experiment (400 coworkers in an experiment is not unusual at CERN - the European center for HEP where Knorr Cetina did her fieldwork). This point alone is certainly a problem for experimental HEP since it is difficult to determine completely where the facts came from. Thus, I agree with Knorr Cetina that there are cultural influences in experimental HEP like in any other field but, of course, this does not imply that the facts obtained are culturally or socially determined. As far as I can see, the notion of construction in the work of Knorr Cetina on HEP is somewhat ambiguous. It is not clear how much is implied by the construction metaphor or what the role of a material world independent of science is (recall Knorr Cetina's quote where she notes that scientific facts are constructed against the resistances of the material world). This ambiguity may be illustrated by contrasting the claim that nature is the consequence of scientific facts with the following two quotes:

"...particle physics is perfectly capable to derive truth effects from its representing operations." (Knorr Cetina 1994a p.3)

and:

"...if one asks a physicist in this area he or she will say that the goal of it all remains to catch the (positive, phenomenal) particles which are still on the loose, to measure their mass and other (positive, phenomenal) properties, and nothing less. All other things are ways and means to approach this goal. There is no doubt that this goal is indeed what one wishes to achieve, and *occasionally succeeds in achieving*, as with the Nobel prize winning *discovery* of the vector bosons at CERN in 1983." (Knorr Cetina 1994a p.9, emphasis added.)

Knorr Cetina states that 'truth effects' are derived by replacing the care of unreal objects (only a very limited amount of time in the experimental procedure is devoted to the actual study of elementary particles) with *"the care of the self"* (analyses of how the detector works) (Knorr Cetina 1994a) but she does not explain what she means by truth effects. According to constructivist ideas the notion of 'truth' cannot refer to truth outside the detector, or at least not to truth outside the minds of the physicists. But in this case one can hardly talk about success in catching positive, phenomenal particles[115], or discovery of new particles. It could be that Knorr Cetina is simply using words that a physicist would use in describing what he/she is doing but it is, at best, a very confusing language.

[115] the word 'positive' refers to the confirmation of actual particles as opposed to the negative knowledge i.e. all the experiments aiming to limit what one can observe.

Summing up, moving on

I have argued that Knorr Cetina and constructivism cannot claim disinterestedness in the relation between the scientific product and the world, and at the same time claim that scientific facts are the result only of negotiations, accidental events and interpretations. Thus, the core of constructivism (as I see it) is not reasonable. At the same time, I have focused on the problems of experimental HEP which, once recognised, give credit to constructivism. In this sense the realist/objectivist position of science is not threatened. On the other hand the focus on what goes on in science reveals aspects of this activity (here HEP) which should be taken seriously as limitations for scientific knowledge - not least by scientists.

Steve Fuller (Fuller 1994) and Steven Weinberg (Weinberg 1994) have had discussions in 'Social Studies of Science' where, after reviewing Weinberg's book "Dreams of a final theory", Fuller poses the question: *"But can scientists and science practitioners go beyond mutual fear and suspicion - and towards public-spirited debate?"*. Weinbergs reply to Fuller (Weinberg 1994) seems to me both a much too quick reply to science studies and as emphasizing charges of misreading rather than concentrating on the central issues at stake. Another event which may also be seen in the light of 'Science Wars', took place at a session at the 4S meeting in Charlottesville 1995, where D. Haraway (1995) and the authors behind "Higher Superstition" (Gross 1994) focused almost entirely on misreading of each others texts.

One reason for the controversy between science studies and scientists may be linked to the distinction between natural and social science as 'hard' and 'soft' science respectively. Though the validity of this distinction may be questioned (a strong social constructivist would probably deny it due to the claim that all science is social), it is difficult to avoid since natural scientists from the very beginning of their careers are taught that what they deal with most of the time are brute facts. If, on the other hand, a career in social science implies professors and books which have a variety of ways of looking at the same subject, cultural clashes such as 'Science Wars' are to be expected. Nevertheless, since there are both natural and social constraints to science, both the context and content of science are worthwhile studying, and though these two aspects are mutually dependent, it is not only unfruitful, but wrong, to claim that one determines the other.

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B Conditions for Objectivity

Introduction

This is a short paper reflecting on a course in scientific objectivity given at Harvard by Hilary Putnam and Peter Galison in the fall 1996. Putnam's lectures focused primarily on Wright's book 'Truth and Objectivity' [201], where Wright attempts to provide 'neutral' ground for debates between realists and anti-realists. Galison's lectures were a guided tour through the history of objectivity, with emphasis on the different meanings this notion has had at different times. In the following, I will use Daston and Galison's text in "Representations" [44] as a starting point for some remarks on possible conditions for the discussion of objectivity. In particular, I will review a Danish angle on the concept of objectivity as expressed in the philosophy of Peter Zinkernagel, with some remarks regarding the philosophical context of his work[116]. I will also discuss how the philosophy of Peter Zinkernagel is connected, and in some sense extends, the philosophical views expressed by Niels Bohr[117].

Objectivity according to Daston and Galison

During the course we were presented with examples of different approaches to objectivity - or modes of conceiving objectivity - from Bacon to Descartes to Kant (though the distinction between objectivity and subjectivity, as we know it, is a relatively recent invention). As a particular aspect of the concept of objectivity, Galison (referring, in part, to a text co-authored by Lorraine Daston [44]) has focused on the distinction between objectivity and subjectivity in connection with visual representations of scientific results. Through examples, Daston and Galison have demonstrated that the concept of objectivity - in visual representations of scientific results - has changed significantly during the last two centuries, from a 'truth-to-nature' approach (allowing for artistic intervention in representations), via 'mechanical objectivity' (having the machine as the ideal - excluding fallibility of the senses), to a 'judgment-to-the-audience' (acknowledging a need for audience-judgment of the representations) [44].

An 'image of objectivity' connected to representations of scientific results (such as atlases - the key example for Daston and Galison) need not be connected with a more general notion of objectivity, e.g. in association with questions like "are the social sciences objective?". One can, however, speculate whether a contingency in representational objectivity implies some sort of contingency for the concept as a whole[118]. Indeed, Daston and Galison acknowledge the possibility for a generalization of representational objectivity: "...we address the history of only one compo-

[116] His original views are given in the book 'Conditions for Description' [202] from 1962. Though the main points of this book - to be discussed below - have remained intact, there has been some development of his views which are not translated into english [204]. The views expressed here are my interpretation of Peter Zinkernagel.

[117] In [202] p.11 it is noted that "Our epistemological attitude is in accordance with the attitude expressed in the papers of Niels Bohr".

[118] The emphasis on differences in general notions of objectivity, facts and truth from Bacon to Descartes to Kant suffice, of course, to justify such an assumption

nent of objectivity, but we believe that this component reveals a common pattern, namely the negative character of all forms of objectivity" ([44] p.82). The 'negative character' of a form of objectivity is explained as the struggle against some sort of subjectivity, for instance, personal idiosyncrasies, mistrust of the senses or biases from theory.

Despite the suggested contingency of at least one form of objectivity, the concept as a whole has, as Daston and Galison describe, been identified with 'a view from nowhere' in the twentieth century (Thomas Nagel [136]). Daston and Galison warn against such a one dimensional aperspectival view - removed as far as possible from subjective perspectives - since objectivity has been used in many different modes and therefore there can be no single definition of the concept. Nevertheless, Daston and Galison's emphasis on the general 'negative' character of all forms or modes of objectivity seems to coincide with Nagel's aperspectival view[119]. If Daston and Galison are right in their characterization of objectivity as a historically contingent concept, a natural question is whether normative considerations of objectivity are out of reach i.e. if it is at all possible to have a 'transcendental' notion of objectivity. Presumably, Nagel would not reject a historical development of the concept of objectivity. Despite this, his characterization of objectivity as the view from nowhere, and as a method to reach that view (by gradually detaching the subjective in an enquiry) seems to transcend any historical issues. If the notion of objectivity, however, is truly contingent, any discussion of a 'right' definition or content of the term will likewise be contingent. However, as I will discuss below, there might be arguments that the notion of objectivity does include transcendent features. In effect, the present text is an attempt to take one step backwards and investigate what conditions there might be for discussing objectivity in an 'objective' manner.

Prerequisites for discussing objectivity

Consider, as an example of objectivity discussions, Galison's and Daston's text in "Representations" and the following, admittedly very simple, reconstruction of their work. Daston and Galison have an idea that the term 'objectivity' has carried different meanings at different times or, more specifically, that representational objectivity is a nineteenth-century category. The origin of their idea - or its precise formulation - need not concern us here: at least in principle, it seems possible to undertake an investigation without explicit ideas of what kind of argument will result[120]. Whether such an initial step is present or not, the next step is to look up various source materials (influential in some way, or characteristic of the period under study, since a general conclusion would otherwise be hard to obtain) and examine the explicit or implicit notions of objectivity and subjectivity. By showing that these notions were employed and evaluated differently in various epochs, Daston and Galison can make

[119] Moreover, Nagel does distinguish between various forms of objectivity. For instance, in an argument against reducing all phenomena to the physical realm ([136] p.27), he refers to both a 'physical' and a 'mental' objectivity.

[120] This is akin to Shapin and Shaffer's approach in "The Leviathan and the Air Pump"[172] where they attempt to forget all of their present knowledge and regard the Boyle/Hobbes debate in a symmetrical way to avoid using the hindsight of the winning side.

their point on the contingency of representational objectivity.

It is clear that Daston and Galison - like anyone who does science - have made judgments on what evidence to present, what notions to use, how exactly to formulate the argument etc. Nevertheless, they argue for a particular view, and the examples presented are meant as evidence in favor of such a view. But if there is no aperspectival notion of objectivity (in a general sense), to what extent then can Daston and Galisons argument be anything but their own subjective statement? A response from the authors could be that anybody is free to check their references and thus arrive at the same conclusion, but this merely strengthens the point that the argument has some sort of objective validity. Is the argument valid also tomorrow? next year? next century? My guess would be that Daston and Galison would retreat from their position (that the notion of, at least a particular mode of, objectivity is historically contingent) - or at least be willing to question its general validity - only by the appearance of new studies which illustrated that other influential texts from the periods discussed did in fact employ the same notion of objectivity. In this sense, their own argument concerning objectivity is historically contingent. Besides, I assume that the authors consider their argument valid in an aperspectival sense.

Why is it at all interesting to examine the assumptions of Daston and Galison? Could one not contend that the reflexivity discussions within the history of science or science studies in general have been taken far enough[121]? Taking the risk of beating a dead horse, I do think that Daston and Galison provide reasons for examining their assumptions. This can be illustrated with the following quote ([44] p.82):

> As historians of objectivity, we will not be concerned with recent controversies over whether objectivity exists and, if so, which disciplines have it. We believe, however, that a history of scientific objectivity may clarify these debates by revealing both the diversity and contingency of the components that make up the current concept. Without knowing what we mean and why we mean it in asking such questions as "Is scientific knowledge objective?" it is hard to imagine what a sensible answer would look like.

An agnostic position towards the existence of objectivity seems to lend itself to the question of the validity of scientific arguments. Leaving aside the fact that Daston and Galison are primarily concerned with representational objectivity, the implied possibility of non-existence of objectivity is a serious matter. It could be that an agnostic or methodic relativist position towards objectivity is taken merely as a challenge to the adequacy of the one-dimensional objectivity/subjectivity dichotomy (which would seem justified from the study of representational objectivity, since different kinds of subjective factors have been opposed to the objective at different times, [44] p.82). In any case, any argument aiming to clarify debates on objectivity must presumably rest on some more or less transparent conditions which makes it reasonable for others than the authors.

What is it then, that guarantees the robustness of an argument? Can one point to some necessary conditions for an argument to be valid or to make sense? It seems

[121] In a similar way to Shapin and Schaffer who in "The Leviathan and the Air Pump" [172] want to do sociology of scientific knowledge instead of discussing the possibilities for this kind of studies.

that formal logic provides a prime example of such a condition[122].

History of formal logic

Now, are the rules of formal logic historically contingent or are they valid in some sort of transcendental sense? Surely, there has been development in the concept of formal logic since the original formulation by Aristotle[123], most notably, perhaps, with the introduction of symbolic logic in the late nineteenth century. But have the essentials of formal logic changed with refinement of the rules? It is hard to see how one should make sense of such an assertion, in particular if we understand it in a strict sense: at one time it was meaningful to be self-contradictory, later it was not. This is not to say that self-contradictions must always be avoided. Of course, in daily life, we contradict ourselves all the time: "I want to go to the movies, and yet - at the same time - I don't want to go to the movies" can be a perfectly meaningful statement for instance pointing to a situation where it is raining and that the person speaking has not yet made up her mind of whether the movie is worth the effort. A scientific text on the other hand better not contain explicit self-contradictions unless the precise meaning is pointed out (as I just did for the movie example). The reason seems obvious: scientific texts differ from poetry, for instance, in its use of logical and/or empirical arguments which include attempts to convince other scientists of a 'best' approach or, at least, that one's own position and/or studies are reasonable. Clearly, if an author is trying to convince her readers of a particular point, self-contradictory statements are a bad strategy. In fact, the need for avoiding self-contradictions extends much further than the example of a scientific text: whenever we want to communicate meaningfully in a logical or practical way, we better do it - as far as possible - unambiguously. In some sense then, formal logic establishes a demarcation line between meaningful communication and its counterpart.

Though the rules of formal logic may not be contingent, they nevertheless have a history and have played a key role in intellectual development. With some right, it can be claimed that formal logic has shaped our (western) culture as the foundation on which rational thought has been built. Though written down explicitly by Arisotle (see [161]), it is hard to imagine Aristotle as crucial for the validity of the concept itself: any man selling horses in ancient Athens must have known not to contradict himself, if he wanted to survive as a salesman. Such an example might suggest that the law of contradiction is merely a practical rule. As will be discussed below, however, the law of contradiction can hardly be seen as merely a practical rule of thumb. What the example does show is that an explicit formulation of a rule for meaningful communication need not be available in order for the rule to be

[122] By formal logic I understand the law of identity, the law of contradiction and the law of the excluded middle. I shall not enter the discussion on whether the law of the excluded middle should or should not be regarded as necessary for the validity of arguments - a point disputed by e.g. the intuitionists, see Dummett [51] p.9 (I know even less of the Hegelian system which at the outset apparently also dismisses the law of contradiction - see e.g. Scholz' critical remarks [161] p.19). In the discussion below, reference to formal logic will primarily be a reference to the law of contradiction.

[123] The name 'formal logic' was first used by Kant, see e.g. [161] p.15.

valid. Thus, the knowledge of the horse seller, presumably, had the form of implicit or tacit knowledge.

That conditions for meaningful communication can be tacit is not surprising and it is in fact quite fortunate that we are able to use language without knowing explicitly the conditions for using language. Take the first point about lack of surprise. Presumably, children are not taught all the rules for using language explicitly, but learn language through hearing and practising (though they may be corrected some times when making mistakes). That it is also a fortunate circumstance (that we often do not know the conditions explicitly) follows from the fact that we cannot know if we have recognized a sufficient set of conditions for meaningful communication, but only that we - perhaps - know some necessary conditions. The situation may be likened to being able to ride a bike, without knowing explicit the physical laws governing the motion. Intuitively, we know the necessity of keeping the bike in motion (unless we are bike artists), but it is certainly not required to have ever heard of the moments of inertia which make the bike much more stable in motion.

At this point one might ask: if formal logic represents necessary conditions for meaningful communication, could there not be other such conditions? Indeed, from the assertion that formal logic represents necessary conditions for meaningful communication, it does not follow that there could not be more conditions of this kind.

Conditions for description

The Danish philosopher Peter Zinkernagel has been concerned with such conditions for meaningful communication or 'Conditions for Description' - the English title of his book [202]. If such conditions exist, they are general language rules which - just like formal logic - must be observed when one wants to describe or communicate information in an unambiguous manner[124]. In the following, I will discuss Peter Zinkernagel's candidates for such general conditions for description[125]. Moreover, I will try to indicate points of intersection between his theory of conditions for description and other philosophical directions, in particular the views expressed by Niels Bohr. In what follows, I will not distinguish between 'meaningful communication' and 'unambiguous description' (it is my impression that such a distinction is unimportant for both Peter Zinkernagel and for Niels Bohr, see also Favrholdt [58] p.69).[126]

The philosophy of Peter Zinkernagel has some points in common with the later Wittgenstein, in particular with the emphasis on ordinary (or everyday) language. For instance, Peter Zinkernagel writes: "In recent years, a number of philosophers have begun to attach increasing importance to ordinary language; the reason for this is, in our opinion, the kind of insight which emerged in Wittgenstein's later

[124] That language in order to be meaningful must also be consistent with certain grammatical and syntactical rules is irrelevant for the present discussion.

[125] This short essay cannot serve as a comprehensive introduction to the philosophy of Peter Zinkernagel - general references are [202] and [92].

[126] Thus the discussion here is about what may be called logical or practical language and not, for instance, poetic language: poetry can be meaningful without being unambiguous.

period: no matter how we intend to explain and discuss what is correct and what is incorrect, we must, in such discussions, make use of language." ([202] p.104). The central idea is that the language rules - or conditions for description - form the basis for correct use of ordinary language. Since all descriptive language - e.g. descriptions of physical experiences - is ultimately to be related to ordinary language, the rules of language are preconditions for any meaningful description in science. The significant role of ordinary language was also strongly emphasized by Bohr. In summarizing what may be called Bohr's conception of conditions for description, John Honner [90] p.14 writes:

> It is a (necessary) condition for the possibility of unambiguous communication that (suitably refined) everyday concepts be used, no matter how far the processes concerned transcend the range of ordinary experience.

Suitable refinements of everyday language mean that the concepts should be used in accordance with classical physics. The argument for the necessity of ordinary language and the concepts of classical physics is, in Bohr's words, "...simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics." [11] p.39.

Elsewhere, Bohr expresses the necessity of an ordinary language description of experiments even stronger by denoting it "a clear logical demand" [11] p.72. When Peter Zinkernagel states conditions for description, it is thus an attempt to make explicit what is to be understood by 'unambiguous language'. Moreover, the formulation of conditions for description is an attempt to give a precise formulation of Bohr's logical demand ([202] p.120). I have already indicated some points of agreement with the later Wittgenstein, but there are other sources of inspiration in the theory of conditions for description ([202] p.9): "In so far as we consider it right to talk about the logical conditions for describing experience, we are on a par with Kant; still, we do not ascribe to sense perception (*Anschauung*) the epistemological significance he attached to it. It is only natural, when we consider the development that has taken place in the last century in the fields of mathematics and physics, that we should deem it wrong to identify our powers of understanding with our powers of visualization"[127].

In a study of the relation between Bohr's views and those of Kant, John Honner has labelled Bohr's views 'transcendental' [90]. This is to be understood in the sense that Bohr - like Kant - was concerned with "the necessary conditions for the possibility of (experimental) knowledge" [90] p.1. Regardless of whether Bohr's views may, for this reason, be called 'Kantian', it seems clear that some useful analogies may be drawn. Consider, for instance, Kant's distinction between things in themselves (noumena) and things as they appear to us (phenomena) guided by the categories of understanding [94] p.265. When Bohr describes the situation in quantum mechanics, he also makes use of a distinction between properties in themselves and as

[127] The last part of this quote refers, at least partly, to the situation in quantum mechanics where direct sense impressions of the objects under investigation are ruled out.

they appear to us through experiments: "...no result of an experiment concerning a phenomenon which, in principle, lies outside the range of classical physics can be interpreted as giving information about independent properties of the objects, but is inherently connected with a definite situation in the description of which the measuring instruments interacting with the objects also enter essentially." [11] p.26. It should be mentioned in connection with this quote that it does not obviously follow that Bohr subscribed to some sort of idealism towards the atomic objects 'in them-selves'. Whether Bohr may for this reason be called realist or not is controversial as witnessed e.g. in Faye's and Favrholdt's contributions in [57][128].

When discussing Bohr's views, it is important to distinguish the classical case from the quantum mechanical. Perhaps this can be expressed by saying that there are differences in the 'quality' of our knowledge. It seems that, for Bohr, the classical framework is compatible with a simple correspondence between reality and our conceptions of it (see e.g. [10] p.54). Far from denying the (subject-) independent existence of atoms, it is simply our possibilities of experience that change at the quantum level. An example of this is the claim that one cannot give a simple pictorial representation of electrons orbiting nuclei, corresponding to the fact that one cannot ascribe simultaneously both definite location in space-time and definite momentum to electrons.

For Peter Zinkernagel, the question of things 'in them-selves' is in any case intimately tied to language. Language and reality co-exist so to say. A correct statement about reality is a correct statement about language and vice versa. This does not imply, for instance, that the earth did not exist before humans and their language, but that we - if we are to talk meaningfully about this fact - are committed to use language in a certain way.

Rules of language

Having now given some of the background of Peter Zinkernagel's thinking, we can formulate his attempt to state necessary rules for unambiguous use of descriptive language. The rules of language in "Conditions for Description" read[129] ([202] p.51):

> 1. We must not use names of ordinary things and expressions for possibilities of action independently of each other.

[128]As Folse remarks in the same volume, this debate is strongly influenced by the exact location of the division line one draws between realism and anti-realism. For instance, an anti-realist may accept that the world exists independently of our minds, but deny that the notion of truth is independent of our ability to establish whether something is true or not (the principle of transcendence of truth conditions) [57] p.128

[129]With the formulation, a disclaimer is given (p.52): "It will hardly be necessary to emphasize that this is merely the first attempt at a formulation which we have no reason to regard as definitive". For a discussion of the precise formulation of language rules see [92] p.47. (a contemporary exposition of Peter Zinkernagel's view's are so far only published in danish [204]). Though the formulation of the rules have been refined over the years , the general argument in favor of conditions for description remains unaltered (for instance 'personal pronouns' have been substituted with 'persons', forming the rule 'We must not use designations of persons independently of designations of bodies' [204]).

> 2. We must not use psychological expressions [*] independently of the personal pronouns.
>
> 3. We must not use the personal pronouns independently of designations of bodies and, in consequence, of names of ordinary things.
>
> [*] By psychological expressions we understand such expressions as are most often used to indicate states, or contents, of consciousness, e.g., 'think', 'feel', 'see', 'enjoy', 'experience', etc.

The point is that these rules cannot be denied neither explicitly nor implicitly in a description. Thus, to qualify as a general rule of language, it must be demonstrated that violations of the rule will be in conflict with the use of ordinary language. Moreover, by breaking the rule, it must be shown that the words cannot be used in a well-defined manner, hence making the language unsuitable for description and unambiguous communication.

Just as formal logic may be seen to establish necessary relations between words like 'both-and' and 'not', the language rules above establish necessary relations between e.g. 'things' and 'possibilities of action'. Thus, to use the words indicated by the language rules in a well-defined manner in a description, the rules - if they are condition for descriptions - must be observed. I will now review some of the examples which Peter Zinkernagel ([202]) and Klaus-Henrik Jacobsen ([92]) discuss to illustrate the necessary conditions for description.

