Kuhnian theory-choice, the GWS model and the neutral current

Abstract

In the Kuhnian view of theory choice, theories, as a matter of empirical fact, often score differently well with regard to the standard theoretical virtues. The case I discuss in this paper, however, is a case in which there was one theory which was more virtuous than all its competitors. In such cases practitioners' disparate weighting preferences, which Kuhn is so keen to emphasise, make no difference to theory choice: practitioners' choices will converge on one theory despite their different weighting preferences. The case I discuss in this paper concerns the Glashow-Weinberg-Salam (GWS) model of electroweak interactions in the early 1970s. After considering the contemporary experimental evidence in its favour in detail, I argue that the GWS model was chosen not because the evidence in its favour was compelling, but rather because its virtues exceeded those of its competitors.

1 INTRODUCTION

In a widely received paper on theory choice, Kuhn (1977, 357-9) made three central claims. First, as a matter of empirical fact, different theories tend to score differently with regard to what Kuhn considered to be the standard set of theoretical virtues, i.e., empirical accuracy, internal and external consistency, scope, simplicity, and fertility. Whereas some theories will for instance be more empirically accurate than others, other theories will have greater external coherence with our background theories. Second, hardly ever does a theory's being virtuous in one particular respect legitimize a choice for that theory—not even when that virtue is empirical accuracy. Third, following from the first two claims, since different theories normally score differently with regard to the standard set of virtues, and since even a theory's being empirically accurate usually does not suffice for unequivocally tipping the balance in that theory's favour, different practitioners may therefore *legitimately* choose different theories. There is then no neutral algorithm for theory-choice to which all practitioners would be bound (for a recent discussion see Okasha 2011).[1]

There is a further claim Kuhn makes which *prima facie* makes things appear even gloomier: he holds that the virtues themselves are often "imprecise" so that different

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[1] Okasha (2011) applies Arrow's impossibility theorem of social choice theory to the problem of theory-choice, as described by Kuhn, and argues that Kuhn's "no neutral algorithm" claim is to be re-interpreted as there being *no* algorithm for theory-choice whatsoever, rather than too many algorithms (as Kuhn had it).

practitioners might come to different conclusions when assessing a theory with regard to a single virtue. There is for example a sense in which Copernican system is simpler than the Ptolemaic system: it requires only six planetary orbits in order to explain planetary retrogression qualitatively. On the other hand, the Copernican system required just as many epicycles as the Ptolemaic system in order to save the phenomena in quantitatively accurate way (Kuhn 1957). Thus, Kuhn concludes, two practitioners may prefer either theory on grounds of simplicity. But this problem seems to be no more than a communication problem. Although we may use the same label for two different kinds of simplicity (e.g. qualitative and quantitative parsimony) and therefore talk past each other when talking about 'the' simplicity of the theory, we should be able to sort out our differences once we sit down and argue about the details. Once we've done that, however, we still may weight those different kinds of simplicity differently. The 'no-neutral-algorithm' problem thus seems to be much more problematic than the 'ambiguity problem' (see Okasha 2011). One thing should be clear in any case: theoretical virtues are not some kind of subjective projection but rather objective properties of theories. It is primarily the *judging* of the relative importance of these properties which is subjective and which may cause trouble. And yet it need not. It need not in cases in which theories score highly on all (or nearly all) dimensions of theory choice, and moreover, score better than their competitors. In such cases, different practitioners may indeed have different weighting preferences but since one theory sticks out from the rest with regard to its virtues, different weighting preferences simply make no difference. If you prefer a simpler theory (of some kind) over a theory with broader scope, and I have the reverse preference, then if there's a theory that is both simpler and has a broader scope than any other available competitor, we should both accept that theory (despite our weighting preferences).

In this paper I shall argue that the acceptance of the so-called Glashow-Weinberg-Salam (GWS) model of electroweak interactions in the early 1970s is a good example for such an extreme case where all (but one) of the standard virtues clearly favored the GWS model. This episode then is an example for a theory choice scenario that Kuhn would have considered rather untypical. But only in the typical cases is the whole paraphernalia of subjective weighting preferences—which is really at the core of the Kuhnian view of theory choice—relevant. Hence, in this sense, this case constitutes no matter of Kuhnian theory choice. Where my case confirms the Kuhnian view of theory choice, however, is that empirical accuracy was not sufficient for choosing the GWS model. This is where the main focus of this paper shall lie.