To indicate what is included in the assertion of a necessary relation between possibilities of action and things, it is illustrative to discuss the following example of a situation from everyday life ([202] p.57): imagine sitting at a table on which there is an object (for example an ashtray). In this case, there are things which we can do and things that we cannot do. If we want to be consistent with everyday language we can, for example, move the object with our hand, but not move our hand across the spot where the ashtray stands without moving the ashtray. In other words, we have certain possibilities of action. Should we choose to move the ashtray, our possibilities of action are changed: now we can move our hand freely across the spot where the ashtray used to be, but not across the spot where it stands now. This may appear a trivial example, but it shows that in order to use ordinary language consistently, names of 'things' and expressions for 'possibilities of action' cannot be used independently of each other.

Before discussing examples of the second and third language rule, it is relevant to review Bohr's conception of the object/subject distinction. As argued by e.g. Favrholdt [57], the possibility of drawing a clear line of separation between the object and the subject is for Bohr a prerequisite in both classical physics and ordinary language. At the quantum level this clear line of distinction is complicated due to the necessity of incorporating the measurement apparatus in the description of the atomic phenomena. The subject can, however, neither on the classical nor on the quantum level be excluded completely since "...as human beings and subjects with faculties of cognition we are part of the world we explore" [57] p.87. To describe the peculiar situation in quantum mechanics with respect to the subject/object distinction, Bohr sometimes used an analogy from psychology. According to Bohr, the problem of observation in human psychology is elucidated by the difficulty of distinguishing between the phenomena themselves, e.g. feelings, and their conscious perception ([11] p.27). In the same connection Bohr remarks "Every unambiguous communication about the state and activity of our mind implies, of course, a separation between the content of our consciousness and the background loosely referred to as 'ourselves', but any attempt at an exhaustive description of the richness of conscious life demands in various situations a different placing of the section between subject and object." [12] p.13. As far as I can see, the second language rule above implies a separation between the content of our consciousness and 'ourselves' (as persons) since it is clearly not the case that psychological expressions are identical to personal pronouns. On the other hand, this separation cannot be total since psychological expressions and personal pronouns cannot be used independently of each other. By virtue of the third rule a similar relation exists between the subject and the object. Hence, one may regard the second and third language rule as substantiations of Bohr's remarks on subjects and objects.

As an illustration of the second language rule, consider the well-known experience of having had a dream ([202] p.71 and p.73). It is clear that we, if we want to use ordinary language meaningfully, must imply that somebody (e.g. a person) had the dream: there was a dream, but nobody had it, is clearly at variance with ordinary language. This is an illustration of the second rule connecting psychological expressions like 'having a dream' with personal pronouns (or simply persons). Naturally, this holds for other psychological expressions too, e.g. the expression 'having an experience' is meaningful only when it is assumed that someone had the experience. The third language rule may be illustrated through the same example by noting the conflict with ordinary language in a statement such as 'last night I had a dream but I did not have a body' or 'last night I had a dream but I was at no particular place'. Despite the fact that dreams refer to psychological phenomena, it is - as far as ordinary language is concerned - meaningless to refer to dreams or psychological phenomena without referring to persons with bodies situated in space (thus referring to the body as a material object).

In connection with the second rule of language, it is at this point, if not before, natural to ask questions like "Is it not meaningful to speak of a rabbit having an experience?". An answer to such a question might be that the interest lies in conditions under which we can describe phenomena in an unambiguous manner for which the possible experiences, or states of consciousness, of rabbits are quite irrelevant. If we replace neural networks, or computers, for rabbits, however, the answer may not be as simple (due to the 'could we all be brains in a vat'-scenario discussed in [146]). As far as I understand Peter Zinkernagel, the point is that since computers are not persons, 'states of consciousness' and 'experiences', if assigned to computers, mean something altogether different from what persons can have. Nevertheless, it seems that the relation between language rules and concepts, for instance 'personal pronouns' or 'persons', needs further clarification. What is at stake here is the relation between logic and meaning. Dummett asserts that the correct logic will follow from a theory of meaning ([51] p.14). Peter Zinkernagel's theory of conditions for description puts the logical relations (rules of language) before a theory of meaning. However, it seems that both positions need refinement: one cannot discuss a theory of meaning without presupposing some rules for discussing. On the other hand, the focus on necessary (logical) relations between concepts presupposes some stability of the meaning of these concepts.

In any case, if conditions for descriptions are preconditions for the meaningful employment of words in a description, preconditions establishing necessary relations between different concepts, then there can be no definitions of fundamental concepts independent of other concepts or independent of the language rules. Specifically, it cannot be possible to give ostensive definitions of all concepts, completely independent of other concepts, since this would violate the principle just stated[130]. To appreciate further the necessity of language rules, it is instructive to consider an example of the presuppositions involved in ostensive definitions. Imagine that we want to define the term 'chair' ostensively, and hence proceed by pointing at a chair while proclaiming 'this is a chair'. In order first to see how formal logic is presupposed in such a statement, we just have to add - while pointing - 'and this is not a chair'. Clearly, by saying 'this is and this is not a chair' we make the definition unintelligible, hence unsuitable in any description. Thus, though formal logic is not directly involved in the ostensive definition of a chair it nevertheless follows that we have to observe (that is, not be in conflict with) the rules for using the logical constants ('and', 'or', 'not' etc.) if we are to have meaningful definitions of words.

Now, the question is whether the rules expressed in formal logic are the only rules of language which have to be presupposed in ostensive definitions. For instance, we can consider the following additions and alterations to the original sentence 'We can point at a chair and say: This is a chair.' (quoted from [202] p.81):

'We can point at a chair without doing anything.'

'When we point at a chair, we do something, yet we do not alter our possibilities of action.'

'In pointing, we alter our possibilities of action, yet we do not alter them by moving a finger, or by moving something else.'

'We can point without having a body.'

'We can point at a chair which does not limit our possibilities of action.'

None of these sentences are unintelligible from the standpoint of formal logic. All of them, however, are unintelligible from the standpoint of ordinary language and hence we cannot ascribe meaning to ostensive definitions without presupposing the general rules of language as stated above. This suffices to make plausible that we in order to use our words in a well-defined manner have to observe certain general features of language, among others, the rules expressed in formal logic.

The generality of language rules

For conditions for description to be general and fundamental rules, they cannot, of course, be dependent on specific definitions of e.g. things being impenetrable (which might be suggested when discussing possibilities of action in relation to things). If that was the case, the conditions themselves would have preconditions and hence could not have the generality claimed. Another example will illustrate that the

[130] Also the later Wittgenstein objects to the general applicability of ostensive definitions, see e.g. the discussion in [92] p.24 (an ostensive definition of a thing - e.g. an apple - refers to a definition constituted by pointing to the thing in question).

rules above are invariant under alterations of definitions of things (adapted from [92] p.53):

The question of generality of the language rules may arise, for example, while discussing whether a cloud is a thing or not and hence is affected by the rules stated above. As argued by Klaus-Henrik Jacobsen, we may choose to think of a cloud either as a fog - which we can move through - or as an object at a certain distance from the earth which we cannot know how will affect our possibilities of action. In the first case of the cloud as a penetrable fog, we may observe that the distinction penetrable/impenetrable is about possibilities of actions for bodies e.g. if the fog is penetrable, a body can be moved through it. Thus, the distinction penetrable/impenetrable - as an expression for possibilities of action - in everyday language presupposes designations of things, in agreement with the first rule stated above. In the second case where we conceive the cloud as an object far away from us, we imply that some distance can be stated, e.g. that the cloud is 15 miles from the surface of the earth. But the earth is an object which does limit our actions. In other words, when we say that something is 'far away' it presupposes some sort of distance. This, in turn, presupposes a distance to another object, namely an object which does limit our actions. Therefore, it does not matter whether or not we call a cloud a thing. To quote Klaus-Henrik Jacobsen: "Regardless of whether one would call a cloud a thing, a material object, or not, the first linguistic rule's claim that the concept of thing and the concept of possibility of action are interdependently connected concepts would seem at any rate to be presupposed in every such discussion." ([92] p.53).

The language rules as stated above may not be surprising, since the examples I have mentioned as violations of the rules are what most people will regard as violations of common sense. Nevertheless, stating language rules explicitly indicates that, not only do the rules represent common sense, but they are in fact necessary conditions for unambiguous communication. At this point it is valuable to point out why the theory of conditions for description, while building on the insight expressed by the later Wittgenstein, also deviates from his philosophy, in particular with respect to the strong analogy between language and games. Whereas we can easily imagine changing a rule in a game (for instance, in the game of chess) we cannot imagine changing the rules of language. This is perhaps easiest to see in the case of the law of contradiction: altering or abolishing the law of contradiction implies making the language unsuitable for unambiguous description or communication or, to quote Peter Zinkernagel: "The part played by the law of contradiction in a language seems impossible to understand except by reference to the descriptive function of the language; hence the apparent difficulty of regarding the law of contradiction on the analogy of chessrules." ([202] p.31). Whether the same circumstances apply to language rules such as the above stated demands a somewhat longer discussion and I shall just refer to Klaus-Henrik Jacobsen's treatment [92] p.66-67 (where it is concluded that the language rules outside formal logic are equally impossible to abolish). In any case, as the examples above illustrate, it is by no means obvious what one should understand by unambiguous language if the rules were not observed.

There are several other issues which I have not touched upon here but which are nevertheless important in connection with the theory of conditions for description. One is the question of the relation between the different conditions e.g. are some of them more fundamental than others? Another question is how exactly descriptive language can be demarcated from non-descriptive language. Both these questions are treated in detail in reference [92]. The goal here has solely been to make plausible the general claim of the theory of conditions for description: that we in order to use language in an unambiguous and meaningful way to communicate and describe have to observe certain general language rules, among others those expressed in formal logic.

The problem of the external world

One of the original aims for the philosophy of Peter Zinkernagel was to solve or rather dissolve the Berkeleyian problem of the existence of the external world. From the brief presentation of conditions for description given above, I can hardly assume that the theory has been presented in a convincing manner. Nevertheless, it is interesting to note the consequences of the assumption that the language rules stated above are necessary conditions for meaningful description. Dummett has remarked that solutions to the problem of the external world rarely have satisfied anybody but the authors who provided them ([51] p.19). Despite the lack of solutions to the problem, philosophers have been reluctant to accept that we cannot know if things in front of us exist or not. For instance, one finds the following quote in the preface to the second edition of Kant's *Critique of Pure Reason*: "However harmless idealism may be considered in respect of the essential aims of metaphysics (though, in fact, it is not thus harmless), it still remains a scandal to philosophy and to human reason in general that the existence of things outside us (from which we derive the whole material of knowledge, even for our inner sense) must be accepted merely on *faith*, and that if anyone thinks good to doubt their existence, we are unable to counter his doubts by any satisfactory proof." (p.34 in [94])

In *Meditations*, Descartes established the link to the external world, and thus the independent existence of material objects, with the help of God. The goodness of the perfect God was, for Descartes, incompatible with the idea of minds only being deceived to believe in material objects. If, however, Descartes proof for the existence of God fails, then he has no way of establishing, with absolute certainty, the existence of the material world. To see the implication of language rules on Descartes problem, we merely have to consider his famous line "I think, therefore I am". With this statement, Descartes thought it possible to conceive of himself as a thinking being independently of his body or of the existence of material things. Consider, for example, the following quote (from the John Veitch translation [184]):

> "And although I may, or rather, as I will shortly say, although I certainly do possess a body with which I am very closely conjoined; nevertheless, because, on the one hand, I have a clear and distinct idea of myself, in as far as I am only a thinking and unextended thing, and as, on the other hand, I possess a distinct idea of body, in as far as it is only an extended and unthinking thing, it is certain that I, [that is, my mind, by which

> I am what I am], is entirely and truly distinct from my body, and may exist without it."

Thus, one may ascribe to Descartes the view that 'I think' can be said independently of 'things exist' (or 'bodies exist', in so far as bodies are things) or that there exists no necessary relations between 'things' and 'I'. If, however, it is impossible to use, meaningfully, designations of things independently of persons (implied by the personal pronoun 'I'), then Descartes' assumption must be wrong.

Following in the footsteps of Descartes, Berkeley contended that psychological expressions, like 'see', 'feel', 'hear' etc. could be used independently of the existence of things. From the second rule of language, however, we can speak meaningfully of sense impressions (or experiences) only in connection with personal pronouns (or simply persons). Indirectly, Berkeley accepts this condition for description by acknowledging the relation between the mind and the sense impressions - the sense impressions take place in the mind (if one identify Berkeley's 'mind' with Peter Zinkernagel's 'person'). As Descartes, however, Berkeley is at variance with the third language rule, relating persons to bodies, and thus to material things.

In this sense, the problem of the external world is not 'solved' but rather dissolved: if there are conditions for meaningful use of language the formulation of the problem becomes meaningless. In other words, the language rules prohibit any sensible argument in favor of an idealist position: one must assume the existence of an external world to say something meaningful about it.

Classical mechanics as rules of language

I have indicated above that it is probably impossible to know how many conditions for description there are. I will now turn the attention to another language rule candidate related to the situation in classical physics which, as described above, is a refinement of ordinary language[131]. The rule which I will mention here is related to the Kantian response to Hume with respect to the causal laws: all effects have a cause. For Hume, the general validity of such a statement was but a habit of our experiences, since it could not be derived from any formal logical considerations (see e.g. [94] p.44). Hume thus contended that we cannot know whether the causal laws will be valid also in the future: although we have seen billiard balls move according to causal laws many times in the past, we cannot be sure that they will do so also in the future. Kant, on the other hand, argued that the concepts of space and time, and, cause and effect have an *a priori* status[132]. Thus, if empirical knowledge about objects (billiard balls, for example) can be understood only if the objects

[131] The general situation in physics has been elaborated in [204]. In that book, Peter Zinkernagel argues that laws of nature are necessary relations between different quantities and discuss such laws within the theory of relativity and quantum mechanics. Within their field of application the laws of nature represent conditions for meaningful description.

[132] Space and time are forms of sensibility (i.e. preconditions for sensations of objects) whereas cause and effect are categories forming the necessary basis from which objects must be viewed to become objects of empirical knowledge. Both space-time and cause-effect, however, relate to objects in an *a priori* manner cf. [94] p.121

are situated in space-time and subjected to causal laws, it does not make sense to question whether causal laws will be valid also for future experiences with objects.

Such a conclusion can be strengthened by stating the laws of classical physics as necessary conditions for description or meaningful communication[133]. Such a 'language' rule has, of course, a limited field of application which can be demarcated quite clearly: as a rule governing classical phenomena where the quantum of action can be neglected. When discussing causal laws, however, one also needs to take into account the special theory of relativity. Though this theory changed the absolute character of space and time, the situation as regards to causal laws remained essentially unaltered as noted by Bohr: "...as stressed by Einstein, the space-time coordination of different observers never implies reversal of what may be termed the causal sequence of events, relativity theory has not only widened the scope but also strengthened the foundation of the deterministic account, characteristic of the imposing edifice generally referred to as classical physics" [12] p.2.

I noted above that classical physics - for both Niels Bohr and Peter Zinkernagel - is a conceptual refinement of the ordinary language. This implies that classical physics is also a conceptual refinement of the language of things[134]. If this is the case, it is reasonable to claim that the laws expressed in classical physics state general conditions for description also in ordinary language.

To illuminate such a suggestion, consider the following formulation of Newton's first two laws: If we do not alter the state of motion of an object by exposing it to some force, the object will continue to be in that same state of motion. Seen as a language rule, this means that we in order to assume that we can stop e.g. a ball by exposing it to a force, we must also assume that the ball will continue its motion (remain in the same state of motion) if the ball is not exposed to a force. A natural objection to such an idea is that contrary to what we might say, all experience does, in fact, point in the direction that objects are capable of altering their state of motion independently of whether they are exposed to some force or not (referring indirectly to frictional forces). As far as I understand Peter Zinkernagel, however, the point is to focus on the possibilities for characterizing non-uniform motion - changes of states of motion - in an unambiguous manner. To do this one must assume a distinction between non-uniform and uniform motion[135]. Thus, when one refers to changes of states of motion, the concept and possibility of conserved states of motion are implied. In this sense, there is no conflict between our daily life experiences of apparently spontaneous changes of states of motion and Newton's laws[136].

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[133]This is suggested in [202] p.223 and developed further in [204]. Classical physics is here taken to be the laws of Newtonian mechanics, that is, the laws of motion governing material objects

[134]Recall that the rules for ordinary language include conditions for meaningful use of designations of things and that motion of things is what is dealt with in classical mechanics.

[135]That uniform motion perhaps only exists as an idealization, except in the special case of the object being at rest (relative to some frame of reference), is not a problem. The situation is analogous to the idealization of geometrical points without extension: though such points are an idealization, and all physical points (which we could draw on a blackboard for example) do have extension, we can speak unambiguously of extension only in the light of geometry, see the discussion in [202] p.3.

[136]In effect this means that Newton's laws must be presupposed if we are to discuss motion of

If classical physics states necessary conditions for description, it has important bearings on our knowledge of the future - that is - what we can meaningfully say about it. Assuming that the laws of classical physics are necessary conditions for meaningful communication the statement 'we cannot be certain the sun will rise tomorrow' (or that we cannot be certain that the laws of classical physics are valid also tomorrow) becomes meaningless[137]. If language rules establish necessary relations between different concepts we must say that the laws of classical physics establish necessary relations between concepts like 'mass', 'force' and 'state of motion'. Thus, by referring to the sun and earth as massive objects, we imply that their motion is governed by classical mechanics. Moreover, classical physics provides us with the necessary conditions for understanding, for example, the concept 'tomorrow': we can only refer to 'tomorrow' in an unambiguous manner by referring to time in the sense of ordinary language. Since classical physics is a refinement of this language, a meaningful use of the word 'time' implies reference to some sort of mechanical system (a clock)[138].

Objectivity

The question is now to which extent the theory of conditions for description has any bearing on the question of the contingency of the notion of objectivity. To answer this question it is illustrative to review Bohr's conception of objectivity. A clear indication of his view is found in [12] p.10, where Bohr talks about physics: "In this respect our task must be to account for such experience in a manner independent of individual subjective judgement and therefore objective in the sense that it can be communicated in the common human language."[139].

Bohr's account of objectivity fits into the 'negative' character described by Daston and Galison or Nagel's 'view from nowhere': objective knowledge is knowledge which is independent of subjective judgment. If the language rules exist as general conditions for unambiguous communication then they give a somewhat different understanding of the concept of objectivity. According to Peter Zinkernagel by objective knowledge we "must understand knowledge which cannot be meaningfully denied..." [202] p.1. Examples of objective knowledge then, are the language rules insofar as they cannot be meaningfully denied if we are to use language in an unambiguous manner.

This notion of objectivity is not in contradiction to Bohr or Nagel's concept aim-

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objects in an unambiguous manner. Perhaps this point of view implies that Aristotelian physics is ambiguous since it fails to characterize the distinction between uniform and non-uniform motion (?)

[137] Disregarding possibilities such as explosions in the core of the sun, which have, of course, nothing to do with the laws of classical physics within their field of application.

[138] Though quantum mechanics and the theory of relativity employ different concepts of time and space one can again refer to the Bohrian argument for the necessity of our experiences being formulated ultimately in classical terms.

[139] As Favrholdt shows in [58] p.69, Bohr's emphasis on the communication aspect of objective knowledge, does not imply that Bohr contrast personal conditions for experiences to those of a community: the criterions for making sensible descriptions on the personal level are the same as what may be communicated.

ing at an - as far as possible - elimination of subjective elements in a description. Rather it is what could be called the strongest mode of objectivity. A fact may be objective in the sense that it is independent of where or by whom it is formulated, but it must be formulated in accordance with the language rules in order to be meaningful. It may be possible to criticize or even deny a fact as objective, for instance by questioning whether the fact was established under good enough epistemic conditions or not. For instance, if a man was under the influence of alcohol at a time he claimed to have seen a pink elephant in a park, one may question the objective validity of facts about pink elephants in parks. On the other hand, if language rules are indeed the conditions for meaningful description, it does not make sense to criticize or deny them (disregarding questions of their exact formulation, or, for 'language' rules within physics, their range of application).

For the specific question of the 'image of objectivity', that is, in which sense objectivity is employed in visual representations, the theory has little to say. However, any scientific argument concerning objectivity must, according to the theory of conditions for description, ultimately rest on the language rules. More specifically, if every account of the possible contingency of 'visual' objectivity must observe a set of language rules in order to be sensible, the theory does provide a suggestion to a non-contingent, or normative, notion of objectivity. In this sense an agnosticism towards the concept of objectivity is not tenable, i.e. the question whether objectivity exists is not meaningful if objectivity is what cannot be denied: any argument in favor of an agnostic position must rest on relations of both formal and non-formal logic (language rules).

In conclusion, it must be recognized that the status of the language rules has not been elaborated in detail in the present paper. Nevertheless, it is obvious that our ability to communicate and describe meaningfully - at least to some extent without ambiguity - is part of the prerequisites for scientific work to be interesting, be that natural science, social science or whatever.

C An interview with C. LLewellyn Smith

The following is an almost verbatim transcription from the tape recorded during an interview with Christopher LLewellyn Smith, the Director General (DG) of CERN August 25 1995. The text has not been edited much, only punctuation has been added. Also a few things have been left out, mostly because of repetition. The following are included as editorial remarks: [d-t-h]: difficult-to-hear words. [nq]: non-quotation; a summary of actual DG statements which, however, are not easily transcribed. [text] are words which I included for readability of the text. [...] is neutral and means that the DG is taking a small break. [HZ] in the text are my questions/comments during the interview. The DG repeated my questions (from a letter send to him prior to the interview) just before answering.

<u>question 1</u>

Possible problems with the high complexity of the experiments. To what extent can we trust that the experimental results of CERN represent properties of Nature bearing in mind the model dependence of these results. In other words, how do you view the influence of the Standard Model and Phenomenological Models on data selection and analysis (theory-laden-ness)?

DG: I think the first problem [...] the problem of the interpretation and the expectations determining what you then discover basically is a problem in the sense that: In the old days when you looked at bubble chambers for example - we had nice girls who went and look for specific things they were told to look for - but the physicists could also go and look at every picture if they wanted to, so you could discover something unexpected. Now, there is absolutely no way - you're already, at the time you take the data, deciding what to record with a trigger so there is certainly a worry [...] [that] it's very difficult to find things which you are not looking for - something about nature, let's say.

On the other hand it's also true, that in the old days you had no theory - so you really had to look [whereas] now you do know that the Standard Model is, more or less, correct so it is not such a big problem because you have some indications where the deviations might be, but it is certainly a worry. If [the] Higgs boson, for example, has very unexpected decay modes [...], in a model which nobody thought about, the detectors may just miss it because they are looking for specific decay modes - that's one worry. The other problem is that the analysis is entirely dependent on these huge Monte Carlo programs for the background - we are talking about looking at incredible small signals - a Higgs boson of 800 Gev that decays into four muons is something like 1 event in 10^{14}. Now at that level there really are sev... very rare and unexpected things going on so you worry first that the input to the MC really is sophisticated enough to have those very unusual [...?d-t-h] events - which could be background - but maybe it isn't such a worry, because once somebody finds a signal - then they really start worrying about those background.

There is also the worry that these MC programs - with their thousands of lines of codes written by many generation of graduate students - who remembers what was written into it? And there is a problem about correlation. A problem with the lab experiments that some of the parts of the MC programs - the event generators - are common to the different experiments. As far as possible one has to try to keep the experiments independent - not just physically independent - but if they are sharing the same software there is a danger also that they look like independent confirmation of the same effect [...] because you misestimated the background from the same program - it's a problem. That is certainly a worry if you like, but I don't think it is a trem... it is not a serious enough worry to say 'you should give up, it's hopeless' and I certainly don't take the point of view that - I don't know what it is called philosophically - that the theories we have are not real theories at all [ie.] they are just biased by our sociology etc. - I think there are real facts out there which we are discovering. I don't know if that's the sort of questions you are trying to get an answer to? *HZ: Sure.*

<u>question 2</u>
The economical perspectives of CERN. How do the physicists arguments for the continued operation of CERN relate to the arguments used in political discussions, especially in relation to the focus on 'spin-off' products of CERN?

DG: I think the first point you have to make about CERN is - people say it's very expensive [but] compared to what? The cost of CERN is about the cost of a typically medium to large sized European university - so, if you like, the 19 countries of CERN have said: We will fund, on top of the usual universities, one more university between us - which is supplying facilities for all the other ones - at least it's not a university as far as the funding is concerned. So that gives some sort of scale of the funding - it's not so enormous.

The second point is: If you look at the history of CERN, and you correct for inflation, the budget is less than it was 20 years ago. In that period the gross national product of the CERN member states went up 60%. Funding of more or less every other field of science also went up at least as fast as gross national product - in some cases faster than that - so although we are more expensive than most other areas of science we have actually kept constant, and we realize that we are not gonna get anymore - so we have to keep constant. We manage to do that by being rather efficient - every machine at CERN incorporates the previous machine - so we manage to keep the cost down.

That's the first remark: it's not going up - our share is going down - and if you compare to the sort of 'scale' of research and education in Europe the budget of CERN is not so enormous - nevertheless it is expensive. Now, how can you justify that? I personally think that in the end the justification is that we are contributing to part of civilization – we are looking for new knowledge and that is something which a civilized society should do. There is a very nice answer that the first director of Fermilab gave when he was asked by a US congress committee. They asked the question "what will Fermilab contribute to the defense of the United States?" and he said "nothing, but it will make it worth defending". It is a very beautiful answer and I mean, what sort of society is not prepared to support the curiosity to find out more about the universe we are in? Of course it's possible that we will discover new laws of physics which turn out to have tremendous impact, but you can never guarantee that. So I don't think you can base the funding on that though it's possible, and history shows that usually it's happened. The other things you can put in as spin-off and education [...] - I think personally you cannot justify particle physics on spin-off, but I think there is spin-off and that, somehow, should be subtracted from the cost, if you like.

To give an example of spin-off - the easiest ones to give from CERN - are, first of all, the work of Charpak in developing detectors - those are used in medicine all over the world. Another good one is the World Wide Web. [nq:] Of course it has nothing to do with CERN and WWW would most likely have been developed anyway, but probably not for another 5 or 10 years [:nq]. What the economic value of that [WWW] is I have no idea, [...] not so much in terms of profit, but it must be speeding up life - improving life and so on. Probably the value of five years of the web is enough to pay for other services [?d-t-h] as foundation but I would not say [that] therefore CERN is justified, because you can't guarantee that. If you spend a lot of money on a high-tech activity there is always gonna be some spin-off, so, I don't think you can say 'that justifies CERN' but you can say 'there is spin-off' [and] you should subtract it somehow from the cost.

I think the other argument you can make is on education of the experimental graduate students who work here. These work in a multinational environment, in big projects and learn skills that are very useful in industry and industry likes those people. Of course it is an expensive training, but nevertheless there is a human eye [d-t-h] output of particle physics. And I think that there is also an educational advantage [...] [of] a much younger age — teenagers: There is a worry in most countries in Europe, and also in Japan and in the United States, that young people are not interested in science, technology, industry and these things anymore - they only want to study philosophy and music etc. Now, it's a good thing with this [d-t-h] and good people studying philosophy and so on, but if science doesn't continue to get some share of bright people - we have a problem. Experience in the United Kingdom anyway is, when we ask university students 'how did you become interested in science?' they say 'Ah - when I was thirteen I read about quarks, I read about black holes' and I think those people, they don't become particle physicists or astronomers necessarily, but it pushes them in that direction - so I think that's one argument.

<u>question 3</u>
The role of public communications from CERN. Is there an official 'marketing'-strategy and if so, how is the correspondence with question number 2?

DG: If you argue, as I was doing, that what we are doing is contributing to part of our culture then, if you wish the tax-payers to support us, you have to share that culture with them [d-t-h] - so I think that we actually have a duty to try to communicate with the public what we're doing and it is also a necessity cause [otherwise] they would stop funding us. I think it's being realized more and more in Europe that it is a duty [...], but I think it is mainly the duty of the physicists in the CERN member states. You have to remember that at CERN we have a large staff, but there is rather few research physicists - [...] only about one hundred. On the other hand the CERN users are about five thousand physicists outside. Those are the people who should be, in their own countries, communicating with the general public - at least they have the final responsibility.

Of course we should be helping them, we should be providing back-up - I mean we should also be doing it - and indeed I go around giving public talks; many people at CERN do, but we have only a small number of the particle physicists in Europe so we can only make a relatively small direct contribution. But we can provide the back-up by providing traveling exhibitions, giving help to people, providing information [and] things like that. [...] I don't know if we have an official marketing strategy - we have an official strategy policy that public communication is important, we have a public communication group, we have the MicroCosm - which we are just reviewing to see if it can be improved - and so on.

HZ question: When this public communication is done then how strong are the physicists arguments - the quest for deeper things [understanding of] in nature - compared to the spin-off perspective?

DG: The funny thing is that, to a certain extent, it depends who you talk to: The people who are outside science, if you have time to talk to them, they are very impressed by the contribution to fundamental knowledge and they feel that CERN should be supported on those grounds, but the people in other sciences are of course competing for the funds. They are also contributing to fundamental knowledge, so they are not so impressed by that - they just notice 'your science is more expensive than ours so if we cut yours down it will [d-t-h] give us some of the money'. So for them arguments of spin-off and so on can also be important but they can also point to examples of spin-off, so it's not so easy. I find myself that the best strategy is to emphasize the contribution to culture, while mentioning these other factors which are important [...]. To a certain extent - it sounds a little bit cynical - but you adjust the argument depending on who you are talking with and to which arguments impress people most.

<u>question 4</u>
The prospects for CERN. Considering the recent shutdown of the SSC - what are the advantages/disadvantages of a possible 'monopoly' at the forefront of experimental high energy physics or what role will CERN play in the future?

DG: The question of a monopoly [...]. To do the experiments at very high energy - especially with proton colliders - it's absolutely essential to have competition and different ways of doing it, but this can be competition between different experiments at the same laboratory. To make sure that the physics of the LHC is done properly it is absolutely essential we have two general purpose experiments, which have independent groups, with different technological methods [? d-t-h], advantages, capabilities for looking for different channels and so on. I don't think it's necessary to have two such accelerators, so I don't think there is a danger to the LHC physics having only one LHC machine. On the other hand I think it would be a danger if, really, CERN became the only laboratory at the front-line in this field. I think that would be rather bad, because you need competition also in strategy of what you gonna do.

Now, we decided to build the proton machine. I certainly hope that someone in the world will build another big electron-positron collider, it will have to be a linear collider, and I think that would be healthy that there are competing laboratories [...]. I think, CERN is becoming 'a' world lab. If CERN became 'the' world lab that would be dangerous. It would also, again from a more cynical point of view, make it maybe easier to justify, because we could say 'look, we really concentrated all our resources in one place so we are doing it in a most economical way' but it would also make it relatively easy to kill the subject, because everyone is in one boat - you'll only sink one boat and that's the end of the subject.

I think the role that CERN will play in the future [...]. CERN is already a global organization, in that, about 30 percent of our users right now come from outside the CERN member states. For the LHC that will be almost 50 percent, so in fact we are a world organization. I think that it is not healthy to have an organization which is in practice a world organization, but is only funded with one region - it's not gonna be very stable.

And I don't think it is very good that there are huge number of users from, lets say the United States, who have no institutional relationship with CERN, so they have no way of influencing the policy - it doesn't seem to be a very stable or healthy situation. Therefore I think that CERN should be trying to bring in these groups [...] [from] other countries to have a major involvement.

<u>Additional questions and answers</u>

HZ question: Concerning the LHC you said that there are two experiments in it... DG: Two big experiments - and then there are some other smaller experiments. *HZ: But they are also using the same MC fragments to some extent?* DG: [...] thats a good question, that you should probably ask those guys, in fact, why don't you go and talk to the spokesmen of those experiments Drs. Jenni and Della Negra *HZ: I should.*[140]

HZ: Another thing - do you find that there are limits to what we can gain from CERN, concerning limits of knowledge? DG: Yes, I mean, there must be some limits - in the end there will be limits just from the size of the experiments... the accelerators we can build and so on. If you ask me the question 'is the LHC gonna give us the final theory of everything' the answer is probably no. *HZ: So do you believe in a final TOE?*

DG: I personally think there probably is a final theory - well it depends on how you define a theory [...]. If you read St. Augustin you can interpret him in saying that the most important question that a human being can ask is 'why is there something, rather than nothing?'. Now, that question, I personally feel, is probably always gonna be outside the domain of science. There is a different question which you can ask: Given that there is something, why is it this and not that? That question I feel we can probably answer at CERN [d-t-h] - in that sense I think there is probably some sort of a final theory, eventually [CERN probably can] answer that.

[140]Unfortunately, I was not able to get in contact with either Jenni or Della Negra during my stay at CERN.

But I think the idea that to say LHC will find the final theory [...]. I think there is a final theory - I'm much more cynical about, sceptical about, whether we will ever find it - I think we will incremently build up knowledge. There are people who think we just find the theory so perfect [...].

D Referential Realism and Appropriate Technology

Review of: Hans Radder, *In and About the World: Philosophical Studies of Science and Technology*, SUNY, 1996.[141]

There was a time when philosophers of science neglected the role of the sociology of science by insisting on the good old distinction between analytical philosophical studies of scientific justification vs. the historical contingent settings for scientific discoveries. Even if this distinction has somehow faded, philosophers of science may still doubt the relevance of the sociology of science, especially constructivist versions, by finding associated relativist claims wildly implausible.

But none of these attitudes will do, says the Dutch philosopher Hans Radder. Given the amount of, for example, constructivist studies that has documented the social dimension of all levels of the scientific enterprise during the last twenty years or so, Radder's view is hardly surprising. Still, it remains a good question exactly how constructivst insights can be adapted to a philosophical understanding of science. At least a good question for philosophers. But a social analyst who feels the need for some philosophical ground to stand on could also benefit from Radder's discussion of the question. Even a philosophically disinterested science studies scholar should be moved by the question since Radder lines up a number of normative reasons to take philosophy seriously. For instance, Radder points out that if we were to take constructivist relativism at face value, we would not be able to deal with science-related environmental problems. As a concrete case, Radder notes that the hole in the ozone layer will not disappear just by ceasing to talk about it as some relativists seem to suggest. The relevance of the example is based, of course, on the assertion that the hole constitutes a problem in the first place, which some may be inclined to dispute. But few would deny that there are real problems related to the technoscientific society which need to be addressed by means other than just talking. Action is needed. And Radder's aim is to show that such action can and ought to be philosophically guided.

In and About the World is largely adapted from Radder's previous publications (but with new introductory and final chapters) and contains a very broad range of topics. Indeed, there is a long way from the role of heuristics in the genesis of quantum mechanics to the implementation of biotechnology in Barbados. Nevertheless, the clear overview of the contents early in the book points out how these diverse fields are related to Radder's philosophical framework.

The argument in the first half of the book, dealing with epistemological and ontological aspects of science, can be summarized as follows: Radder sets off in experimental (physical) science by arguing that reproducibility is a significant feature of experimental practice. Reproducibility of experiments implies non-local patterns of experimental practice which cannot be understood solely as artifacts of specific

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social settings or local circumstances. Next, Radder examines how continuity and discontinuity go hand in hand within theoretical practices in (physical) science. Specifically, he analyses the correpondence principle from quantum mechanics and argues that transitions between successive theories may imply radical conceptual changes. Nevertheless, if successive theories simultanously exhibit some formal-mathematical correspondence it is reasonable to claim that the different conceptual frameworks refer to the same domain of reality — a reality which is bigger than we. Given the premise that theoretical terms, in order to refer, must also relate to reproducible experiments, Radder can state his 'referential' realism as a compromise between trancendental realism and constructivism: "If a (conceptual-theoretical) term from the theoretical description of a reproducible experiment refers, it 'is about' a *persistent potentiality of reality*, which as such is independent of the existence and knowledge of human beings." (p.79). However, Radder continues, to realize these potentialities requires human work which essentially depends on contingent historical conditions.

Radder's case for referential realism in physical science is well argued. It is not obvious, however, that referential realism is always an adequate response to epistemological and ontological questions in science. Radder's key example of successive theories is classical mechanics vs. quantum mechanics. But the relation between these two theories is unique. In particular, because classical mechanics, in addition to standing in a correspondence relation to quantum mechanics, is probably needed to make sense of quantum theory (see, for example, Favrholdt (1994)). Thus, the meaning of reference to 'persistent potentialities of reality' of quantum mechanical terms is crucially linked to terms in classical mechanics. This somehow mutes the conceptual discontinuity between these two theories and implies, in my view, that the realism issue within physics might be more differentiated than Radder suggests. Moreover, there are sciences where reproducible experiments and/or successive theoretical frameworks play a less distinctive role than in physics but where some reference to a human-independent reality might nevertheless come in handy. Take for instance marine biology where some science studies scholars have argued that scallops cannot be taken as the cause of scientists beliefs about them (see Collins and Yearley (1992)). Before attempting to defend referential realism in this case, it may be more relevant to ask the science studies scholars why the marine biologists are more real than the scallops.

Nevertheless, the referential realism that Radder argues proves relevant when linked to the latter half of the book which deals with reflexive and normative aspects of philosophy. First, Radder substantiates his criticism of the lack of 'constructive' normative aspects in constructivist studies by examining some inherent philosophical assumptions in social constructivism, ethnographical studies and actor network theory. Radder assures that studies aiming to document the social and/or local character of scientific or technological knowledge are important. But such studies by-pass actual problems of how to deal with issues like technology assessment or environmental controversies. Though Radder's referential realism in itself does not point to any specific solutions either, the argument that scientific and technological knowledge has also to do with the material world seems to be a sensible starting point for normative reflextions on science and technology. It should be noted that

Radder does not lump all versions of constructivism into one. For instance, Radder finds that actor-network theory, for Latour, has 'nature' solely as an artifact of network interactions but that Law's version of the same theory grants more to the role of an autonomous nature (even though it may require network activity to reveal this role).

Radder's criticism of the (missing) normative aspects in some constructivist studies of science and technology is convincing. But the practical advantage of Radder's alternative might be met with some scepticism. Radder suggests that even though prediction and control in science and technology is essentially uncertain, we should "try to implement such scientific or technological projects that, according to the best of our knowledge, would cause minimal damage should they fail, and the material and social realization of which is democratically supported by all the people involved" (p.114). Few would probably disagree with Radder, but what is one to do in a practical situation? Radder continues as follows: A technology is *appropriate* if it satisfies three criteria: 1) The products of the technological system and the possible courses of action resulting from it are desirable, 2) The material, psychological, social and cultural conditions required for the successful realization of the technological system are feasible and desirable, 3) Enough is known about the realization of the technological system to approach 1. and 2. in a sensible manner. (p.147). As Radder recognizes, this prescription is somewhat idealized and consequently it is better to speak about a 'degree' of appropriateness for a given technology. Clearly, the importance of assigning a degree of appropriateness to a given technological system comes from the possibility to compare it with 'degrees' of the alternatives to the technology in question (for instance, the degree of appropriateness of *not* implementing the technological system).

The notion of appropriateness "makes explicit what is normatively at stake in modern technology and what should therefore be on the agenda in assessing the feasibility and desirability of technological systems in a just and democratic manner" (p.151). In practice, however, the recommendations to the actors involved in the realization of technological systems "mostly amount to pinning down and criticizing less appropriate aspects of technologies and clarifying the conditions under which more appropriate realizations might be obtained" (p.151). In addition to setting up a theoretical framework for normative considerations of technology (including analyses of the intimate connections between technoscientific knowledge and power), Radder discusses a number of concrete cases such as aspects of nuclear energy technology in the Netherlands and the implementation of agricultural biotechnology in the Caribbean. Judging from these cases, relevant normative issues have indeed not always been taken into account in technology assessment and implementation.

But exactly how relevant normative issues should enter into a degree of appropriateness is not discussed in detail. For instance, how much does the desire of a third-world farmer to implement biotechnology count in appropriateness relative to a non-govermental organisation's concern for the environment? Or how much does the opinion of other farmers, who are not asked to participate in the project, count? To be sure, I do agree that it would be wonderful if one could set up ways of measuring degrees of appropriateness. But in practical matters this may turn out to be a difficult task.

One thing that Radder stresses as particularly important is a substantive dialogue between prospective users of a technological system and the other actors involved in the realization of that system. It is, as Radder points out, necessary to hear the prospective users opinion about the desirability and feasibility of proposed projects. I have to admit that my knowledge of technology implementation is very limited. But hopefully this 'minimum' criterion in technology assessment does not come as any surprise for people dealing with such assessments. In any case, Radder has an important point in arguing that a central normative question in technology discussions is appropriateness evaluations — even though it may be hard to get a firm grip on a quantitative 'degree'.

Throughout the book, Radder states his points clearly and with detailed information on how his approach and ideas are situated with respect to other voices on the topics he addresses. It will, I think, be difficult to find scholars within science and technology studies or philosophers of science and technology who could not take something home from Radder's book. Radder successfully points to ways in which philosophy is also important away from the armchair of analytical philosophy. In this sense Radder achives his goal of showing how philosophy can be both theoretical, reflexive, and normative. Thus, Radder advocates a philosophy that has a role to play not only in thinking about science and technology but also when it comes to concrete action. Or, as Radder puts it, a philosophy that is at once in and about the world.

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E $g-2$ and the trust in experimental results

Introduction[142]

There was a time when it was often held that it is the reproducibility of experiments which establishes experimental results as objective facts. In the wake of Kuhn, however, it was argued that theoretical presuppositions shape or even determine experimental results. And since theories change, so will the results of experiments. Consequently the 'objectiveness' of experimental results became relative to their theoretical framework. To be sure, there has been numerous objections to just how radical theory changes were and thus how different experimental results could be in different theories. In any case, this Kuhn-inspired emphasis on theories has been opposed by recent philosophy of experiments which has argued that experiments can remain stable when theories change — experiments have a life of their own (see for example [80], [65] and [70]).

Have experimental results then again become objective facts about nature? Answers differ, but it should be stressed that philosophy of experiments have not renounced theory as such. Rather the relation between theory and experiment has been seen in a new light. For instance, even though theory does not determine experimental results, some theory or background suppositions are needed in order to make sense of experiments. Building on such insights the question has been raised of how experiments end. When is the scientific community prepared to believe in an experimental result? This way of putting the question, however, assumes that experiments *do* end. Which of course they often do. But some experiments, or rather experimental studies of the same questions, are repeated again and again.

In this paper we want to reflect on the development since 1947 of experiments on the magnetic moment of the electron, commonly referred to as $g-2$ [g minus 2] experiments. The ancestors to these experiments were the gyromagnetic experiments which have been chronicled by Galison in his book *How Experiments End* [70]. Galison provides an analysis of gyromagnetic experiments from 1913 until around 1933 and discusses how the experiments survived through major theory changes. The period covered by Galison is spanned by classical electromagnetism, the old and new quantum mechanics and relativistic quantum mechanics. But experiments on the magnetic properties of electrons did not end with Galison's analysis. In fact, the continuing series of experiments on the magnetic moment of the free electron covered in this article provides the most accurate test of Quantum Electrodynamics (QED), and refinements continue to this day[143]. Nowhere else in physics has a theory been confronted with experimental results to such high accuracy.

It is sometimes assumed that repetitions of experiments only take place in areas of controversy, for instance to test the stability of a new effect under variation of the experimental circumstances (see e.g. [36]). The $g-2$ experiments have all been performed in a period under a fixed theoretical framework, QED. Nevertheless, the development of these experiments provides an interesting example of the interplay

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[143] Some remarks about the experimental situation in the period 1933–1947 may be found in [163].

between theory and experiment. As we shall see, the $g-2$ experiments appear well suited for a discussion of questions raised by recent philosophy and history of science, for instance regarding some of the elements contributing to the trust in experimental results.

Our point of departure will be on the concept of errors which couples nicely to the debate about theory-ladenness of experiments. At every point in the history of the $g-2$ experiments, a certain amount of theory was necessary to convert the raw measurements into a value for $g-2$. To discuss what theoretical considerations were involved in the various $g-2$ experiment, we explain below some of the ideas on which the experiments were built.

Concerning our case-study, it is important to stress that we do not undertake a detailed historical analysis of the circumstances leading to the published articles on the magnetic properties of the electron[144]. Instead we shall attempt to extract philosophical lessons from the published articles by seeing them in relation to the historical development of the experiments. Thus, rather than asking how experiments end, we will be asking why experiments *continue*.

Errors and error bars

Experiments are beset with errors. As has been pointed out before in the literature, a significant and constitutive part of experimental practice is estimations of, and corrections for, errors — extracting signal from noise or foreground from background. Since a central emphasis in this article is on experimental errors it is appropriate to give a short introduction to the concepts of statistical and systematical errors.

Statistical errors are random errors. They arise from the finite accuracy of the measuring and monitoring apparatus or inherent randomness of the phenomena under scrutiny and lead to a spread of the experimental result around an average value. The statistical errors are assumed truly random, so the size of the statistical error in one measurement is independent of errors in other measurements. Ideally, if there were no other errors than statistical errors, the average value taken from an indefinite number of measurements would constitute the 'true' value for the measured quantity. Statistics deals with these errors by taking into account that a quantity can only be determined a finite number of times.

But experimental results are also subject to systematical errors. These arise from experimental effects not taken into account and/or bias in the extraction of results from data. In contrast to the statistical errors, a systematical error does not imply a spread around a central value but merely shifts the result away from the true value.

It is common to state an experimental result with error bars. For instance, if one has measured a physical quantity, the result x_{exp} can be reported as $x_{\text{exp}} \pm \Delta$ where Δ indicates the error. If the error is expected to be mainly statistical, this is usually taken to mean that the interval $[x_{\text{exp}} - \Delta; x_{\text{exp}} + \Delta]$ includes the true value x_{true} with 68.3% probability (see e.g. [140] p.1278). Accordingly, it will not

[144] By this we do not mean to neglect the value of such studies. The importance of going behind the reconstructed logical ordering of the published paper has been emphasized by many recent scholars, see for instance [36] and [70] p.244.

raise concerns if a later experiment gives x'_{exp} with error Δ' and the corresponding interval are slightly outside the interval of x_{exp} and Δ. In the $g-2$ experiment we shall see that the systematical errors have often been the major source of error and hence that deviations between succeeding measurements and their error bars have been regarded as problematic.