Two senses in which a theory's empirical accuracy is not sufficient for theory-choice are mentioned by Kuhn (1977) and are rather pedestrian. First, theories can be transiently underdetermined by the evidence as for example the Copernican and Ptolemaic system around the mid until late 16th century. Obviously, if two theories are on a par with regard to empirical accuracy, the evidence cannot be used to decide between them. Relatedly, theories are never tested in isolation, so one theory entailing the evidence and another theory not entailing the evidence is not sufficient for accepting the former but not the latter theory; one of the auxiliaries of the latter theory, rather than the theory itself, may be the culprit.[2] Further, and most trivially perhaps, different theories can match experience better in different phenomenological realms without one theory gaining a clear empirical edge over the other. Empirical accuracy, in such cases, is of course not sufficient for choosing either theory for they are both empirically accurate (although in different domains). Kuhn claims that the accuracy in different domains and the problem of underdetermination are frequent and therefore to a large extent disqualify empirical accuracy as a tie-breaker in theory-choice.[3]

There is yet another sense in which empirical accuracy is insufficient for theory-choice which has played hardly any role in philosophical discussions: the problem of conflicting evidence. That is, different (carefully checked) experiments have produced data some of which support a theory's prediction and others do not support it. When there is conflicting evidence the evidence does not determine the choice for that theory, just as it wouldn't in a cases of underdetermination or in cases of different theories being empirically accurate in different domains. But may one rationally choose a theory for which there is conflicting evidence, just as one may rationally choose a theory that, for instance, is empirically accurate in one domain but not another one (as in the Kuhnian scenario)?

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[2] Often, the Duhem-Quine thesis is mentioned in the literature as a way of generating empirically equivalent theories, i.e., the central condition for the underdetermination thesis. Kuhn himself does not mention the Duhem-Quine thesis.

[3] It is worth noting that Okasha's rendering of Kuhn (see first footnote) is not entirely correct. Whereas Okasha assumes that in Kuhn's view *no* criterion can by default overrule any other criterion (Okasha calls this the 'non-dictatorship' condition of theory choice), Kuhn's view was much weaker than that. Since for Kuhn empirical accuracy is the "most nearly decisive criterion of all" (357) it would presumably overrule the other criteria when empirically accurate theories are compared with theories that are not (although Kuhn does not mention such cases). The problem of theory choice, as described by Kuhn, is therefore not "formally identically to a standard social choice problem". For another more extensive criticism of Okasha (2011) see Morreau (ms).

The answer to this question, at least to a large measure, of course depends on what is meant by 'choice'. Choosing a theory can mean two things: one can choose to *pursue* a theory and one can choose to *believe* a theory. Pursuit and belief are logically independent notions: one can pursue a theory, for example by spelling out its assumptions and its consequences, by doing experiments, etc., without believing that it is true, and one can believe in a theory without pursuing it. For example, a physicist might pursue string theory without believing in it. Conversely, a physicist might believe that the general theory of relativity is true without ever having worked on it. The constraints of rationality are of course much tighter on belief than on pursuit. In theory pursuit, as long as one remains consistent, and as long as one doesn't counteract one's other goals by pursuing a theory (and as long as one acts ethically), anything seems to be permissible. It would seem that it matters little in theory pursuit whether the evidence is conflicting. In fact, there might be no evidence whatsoever, like in string theory. In contrast, it seems as though one is entitled to rationally believe in a theory only if there is some evidence for the theory being true. Although conceptually we can neatly distinguish between pursuit and belief things get more complicated when we try to map those concepts onto the practice of science. Did physicists decide to merely keep pursuing Einstein's general theory of relativity after the famous 1919 light bending measurements or did these experiments raise their degree of belief in the theory (to put it in Bayesian terms)? It is difficult to establish these things. Scientists are usually rather tight lipped when it comes to making their (more fine grained) epistemic attitudes public. But in principle a whole range of attitudes is possible. A sceptical scientist might have withheld her judgment until, for instance, the famous Eötvös experiment provided evidence for Einstein's central postulate of the equivalence of gravitational and inertial mass in 1964. A full-blooded pragmatic scientist might withhold her belief from the theory as a matter of principle (maybe, because she's well-informed, bullet-biting, Popperian falsificationist) and merely pursue the theory as long as it is consistent with the phenomena. There is no reason to think that one would find any one of these attitudes to be particularly predominant. Even if one would, this would be completely separate from the issue of how to best interpret science. And most philosophers would think— and rightly so—that Einstein's theory was confirmed by the 1919 light bending experiments. Indeed most philosophers would think that Einstein's theory was strongly confirmed, given that it *predicted* light bending. Accordingly, it was rational to increase one's degree of belief in the theory given those results, and conversely, it would have been

irrational not to do so, regardless of the particular epistemic attitudes the members of the scientific community so happened to sport.

Let us then bracket off for a moment physicists' actual epistemic attitudes and ask whether one can, in principle, rationally believe a theory in cases in which the relevant evidence is conflicting. According to one view, one ought to withhold one's judgment since the different pieces of evidence simply neutralize each other. A theory with relevant conflicting evidence would thus be as good as a theory without *any* evidence. But I don't think this is so straightforward. It could of course turn out that all available data, despite the careful checking that has taken place beforehand (Franklin 2002), eventually turn out to be unreliable. In that case conflicting evidence is as good as a situation where there is no evidence at all for a theory. It may however also be the case that some of the data eventually turn out to be reliable and others do not. In that case there is *some* evidence. We just don't know whether the evidence is going to turn out to be evidence *for* or *against* the theory in question. At any rate, it would seem that in cases of conflicting evidence (produced by well-checked experiments) we need some constraints that compel us to consider one or the other data set as reliable (or none), and subsequently, to choose the appropriate theory (or none). Might those constraints be provided by a theory's other theoretical virtues? Perhaps. If in the past it regularly turned out that, say, a simple theory with unifying power generally tends to garner empirical support (after periods in which the relevant evidence is ambiguous), then one might be inclined to consider such a theory to be confirmed by the relevant positive evidence even in periods in which only conflicting evidence is available (see *blinded*). Although any particular simple and unifying theory may in fact turn out *not* to garner empirical support subsequently, I don't think it would be irrational to base one's belief in a theory on these inductive grounds. After all, much of our reasoning about the world is based on inductive inferences. Many of our beliefs would thus turn out irrational if inductive grounds were to be considered *no* grounds for rational belief. At any rate, it is not the aim of this paper to provide an epistemic account of theoretical virtues. Rather, the primary focus of this paper will be to argue *that* there exists an example where the facts suggest that a virtuous theory played the above-outlined role in the disambiguation of conflicting evidence, and that the so disambiguated evidence was used to justify that theory.

As mentioned above, the historical case in question concerns the Glashow-Weinberg-Salam (GWS) model in the early 1970s and the only available evidence relevant to confirming the GWS model at the time, namely evidence concerning the existence of the

so-called weak neutral current (NC). My discussion will take its lead from a remarkable statement about the acceptance of the GWS model by one of its 'discoverers' Steven Weinberg, made in his well-known book *Dreams of a Final Theory*, which I want to quote in full:

> One may ask why the *acceptance of the validity* of the electroweak theory was so rapid and widespread. Well, of course, the neutral currents had been predicted, and then they were found. Isn't that the way that any theory becomes established? I do not think that one can look at it so simply. […] *what really made 1973 different was that a theory had come along that had the kind of compelling quality, the internal consistency and rigidity, that made it reasonable for physicists* to believe they would make more progress in their own scientific work by *believing the theory to be true* than by waiting for it to go away. (Weinberg 1993, 97, my emphasis)