The systematic errors can be divided into three groups: First, there are systematic effects which are discovered or well-known and can be corrected for either experimentally or theoretically (for this reason these effects are called corrections). In the final reported result of the measurement, corrections will be incorporated directly and are not reflected in the error bars. The second type of systematic errors refer to effects which are thought or known to play a role but whose exact influence cannot be determined. In such cases the error may also be estimated either by theoretical or experimental arguments. It may happen, however, that an effect is thought or known to play a role but that it for some reason cannot be theoretically or experimentally estimated, in which case the estimate may be a more or less educated guess. In any case, if an estimate of a systematic effect is made, it is reflected in the error bars. Finally, the third category of systematic errors are those which are unknown at the time of the measurement and consequently cannot be corrected or show up in the error bars.

Thus, theory will dictate what systematic errors can be expected in an experiment and theory may be used to estimate their influence. In the account of the $g-2$ experiments below we shall pay attention to what kind of theoretical arguments were involved in the process of extracting a value for $g-2$ from the measurements. Before turning to our case study we provide some background on the origin of magnetism and the g-factor. (see [70] for a full account).

The g-factor

In Maxwell's electrodynamics there are no elementary magnetic charges. Although his equations make room for the possibility, and although many authors have speculated upon it during the last century, it seems now after many years of fruitless experiments that free magnetic charges in the form of lonely north poles or south poles (monopoles) indeed do not exist (see e.g. [98]).

From the assumption of the electron being the only charge carrier responsible for magnetism and the fact that all electrons have the same charge and mass, follows the prediction that the density of charge should be proportional to the density of mass for these charge carriers. This proportionality leads in turn to a relation between the magnetic moment of a current distribution which is a measure of its magnetic field and the angular momentum of its mass distribution which is a measure of its state of rotation. They must in fact be proportional to each other with a constant of proportionality given by the ratio of the electron's charge to twice its mass ($e/2m$).

If the electron were not the only charge carrier things would be different. Contamination from another charge carrier with different ratio between charge and mass would lead to a different constant of proportionality. In order to include this possibility a 'fudge-factor' g was introduced (by Landé in 1921 [70] p.64) to take care of such deviations (so the ratio was written $g\,e/2m$). This g-factor or gyromagnetic ratio would accordingly be exactly 1 (i.e. $g = 1$) if the electron were the only charge carrier.

The curious history of the gyromagnetic experiments illustrates the importance of theoretical ideas for the outcome of experiments. Around 1915 the prejudice of theorists (among them Einstein) was strongly in favor of $g = 1$ and experimental results were also found in this neighborhood at that time and as late as 1923 by Einstein's collaborator. Other experimentalists were also influenced but did eventually abandon their theoretical prejudices. It took nevertheless the concerted efforts of many experiments to indisputably dislodge the measured value from the expected one. Galison concludes in his analysis of the background for this situation that the theoretical prejudice would not by itself bias the experimental result, but could possibly have created a mindset in which experiments were terminated and the search for systematic errors given up when a result was found near the strongly expected one [70].

In 1925 Goudsmit and Uhlenbeck explained the so-called anomalous Zeeman effect (see below) by introducing the electron spin, a quantum phenomenon akin to an internal rotation of the electron about an axis. Using a gyromagnetic factor of $g = 2$ for the spin they were able to explain the fine structure doubling of spectral lines and the Zeeman effect. By then it had also become clear that magnetism in materials was a much more complicated phenomenon owing its properties to a mixture of orbital electron motion with $g = 1$ and the intrinsic magnetic moment of the electron spin with $g = 2$. In 1928 Dirac published his relativistic theory of the electron. In this theory the electron has a built-in spin with an exact gyromagnetic factor of $g = 2$. For the next two decades this became a theoretical prejudice which agreed comfortably with experiments (see [163] p.211 ff).

Modern experiments on the electron g-factor

The first suggestion that the g-factor of the electron might be different from 2 was made by Breit in 1947 [17] (see also [41] and [163] p. 220), and was prompted by a disagreement between theory and precise measurements of the hyperfine structure of hydrogen obtained by Nafe, Nelson and Rabi [135].

This began a series of experiments for determining the precise value of the difference between the actual g-factor of the electron and the Dirac value 2. One may roughly divide the modern development into three different phases that more or less follow each other sequentially in time: 1) atomic level experiments, 2) free electron spin precession experiments, and 3) free electron spin resonance experiments. We shall discuss these in turn below.

In table 1 the experimental determinations of the g-factor of the electron or rather the corresponding anomaly $a = (g - 2)/2$ are listed. The same data is also plotted in fig. 2, but because of the rapid drop in the size of error bars (see fig. 3) the plot is not representative of the two later phases. See fig. 3 for another presentation of the data. It should be noted that not all experimental values refer to independent measurements.

The decrease in experimental errors over the years has been remarkable. As shown in fig. 3 the decreasing errors roughly follow an exponential curve from 1958

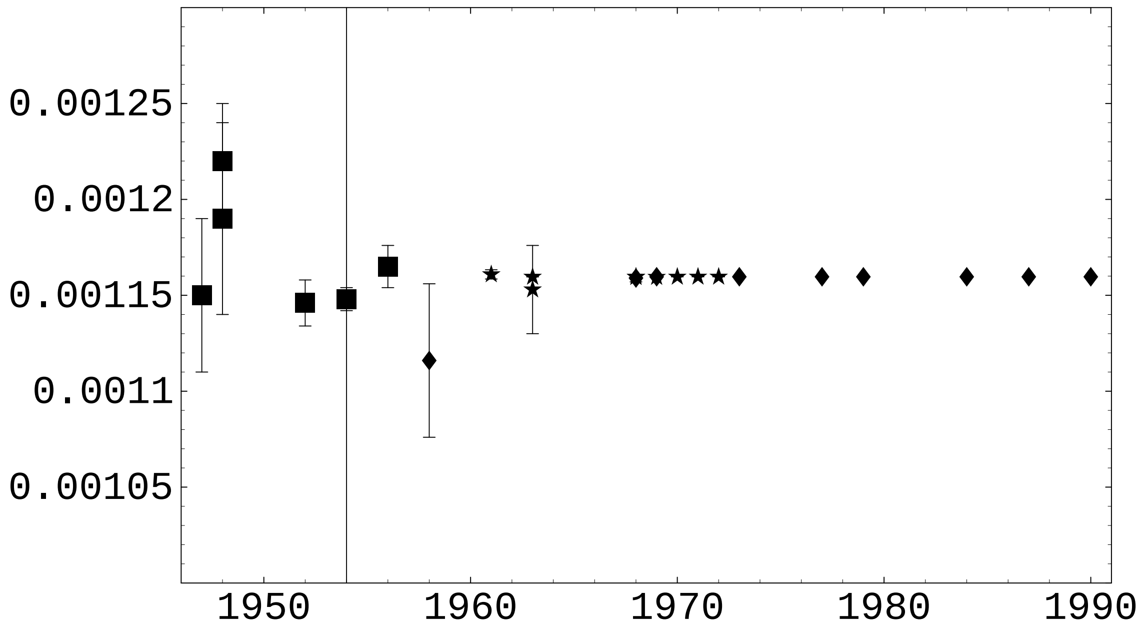

Figure 2: Modern measurements from 1947 to 1987 in terms of the anomaly $a = (g-2)/2$. The square boxes represent atomic level experiments, the stars free electron spin precession experiments, and the diamonds free electron spin resonance experiments. Error bars are included everywhere, but are too small to be seem in the plot after 1964. The vertical line in 1954 is part of the large error bar from the pilot experiment on spin precession by Louisell, Pidd and Crane [122]. See also Table.

to 1984. On average the error decreases by a factor of 1.8 per year during this 26-year period.

Apart from the very first experiment [106] and the most recent series of experiments [178], the theoretical value for the electron g-factor was always known to higher precision than the experimental values. The theoretical predictions changed due to more precise QED calculations [114, 96] and to changes in the measured values for the fine structure constant α (see below). We shall discuss the theoretical calculations of $g-2$ only insofar as they are directly related to the experiments. In the following we describe the physical principles behind the experiments in order to highlight some of the systematic corrections applied to the raw data when obtaining the final quoted experimental results. Our account below is by no means exhaustive but covers mainly those features of the experiments that are relevant to our discussion.

Atomic level experiments

Stimulated by suggestions by Breit and Rabi, Kusch and Foley [106] in 1947 carried out high precision measurements of atomic levels revealing a discrepancy which might be due to an anomalous g-factor.

The experiments were based on the so-called Zeeman effect which denotes the

Authors	Year	Type	Anomaly(error)
Kusch, Foley [106]	1947	■	0.001,15(4)
Foley, Kusch [62]	1948	■	0.001,22(3)
Kusch, Foley [107]	1948	■	0.001,19(5)
Koenig, Prodell, Kusch [102]	1952	■	0.001,146(12)
Beringer, Heald [7]	1954	■	0.001,148(6)
Louisell, Pidd, Crane [122]	1954	★	0.000(5)
Franken, Liebes [64]	1956	■	0.001,165(11)
Dehmelt [45]	1958	♦	0.001,116(40)
Schupp, Pidd, Crane [162]	1961	★	0.001,160,9(24)
Farago, Gardiner, Muir, Rae [55]	1963	★	0.001,153(23)
Wilkinson,Crane [199]	1963	★	0.001,159,622(27)
• Rich [151]	1968	★	0.001,159,557(30)
• Farley [56]	1968	★	0.001,159,596(22)
Graff, Major, Roeder, Werth [75]	1968	♦	0.001,159(2)
Graff, Klempt, Werth [76]	1969	♦	0.001,159,66(30)
• Henry, Silver [87]	1969	★	0.001,159,549(30)
Wesley, Rich [194]	1970	★	0.001,159,644(7)
Wesley, Rich [195]	1971	★	0.001,159,657,7(35)
• Granger, Ford [77]	1972	★	0.001,159,656,7(35)
Walls, Stein [185]	1973	♦	0.001,159,667(24)
Van Dyck, Schwinberg, Dehmelt [178]	1977	♦	0.001,159,652,410(200)
Van Dyck, Schwinberg, Dehmelt [179]	1979	♦	0.001,159,652,200(40)
Van Dyck, Schwinberg, Dehmelt [180]	1984	♦	0.001,159,652,193(4)
Van Dyck, Schwinberg, Dehmelt [181]	1987	♦	0.001,159,652,188,4(43)
• Van Dyck[182]	1990	♦	0.001,159,652,189(4)

Table 1: Experimental determinations of the electron g-factor anomaly ($a = (g-2)/2$). The error is written in the parenthesis after the value and refers to the last digits. The square boxes represent atomic level experiments, the stars free electron spin precession experiments, and the diamonds free electron spin resonance experiments. A bullet indicates a reevaluation of earlier experiments with no new data taken.

splitting of atomic levels into sublevels in a magnetic field. The effect is caused by interaction between the magnetic field and the total magnetic moment of the atom and each sublevel corresponds to a different orientation of the magnetic moment. The actual measurement consisted in subjecting a beam of atoms to a weak oscillating magnetic field and determining the frequency required to excite transitions between the Zeeman levels. The state of the atoms after the excitation was observed by splitting the beam into subbeams corresponding to the different Zeeman levels (a Stern-Gerlach type setup). Sweeping the oscillation frequency across the natural transition frequency of the atoms, a marked peak could be observed in one of these subbeams.

According to quantum mechanics the atomic transition frequency is

$$\omega_A = g_A \frac{e}{2m} B \tag{8}$$

where $e/2m$ is the Bohr magneton, B is the magnetic field and g_A (in analogy with the electron g-factor) is the magnetic moment of the atom in units of the Bohr magneton[145]. The atomic g-factor is partly due to the g-factor for orbital motion of

[145]The Bohr magneton is given by $e\hbar/2m$ where e is the magnitude of the electron's charge, m

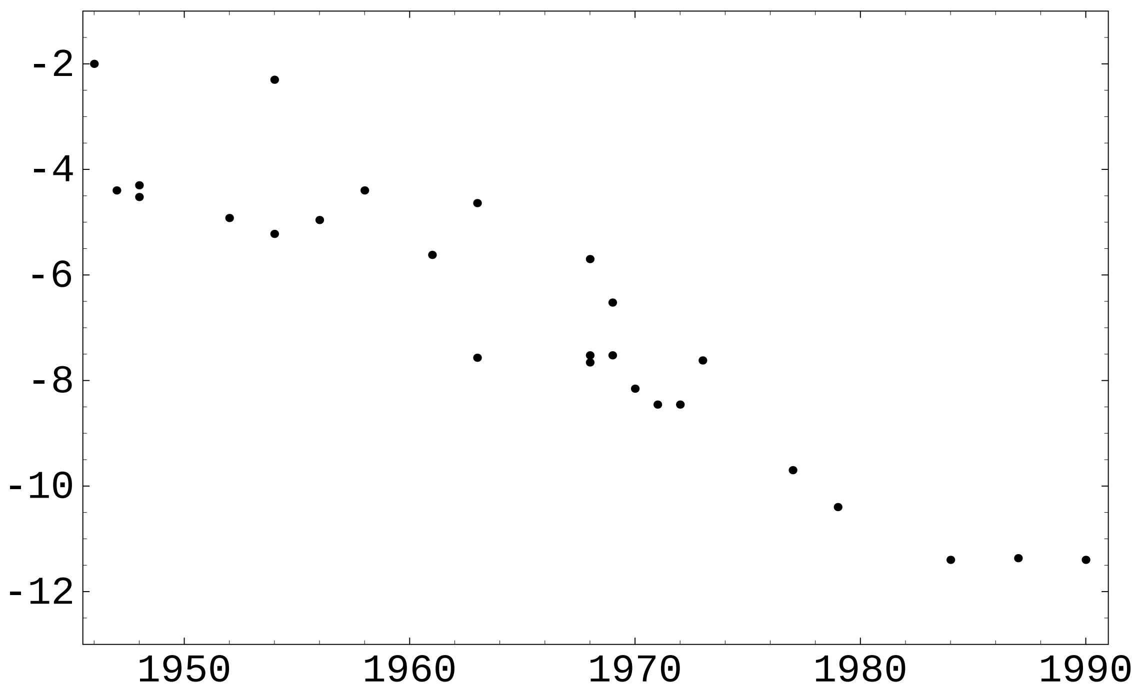

Figure 3: Logarithm of experimental errors plotted versus year. The square boxes represent atomic level experiments, the stars free electron spin precession experiments, and the diamonds free electron spin resonance experiments. Note that the precision of the spin precession experiment by Louisell is too low to be seen in the figure (see Table).

electrons around the nucleus and partly to the g-factor for the intrinsic spin of the electrons. In such experiments it is only possible to determine the combined effect of these two interactions, not each contribution by itself.

It is technically difficult to obtain a sufficiently precise value for the magnetic field strength. This problem disappears if one calculates the ratio of transition frequencies for two different transitions, 1 and 2, in the same or in different atoms,

$$\frac{g_1}{g_2} = \frac{\omega_1}{\omega_2} \tag{9}$$

From such ratios of atomic g-factors the ratio of the spin and orbital g-factors could be extracted.

As we mentioned above, the theoretical expectations after Dirac's relativistic theory of the electron was that orbital motion was associated with $g = 1$ and spin motion with $g = 2$. In their first paper, Kusch and Foley [106] found a discrepancy with these assumptions and noted that it could be corrected by adding an anomalous contribution to the g-factor of either the orbital motion or the spin. In their second paper [62], Foley and Kusch abandon the first possibility and in a footnote quote Schwinger for the theoretical justification (see below). The anomaly is consequently understood as entirely due to an *anomalous* magnetic moment for the electron. With

its mass and $\hbar = h/2\pi$ is Planck's reduced constant. The g-factor of the electron is simply its magnetic moment in units of the Bohr magneton.

this interpretation they found the final value[146] of the electron g-factor anomaly to be $a = 0.00119(5)$ [107].

From this moment onwards the experimental focus was on the deviation between the Dirac value 2 and the actual value of g. It is now customary to quote all experimental as well as theoretical results in terms of the electron anomaly which as mentioned above is half the difference between the actual value and the Dirac value (see table 1).

The first theoretical calculation of the free (i.e. not bound to any system) electron's anomaly by Schwinger in 1948 gave the result $a = \alpha/2\pi \simeq 0.001162$, where $\alpha \approx 1/137$ is the dimensionless fine structure constant known from atomic physics. The fine agreement between theory and experiment was perceived as a confirmation of the internal radiative processes involving a single photon predicted by QED[147]. Later theoretical efforts have entirely been concerned with the calculation of higher order radiative effects involving more than one internal photon, corresponding to higher powers of α.

Foley and Kusch [62] explicitly point out that the theoretical estimate by Breit [17] who among others had inspired them to do the experiment, was in disagreement with their experimental result[148]. But they were as mentioned aware of Schwinger's work in progress as he was of theirs [165]. From the published papers [62, 165] it is not clear whether they knew Schwinger's specific value for a, which was in perfect agreement with their measurement (in his paper Schwinger points out that Breit has not done the calculation correctly). In any case, it clearly took QED to associate the experimental result with the deviation in the spin rather than the orbital g-factor.

In the decade following the pioneering experiments by Kusch and Foley similar experiments were carried out with somewhat improved precision (see table 1). A serious problem with atomic determinations of the g-factor for the electron arises, however, from the complicated corrections due to the electron being bound in an atom and not free. At the level of the Schwinger calculation these atomic corrections may be ignored, but at the next level (of order α^2) where radiative processes involving two internal photons come in, it is necessary to include various types of atomic corrections to the same precision.

For this reason Koenig, Prodell, and Kusch [102] in 1952 applied a relativistic mass correction to their measured g-factor in hydrogen in order to compare with the theoretical second order result for free electrons obtained by Karplus and Kroll [95]. They found excellent agreement. Beringer and Heald [7] carried out a slightly modified experiment in 1954 and obtained a result which was in good agreement with Koenig et al. At this point in time the agreement between theory and experiment seemed perfect, even if unbeknownst to everybody the theoretical value was in error.

In the experiments following Kusch and Foley's the actually measured quantities are also transition frequencies. Since the experimental setup with hydrogen beams

[146] This value is actually an average over three experimental runs using different choices of atoms and levels.

[147] The difficulties in calculating radiative corrections from QED arose from mathematical infinities which were first circumvented with the renormalization scheme of Schwinger, Feynman and Tomonaga in and around 1947 (see e. g. [163]).

[148] See Schweber [163] p. 222 for an account of Breit's discontent with Foley and Kusch's remarks.

only permits the determination of one transition frequency, ω_H, the second transition frequency, ω_P, is obtained from nuclear spin resonance on protons in the same magnetic field. The g-factor of hydrogen is then determined by

$$g_H = g_P \frac{\omega_H}{\omega_P} \tag{10}$$

where g_P is the proton magnetic moment in units of the Bohr magneton. Actually the right hand side of this relationship does need systematic corrections mainly due to non-linearities in the atomic Zeeman levels as a function of magnetic field, effects well-known at the time.

In order to determine the hydrogen g-factor g_H (which contains the electron g-factor) in these experiments, it is necessary to obtain a value for g_P. In both experiments this value was taken from an earlier experiment by Gardner and Purcell [74] and the uncertainty on this quantity dominated the resulting uncertainty in g_H and thereby g.

In 1956 Franken and Liebes [64] remeasured the proton magnetic moment in an experiment designed to eliminate the errors in the g_P determination due to the influence of non-vanishing electrostatic fields present in their apparatus. These errors were only estimated theoretically but not measured directly in the earlier experiment by Gardner and Purcell. The improvement was based on the idea that even if the actual electric field strength were unknown, its influence on the g-factor of the proton depended on the magnetic field strength and the electron velocity. Carrying out the experiments for a range of magnetic field strengths, Franken and Liebes were able to determine the size of the correction experimentally and subtract it from the measured values of g_P.

The new result for g_P disagreed with Gardner and Purcell's by about twice the quoted errors. Furthermore, in combination with the previous results by Koenig, Prodell and Kusch, and by Beringer and Heald, this measurement of g_P lead to a new value for the g-factor in disagreement with the theoretical value of Karplus and Kroll by about twice the experimental error.

Franken and Liebes' experiment raised doubts about the agreement between theory and experiment. Even without the benefit of hindsight, which indicates that Gardner and Purcell must have underestimated their errors by at least a factor of two, the experiment of Franken and Liebes — in spite of the quoted uncertainties being the same as in the previous experiment — appears to be a better experiment, because they turned an educated guess by Gardner and Purcell into an experimentally determined correction.

The theoretical value of Karplus and Kroll was found to be in error by Petermann [141] and Sommerfield [175] in 1957. The experiment of Franken and Liebes served as an inspiration for the theoretical recalculations which again brought theory and experiment into agreement within about half the experimental error.

Free electron spin precession experiments

In the preceding experiments the g-factor was measured on electrons bound in atoms. The complicated corrections due to the binding influenced as we have discussed the interpretation of the atomic experiments. If on the other hand it were possible to determine the g-factor of the *free* electron, such corrections would be absent.

In 1954 a pilot experiment by Louisell, Pidd and Crane [122] demonstrated the feasibility of a new method for determining the g-factor anomaly of the free electron. The central feature of the method consists in setting electrons in circular motion in a plane orthogonal to a homogenous magnetic field. The number of revolutions per second is measured by the angular velocity, ω_c, called the cyclotron frequency. The magnetic moment of the electron will simultaneously precess around the direction of the magnetic field and if the g-factor were exactly 2, its precession rate, or angular velocity ω_s, would be equal to the cyclotron frequency ω_c, implying that spin magnetic moment and velocity would rotate at the same rate and maintain a constant angular separation.

Conversely, if g is not exactly 2, the angle between the direction of the electron velocity and the direction of the spin magnetic moment will change with a rate given by the difference $\omega_a = \omega_s - \omega_c$, which is proportional to the anomaly $a = (g-2)/2$. This means that the anomaly may be determined as a ratio between two frequencies

$$a = \frac{\omega_a}{\omega_0} \tag{11}$$

where the frequency in the denominator is $\omega_0 = eB/m$ with e and m being charge and mass of the electron[149]. This frequency is a direct measure of the magnetic field strength B. As before there are systematic corrections to this simple relation (see below).

The actual experimental setup of Louisell, Pidd and Crane is based on an observation by Mott in 1929 [134] that unpolarized electrons scattering off atomic nuclei will get their spins partially polarized. The polarized electrons are then allowed to circulate in a homogenous magnetic field for a certain number of revolutions. Finally the electrons are scattered once more and here it is used that the intensity of the scattered polarized electrons for a particular scattering angle will depend on the direction of polarization [134]. Thus by observing the intensity variation of the scattered electrons as a function of scattering angle, Louisell et al could determine the final spin direction.