In other words, for Weinberg, the difference-maker for the "acceptance of the validity", i.e. the belief that the GWS model was confirmed, was *not* the discovery of the neutral currents, but rather the "compelling quality" of GWS's internal properties. Consistently with Weinberg's assessment and contrary to standard historical accounts I shall argue that the NC evidence in the early 1970s was conflicting and ambiguous. I shall point out that physicists nevertheless cited the available positive NC evidence in support of the GWS model, neglected the negative evidence, and did not pursue alternative interpretations of the results produced by the "NC discovery experiments". The explanation I want to offer for this behaviour is along the lines sketched above: it was the virtues of the GWS model that reinforced the belief that the neutral current existed. Accordingly all data contradicting this belief had to be flawed. The positive evidence was then used to motivate the choice for the GWS model. Whether the relevant members of the physics community *explicitly* committed to believing the model or not, if the GWS model did indeed play the role in the disambiguation of the evidence that I will argue it did, then physicists did not just pursue the theory. They believed it. Of course that belief had to be backed up by empirical evidence. And there was positive (well-checked) evidence that they could quote in support of the theory after they had disambiguated it. But whether that initial belief in the theory was justified or not of course depends on whether or not theoretical virtues are truth-conducive. And this, regardless of the inductive argument for it mentioned above, may well not be the case. But again, it is not the aim of this paper to provide a detailed epistemic account of theoretical virtues. What I do want to argue here is that this explanation is the most plausible one if we do not want to make the behaviour of the physics community irrational.

This is how I shall proceed. In Section 2 I shall briefly introduce the GWS model and its most important empirical prediction in the early 1970s, i.e. the existence of the NC. In Section 3 I shall discuss the experiments that were performed in the search for the NC and the evidence that they produced. Since the discovery of the NC has been studied in great detail by historians (Galison 1983; Pickering 1984a; Galison 1987; Miller and Bullock 1994), I will emphasise those aspects of the discovery which undermine certain claims made by standard accounts of the NC discovery, which have to be considered either false of simplistic. In particular, I will argue against that the view that single events were enough to support the NC discovery claim (Galison 1997), debunk the view that there was an argument made in one of the relevant experiments that was evidence "beyond doubt" for the existence of NC (Miller and Bullock 1994), and draw attention to open questions in the standard account about the "ending" of the discovery of the NC (Galison 1987). By venturing into territory that has not been touched upon by standard accounts of the NC discovery, I shall point out that (i) one of the experiments that produced a positive result that was taken to be evidence for the existence of the NC, in fact also produced a negative result that, despite being more informative with regard to the GWS model, was largely ignored, that (ii) the convergence of various experimental results, prima facie excellent evidence for the NC, is to be considered suspect, and that finally (iii) alternative interpretations of the results produced by the "NC discovery experiments" were available, but not pursued to a degree one might have expected. In Section 4 I will elicit the theoretical virtues of the GWS model, which provided the reasons not only for acceptance but also for belief, despite the ambiguous and conflicting NC evidence. Section 5 will draw the in my view appropriate philosophical conclusions from the historical case study.

2 THE GWS MODEL AND THE NEUTRAL CURRENT

There are four fundamental forces in nature: gravity, the electromagnetic, the strong, and the weak force. The recent history of physics can be viewed as an attempt to unify these forces in a single theory. One important step towards the (misleadingly coined) "theory of everything" was taken in the 1950s-1970s with the unification of electromagnetic and weak forces into 'electroweak' forces. The development of electroweak models go back to the early 1950s (see e.g. Pickering 1984b; Morrison 2000), but the first lasting contribution came from Sheldon Glashow. Motivated by several analogies between photons, the mediators of the electromagnetic force, and the hypothesized mediators of the weak force,