Due to the anomaly being of the order of one part in a thousand it takes a thousand orbital revolutions in the magnetic field for the direction of the spin magnetic moment to make one revolution relative to the initial direction. Louisell, Pidd and Crane only studied the electrons for five cycles of revolution, corresponding to a change of only two degrees in the angular separation of spin and velocity. The experiment only allowed them to conclude that the spin and velocity direction rotated at the same rate within the experimental resolution which was five times larger than the one necessary to observe the anomaly.

In 1961 Schupp, Pidd and Crane [162] reported a highly improved version of this experiment in which the electrons were trapped for many thousands of revolutions instead of just five. By registering the actual trapping times for the polarized electrons they could determine the cyclic change in spin direction relative to velocity

[149] This equation is also valid in the relativistic case. For a non-relativistic electron the cyclotron frequency is equal to ω_0. For a full discussion of relativistic corrections see for example [41].

as a function of trapping time and thereby ω_a. The frequency $\omega_0 = eB/m$ was determined by direct measurement of the magnetic field using Franken and Liebes' result. The final quoted result $a = 0.0011609(24)$ agreed with the theoretical calculation by Sommerfield [175] and Peterman [141], $a = 0.0011596$, to within half the experimental error.

The authors' own trust in their result is however somewhat less than complete. The experiment was carried out for several values of the magnetic field strength and the simple weighted average over all runs came to $a = 0.0011627$ with a statistical error of less than half a unit at the last digit. The distance between this result and theory is more than 60 times the statistical error. The largest estimated systematic error stems from inhomogeneities of the magnetic field necessary to create the magnetic trap. Put together with a number of smaller estimated systematic errors the authors end by using an estimated total value for the systematic error of 14 units (on the last two digits). This brings the distance between theory and experiment down to about twice the experimental error.

In the experiment by Franken and Liebes[64] discussed above, important systematic corrections due to stray electric fields could be eliminated by varying the magnetic field strength. Applying the same type of correction to their experiment, Schupp, Pidd and Crane were able to arrive at a measured value of $a = 0.0011609$ which brings theory and experiment into agreement within the above-mentioned error of 14 units on the last digit.

The authors are however not quite sure about this correction, in particular because it does not behave as expected under variation of some of the experimental conditions. They state that the correction is "based on an uncertain hypothesis", namely that the dependency on the magnetic field strength is actually due to electric fields and not to some other instrumental cause, or even a real variation in the g-factor with the magnetic field (or equivalently, the electron velocity). The authors make the following comment about the use of this correction:

> In deciding upon a single value for a to give as the result of the experiment, our judgement is that we should recognize the trend [in the data corresponding to measurements with different magnetic fields], and proceed on the assumption that a radial electric field is present, in spite of certain weaknesses in the evidence for it.

In the end they published the value $a = 0.0011609$ but assigned to it an error which was great enough to include the weighted average over all measurements. The final published error thus became 24 on the last digits. This correction brought theory and experiment into agreement within half the experimental error.

In an experiment from 1963 by Farago, Gardiner, Muir and Rae [55] a transverse electric field was explicitly introduced to control the number of cyclotron revolutions. The experimental precision (see Table 1) was limited by unsurmountable technical problems [152] and only attained the 1% level, but did otherwise agree with the previous experiments and theory.

In the same year an experiment by Wilkinson and Crane [199] resulted in an order of magnitude increase in the precision obtained by Schupp, Pidd and Crane [162]. The experiment was an advance over the earlier one at several points. Central to the

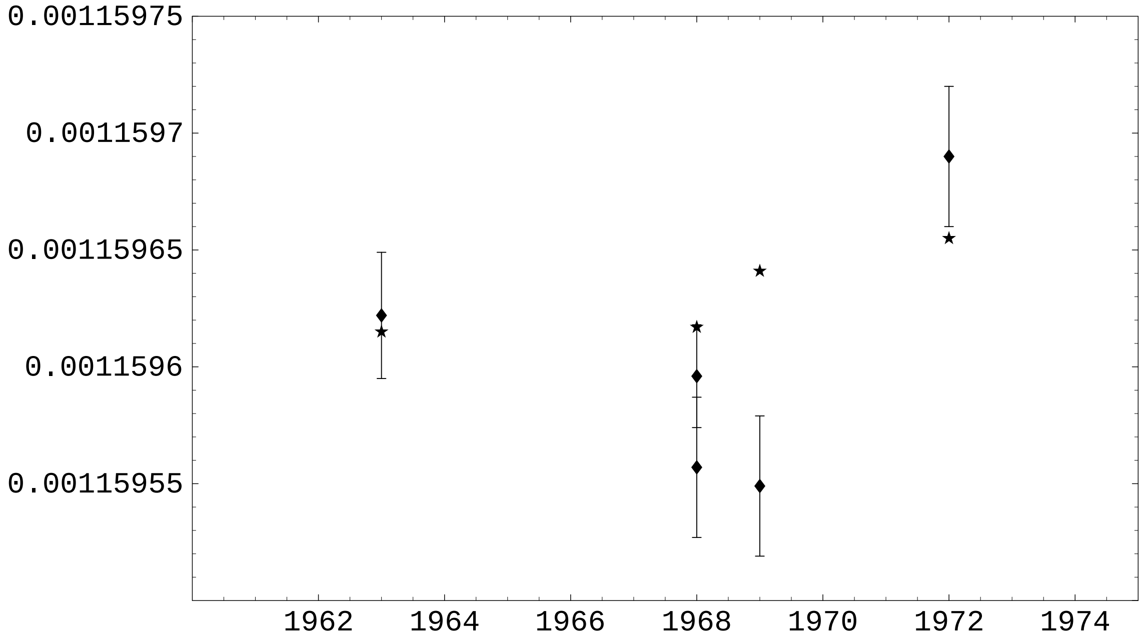

Figure 4: Plot of the 1963 Wilkinson and Crane result for the electron g-factor anomaly and subsequent reevaluations. Theoretical values cited in these papers are also plotted. These values shifted during this period partly due to changes in the fine structure constant and partly due to refinements in theory.

improvement was a reduction of the effects of stray electrostatic fields by increasing the separation between the electron orbits and the material of the vacuum chamber. The authors expressed no doubts about the need for the electric field correction which as before was applied after all other errors had been estimated.

This time the trend in the data was clear and eliminated the need for an ad hoc assignment of error. Instead the error was deduced from the estimated errors on the individual data points at different magnetic field strengths. The final published value for the anomaly became $a = 0.001, 159, 622(27)$. The theoretical value which still only included the two first radiative corrections was at this point in time $a = 0.001, 159, 615$. The agreement between theory and experiment was impressive, amounting only to one quarter of the experimental error. However, as we shall see below, this agreement was later to be cast into doubt.

The precision of the theoretical result was limited by the still unknown third radiative correction (amounting to 10 on the last digits) and the current experimental error in the fine structure constant (5 on the last digit) which goes into the calculation. In the end of their paper, Wilkinson and Crane state that "partly for this reason, but mainly for experimental reasons. we here conclude the 10-year effort of the laboratory on the g factor of the free negative electron". Nevertheless, just seven years later a new experiment with an order of magnitude improvement in precision was reported from the same laboratory.

In the meantime reevaluations appeared of the Wilkinson and Crane experiment worsening the agreement between theory and experiment. Farley [56] pointed out a theoretical error in the relativistic calculation of electron motion, Rich [151] improved the numerical precision in averaging the actual magnetic field, and Henry and Silver [87] made further relativistic corrections. Finally, in 1972 Granger and Ford [77] made a careful reevaluation of the theoretical basis for the experiment. In 1971 a significant change also happened in the theoretical prediction of the free electron anomaly due to the first calculation of the third radiative correction by Levine and Wright [117]. As seen in Fig. 4 the first corrections tended to worsen the agreement between theory and experiment whereas the Granger and Ford reevaluation comfortably agreed with the Levine and Wright result.

In 1970 Wesley and Rich [194, 195] rebuilt the spin-precession experiment allowing for an order of magnitude increase of the magnetic field strength along with other improvements. The increased magnetic field diminished the relative size of the correction due to stray electric fields. The stronger magnetic field also allowed to trap the electrons for millions of orbit revolutions leading to a smaller error on the $g-2$ frequency ω_a. The final result $a = 0.001,159,657,7(35)$ agreed perfectly with the current theoretical value by Levine and Wright [117]. It almost agreed with the original Wilkinson and Crane value of $a = 0.001,159,622(27)$ within the quoted errors, but disagreed significantly with all but the last of the later reevaluations of this experiment. The authors express worries about the disagreement, but in spite of an extensive critical review "no concrete basis for the discrepancy has yet been found".

In fact Granger and Ford were able to explain also this discrepancy in their reevaluation [77]. Nevertheless when Rich and Wesley reviewed the situation later that year (1972) they wrote ([152] p. 255)

> The agreement [between theory and experiment] should be treated with a certain amount of caution, since it is based on a comparison between a single theoretical calculation and a single type of experimental measurement. In view of the complexities of the theoretical calculation, and the difficulty of accurately estimating the systematic errors associated with a specific experiment, independent checks of both theory and experiment are of great importance.

At the end of the period of free electron precession experiments there was essentially only one experiment [195] with several interpretations [151, 56, 87, 77], and one theoretical calculation [117] at the highest level of precision. Apparently, this situation was considered uncomfortable.

Free electron spin resonance experiments

The third kind of experiments has its origin in an early experiment from 1958 by Dehmelt [45]. Although the experimental precision was too small to compete with the atomic level experiments of that time, his method also avoided the binding corrections that limited the atomic experiments.

Free electrons in a magnetic field have on top of the energy levels associated with orbital motion two distinct levels, corresponding to spin up and down. The level separation between these spin states is given by $\hbar\omega_s$ where ω_s is the spin-flip frequency $\omega_s = geB/2m$, which is proportional to both the g-factor of the free electron and the magnetic field B. In a magnetic field free electrons will tend to become polarized by aligning their spin directions with the field in a relaxation time depending on the environment of the electrons. Subjecting the electrons to a magnetic field oscillating at a frequency in the neighborhood of the spin-flip frequency, the electrons become depolarized, and the strongest depolarization happens exactly at the spin-flip frequency, a phenomenon akin to acoustic resonance.

By mixing the free electrons with a gas of sodium atoms, Dehmelt was able to exploit a coupling between the polarization of the sodium atoms and the polarization of the electrons. When the electrons were depolarized, the sodium atoms were also depolarized due to this coupling. The atomic depolarization could in turn be determined from the light absorption properties of the sodium gas. By varying the magnetic field, Dehmelt was able to sweep over the resonance and determine its position. The magnetic field itself was not measured directly, but determined indirectly in the same experiment from similar resonance measurements on the hyperfine Zeeman levels of the sodium atoms. Because the experimental conditions were unchanged, the ratios between the observed frequencies did not depend on the magnetic field and provided a field-independent basis for determining the g-factor.

In 1968 Gräff et al [75] continued the work started by Dehmelt on free electrons. The experimental resolution could not compete with the precision of the precession experiments by Wilkinson and Crane [199], but the experiment demonstrated [76] the feasibility of a direct resonance measurement of the anomaly. In the following we shall describe the principles of this experiment which forms the basis for all subsequent spin resonance experiments.

As already mentioned, a non-relativistic electron in a homogenous magnetic field B will move in a circular orbit with cyclotron frequency $\omega_c = \omega_0 = eB/m$. The radius of the orbit is given by $r = v/\omega_c$ and is proportional to the electron's velocity (and inversely proportional to the magnetic field). This means that if the velocity is lowered, the radius of the orbit is shrunk correspondingly. In classical (non-quantum) mechanics there is no lower limit to this phenomenon, and an electron in a circular orbit will in analogy with a classical atom lose energy to electromagnetic radiation and spiral in towards the center. Classically then, the electron velocity will grow towards infinity in the atom whereas in the magnetic field it will decrease towards zero.

Quantum mechanically both cases are, however, impossible because of the uncertainty relations which state that one cannot at the same time determine the velocity and the position of a particle with arbitrary precision. Consequently, for an electron in a magnetic field, like for an electron in an atom, there will be a lowest level of energy, a ground state, below which the electron cannot be found. Above the ground state there will be an infinite sequence of states, which for the electron in the homogenous magnetic field forms a ladder of equidistant levels, called Landau levels[150].

The distance between the Landau levels is (in the non-relativistic case) given by the cyclotron frequency $\hbar\omega_c$. If circulating electrons are subjected to oscillating

[150] Because of the similarity between the electron's behaviour in an atom and in a magnetic field and since the field in these experiments may be viewed as "anchored to the earth", the magnetically bound electron has been called geonium [178].

electromagnetic fields of this frequency, transitions to higher Landau levels with larger radius will occur. Eventually the electrons may collide with the surrounding material. In their first experiment Gräff et al [75] used this effect to determine the cyclotron frequency.

The electron's spin only slightly changes this simple picture in a way which is reminiscent of the precession case, although the physics is quite different[151]. If the g-factor of the electron were equal to 2, the spin-flip frequency $\omega_s = geB/2m$ would be equal to the cyclotron frequency ω_c, and an electron with spin up in a given Landau level would have precisely the same energy as an electron with spin down in next higher Landau level. Due to the g-factor anomaly, this is not strictly the case and there is a small difference in energy between the two situations. The frequency corresponding to this difference is $\omega_a = \omega_s - \omega_c = a\omega_0$, which is directly proportional to the anomaly. The anomaly may thus be determined from the same formula as in the precession experiments

$$a = \frac{\omega_a}{\omega_0} \tag{12}$$

Technically, an important advance entered in this experiment. The electronic orbits may drift along the magnetic field lines. In order to contain the electrons, Gräff et al employed a so-called Penning trap, in which an electric field is superimposed on the magnetic field. The electric field is generated by two negatively charged electrodes repelling the negatively charged electrons from the end of the containment region, together with a positively charged cylindrical electrode surrounding it. Although attracted by the positive cylinder the electrons are prevented from moving towards it by the circular motion imposed by the magnetic field, as long as the electrode voltage is not too high. The levels are influenced by the imposed voltage, and in the end Gräff et al [76] could extrapolate to zero voltage in order to extract the desired frequencies.

Another technical advance due to Gräff et al was that they were able to monitor the depolarization around the anomaly transition ω_a as well as around the spin-flip frequency ω_s using the same technique (spin-dependent inelastic scattering off polarized sodium atoms). The anomaly could then be extracted by forming the ratio of these frequencies, using that $a/(1 + a) = \omega_a/\omega_s$. In the end their experiments did not provide a high precision value for the anomaly, mainly due to problems in understanding the detailed shape of the observed resonance line widths.

In 1973 Walls and Stein [185] employed a different technique for monitoring the two frequencies in the Penning trap. The slow axial oscillation of the electron orbits along the direction of the magnetic field gives rise to a noise in the circuit connected to the end plate electrodes. The amplitude of this noise is coupled to the polarization of the trapped electrons and by monitoring the noise around the spin-flip resonance and the anomaly resonance, the frequencies may be extracted. As in the preceding experiments the value for the anomaly was not competitive with

[151] In the precession experiments the electron spin direction rotates at almost the same rate as the velocity direction, whereas in the resonance experiments the spin-flip level spacing is almost equal to the Landau level spacing. Loosely speaking, one may say that the electron precession experiments were classical, whereas the electron resonance experiments belong to the quantum realm.

the electron precession experiments of the time[152]. Problems with understanding the line widths also played a major role in this experiment, but the influence of the electric fields from the spatially distributed cloud of trapped electrons seemed to ultimately limit experiments of this particular type (see [182], p. 326).

The space charge problem would however be absent, if it were possible to trap a single electron and keep it under controlled conditions. In 1973 this was achieved in an extraordinary experiment [200] based on the observation that electrons in a Penning trap effectively behave like an electronic circuit resonating at the natural axial oscillation frequency of the trapped electrons. When brought into forced oscillations by means of a driving potential at a frequency near the natural one, the amplitude of the current in the circuit depends on the difference of the applied driving frequency and the resonance frequency, so that the response current is strongest closest to resonance.

The response is also proportional to the number of electrons in the trap. In the experiment it was demonstrated that it was possible to trap a few electrons and, by observing the circuit current, follow how they one by one were ejected from the trap when driven by a sufficiently strong potential. The last electron was held for a few minutes but that time could be made much longer by lowering the driving potential.

In 1977 this technique lead to a new high precision value [178] for the electron anomaly with a quoted uncertainty seventeen times smaller than the best spin precession result. By perturbing the large homogenous magnetic field with a small bottle shaped magnetic field, the resonance frequency in the axial motion could be made dependent on the quantum state of a single trapped electron. The changes were minute but could be observed through the changes in the circuit response. This made possible the essentially simultaneous determination of both the cyclotron frequency ω_c and the anomaly frequency ω_a.

At that time there were three different theoretical calculations of the third order term in the anomaly, leading to three different predictions for the theoretical value. The experimental value fell almost within the ranges of these predictions, which had a spread over about five times the experimental error [178].

Over the next decade [179, 180, 181] this method was refined to yield a further reduction of the uncertainty by a factor of fifty. The quoted error on the anomaly is now four parts per billion with the statistical error being much smaller than the systematic error. The systematic error is dominated by the influence of the cavity walls on the resonances, effects that have only been estimated.

The smallness of the statistical errors make the determination of cavity shifts paramount for getting even better experimental values for the electron anomaly. The authors themselves worry [182] whether their previous work could be "plagued by large cavity shifts". As of 1997 no new experimental results have been reported, apart from a preliminary result in agreement with earlier experiments [183], but new experiments are underway[153].

[152] Walls reported already in 1970 a preliminary value using this technique for the anomaly in his PhD thesis. As the result $a = 0.001,159,580(80)$ was never published we have omitted it in Table 1 (see [152]). It agrees but does not compete with with previous experiments and theory at the time.

[153] R. Van Dyck, private communication

It should be noted that this experiment is carried out by a single group in the world and thus lacks the dialog with competing experiments. In this sense the experimental situation appears not much different from the one at the end of the period of the precession experiments.

Theoretically the calculation of the electron anomaly has been carried through to fourth order in the fine structure constant [96]. The first three orders are now known analytically, whereas the fourth has been evaluated numerically including muonic, weak and strong contributions. The intrinsic error on the theoretical calculation is about four times smaller than the current experimental error. In calculating a theoretical value it is however necessary to employ a value for the fine structure constant. The problem is however that the error on this quantity carries through to the error on the anomaly which thereby becomes several times larger than the experimental error. Thus, in the current situation a limit has been reached in the comparison between theory and experiment[154].

Discussion

In the above account we have highlighted some of the places where inspiration from the theory under scrutiny, QED, *might* have been an important factor for the experimentalists when they established their final results. We emphasize again the uncertainty on this point since, as already indicated, the published papers cannot be taken to represent the full range of the experimentalists' motivations. Nevertheless, the published results indicate what the authors can be expected to defend. Moreover, by seeing these experiments in a historical perspective, it is possible to extract some important lessons on the interplay between theory and experiment.

Consider the question of theory-ladenness of the experiments. The first thing to note is that the QED formalism at no point was directly involved in the extraction of a $g-2$ value from the experiments. This is also true when corrections and estimations of systematic errors are taken into account. As we saw the experiments relied at most on Dirac's relativistic quantum mechanics which has $g=2$ exactly.

But a theory under test can also in other ways be involved in the experiment. In connection with the ending of the gyromagnetic experiments, Galison provides the following conjecture ([70] p.74):

> One might expect that in experiments where both strong theoretical predispositions and a definite quantitative prediction are present, it will often happen that the experimenter will end an experiment by finding the anticipated result, whether or not it corresponds with what is later found to be the case.

It is clear that the $g-2$ experiments (except for the very first) were pursued in the specific theoretical environment of QED. Without this theory there would not have been much point in pushing the experiments to higher and higher precision. But to

[154]Furthermore there are currently three different determinations of the fine structure constant which fall outside each other's error bars [97]. There are thus three different theoretical predictions of the electron anomaly, all of which disagree with the experimental value, although the disagreements are all only a few times the quoted errors.

what extent can history support a hypothesis that the theoretical environment of QED prompted the experimenters to get agreement with theory (e.g. by looking for systematic errors until an agreement was obtained)? Let us summarize the three phases of the $g-2$ experiments with this question in mind.

For the first atomic resonance experiment by Kusch and Foley there was only Breit's theoretical estimate for the g-factor of the electron, and Kusch and Foley's result was in disagreement with it. The following atomic resonance experiments initially agreed with theoretical calculations (by Schwinger, and Karplus and Kroll), but the last in this series (by Franken and Liebes) did not conform with theory. In turn, this led to reexaminations of the theoretical prediction revealing an error in the previous calculation. Accordingly, theoretical bias can hardly be blamed as the main factor in reporting the final result. While the anomalous magnetic moment of the electron "is one of the simplest quantities precisely calculable from first principles" [96], the history of the calculation shows that this does not imply simplicity in determining its actual value[155].

The free electron spin precession experiments with highest accuracy agreed in all cases with the theoretical prediction when they were published. However, the experimenters were not without reservations to this agreement. Schupp, Pidd and Crane [162] worried about the reasonability of their systematic corrections. Though Wilkinson and Crane were content enough with the situation so as to consider it to be the end of this series of the experiments, their laboratory was back in the same business only seven years later. At this time it was Wesley and Rich [194, 195] who came up with a more precise value and at the same time expressed clear concerns that their value did not agree with that of Wilkinson and Crane (or its three reevaluations [56, 151, 87]). Moreover, as we saw in fig. 4, the first three reevaluations of Wilkinson and Crane's result tended to shift the experimental $g-2$ value away from the theoretical value. Even when the struggle over the systematic errors in Wilkinson and Crane's experiment had ended with Granger and Ford's reanalysis [77], Rich and Wesley were uneasy with the situation, because there was only one experimental value at the highest precision, only one equally precise theoretical calculation, and only one analysis of the systematic errors which brought theory and experiment into complete agreement [152].

The free electron spin resonance experiments only became competitive with the best free electron spin precession experiments after 19 years (recall table 1). Since that time (1977) the Van Dyck-Schwinberg-Dehmelt group has been the only one reporting new measurements of $g-2$. Without exceptions these results have been in agreement with theory although, as we mentioned above, the current situation does not really permit detailed comparison between theory and experiment. Nevertheless, the Van Dyck-Schwinberg-Dehmelt group continues to work on getting the systematic errors under better control.

Turn now to the question of trust in experimental results in the light of the above discussion. An often cited criterion for belief in experimental results is its stability

[155] The calculation of higher order terms in the fine structure constant becomes increasingly complicated in higher orders. The numbers of Feynman diagrams involved in the calculation from first to fourth order in α are 1, 7, 72, and 891. The first three orders have been evaluated analytically, whereas the fourth has only been calculated numerically. [96].

under variation of the experimental circumstances [148]. By this criterion the result of Kusch and Foley from 1947 has been amply confirmed by a number of different experimental methods. By the same criterion we can have less confidence in the last three digits of the present $g-2$ value than in the first few digits.

Besides trust, the historical development of the experiments also supports a certain kind of realism. In *Representing and Intervening* Hacking argues that we ought to believe in the existence of electrons, as these can be manipulated to study other phenomena [80]. In the case of $g-2$ experiments the criterion of manipulability seems especially relevant.

Seen as a whole, the historical development consists in a gradual stripping away of the electron's environment, with a corresponding elimination of systematic errors. In the atomic resonance experiments, the electrons were found deep inside the atoms, making the extracted value dependent on complicated atomic-physics calculations. In the free electron spin precession experiments the electrons were removed from the atom and studied collectively in a magnetic field trap, but space charge problems due to their collective charges ultimately set the limit to this kind of experiment. Finally, in the single electron spin resonance experiments, the electrons in a Penning trap could eventually be controlled so well as to eject all but one of them from the trap.

Conclusion

In our view the historical progression of the experiments not only speaks in favor of the trust in the experimental results, but also supports the existence of electrons through their sublime manipulability. Thus, insofar as there are electrons, they have an anomalous magnetic moment.

We have not proven that the data analysis in the $g-2$ experiments was not influenced by knowledge of the QED predictions. We find it, however, implausible that this should be the case due to the long sequence of $g-2$ experiments with their continuing stripping of the electron's environment. This stripping process was entirely based on theory that did not involve QED itself. Hypothetically, these experiments could all have been performed and yielded their results without QED having ever been formulated.

The trust in the results constitute a clear empirical success for QED. Whether this implies that QED is necessarily the correct framework for describing the electron is another story[156] . In any case, a different theory would have to face up to the remarkable results for the anomalous magnetic moment of the electron.

[156]Schwinger, who initiated the theoretical calculations of $g-2$ from QED, later became very sceptical towards aspects of the QED framework resulting in his alternative *source theory* [166]. Some discussion of source theory which differs from QED with respect to the physical interpretation of the internal radiation processes leading to renormalization may be found in [156].

F The Casimir effect and the interpretation of the vacuum

Abstract[157] - The Casimir force between two neutral metallic plates is often considered conclusive evidence for the reality of electromagnetic zero-point fluctuations in 'empty space' (i.e. in absence of any boundaries). However, it is only little known that the Casimir force can be derived from many different points of view. The purpose of this note is to supply a conceptually oriented introduction to a representative set of these different interpretations. Our emphasis is on the major conceptual points in the various accounts given of the Casimir force. The different accounts demonstrate that the Casimir effect reveals nothing conclusive about the nature of the vacuum.

Introduction

The present work is part of an investigation of the modern concept of the vacuum and what can be said about its substantial existence on the basis of various experiments in the laboratory, and also evidences from astrophysics and cosmology. In this paper, we will examine the Casimir effect and its impact on the interpretation of the vacuum in quantum electro dynamics (QED). An important motivation for conducting such a study is the observation that a distinctive feature of quantum field theory (QFT) is the highly non-trivial notion of the vacuum. According to the standard QFT point of view, the vacuum is a very complicated *quantum state* (ground state) of the field, which is associated with concepts such as vacuum fluctuations and vacuum zero-point energy. The Casimir effect, according to the original proposal by H.B.G. Casimir in 1948, is the prediction of an attractive force between two electrically neutral and perfectly conducting parallel plates. The expression for this Casimir force is

$$\frac{\mathcal{F}}{\mathcal{A}} = \left(\begin{array}{c} \text{The force per unit} \\ \text{surface area between the} \\ \text{two parallel plates} \end{array} \right) = -\frac{\pi^2}{240} \frac{\hbar c}{d^4} \tag{13}$$

where $\hbar$ is the Planck constant, c is the finite velocity of electromagnetic propagation and d denotes the distance between the plates.

The Casimir effect is usually taken as important evidence for the physical reality of vacuum fluctuations and vacuum zero point energy. Less well known is that the Casimir force can be derived from other points of view, some of which do not employ the concepts of vacuum fluctuations or vacuum zero-point energy. In this paper, we shall briefly sketch and discuss some of these approaches and examine their implications for the current understanding of the vacuum.

Since Lorentz's ether, as a physical medium through which electromagnetic radiation propagated, was rejected by the special theory of relativity at the beginning

[157]Article written together with Svend E. Rugh and Tian Y. Cao. Submitted to Studies in the History and Philosophy of Modern Physics, February 1998

of this century, physicists afterwards were inclined to believe that the vacuum was in fact a void. Dirac's paper 'The Quantum Theory of the Emission and Absorption of Radiation' of 1927 [47] marked the beginning of the transformation of the vacuum concept into its modern versions[158]. For Dirac, the electromagnetic vacuum is a 'zero-state' [47], which is composed of an infinite number of light-quanta with zero energy and zero momentum. The relativistically invariant Dirac equation for the electron contributed further to the complicated picture of the vacuum. As a consequence of the negative energy solutions of the Dirac equation, the famous hole theory was introduced [48] which rendered the vacuum into a highly non-trivial structure. Far from being empty, the (electron) vacuum was reinterpreted as the state with lowest energy, the so-called *Dirac sea*, with the assumption that all negative energy states are filled in the vacuum (giving rise to an infinite charge of the negative sea)[159].

In the present texts on quantum field theory, explicit reference to the hole theory is usually absent, because Dirac's one-particle theory was immediately replaced by the so-called second quantized theory or quantum field theory. Even in the early 1930s when Dirac's conception of the filled vacuum was still in fashion and was the accepted way of dealing with antiparticles, it was already rejected by Pauli and some other physicists. First, Furry and Oppenheimer (1934) [68] recognized that by interchanging consistently the roles of creation and destruction of those operators that act on the negative states, the filled-vacuum assumption could be abandoned without any fundamental change of Dirac's equation. In this way electrons and positrons entered into the formalism symmetrically, as two alternative states of a single particle, and the infinite charge density of the vacuum disappeared. With a suitable rearrangement of the bilinear terms of the creation and destruction operators in the Hamiltonian, the infinite negative energy density can also be eliminated. The same method of exchanging the creation and destruction operators for negative states was also used in the same year by Pauli and Weisskopf (1934) [191] in their work on the quantization of the Klein-Gordon relativistic wave equations for scalar particles. The quantum theory of the scalar field contained all the advantages of the hole theory (particles and antiparticles, and pair creation and annihilation processes, etc.) without introducing a vacuum full of particles.

In sum, the above pair of papers showed that QFT could naturally incorporate the idea of antimatter without introducing the concept of the filled vacuum. However, the existence of vacuum fluctuations as a consequence of the uncertainty relations remain to be a striking feature of the quantum field theory.

For the fields in QED the vacuum is defined as the *ground state* or the lowest energy state of the theory. As we shall see, this 'lowest energy' is in fact infinite but, by arranging the operators in a particular order (normal ordering), the vacuum energy of all QED fields is eliminated (at least to lowest order in the perturbative series). Since Casimir originally explained the observed effect in terms of the vacuum energy of the electromagnetic field, it suggested that the Casimir effect may not be

[158]For an account on the pre-Dirac era of the vacuum concept, see e.g. the review by Saunders [159]. Since the vacuum concept after Dirac is based on an entirely different foundation, that of relativistic quantum mechanics, the pre-Dirac notion of the vacuum is not important here.

[159]For an account of the early developments in QED, see [163].

compatible with a version of QED in which normal ordering is carried out. However, as we will see, this is not the case.

An important reason for investigating the Casimir effect is its manifestation before interactions between the electromagnetic field and the electron/positron fields are taken into consideration. In the language of QED, this means that the Casimir effect appears already in the zeroth order of the perturbative expansion. In this sense, the Casimir effect is the most transparent feature of the vacuum. On the experimental side the Casimir effect has been tested on a number of occasions[160].

It is often held that a number of other experimental verified features of quantum field theory points to the reality of vacuum fluctuations and vacuum zero-point energy. Besides the Casimir effect some well known 'vacuum experiments' are the Lamb shift in the Hydrogen-atom and spontaneous emission of electromagnetic radiation from an excited atom[161]. However, it may be noted that all of these effects are related to material systems, e.g. atoms in the cases of Lamb shift and spontaneous emission. It is therefore not obvious whether the vacuum effects are inherent features of the material systems or of the vacuum.

In this paper we shall examine two essentially different ways of looking at the Casimir effect:

1. *The boundary plates modify an already existing QED vacuum.* I.e. the introduction of the boundaries (e.g. two electrically neutral, parallel plates) modify something (a medium of vacuum zero-point energy/vacuum fluctuations) which already existed prior to the introduction of the boundaries.

2. *The effect is due to interactions between the microscopic constituents in the boundary plates.* I.e the boundaries introduce something (the media) which give rise to the effect: the atomic or molecular constituents in the boundary plates act as (fluctuating) sources which generate the interactions between the constituents. The macroscopic effect (i.e. the macroscopic attractive force between the two plates) arises as a summed up (integrated) effect of the mutual interactions between the many microscopic constituents in these boundary plates.

The first view refers explicitly to the omnipresent existence of a fluctuating QED vacuum or, at least, to a vacuum with non-trivial properties which would exist (with these properties) also in the absence of the modifying boundary plates. Depending on the origin of the forces between the individual constituents in the plates, the second view may or may not support or refer to the existence of a non-trivial vacuum. When the boundary plates are viewed as dielectrical materials the plates are considered to be composed of atoms or molecules with fluctuating dipole moments (and, in principle, higher order multiple moments as well). The interactions between these fluctuating components are called *van der Waals forces.*

[160] The first experimental support for the original suggestion by Casimir of the attraction between two neutral perfectly conducting plates were given by Sparnaay in 1958 [176]. The most precise experiment measuring the originally proposed Casimir force has been reported by Lamoreaux (1997) [108].

[161] See, e.g., discussions and references in Milonni [128].

In the following, we want to illustrate the conceptual ambiguity of interpreting the very same quantitative force (13) by referring to four different interpretations of the Casimir effect[162].

The first interpretation is Casimir's original proposal in terms of vacuum zero-point energy of the electromagnetic field. Casimir's calculation (1948) is directly linked to the existence of a vacuum field between the plates. A second interpretation is Lifshitz's theory (1956, in English) where the Casimir effect is a limiting case (perfectly conducting plates) for macroscopic forces between dielectrica. Lifshitz theory employs a random fluctuating field in the plates whereas no explicit reference is given to an independent fluctuating QED vacuum field in between the plates. In fact, the electromagnetic field between the plates, which is generated by the fluctuations in the plates, is not treated as a quantum field, but as the solution to the classical Maxwell equations with sources generated in the plates. (However, as we shall see, the argument has been put forward, that QED vacuum fluctuations are needed, indirectly, in order to sustain the fluctuating sources in the plates). As a third approach we mention, briefly, that a calculation of the Casimir effect can proceed perfectly well within standard QED in which a systematic normal ordering of the field operators has been carried out (thus no zero point energy will be present while the vacuum fluctuations will remain). The fourth interpretation is based on Schwinger's source theory in which the vacuum is taken to be completely void (without fluctuating fields in the 'empty space').

The Duhem-Quine thesis of the underdetermination of theory by data implies that it is impossible for a theory to be verified, because, in principle, there is always more than one theory capable of explaining the same data. This thesis becomes much more interesting when one can actually point to other theories (rather than mere conjectures) with such capabilities[163]. In this case one must rely on other criteria for theory selection. Although it is not our main task to discuss such criteria, this paper will shed some light on the issue[164].

An 'experimental test' on aspects of the vacuum concept may also come from cosmology which could eventually speak in favor of particular interpretations of the Casimir effect and the vacuum. We have in mind the connection between vacuum zero-point energy and the so-called *cosmological constant problem*, see e.g. [187, 189].

[162]These four interpretations are representative for our purpose. In fact, there are more interpretations which are fully compatible with the quantitative predictions of the Casimir force, e.g interpretations in terms of stochastic EM (see e.g. [128]) and in path integral formulations of QED [19].

[163]Another interesting and much debated example of 'underdetermination of theory by data' is the different versions of quantum mechanics which have been constructed and which all appear to be compatible with presently known experimental constraints (see, e.g., J.S. Bell [6]). For a discussion on the relation between the underdetermination thesis and Bohr's view on quantum mechanics, see e.g. [123].

[164]We may note that the criterion 'simplicity' is difficult to use in the present case: Casimir's calculation is certainly both conceptually and mathematically simpler that either Lifshitz' or Schwinger's. However, as will be discussed, Lifshitz and Schwinger address a more general problem. Moreover, the resulting concept of the vacuum may also play a role in an evaluation of the simplicity of the theory.

The cosmological constant (Λ) refer to the term $\Lambda g_{\mu\nu}$ in the Einstein equations,

$$R_{\mu\nu} - \frac{1}{2} g_{\mu\nu} R - \Lambda g_{\mu\nu} = \frac{8\pi G}{c^4} T_{\mu\nu} \tag{14}$$

and a non-vanishing value of Λ will have measurable consequences for astrophysics and cosmology (see also e.g. [30]). According to standard QFT, the cosmological constant (Λ) is equivalent to the summed vacuum energy density from any known (and unknown) quantum field. The essence of the problem is that any reasonable theoretical estimate of Λ lies at least $\sim 50 - 100$ orders of magnitude above any observed value (which is either zero or close to zero). In turn, this discrepancy can only be circumvented by assuming that the various contributions to Λ cancel each other to an extreme accuracy, which at present understanding of QFT seems absurd[165].

Thus, the structure of the vacuum has important bearings on the conceptual foundations of QFT as well as contemporary discussions in physics and cosmology. Following the sections on the various approaches to the Casimir effect below, we shall return to their philosophical implications for current QFT. First, however, we provide some background for the vacuum concept in conventional QED.

Vacuum fluctuations and vacuum energy

1. Field Quantization

As we shall see in the next section, H.B.G. Casimir derived the Casimir effect from changes of the zero-point energy of the 'free' electromagnetic field that is confined between the plates. That a non-vanishing (fluctuating) electromagnetic field is present between the plates is entailed by the standard quantum field theory procedure known as *canonical quantization* in which the field modes of the electromagnetic field are represented as a set of quantum harmonic oscillators. In this quantization procedure, the electromagnetic field is first confined in a 'quantization volume' V giving rise to a certain discrete set of mode vibrations (normal modes) of the field[166]. The field is then Fourier expanded in terms of the normal modes and the coefficients (the amplitudes) in this Fourier expansion are replaced by operators, namely, annihilation ($\hat{a}_k$) and creation ($\hat{a}_k^\dagger$) operators, subject to a definite set of commutation relations. The *quantum state* of the field is specified by a set of integers $n_k = a_k^\dagger a_k$, one for each normal mode k (n_k is called the number operator and may be thought of as

[165] The cosmological constant problem does not, however, concern the "QED vacuum" in isolation. Other phenomena in modern quantum field theory, such as the process of spontaneous symmetry breaking (e.g. associated with the Higgs field) also contribute in building up an effective cosmological constant. The connection between the vanishing of the cosmological constant, which has been called a veritable crisis for high energy physics [187], and the vacuum concept was a main focus of some previous work [155].

[166] The utility of the harmonic oscillator picture rests on the *linearity* of the theory which is quantized and it finds application in the theory of electromagnetic interactions. By contrast, the theory of strong interactions, quantum chromo dynamics (QCD), is a highly non-linear theory, and its quantum states, in particular its ground state, cannot with good approximation be expressed in terms of harmonic oscillators.

the number of field quanta in the mode k with frequency $\omega = ck$). The Hamiltonian may be written

$$\hat{H} = \sum_k (\hat{n}_k + \frac{1}{2})\hbar\omega_k \equiv \sum_k (\hat{a}_k^\dagger \hat{a}_k + \frac{1}{2})\hbar\omega_k \tag{15}$$

where the sum is extended over all the possible normal modes compatible with the boundary conditions of the quantization volume. Contrary to its classical counterpart, each quantized harmonic oscillator has a zero-point energy ($\frac{1}{2}\hbar\omega_k$) and since the quantized electromagnetic field has an infinite number of modes, the resulting field energy is infinite.

The vacuum state of the theory is defined as the quantum state with lowest energy (i.e. the ground state of the theory). From eqn (15) we see that this is the state where there is no field quanta in any mode, i.e. $\hat{n}_k|0> = \hat{a}_k^\dagger \hat{a}_k|0> = 0$ (we shall return to the infinite value of the energy of this vacuum state shortly).

Moreover, when the field is quantized, neither the electric ($\boldsymbol{E}$) nor the magnetic field ($\boldsymbol{B}$) commute with the operator describing the number of photons in each field mode[167]. This means that when the number of photons has a given value (i.e. according to eqn (15), when the energy has a given, fixed value), the values of $\boldsymbol{E}$ and $\boldsymbol{B}$ necessarily fluctuate[168]. Equivalently, one may note that the commutation relations between the operators $\boldsymbol{E}$ and $\boldsymbol{B}$ precludes the possibility of having zero values for both the magnetic and the electric field in the vacuum state[169]. This is in sharp contrast to classical electromagnetism where $\boldsymbol{E} = 0, \boldsymbol{B} = 0$ is a valid solution to the Maxwell equations in the vacuum.

It follows from this discussion that both the zero point energy of the vacuum and the vacuum fluctuations are consequences of the quantization of the electromagnetic field. However, the zero point energy has a more formal character and can be removed by reordering the operators in the Hamiltonian by a specific operational procedure called normal (or 'Wick') ordering [197] (placing creation operators to the left of annihilation operators). It should be emphasized that the ordering of operators in a quantum theory is quite arbitrary and is not fixed in the transition from the classical to the quantum mechanical description of, say, the electromagnetic field.

[167]See e.g. Heitler [86] p.64.

[168]In general, the meaning of 'fluctuations' in a quantum mechanical quantity $\hat{\xi}$ is that the mean value (expectation value) may be zero, $< \hat{\xi} >= 0$ but $< \hat{\xi}^2 > \neq 0$. Thus, fluctuations of $\boldsymbol{E}$ and $\boldsymbol{B}$ in the vacuum state refer to the situation where their mean values are zero, but the mean values of $\boldsymbol{E}^2$ and $\boldsymbol{B}^2$ are non-zero.

[169]The commutation relations between the field components of $\boldsymbol{E}$ and $\boldsymbol{B}$ may be inferred from the commutation relations for the creation and annihilation operators in terms of which the quantized electromagnetic field components are written (see e.g. [86] pp. 76-87). According to the commutation relations, field strengths at two points of space-time which cannot be connected by light signals (the two points are space-like separated) commute with each other. This means, that at a *given instant of time t*, field strengths in different space points commute. In a given space-time point the different field components does not commute, however, and the commutator is in fact formally infinite. In view of the commutation relations between field components of $\boldsymbol{E}$ and $\boldsymbol{B}$ Landau and Peirls (1931) and subsequently Bohr and Rosenfeld (1933) [13] set out to investigate the physical interpretation of these commutator relations. It is important to note that these considerations, e.g. by Bohr and Rosenfeld, are confined to an analysis not about the fluctuating QED vacuum (when left alone) but to what one may operationally measure (with the aid of a measurement apparatus, viz. various test charge distributions etc.).

The normal ordering amounts to a formal subtraction of the vacuum zero point energy (a subtraction of a c-number) from the Hamiltonian. This leaves the dynamics of the theory and its physical properties, including the vacuum fluctuations, unchanged[170]. For example, the interaction between an atom and the electromagnetic field will remain the same irrespective of our choice of ordering of the operators.

Besides canonical quantization, another standard quantization procedure is the *path integral quantization* in which the amplitude for a quantum mechanical process is written as a sum (an integral) over many 'histories' (paths). In several respects, the path integral quantization is equivalent to canonical quantization (for some differences between these approaches, see e.g. Weinberg [190], Sec.9). However, it should be noted in passing that the vacuum fluctuations in QED are entailed by a field ontology interpretation of the canonical formalism of QED. In the path-integral formalism of QED, it is possible to give a particle ontology interpretation, which, at least, will render the concept of the vacuum fluctuations ontologically unclear (see e.g. [29]). Conceptually, the path-integral formalism resembles Schwinger's source theory approach (which will be examined in a later section) in the sense that its machinery for generating functionals (related to the Green's functions of the theory) utilizes the presence of sources.

2. Origin of fluctuations and the fluctuation-dissipation theorem

As we shall see, arguments in favor of an ontological substantial vacuum can sometimes be traced to the so-called fluctuation-dissipation theorem. In order to formulate this theorem we shall first discuss the general *origin of fluctuations*. One may note that

(1) In a given quantum system $\mathcal{S}$ which is in an eigenstate corresponding to some quantity (some quantum operator), say the energy $\hat{H}$, then another quantity which does not commute with $\hat{H}$ will generally fluctuate by necessity of the quantum rules. For example, if we consider an energy eigenstate (e.g. the ground state) in a harmonic oscillator with Hamiltonian $\hat{H} = \hat{p}^2/2 + 1/2m\omega_0^2\hat{x}^2$, the dipole moment operator $\hat{d} = e\hat{x}$ does not commute with the energy operator $\hat{H}$ and the ground state is not an eigenstate of the dipole moment operator $\hat{d}$. Thus, the dipole moment will fluctuate $<\hat{d}^2>=<e^2\hat{x}^2>\neq 0$ when the atom is in its ground state, yet its average value is zero, $<\hat{d}>=<e\hat{x}>=0$.

(2) If one has a quantum system $\mathcal{S}$ comprised of two *interacting* sub-systems $\mathcal{S}_1$ and $\mathcal{S}_2$ and the quantum state of the total system $\mathcal{S} = \mathcal{S}_1 + \mathcal{S}_2$ is an eigenstate of some quantity, then the two sub-systems will in general not be in an eigenstate of the same quantity. For example, consider the quantum system $\mathcal{S}$ which consist of an atom ($\mathcal{S}_1$) interacting with the QED vacuum field ($\mathcal{S}_2$). The total system is described by a Hamiltonian

$$\hat{H} = \hat{H}_{atom} + \hat{H}_{interaction} + \hat{H}_{vacuum}$$

Due to the interaction term the eigenstate of the total system will in general not be simultaneously an eigenstate of either the atom or the vacuum field. Thus, for

[170] The normal ordered Hamiltonian has expectation value equal to zero but $\boldsymbol{E}^2$ has a non-zero (in fact, infinite) expectation value implying that fluctuations are still present.

example, if we assume that the total quantum system $\mathcal{S}$ is in the ground state of the energy operator $\hat{H}$ and we restrict attention only to one of the two sub-systems, say the atom, then its energy ($\hat{H}_{atom}$) will fluctuate while the energy of the total system is conserved (and, thus, does not fluctuate). Quite generally, fluctuations appear when one restricts attention to a sub-system ($\mathcal{S}_1$), by tracing (or integrating) out some degrees of freedom (from system $\mathcal{S}_2$) which are present in the total system $\mathcal{S}$.

We now turn to *the fluctuation-dissipation theorem*, first derived by Callen and Welton in 1951 [23]. It will be of importance in the discussion about the origin of electromagnetic fluctuations in the microscopic constituents (atoms or molecules) comprising the two boundary plates in the Lifshitz approach, and also for the standard QED in the normal ordered picture.

We consider a quantum mechanical quantity $\hat{\xi}$ and a quantum mechanical system $\mathcal{S}$. If the quantum mechanical system is to remain in equilibrium, there will in general be an intimate relationship between the fluctuations $<\hat{\xi}^2>$ and the dissipation taking place in the system. A system is said to be *dissipative* if it is able to absorb energy when subjected to a time periodic perturbation. The system is said to be *linear* if the power dissipation is quadratic in the magnitude of the perturbation. For a linear, dissipative quantum system one may define a susceptibility $\alpha(\omega)$ which describes the response of the quantum system when it is subjected to a perturbation.