Glashow in 1961 devised an electroweak *gauge* theory of leptons, i.e. a quantum field theory in which the Lagrangian was partially invariant under *local* transformations. The gauge symmetry in Glashow's model was SU(2) × U(1), with four intermediate vector bosons (IVBs), as the mediators of force are also referred to, namely a triplet consisting of W^+, W^-, W^0 and a singlet (B^0). The neutral mediator of the weak interactions (later referred to as Z^0) was produced by 'mixing' the neutral member of the triplet and the neutral singlet (Glashow 1961). A major drawback of this model was the fact that the masses for the IVBs were inserted 'by hand' into the Lagrangian, rendering the model not only non-rigorous but effectively also non-renormalizable.[4] Progress was made six years later when Steven Weinberg and Abdus Salam, independently of each other (but Weinberg slightly earlier than Salam), developed a model in which the masses for the IVBs would be produced through *spontaneous symmetry breaking*[5] from the four massless IVBs assumed in Glashow's model. In Weinberg's model, these IVBs gained their masses through the Higgs mechanism, which postulates a set of scalar bosons at high energy ranges, namely an isospin doublet H^+, H^0 and its antiparticles $\overline{H}^+$, $\overline{H}^0$, which themselves gain mass through mutual self-interaction. When spontaneous symmetry breaking occurs, the masses of the $H^\pm$ and the H^0-$\overline{H}^0$ pair are absorbed by the $W^\pm$ and the Z^0, respectively. Since the photon absorbs no mass, a massive Higgs boson remains, which should be observable, but which, despite great efforts, to this day has not been observed. The only parameter left free in Weinberg's model (besides of the mass of the Higgs boson) is the so-called Weinberg angle (θ_w), which determines the 'mixing' of the initial neutral vector bosons W^0 and B^0 to yield a massless photon and a massive Z^0 boson.

An immediate challenge the GWS model was facing after its inception was the question of whether it would be renormalizable. In his proposal of the model, Weinberg had speculated that it would be, but was not able to prove it. This was one of the reasons, why the model was initially more or less ignored by the physics community (Koester et al. 1982). It took several years until the young physicist 't Hooft managed to come up with a

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[4] Renormalizability is the property of a theory by which certain types of infinites occurring in higher order approximations in those theories are eliminable by replacing them with the appropriate measured values (e.g. electron mass and charge).

[5] In spontaneous symmetry breaking (SSB) the field theory Lagrangian possesses a symmetry that the described physical system, *on the face of it*, does not possess. A well-worn standard example for SSB is ferromagnetism. A ferromagnet such as a bar magnet is made up of spinning particles which all align in one direction. However the Lagrangian for interacting spin particles is 'rotationally invariant', i.e., it shows no preference for any one direction. It is therefore assumed that the observed physical state (displaying no symmetry) comes about from a symmetric state after SSB at the critical temperature.

rather complicated version of the required proof (t Hooft 1971) that was subsequently simplified (cf. Pickering 1984b).

The clearest empirical prediction of the GWS model, which distinguished it from the then prevalent V-A theory of weak interactions by Feynman and Gell-Mann (1958),[6] was the existence of weak neutral currents as mediated by the Z^0 IVB. Since there were no detectors with energies high enough to detect the Z^0 particle (this became feasible only in the early 1980s), physicists set out to provide evidence for the existence for the Z^0 particle *indirectly*, as it were, through the detection of weak neutral currents (NC), which, if they were to exist, would leave characteristic signatures in particles interactions (see next section). Initially, however, it looked as though neutral currents were non-existent.

A well-known form of weak interaction in the 1960s, the hadronic decay of K-mesons, showed practically no signs of NC. These kinds of weak interactions involved only a particular type of weak interaction, namely ones in which the '*strangeness*' of the involved particles would *change*.[7] It was generally assumed that strangeness conserving interactions would behave no different than strangeness changing interactions, and there appeared to be little interest to establish this belief experimentally (Galison 1987). This of course changed dramatically with the NC prediction by the GWS model. Although the proponents of the GWS model could help themselves to the so-called GIM-mechanism when extending the model from leptons to hadrons, invented by Glashow Iliopoulos and Maiani in 1970 to suppress strangeness changing currents,[8] it still required the existence of strangeness conserving NC.

3   THE NEUTRAL CURRENT EXPERIMENTS

Clearly motivated by the predictions made by the renormalizable GWS model, several experimenters set out to detect strangeness conserving NC in deep inelastic scattering experiments. In these experiments, neutrinos were fired at nucleons with the aim to recover

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[6] According to the V-A theory, current—current weak interactions are a mixture of *v*ector and *a*xial vector parts, leading to the observed parity violation in weak interactions. In the first theory of weak interactions, Fermi had assumed that weak interactions have only vector character.

[7] Strangeness is a quantum number that was introduced as a "bookkeeping device" to accommodate the "strange" fact that kaon and lambda particles were produced at very high rates in particle collisions but decayed very slowly.