The susceptibility plays a fundamental part in the theory establishing the fluctuation-dissipation theorem, since the fluctuations $<\hat{\xi}^2>$ of the quantity $\hat{\xi}$ can be expressed in terms of $\alpha(\omega)$. Moreover, the dissipation in the quantum system may be expressed in terms of the imaginary part of the susceptibility $\alpha(\omega)$ (see e.g. [110] §123)

By invoking some simplifying assumptions about the quantum system, one may derive a simple quantitative relationship between the fluctuations $<\hat{\xi}^2>$ in the quantity $\hat{\xi}$ and the imaginary part of the susceptibility[171].

$$\begin{pmatrix} \text{Mean square fluctuation} \\ \text{in the quantity } \hat{\xi} \\ \text{(at frequency } \omega) \end{pmatrix} = 2\times \begin{pmatrix} \text{Imaginary part of} \\ \text{susceptibility } \alpha(\omega) \\ \text{(at frequency } \omega) \end{pmatrix} \times \left\{ \frac{1}{2}\hbar\omega + \frac{\hbar\omega}{\exp(\hbar\omega/kT)-1} \right\} \tag{16}$$

The factor in the braces {..} in equation (16) is the mean energy of an oscillator at temperature T; the term $\frac{1}{2}\hbar\omega$ corresponds to the zero-point oscillations of the oscillator. Since the analysis has been carried out for one monocromatic component (one frequency), the total mean square of the fluctuating quantity $\hat{\xi}$ can be obtained by integrating the expression (16) over all frequencies ω. The resulting formula constitute the fluctuation-dissipation theorem, established in the quantum context by Callen and Welton[172].

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[171] As examples of these simplifying assumptions, we mention that the energy eigenstates $\{E_n\}$ are assumed to be densely distributed and that the perturbation of the quantum system is of the form $\hat{V} = \hat{\xi} f(t)$ - that is, linearly in $\hat{\xi}$. It is assumed that the average value (expectation value) of $\hat{\xi}$, i.e. $<\hat{\xi}>$, is zero in the equilibrium state of the quantum system in the absence of the perturbation.

[172] They generalize a famous theorem, established in the classical context (at finite temperature

3. The fluctuation-dissipation theorem and the vacuum

An application of the fluctuation-dissipation relation above, of particular interest in connection with the vacuum and our discussion of the Casimir effect, is the case of an oscillating dipole. According to Callen and Welton's and Milonni's ([128] p.53) interpretations such a dipole oscillator dissipates energy which has to be balanced by an influx of energy from the fluctuating QED vacuum in order for the dipole not to die out in its motion.[173] In the language of the last section, the dipole is the system $\mathcal{S}_1$ whereas the QED vacuum is the other system $\mathcal{S}_2$. The equation which governs the motion of the dipole can be schematically represented as[174]:

$$\left(\begin{array}{c}\text{Undamped dipole}\\ \text{oscillation of an}\\ \text{electric charge}\end{array}\right)-\left(\begin{array}{c}\text{Dissipative force}\\ \text{due to the radiation}\\ \text{field of the dipole}\end{array}\right)=\left(\begin{array}{c}\text{Balancing external}\\ \text{fluctuation force}\\ \text{from the QED vacuum}\end{array}\right) \tag{17}$$

Since the dissipation term can be translated into an susceptibility $\alpha(\omega)$, the above relation is in effect a fluctuation-dissipation relation like eqn (16), cf. e.g. Callen and Welton [23].

We note that this argument for the necessity of the vacuum field, based on the fluctuation-dissipation theorem, rests on the assumption that the electromagnetic vacuum field is treated as an independent system (e.g. a system of harmonic oscillators $\mathcal{S}_2$). If an atom is approximately regarded as a fluctuating dipole oscillator, the above considerations imply that the reason why an atom is stable is that the QED vacuum supplies the fluctuation energy to keep the atom going.

The approach by H.B.G. Casimir

1. Forces between neutral, polarizable atoms/molecules.

When Casimir in 1948 [32], considered the attraction of two neutral metallic plates, it was well known that two neutral bodies may attract each other. In the dissertation of J.D. van der Waals[175] weak attractive forces between neutral molecules were introduced and the London theory [119] subsequently gave a quantitative and precise explanation of the nature and strength of the van der Waals forces as due to the interaction of the fluctuating electrical dipole moments of the neutral molecules [112]. Casimir's work on the forces between two metallic plates through the study of QED zero-point energies was a continuation of previous work with Polder on London/Van der Waal's forces [34] . As we shall see, Casimir refers explicitly to a fluctuating QED vacuum field, both in the presence of boundaries (e.g. two metallic plates) and in the absence of boundaries, i.e. electromagnetic vacuum fluctuations in 'empty space' prior to the introduction of any boundaries.

T) by Nyquist (1928) [138].

[173] The relevance of this example is that the van der Waals interaction (from which the Casimir force (13) may be build up) between the microscopic constituents in the two plates may be understood essentially as dipole-dipole interactions between fluctuating dipoles in the two plates.

[174] For a more detailed discussion, see also e.g. [86], [128], Sec.2.6 and [177].

[175] Dissertation, Leiden, 1873. Published in 1881.

We shall briefly introduce some key concepts necessary for the understanding of the following. First, if one has a system consisting of n charges with position vectors $\boldsymbol{r}_n$ the system of charges has a total dipole moment defined as $\boldsymbol{d} = \sum_n q_n \boldsymbol{r}_n$. If an electromagnetic wave is incident upon a quantum system, such as an atom (or a molecule), with a wavelength which is much longer than the linear dimensions of the system, an electrical dipole moment $\boldsymbol{d}$ is induced in the system; this dipole moment is proportional to the electric field strength $\boldsymbol{E}$ at the centre of the atom: $\boldsymbol{d} = \alpha \boldsymbol{E}$. The coefficient α is called the polarizability of the atom.[176] In the quantum theory, the polarizability of a system (an atom) in an assumed eigenstate $|n>$ of the atomic Hamiltonian (a harmonic oscillator) is given by:

$$\alpha(\omega) = \frac{2}{3\hbar} \sum_m \frac{\omega_{mn} |\boldsymbol{d}_{mn}|^2}{\omega_{mn}^2 - \omega^2} \tag{18}$$

where $\omega_{mn} = (E_m - E_n)/\hbar$ is the frequency corresponding to a transition $m \to n$ and $\boldsymbol{d}_{mn} = e < m|\boldsymbol{r}|n >$ is the electric dipole matrix element between states m and n.

Van der Waals forces refer to the attraction forces of electromagnetic origin which exist between two or several neutral atoms (or molecules) separated by distances that are large in comparison with their dimensions. If one neglected all fluctuations in the two neutral constituents there would be no forces of the van der Waals type. Their appearance is due to the fluctuating dipole moments (and, also, to higher multiple moments) that are produced in the two atoms[177].

London's (1930) expression for the potential describing the dipole-dipole interaction between two identical ground state atoms (molecules) with some transition energy $\hbar\omega_0$ between the ground and first excited levels and with a static (zero-frequency) polarizability $\alpha \neq 0$ is

$$\mathcal{U} = \mathcal{U}(r) = -\frac{3}{4} \hbar\omega_0 \frac{\alpha^2}{r^6} \tag{19}$$

The energy of the interaction between the atoms stems from the appearance of a correlation $< d_{1,i} d_{2,j} >$ between fluctuations of the dipole moments $\boldsymbol{d}_1$ and $\boldsymbol{d}_2$ of the two atoms. The London theory for the van der Waals forces is a non-relativistic calculation. Thus, it does not take into account the finite velocity of propagation of the electromagnetic interaction between the two molecules. If the time r/c of propagation of the interaction is much shorter than the characteristic periods of the motion of the electrons in the atoms, it is a reasonable approximation that the

[176] We keep the standard notation but note that α here means something different from the susceptibility in the previous section.

[177] The dipole moment operator for the atom is $\hat{\boldsymbol{d}} = e \sum_i (\boldsymbol{r}_i - \boldsymbol{r}_0)$ where $\boldsymbol{r}_i$ is the coordinate of the i'th electron in the atom and $\boldsymbol{r}_0$ is the coordinate of the nucleus. This dipole moment operator $\hat{\boldsymbol{d}}$ does not commute with the Hamiltonian $\hat{H}$ for the atom. Thus in an energy eigenstate of the atom, e.g. the ground state, the dipole moment (and the electric field it produces) will generally fluctuate by necessity of the quantum principles. The fluctuating $\boldsymbol{E}$ field, produced by the fluctuations of the dipole moment, will induce a dipole moment $\boldsymbol{d} = \alpha \boldsymbol{E}$ in another neighbouring atom (and vice-versa) and dipole-dipole correlations between the two atoms are thus created.

interaction between the atoms can be regarded as electrostatic.[178] However, when the distance of separation between the molecules is not too small it is important to take into account that the polarization of the neighboring molecules which are induced by a given molecule will be delayed as a consequence of the finite velocity of the providing interaction, $c < \infty$. When this relativistic retardation effect is taken into account one denotes the forces as 'long range retarded van der Waals forces'. Casimir and Polder published a paper in 1948 [34] with an expression for the potential energy U corresponding to two atoms (molecules) separated by a distance r,

$$\mathcal{U} = \mathcal{U}(r) = -\frac{23}{4\pi} \times \hbar c \times \frac{\alpha^2}{r^7} \tag{20}$$

In contrast to eqn (19), the velocity of light c appears in this expression (20) since retardation effects has been taken into account and the retardation causes the interaction energy to be proportional to r^{-7} (instead of r^{-6}).

One should consider the two mathematical expressions (19) and (20) for the interaction potential $\mathcal{U}(r)$ arising between two neutral, polarizable microscopic constituents (atoms or molecules) as limiting cases for small versus large distances. For distances in between those two limiting cases, there will be a more complicated formula which interpolates between equations (19) and (20) arising in the two limiting cases of small and large distances.

2. Force between two neutral, macroscopic plates

Whereas the result (20) by Casimir and Polder has been presented as the result of correlations between fluctuating dipole moments of the two atoms, the same result may be restated in terms of zero-point fluctuations of electromagnetic fields between the two atoms. Considerations of zero-point fluctuations of the electromagnetic field may also be applied to the situation of two identical electrically non-charged (neutral) parallel plates, consisting of perfectly conducting material, which are located at a distance d from each other.

Casimir's strategy is to compare two configurations of a conducting plate situated in a cubic cavity of volume L^3: the plate at a small distance d (in the z-direction) from a perfectly conducting wall and the plate at a large distance $L/2$ from the same wall (L is assumed much larger that d which in turn is assumed much larger than atomic distances). Casimir's point is that even though the summed zero-point energies of all the field modes are divergent for both configurations, the difference between these sums is finite. This finite difference is interpreted as the interaction energy between the plate and the wall (since it amounts to the energy which must be expended to move the plate from $z = d$ to $z = L/2$).

Casimir's calculation can be summed in the following four steps:

1. By assuming that the field modes vanishes on the boundaries, Casimir can write down the total zero-point energy for the configuration where the plate

[178] This condition can also be written in the form $r \ll \lambda_0$, where λ_0 is the radiation wavelength that characterizes the given atoms; typically $\lambda_0 \sim 0.1 - 1\ \mu m$.

is at $z = d$:

$$\frac{1}{2}\Sigma\hbar\omega = \hbar c\frac{L^2}{\pi^2}\int_0^\infty\int_0^\infty \Sigma_{n=0}^\infty\sqrt{n^2\frac{\pi^2}{d^2}+k_x^2+k_y^2}\,dk_x dk_y \tag{21}$$

where $k_z = \frac{\pi}{a}n$ (the wave number in the z-direction) and k_x and k_y are assumed continuous variables (yielding the integrals and L^2/π^2, since L is assumed large.[179]

2. By comparing this expression to the situation where the plate is at a large distance from the wall, located at the xy-plane (plate), Casimir obtains an expression for the energy difference. This difference, however, is not finite as it stands, so Casimir multiplies the integrands by a function $f(k/k_m)$ which is unity for $k << k_m$ but tends to zero sufficiently rapidly for $(k/k_m) \to \infty$, where k_m may be defined by $f(1) = \frac{1}{2}$.

3. In effect, this amounts to a frequency cut-off for high frequencies (or short wave-lengths) which is justified by noting that the plate is 'hardly an obstacle at all' for short wave-lengths[180]. That is, the assumption of perfectly conducting plates becomes invalid for wave-lengths comparable with atomic dimensions. Thus, only wave-lengths of the order of d down to interatomic distances contribute to the Casimir effect, i.e. the approach is sensible only if the distance between the plates is macroscopic in the sense that it is much larger than the atomic dimensions ($d\ k_m >> 1$).

4. The energy, and hence the force, per unit surface area can now (after appropriate mathematical manipulations) be expressed as in (13).

3. The attractive force between macroscopic plates build up from (van der Waals) interactions between constituents in the plates.

We shall in the following sections look at several different approaches to how the interactions between the constituents in the two Casimir plates may be considered to be the physical origin of the Casimir effect. Therefore, we note that, in principle, the Casimir effect can also be build up from the inter-molecular forces of the constituents in the plates as given by the Casimir-Polder interaction potential eqn (20). The technical obstacle in doing this is that the inter-molecular forces (van der Waals forces) are not simply additive. That is, the interaction between N constituents ($N \geq 3$) cannot be reduced to a sum of the pairwise interactions between the individual atoms of which the N-body system consist. This non-additivity arises because the superposition principle applies to the amplitudes of the electromagnetic

[179]The first term within the integration corresponding to $n = 0$ is in fact to be multiplied by a factor $\frac{1}{2}$. The reason is, that there is only one independent polarization mode of the wave for the $n = 0$ mode, whereas any other mode has two polarization modes.

[180]Note that the physically justifiable high frequency cut-off introduced by Casimir is quite different from the similar cut-off introduced in the renormalization procedure proposed by Feynman at the same year [60], which, as a formal device has to be removed at the end of calculation by letting it go to the infinity. For a detailed analysis of the difference between the formal cut-off and physical cut-off in the context of renormalization, see e.g. [26].

field, while the interaction energy depends on quadratic combinations of these amplitudes. That is, if we have several atoms, then the energy of their interaction is not equal to the sum of the energies of the interaction between the isolated atoms. In fact, the 3-body interaction can, indeed, be attractive or *repulsive* depending on the geometrical arrangement of the 3 atoms (see e.g. [5] and [128], p. 258). This contradicts the claim of some authors [54, 108] who, under the assumption that the van der Waals forces are always attractive, argue that Casimir forces (which has been reported to be repulsive in spherical geometry) cannot just be macroscopic manifestations of van der Waals forces between the microscopic constituents of the boundaries.

The lack of additivity of the van der Waals force prevents one from deriving the exact expression for the Casimir effect by integration of the forces which exist between the elementary constituents of the two parallel plates. This procedure of simply adding up the pairwise inter-molecular forces is justified only if the distribution of constituents in the two plates is very dilute (a 'gas' of polarizable, neutral atoms). Despite this principal remark, one may nevertheless reproduce the Casimir expression to within only 20% error by adding up pairwise inter-molecular forces ([128], pp. 219-220 and p.250)[181].

To build up the Casimir effect in this way, however, one still needs to account for the origin of the Casimir-Polder interaction potential eqn (20). In view of the fluctuation-dissipation relation discussed in the last section, the vacuum field may be required to explain the dipole fluctuations which give rise to the dipole-dipole correlations and the Casimir-Polder interaction potential between these fluctuating dipoles.

Macroscopic theory by E.M. Lifshitz

Recognizing that the inter-molecular forces (van der Waals forces) are not additive — and thus that pairwise summation of the forces between molecules is insufficient — E.M. Lifshitz (1956) started from the opposite direction in order to calculate forces between macroscopic bodies. As we shall see, his approach is based on classical electrodynamics and quantum mechanics (giving rise to fluctuations in the plates).

Lifshitz treated matter as a continuum, that is, from a purely macroscopic point of view. The initial assumption of Lifshitz is that the problem of forces between dielectrica can be treated macroscopically when the distance between the macroscopic bodies is large compared to those between the atoms. The Lifshitz theory concerns the general situation of forces between dielectric media with general dielectric constants ϵ. The specific Casimir result (13) is obtained in the (formal) limit of infinite

[181] In the case where the non-retarded interaction force (19) apply, i.e. if the distance between the two plates is small, this procedure of summing up the pairwise interactions had been employed in papers by De Boer (1936) [9] and Hamaker (1937) [81]. In that case they predict (a decade before Casimir) a macroscopic force of attraction between the two plates of the form $\mathcal{F}(d) = -A/6\pi d^3$ (i.e. falling off as d^{-3}) where $A = 3/4\pi^2 N^2 \alpha^2 \hbar\omega_0$ is called the Boer-Hamaker constant of the plate material. As the distance d between the two plates increases the force of attraction will interpolate between the d^{-3} (London-Boer-Hamaker) to the d^{-4} (Casimir) dependence.

conductivity ($\epsilon \to \infty$) of the two boundaries whereas the 'empty space' between the boundaries has $\epsilon = 1$. A macroscopic body in Lifshitz theory is assumed to have well-defined and well-known frequency-dependent dielectric susceptibility $\epsilon(\omega)$ and is considered non-magnetic[182].

From Maxwell's equations in a non-magnetic dielectric medium, Lifshitz infers the following wave equation for an electromagnetic field with frequency ω (later on, Lifshitz integrates over all frequencies)

$$\nabla \times \nabla \times \boldsymbol{E}_{\boldsymbol{\omega}} - \frac{\omega^2}{c^2}\epsilon(\omega)\boldsymbol{E}_{\boldsymbol{\omega}} = \frac{\omega^2}{c^2}\boldsymbol{K} \tag{22}$$

where $\epsilon = \epsilon(\omega) = \Re e(\epsilon) + i\Im m(\epsilon) \equiv \epsilon'(\omega) + i\epsilon''(\omega)$ is the complex dielectric constant and $\boldsymbol{K}$ is a random field in the dielectric medium, the origin and nature of which we shall return to in a moment. The imaginary part of $\epsilon(\omega)$ is always positive and determines the dissipation of energy in an electromagnetic wave propagated in the medium. In fact, the function $\epsilon(\omega)$ is related to the refractive index n and the absorption coefficient κ by the expression $\sqrt{\epsilon} = n + i\kappa$.

Conceptually, the interesting feature of equation (22), is the introduction of the random field $\boldsymbol{K}$. The imaginary part $\epsilon''(\omega)$ of the dielectrical susceptibility $\epsilon(\omega)$ determines the amount of absorbtion (dissipation) of energy taking place in the dielectric medium. Since ϵ'' is non-zero, one needs a source of energy to maintain equilibrium of the system, which in this case is provided by $\boldsymbol{K}$ (one may also reverse the argument and say that since a fluctuating field is present, one needs dissipation in the system).

A fluctuation-dissipation formula (like formula (16)) may be derived in this case of electromagnetic field fluctuations ($\boldsymbol{K}$) in the dissipative dielectric medium. The imaginary part, $\epsilon''(\omega)$, of the dielectric susceptibility $\epsilon(\omega)$ corresponds to the imaginary part of the generalized susceptibility, $\alpha''(\omega)$, in formula (16). The resulting equation in this case which corresponds one-to-one to eqn (16) was derived by Rytov [157] (Lifshitz refers in his first paper [118] to this work by Rytov) and reads

$$< \; K_i(\boldsymbol{r})K_j(\boldsymbol{r}^*) \; >_\omega = 2\hbar\, \epsilon''(\omega)\; \delta_{ij}\delta^3(\boldsymbol{r} - \boldsymbol{r}^*) \tag{23}$$

The fluctuations of the $\boldsymbol{K}$-field at two points in the body are correlated only in the limit when the two points coincide ($\boldsymbol{r}* \to \boldsymbol{r}$). In a macroscopic sense, the meaning of this limit (of zero distance) is that the correlations in the fluctuating $\boldsymbol{K}$ fields extend only over distances comparable with the dimensions of the individual atoms of which the dielectric medium is comprised. Note that $\hbar$ enters in the above equation for the random field. This is the only place in Lifshitz treatment where quantum mechanical effects are taken into account (for instance, there is no quantization of the electromagnetic field as opposed to Casimir's treatment). In Lifshitz first paper,

[182] In [111] (chapt. 13) a treatment is offered which also takes into account the possibility of magnetization of the medium, $\boldsymbol{L}$. To say that the medium is not magnetic amounts to setting the magnetic permeability $\mu = 1$ and $\boldsymbol{L} = 0$. Of course, if the magnetic susceptibilities of the interacting particles are not too small in comparison with their electric polarizabilities, then one also needs to consider the contribution to the interaction of spontaneous fluctuations of the magnetic dipole moments of the constituents. But, in most cases, the contribution due to the magnetic properties of the bodies is quite small.

the conceptual origin of the random field $\boldsymbol{K}$ is not obvious, but in a subsequent volume of Landau and Lifshitz one finds the following passage ([111] p.360):

> As a result of fluctuations in the position and motion of the charges in a body, spontaneous local electric and magnetic moments occur in it; let the values of these moments per unit volume be respectively $\boldsymbol{K}/4\pi$ and $\boldsymbol{L}/4\pi$.[183]

Thus, we take it, that for Lifshitz, the random field $\boldsymbol{K}$ is a consequence of the quantum mechanical zero-point fluctuations of the material components constituting the medium.

The remaining problem is to consider the boundaries. Lifshitz considers the configuration of three different media; a semi-infinite half-space ($x < 0$), a region extending from $0 < x < d$ and a semi-infinite half-space $x > d$. The three regions have dielectric constants ϵ_1, ϵ_3 and ϵ_2 respectively. In the following, we shall mainly be concerned with the case where the dielectric constant is the same for the two half-spaces $\epsilon_1 = \epsilon_2 = \epsilon$ and where we have vacuum in between the plates since this is the case where Casimir's original result can be recovered[184]. In this configuration, it is important to note that the field $\boldsymbol{K}$ is set to zero by Lifshitz between the two half-spaces, that is, in the vacuum:

$$\epsilon = 1 \ \ , \ \ \boldsymbol{K} = 0 \tag{24}$$

This means that in the calculation by Lifshitz it is only in the two dissipative media (the plates) that one explicitly considers the electric ($\boldsymbol{E}$) field to fluctuate (generated by the molecular constituents of the plates). In the 'empty space' between the plates there is no absorption ($\epsilon = 1$, no imaginary part of ϵ) and thus no fluctuations in the $\boldsymbol{E}$ field are enforced by eqn (23).