[8] The GIM mechanism was invoked to save the then prevalent theory of weak interactions, the V-A theory by Feynmann and Gell-Mann (cf. fn 6), from being refuted by the absence of strangeness changing NC in kaon decay. Although the V-A theory did not postulate a neutral IVB, the exchange of a W^+ and a W^- could mimic a Z^0 in higher orders of perturbation theory (Pickering 1984b, 183).

NC signatures being characterized by the incoming particles maintaining their identities (Fig. 1). The experiments that ultimately succeeded in doing so were the experiments
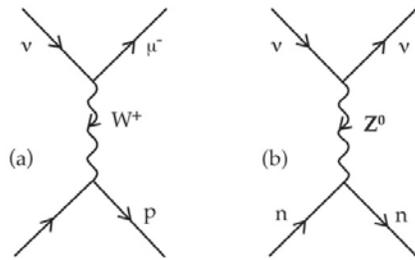

Fig. 1: Neutrino-neutron interactions. A) charged current event, mediated by a W^+ boson, which carries a positive charge from the reaction $v \rightarrow \mu$- to the reaction $n \rightarrow p$ (where v = neutrino, n = neutron, p = proton, μ- = muon); B) neutral current event, mediated by an electrically neutral Z^0 boson, where incoming neutrinos (upper left edges) retain their identity after interacting with a nucleon. Adapted from Pickering (1984b).

performed at CERN and the National Accelerator Laboratory (NAL; now: Fermilab) in 1973-4. A major obstacle to the detection of the NC, as in so many physics experiments, was posed by experimental noise that, observationally, was indistinguishable from the genuine NC events. The type of noise was different in different experimental contexts; its form depended on the type of chamber that was used to detect the neutral current. But in any case, there was no way to detect that noise experimentally. Rather, it had to be estimated computationally. The specific noise problems and the experimenters' struggle to somehow control them will be discussed in more detail in the following sections.

3.1 Qualitative and quantitative arguments at Gargamelle

At CERN, neutral currents were investigated in a bubble chamber called 'Gargamelle'. A bubble chamber consists of a tank superheated liquid (usually hydrogen, but here Freon) held under pressure to prevent boiling. When particles are "shot" into the tank, bubbles form along the tracks of *electrically charged* particles. These events are photographed and then analysed. On these pictures, only charged particles are visible. Neutral particles and interactions, like the NC (see Fig. 1), are not. Neutral particles and interactions must therefore be inferred from the other interaction products. This the CERN physicists did. They presented positive evidence for both the leptonic NC and hadronic NC. Let us consider that evidence in turn.

3.1.1 'Golden' events—no further data needed?

Leptonic neutral current events are very rare. The first event of this sort, in which a neutrino scatters off an electron, was found in a total of not less than 700,000 pictures (Hasert et al. 1973a). Galison (1997), in his discussion of the 'image' and 'logic' traditions

in high energy physics,[9] comments that this single event is "perhaps the best illustration of the demonstrative force that a well-structured *single* picture can carry" (22; added emphasis). In fact, Galison believes that this 'golden event' "swayed many physicists into believing for the first time, in the reality of neutral currents" and "*no further data had to be invoked*" (22-23; added emphasis). And indeed the CERN physicist Perkins (1997), for instance, writes that after the discovery of the leptonic NC event "everything that subsequently happened in the neutral current story was for me something of an anticlimax" (5). It would however be a mistake to believe that 'golden events' alone were sufficient for settling the question of whether or not the NC existed. Steven Weinberg, for instance, in a review article, in 1974 wrote:

> On the experimental side, there is of course the *one* (count them, one!) event of the $\bar{v}_\mu + e^- \rightarrow \bar{v}_\mu + e^-$ observed (Hasert et al. 1973a) recently at CERN. The background expected in this experiment was only 0.03 ± 0.02 events, so this *appears* to be definite evidence for a neutral current, *but with one event, who can tell?* (Weinberg 1974, 259, emphasis added)