Lifshitz decomposes the random field $\boldsymbol{K}$ in Fourier components whereby the fluctuation-dissipation relation (23) becomes a relation between the Fourier components of $\boldsymbol{K}$. He then solves equation (22) with the (standard) boundary conditions that there is continuity of the tangential components of $\boldsymbol{E}$ and $\boldsymbol{H}$ across the boundaries of the different media. Lifshitz next step is to calculate the force between the half-spaces. The collection of boundary conditions and Maxwell's equations determine all the field amplitudes. In turn, these amplitudes determine the Maxwell stress tensor (the energy-momentum tensor for the electromagnetic field, see e.g. [109]) for a given frequency and since the force on an element of the surface of a body is the flux of momentum through it, the force in the x-direction at the boundary $x = 0$ is given by the xx-component of the Maxwell stress tensor at $x = 0$. To obtain the force, which has contributions from electromagnetic fields of all frequencies, Lifshitz performs a frequency integration.[185]

[183] As indicated above, the Lifshitz theory is in a first approximation not concerned with magnetic moments, so $\boldsymbol{L} = 0$.

[184] In Lifshitz' paper, the general case of media with different ϵ is treated. We shall confine the discussion to zero temperatures $T = 0$ (Physically, $T \approx 0$ is a good approximation when the temperature $kT \ll \hbar c/d$ (see e.g. the discussion in [118] p.82).

[185] Although the individual frequency modes are finite, the integrated result contains a term which diverges, hence Lifshitz must discard this (infinite) part of the solution. The argument for this

Lifshitz obtains a general and rather lengthy expression for the force F between the two half-spaces and it will not serve any purpose for us to quote this rather involved formula here. This formula enables Lifshitz to calculate the force between the two half-infinite dielectric media for any separation d, if the dielectric constants $\epsilon = \epsilon(\omega)$ in the bodies are known as a function of the frequency. Lifshitz then considers, as two limiting cases, the case of small and the case of large distances between the two half-spaces. These limits correspond to situations where retardation effects are relevant (long distance between the bodies) or irrelevant (short distance between the bodies).

For small separation d Lifshitz considers the special case of a rarefied medium where the dielectric constant in the two half-spaces $\epsilon \approx 1$, and he then obtains exactly the formula of London, eqn (19). In the opposite limit of large distances d, i.e. when the distances are large compared to the fundamental wavelengths in the absorbtion spectrum of the bodies (implying that retardation effects are important), Lifshitz argues that the frequency dependence of the dielectric constants may be ignored so that $\epsilon(\omega)$ can be replaced by the static dielectric constants $\epsilon(\omega = 0) = \epsilon_0$. For the resulting force between the two half-spaces ($x < 0$ and $x > d$), with dielectric constant ϵ_0 in the two half-spaces (plates) Lifshitz arrives at

$$F = \frac{\hbar c}{d^4}\frac{\pi^2}{240}(\frac{\epsilon_0 - 1}{\epsilon_0 + 1})^2 \varphi(\epsilon_0) \tag{25}$$

where $\varphi(\epsilon_0)$ is a function which has a value ≈ 0.35 for $\epsilon_0 \to 1$ and is monotonically increasing as a function of ϵ_0 to the value $\varphi(\epsilon_0) = 1$ as $\epsilon_0 \to \infty$. In the latter case ($\epsilon_0 \to \infty$) we regain the case of perfectly conducting half-spaces and equation (25) corresponds exactly to Casimir's result eqn (13). Moreover, in the limiting case where Lifshitz considers a situation where the two half-spaces are sufficiently rarefied he recovers exactly the Casimir Polder result for the van der Waals forces including retardation effects (eqn (20)).

We emphasize again that Lifshitz obtains these results without explicit reference to a fluctuating vacuum field between the two plates. In fact, in the approach above, the electromagnetic field is not quantized but it is still possible to regain Casimir's result. Nevertheless, Milonni ([128] p.234) notes that "Lifshitz acknowledges at the outset that his approach is connected with the notion of the vacuum field":

> ...the interaction of the objects is regarded as occurring through the medium of the fluctuating electromagnetic field which is always present in the interior of any absorbing medium, and also extends beyond its boundaries, - partially in the form of travelling waves radiated by the body, partially in the form of standing waves which are damped exponentially as we move away from the surface of the body. It must be emphasized that this field does not vanish even at absolute zero, at which point it is associated with the zero point vibrations of the radiation field.

move is that the divergent term is in fact independent of the separation d of the two half-spaces and thus irrelevant to the problem of obtaining the force between the two. Lifshitz traces the divergent term to the field produced by the body itself.

It is worth noting, however, that Lifshitz is not assuming a zero-point electromagnetic fluctuation in vacuum independently of the fluctuations in the two dielectric half-spaces (as opposed to Casimir for whom the plates only provided necessary boundary conditions for the already existing vacuum field).

Normal ordering of QED and the Casimir effect

A calculation of the Casimir effect in which no explicit reference to zero point energies is made has also been conducted within conventional QED, e.g. by Milonni and Shih [129]. Their strategy is to normal order the operators describing the field and the dipoles in the plates. In effect, they show that there exists a freedom to choose different operator orderings within the standard QED framework to obtain Casimir's result.

The starting point is the quantum expression for the interaction energy between the dipoles in the plates (giving a certain polarization density $\boldsymbol{P}$) and the electric field $\boldsymbol{E}$:

$$< E >= -\frac{1}{2} \int d^3r \ < \boldsymbol{P}\boldsymbol{E} > \tag{26}$$

The plates are actively contributing to the energy and do not only function as boundary conditions for the fields as in Casimir's approach. Milonni and Shih write the electromagnetic field as a sum of two contributions, a free (vacuum) part E_0 and a source part E_S. By normal ordering the expectation value on the right hand side of eqn (26), all reference to the vacuum part is removed. This corresponds to the formal subtraction of the zero-point energy from the Hamiltonian as discussed earlier (normal ordering of the left hand side implies that the energy in the expectation value of the energy in vacuum is zero).

Milonni and Shih are able to derive Lifshitz' and Casimir's results through steps which bear some formal resemblance with Schwinger's approach which we shall return to in the next section[186].

We shall, however, not go further into their calculation. First, Milonni himself emphasizes the necessity of the vacuum field, due to the fluctuation-dissipation theorem, even though the vacuum field is formally removed by the procedure of normal ordering of the operators ([128], p.54). Second, normal ordering only removes the vacuum energy to zeroth order in the perturbative expansion in QED. If one accepts the standard concepts in QED, such as vacuum fluctuations and virtual quanta, then the normal ordering procedure will not remove contributions to the vacuum energy from the so-called 'vacuum-blob diagrams' arising in higher orders of the perturbative calculations.[187]

Source theory approach by Schwinger

[186]Some authors [54] have indicated that normal ordering is inadequate to take phenomena such as the Casimir effect into account. When the normal ordering is carried out for the combined system of fields and plates this point is invalidated.

[187]We thank Sidney Coleman and Holger Bech Nielsen for emphasizing this point to us in private communication.

Schwinger's work on QED can be divided into two periods. In the first period (1947-67), he made important contributions to the renormalization program which was mainly operating within the framework of local operator field theory. In the second period (from around 1967) Schwinger, however, became more critical towards the operator field theory and the related renormalization program. According to Schwinger, the renormalization prescription involved extrapolations of the theory (QED) to domains of very high energy and momentum (or very small spacetime scales) which are far from the region accessible by experiments. Thus, in Schwinger's view, the renormalization program contained unjustified speculations about the inner structures of elementary particles and was conceptually unacceptable ([166], see also [27]). He was also very dismissive to an alternative to QFT, S-matrix theory, which was very popular in the 1960s, because it rejected any microscopic spacetime description[188]. His critique of the major research programs in the 1960s led to the construction of his own alternative theory, the source theory.

The source theory is "a theory intermediate in position between operator field theory and S-matrix theory..." ([166], p.37) restricting attention to experimentally relevant scales. Thus it is phenomenological in nature. Whereas the fields in QED can be expressed by operators capable of changing the number of particles in a system, this role is taken over by the sources in Schwinger's theory: "The function of K [a scalar source function] is to create particles and annihilate antiparticles, while K^* [the conjugate of K] creates antiparticles and annihilates particles." ([166], p.47)[189]. The calculational apparatus in source theory resembles closely that of standard QED (and even closer if QED is formulated in the path integral formulation) and thus can be used, for instance, to calculate the Casimir effect, which is to be expected if source theory is to obtain the same numerical success as QED has achieved.

The crucial point for Schwinger is that the source formulation refers directly to the experimental situation. Nevertheless, in higher orders, the fields play the role of the sources in order to calculate, for instance, the anomalous magnetic moment and the Lamb shift ([167] p.20). In higher order calculations, source theory, like standard QED, encounters divergent expressions. These will be commented on below where we discuss the relation between renormalization and source theory.

The important conceptual difference between standard QED and source theory concerns the status of the field. In source theory, the sources are primary, they represent properties of particles in the particular experimental situation; and the fields are not independent of sources. Rather, the fields result from experimentally imposed sources (and this situation is not changed when fields, in higher order calculations, are given the role of secondary sources). Thus, conceptually, within the source theory framework, the vacuum must be empty: If there are no sources, then there are no field. Hence there are no field fluctuations in the void!

Despite the emphasis on phenomenology, source theory has a vacuum concept that goes beyond mere experimental considerations. In Schwinger's words [168]:

> ...the vacuum is the state in which no particles exist. It carries no physi-

[188] For a discussion of S-matrix theory, see also e.g. [25, 43].

[189] We keep Schwinger's notation but note that K now means something entirely different from Lifshitz' $\mathbf{K}$.

cal properties; it is structureless and uniform. I emphasize this by saying that the vacuum is not only the state of minimum energy, it is the state of *zero* energy, zero momentum, zero angular momentum, zero charge, zero whatever.

However, the usual interpretations of the Casimir effect posed a serious challenge to the source theory concept of the vacuum, and the major motivation for Schwinger's involvement in interpreting the Casimir effect was precisely, and explicitly, for defending his concept of the vacuum ([170] p.2):

> The Casimir effect is usually thought of as arising from zero-point fluctuations in the vacuum [reference to [16]]. This poses a challenge for source theory, where the vacuum is regarded as truly a state with all physical properties equal to zero.

The first response to the 'challenge' of the Casimir effect was given by Schwinger himself in 1975 [169] in an analysis of a simplified toy-model of QED where the photon is treated as spinless[190]. In what follows, we shall present some characterizing features in Schwinger et al's treatment [170] from 1978 where Lifshitz full expression for the force between dielectrica is derived from the standpoint of source theory.

The point of departure for Schwinger et al is an action expression appropriate to an external polarization source $\boldsymbol{P}$, leading to a set of electromagnetic field equations for $\boldsymbol{E}$:

$$-\nabla \times (\nabla \times \boldsymbol{E}) - \epsilon \frac{\partial^2 \boldsymbol{E}}{\partial t^2} = \frac{\partial^2 \boldsymbol{P}}{\partial^2 t} \tag{27}$$

We note the similarity to Lifshitz wave-equation (22) when $\boldsymbol{K}$ is identified with $\boldsymbol{P}$ and with the difference that Lifshitz expression is written down for a monochromatic electromagnetic wave of frequency ω.

The space-time evolution of the particles created by the sources are described by propagation or Green's functions (until the particles enter another source (sink) region). The Green's function Γ relates the source $\mathbf{P}$ to the field $\mathbf{E}$:

$$\boldsymbol{E}(x) = \int (dx')\Gamma(x,x')\boldsymbol{P}(x') \tag{28}$$

The field in eqn (28), is a measure of the effect of pre-existing sources on a weak test source in an intermediate region ([166] p. 145). Thus, the field is to be regarded as a derived quantity (a consequence of the sources), not an operator field with an independent existence. The field in the dielectrics at different points due to the dipoles is given by a field product which is inferred by comparing different expansions of the initial action expression[191] [170]:

$$iE_j(\boldsymbol{r})E_k(\boldsymbol{r'})|_{eff} = \Gamma_{jk}(\boldsymbol{r},\boldsymbol{r'},\omega) \tag{29}$$

[190] Schwinger was nevertheless able to derive the correct result for the Casimir force between two perfectly conducting plates by simply multiplying his final result by a factor 2, corresponding to the two different polarization states of the photon. As mentioned earlier, Casimir took the polarization states of the photon into account explicitly (see eqn (21)).

[191] A similar relation, formulated in the operator language, holds in conventional QED (see e.g. Brevik [18]). The subscript eff indicates that eqn (29) is only approximately valid (the mathematical expression for this (effective) field product is obtained by the comparison of two Taylor expansions of the action).

Substitution of (28) in (27) gives an equation for the Green's function Γ. The remaining problem is to solve the equation for Γ with appropriate boundary conditions. The energy momentum tensor of the electromagnetic field can be expressed in terms of the Green's function through eqn (29) and the force, acting on one of the plates in the Casimir effect, can be calculated from the expression of the energy momentum tensor. The result agrees with that of Lifshitz, hence with Casimir's in the limit of perfectly conducting plates.

In the last section, we noted that calculations within normal ordered QED resembled somewhat the calculations in source theory (more technically, both focus on the evolution of Green's functions). We emphasize here the difference between any conventional QED approach and Schwinger's source theory approach. The field appearing in eqn (28) is the field of the sources and it is a c-number field (associating each point in space with a vector) rather than a q-number (operator) field. In this respect, the source theory approach of Schwinger et al and the Lifshitz approach are more closely related since they both describe the electromagnetic field as a c-number field. The quantum nature of the fluctuations in the material enters only through the sources (suggesting a comparison between the random field $\mathbf{K}$ with the polarization source $\mathbf{P}$). In fact, Schwinger et al use an appendix to make the comparison with the Lifshitz approach explicit.

Why is the fluctuation-dissipation theorem, which seems to make the vacuum field necessary for consistency reasons in standard QED, not a problem for source theory? The answer seems to be that there is no place for the dissipated energy (from the dipole) to go. If there is no vacuum field (in standard QED, an ensemble of harmonic oscillators) where the energy can be dissipated into, then there is no vacuum-dipole fluctuation-dissipation theorem.

Renormalization, source theory and the vacuum

Schwinger's dissatisfaction with renormalization and his emphasis on the empty vacuum are aspects of the same problem because, at least within standard quantum field theory, the conceptual basis for renormalization is the substantial conception of the vacuum. Since our focus is on the Casimir effect, we can at first put renormalization aside because the Casimir effect is usually calculated only to the zeroth order of the perturbative expansion where no renormalization effect is to be taken into account. However, as is well known, there are other effects, usually associated with the vacuum, which involve higher order calculations (such as the Lamb shift or the anomalous magnetic moment of the electron). On the one hand one may therefore say that, for instance, the Lamb shift is a 'deeper probe' of the QED vacuum structure because it involves aspects of the renormalization prescription (which accompanies higher order calculations in QED). On the other hand, it can be argued that the Casimir effect comes closest to the vacuum because only the oscillator ground state energy (a zero order effect) is involved (in some interpretations of this effect). However, it is possible to carry out calculations of the higher order corrections also to the Casimir force[192]. Indeed, such higher order corrections to the

[192] This amounts to take into account the so-called polarization of the vacuum, an effect which in standard field theory is a consequence of the interaction between the electromagnetic and the

Casimir force between two plates have been calculated (see e.g. [14, 103]) [193].

In the framework of Schwinger's source theory, no renormalization procedure will be needed since the physical quantities, such as the electron mass, are fixed directly by experiments. This is conceptually different from the renormalization prescription in conventional QED in which the physical mass refers to the renormalized mass which is the sum of an infinite bare mass parameter and another infinite renormalization factor. Schwinger describes this renormalization prescription in standard QED as a "tortuous manner of introducing physically extraneous hypothesis, only to delete these at the end in order to get physically meaningful results" ([168], p.420).

Nevertheless, in order to account for higher order effects such as the Lamb shift and the anomalous magnetic moment, Schwinger does encounter infinities. Schwinger's treatment of these infinities is not fully transparent but he argues that they are removed by imposing certain physical requirements on the expressions[194]. Schwinger points out that the physical requirements are related to two features of source theory: First, the need of preserving the phenomenological description of initial and final particles in collisions as being without further interactions, i.e. the constraint that free particles do not have self-interactions ([167], p.20). Second, the theory is not extended to experimentally inaccessible regions and thus refrain from attributing physical significance to very large momentum values (or very small distance values) of the Green's functions ([167], p.41).

Discussion

Let us here restate the outcome of the analysis of four different approaches to the Casimir effect. As for Casimir's approach to the problem of the two conducting plates, it is instructive to cite a standard reference such as Itzykson and Zuber ([91] p.138). It is stated that "The original observation of Casimir (1948) is that, *in the vacuum*, the electromagnetic field does not really vanish but rather fluctuates." The modern QFT derivation (which corresponds to Casimir's original approach) can be written as

$$\begin{pmatrix} \text{Casimir energy} \\ \text{corresponding to} \\ \text{given constraints } \mathcal{C} \end{pmatrix} = \begin{pmatrix} \text{zero point energy} \\ \text{corresponding to the} \\ \text{vacuum configuration} \\ \text{with constraints } \mathcal{C} \end{pmatrix} - \begin{pmatrix} \text{zero point energy} \\ \text{corresponding to the} \\ \text{free vacuum} \\ \text{(i.e. without constraints)} \end{pmatrix}$$

This means, that the constraints (for example, the boundary constraints consisting of the two perfectly conducting metallic plates) modify an already existing vacuum energy.

electron-positron fields. In any order of perturbation theory beyond the zeroth order, the fluctuations in the energy of the electromagnetic field leads to formation of virtual electron-positron pairs which polarizes the vacuum.

[193] The recent experimental verification of the Casimir effect between two plates [108] has stimulated hope for verifications of such calculations of higher order corrections to the Casimir force even though these higher order corrections are very small of order $\sim \alpha^2 m_e^{-4} d^{-8}$ [103].

[194] These requirements are enforced on Schwinger's expressions by direct modifications of the divergent parts of the (higher order) Green's functions.

In Lifshitz's derivation, no explicit reference is given to a fluctuating operator field between the plates. However, the random field in the plates, which is taken as the origin of the fluctuations causing the Casimir attraction, rests on the fluctuation-dissipation theorem. At least for Callen and Welton, this theorem shows the necessity of a fluctuating vacuum field. Nevertheless, in Lifshitz theory, there is no assignment of any 'reality' to vacuum energies or fluctuations in the absence of the plates.

In Schwinger's source theory, one finds no necessity of the vacuum field. Instead, the effect is seen as a consequence of the effective fields created by the polarization sources (dipoles) in the plates. This is similar to Lifshitz' approach as far as the c-number electromagnetic field goes. It is different from Lifshitz', however, by emphasizing the strict emptiness of the vacuum.

The calculation based on normal ordered standard QED, conducted by Milonni and Shih, does not explicitly invoke the concept of vacuum zero point energy. Nevertheless, the framework of their calculation is still based on the 'necessity of the vacuum field' interpretation of the fluctuation-dissipation theorem.

As the discussion shows, the Casimir effect alone cannot distinguish between these different notions of the vacuum[195]. In other words, the Casimir effect itself does not unambiguously point to the existence of a fluctuating vacuum with zero-point energy in the absence of sources.

It is often held that Schwinger was a phenemenologist, which seems particularly reasonable when one reads his remarks on renormalization and source theory in the introductory chapter of his source theory books [166]. One can read Schwinger as if he was not concerned with the metaphysical questions concerning the 'reality' of the physical theories, but more with their practical consequences. However, as our quotes of Schwinger above indicate, the strong insistence of the emptiness of the vacuum seems to have more than just a calculational point to it.

The question therefore arises whether the vacuum energy and fluctuations should be ascribed any ontological significance or be regarded solely as conceptual devices. In other words, is 'empty space' a scene of wild activity or is this picture an artifact of certain interpretations of the QED formalism? One author who appears to be clear on the ontological consequences of the Casimir effect is Weinberg. In the context of the cosmological constant problem, he writes [187]:

> Perhaps surprisingly, it was a long time before particle physicists began seriously to worry about this problem, despite the demonstration in the Casimir effect of the reality of zero-point energies [in the vacuum].

As the above discussion indicates, the various explanations for the Casimir effect invalidates Weinberg's assertion that the Casimir effect demonstrated the reality of the vacuum energy. Weinberg notes that it took a long time for physicists to worry about the cosmological constant. We may note that the Casimir result itself did not immediately attract much interest. Indeed, a citation study reveals that the interest in the Casimir effect has been increasing since Casimir obtained his result,

[195] Milonni has argued that certain equations within the various approaches to the Casimir effect can be related mathematically [128] but this should not obscure the fact that they rest on very different properties of the vacuum.

but that only little interest were paid to Casimir's paper in the first ten years after its publication in 1948[196]. Interest in the Casimir effect seems to have increased, also, with the publication in 1956 of the Lifshitz approach and a series of Russian experiments confirming aspects of the Lifshitz theory.

Our main point is thus that the Casimir effect does not give conclusive information about the vacuum. But, if the Casimir effect is not an essential demonstration of the vacuum energy and fluctuations, one may ask how it has nevertheless been used for this purpose. Some authors [54, 145] have suggested that a crucial step on the way of seeing the Casimir effect as an essential demonstration of the vacuum fluctuations was Boyer's result of repulsive Casimir effects, which seems intuitively unexplainable in terms of van der Waals forces that are always attractive (and thus an upintegrated effect seems implausible). There are at least two objections to such a view. First, as we saw, the van der Waal forces are not strictly additive (recall Lifshitz motivation for his theory) which makes repulsive macroscopic effects fully compatible with a van der Waals picture. Second, the calculations by Schwinger et al [132] were able to reproduce Boyer's result within the source theory which, as discussed above, does not employ vacuum fluctuations.

As far as the consequences of vacuum fluctuations go, the higher order corrections to the perturbative series are more directly examined in QED effects such as the Lamb shift. Thus, it should be properly investigated to which extent the Lamb shift and other 'vacuum phenomena' (such as spontaneous emission) can be held to be substantial evidence for a complicated vacuum[197]. Buried in the radiation field of the nucleus, however, the second (and higher) order calculation of the Lamb shift is hardly a direct probe into the vacuum. Much more clear, it seems, is the situation with the Casimir plates since nothing is involved except the very vacuum field and some boundaries. It just so happens, however, that these plates may be viewed as the source of the fluctuating field making the Casimir effect in itself unable to favor one over the other explanation.

These results being well known to, and partly obtained by, Milonni, he writes ([128] p.295):

> ...most physicists would agree on the value of a single concept [the quantum vacuum] that provides intuitive explanations for the "complicated and various facts of electromagnetism".

This may very well be the case. We shall here just remind of the problem of the cosmological constant and conclude with a remark from Abbott [1] where he compares vacuum field fluctuations to the ether of the 19th century: "With the mystery of the cosmological constant, perhaps we are again paying the price for dumping too much into the vacuum". Whether or not the vacuum of QED is a part of 'the ether of our time', its all important role in QFT demands continuous philosophical as well as physical investigations.

[196] Milonni has also reviewed the development and finds that citations of Casimir's article were at a maximum in the period 1975-1980 ([128], p.288). However, upon inspection of the *Science Citation Index* it seems that Milonni has overcounted the amount of articles citing Casimir's paper in the period from 1975-80. We find that the interest in Casimir's paper, judging from the number of citations, has increased at a steady rate up to the present.

[197] We may note here, that both effects can be accounted for in Schwinger's source theory [166].

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