Weinberg's cautiousness toward single event pictures was no exception at the time. The experimental physicist Sciulli, for instance, noted that "[…] particle experimentalists are generally very skeptical of a single event, especially when presented as evidence of a new phenomenon" (Sciulli 1979, 49). And even Perkins, despite the sanguinity he exhibited in the above quotation, in a recent CERN courier article stated much more cautiously that even "three [golden] events [that were found after 1.4 million scans, for which at least 5 events were expected] could hardly establish a new physical process" (Perkins 2003). Thus, although it is undeniable that the CERN physicists felt encouraged by their finding of the first golden leptonic neutral current event the latter was by no means sufficient for a discovery claim. And even if their findings had been more numerous, neutral currents still had to be established in the hadronic sector, to which we will now turn our attention.

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[9] Whereas physicists of Galison's 'image tradition' put emphasis on the production and analysis of detailed images of single events, physicists of the 'logic tradition' seek to generate large amounts of data in order to be able to use statistical arguments. The former tradition Galison associates with bubble chambers like Gargamelle at CERN (among others), and the latter with spark chambers (see below). Staley (1999) has convincingly argued that the persuasive power of these 'golden events' has to do with the probability of background events mimicking lepton NC events being rather low. Essentially, therefore, both traditions employ statistical arguments. The epistemologies associated with the two traditions, contrary Galison, are therefore not distinct.

3.1.2 An argument 'beyond doubt'?

In the experiments concerning the hadronic neutral current at CERN (with a higher event production rate than the leptonic NC), experimenters had to discern the NC signal from so-called 'neutron background'. Neutron background was caused by neutrons mimicking the NC within the visible chamber without the neutrons being associable with the charged currents they were triggered off by (see Fig. 2). In order to argue for background-free NC

Fig. 2: Neutron stars in bubble chamber.
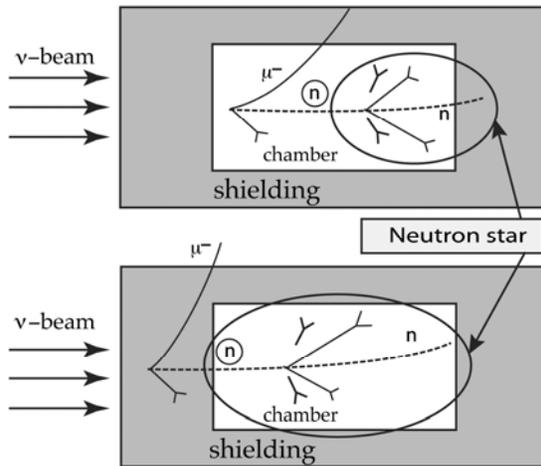
The figure shows two forms of so-called 'neutron stars' triggered by a neutrino beam. *Above*: a neutrino hits a nucleus, producing a muon (μ^-), hadrons, and a neutron (n). The neutron, again, hits another nucleus, producing even more hadrons, but without producing a muon, i.e. a neutron 'star'. Here the neutron star can unequivocally be associated with the charged current event, which is why these stars are also referred to as "associated events". *Below*: a neutron star is triggered in the invisible shielding, making the muon event (μ^-) undetectable. The neutron star within the visible chamber is indistinguishable from a neutral current event. This sort of event is therefore also referred to as non-associated or 'background' event. Diagram adapted from Haidt (2004). Also compare with the problem of „escaping muons" in the HPWF experiment (Fig. 4).

events, physicists at CERN sought to exploit the spatial distribution of the NC and neutrons respectively across the chamber (Hasert et al. 1973b, 1974). Miller and Bullock (1994) in their historical analysis of the discovery of the NC write:

> The problem of *proving beyond doubt* that some of the set of 'muonless events' were produced by neutrinos reduced, mainly, to demonstrating that their spatial distribution could not have been produced by the incoming neutron flux originating from neutrino interactions (Miller and Bullock 1994, 912, original emphasis)

Diagrams a), b) and d), e) of Fig. 3 display the spatial distributions of neutral current type events (NC) and charged current events (CC) for both neutrino (v) and antineutrino induced events ($\bar{v}$), respectively. Diagrams c) and f) display the ratio NC/CC for v and $\bar{v}$, respectively. Both the distribution of NC and CC is uniform along the length of the chamber, in the direction of the neutrino beam from left to right. Hasert et al. argued that *if* the neutral current-*type* events within the chamber were caused by *neutrons* rather than by neutrinos, one would expect an exponential decrease of these events along the chamber length (from left to right), simply because neutrons, in the present context, are secondary
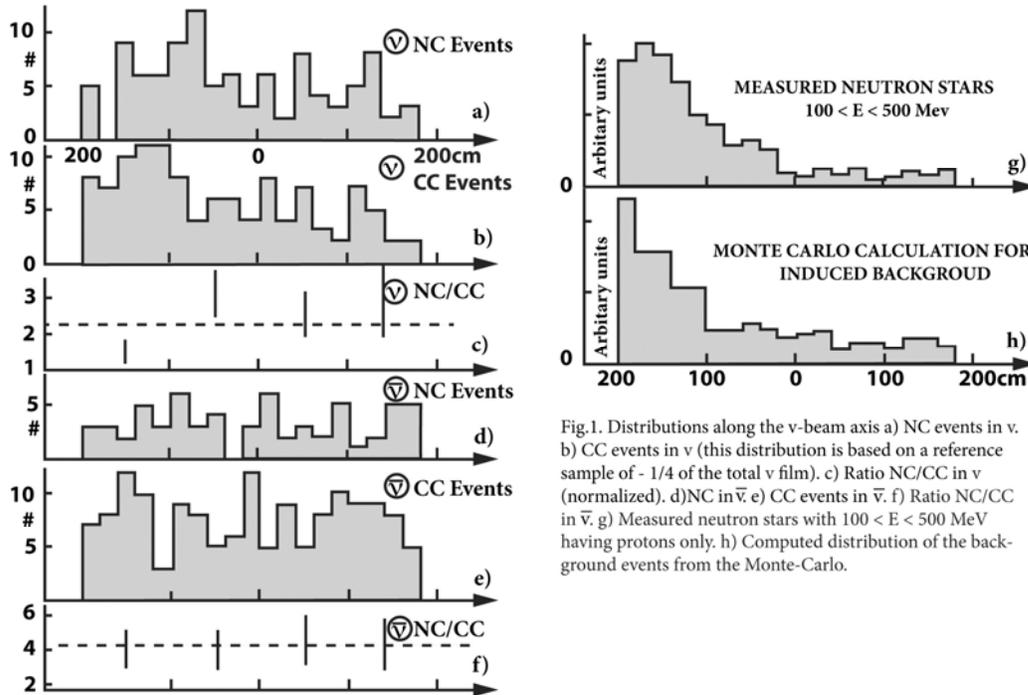
Fig.1. Distributions along the ν-beam axis a) NC events in ν. b) CC events in ν (this distribution is based on a reference sample of - 1/4 of the total ν film). c) Ratio NC/CC in ν (normalized). d)NC in ν̄ e) CC events in ν̄. f) Ratio NC/CC in ν̄. g) Measured neutron stars with $100 < E < 500$ MeV having protons only. h) Computed distribution of the background events from the Monte-Carlo.

Fig. 3: The argument from spatial distributions. See main text for details. Adapted from Hasert (1973b).

events and are thus less energetic than neutrinos. Diagram g) of Fig. 3 plots the *measured* neutron stars within the chamber (with clearly identifiable muons; cf. Fig. 2). This is to be compared with diagram f), which shows the Monte Carlo *estimates* of the neutron background events caused by unidentified CC events in the shielding. Both diagrams exhibit a similar distribution. This indicated to the researchers that the measured neutron stars in the chamber more or less covered all neutron background to be expected—if the estimates were correct. The measured and estimated neutron background could now be subtracted from the NC *candidates* in diagrams a) and d) giving the genuine NC events.

However, Dieter Haidt and Jack Fry, both members of the Gargamelle collaboration, drew attention to two points "which damped the euphoria" that had sprung from the spatial distribution argument (Haidt 2004, 28). The first point, although simple, was quite devastating. In the argument from distribution, it was assumed that neutrons would enter from the front of the chamber along the neutrino-/antineutrino beam only. This, however, was a gross oversimplification, because "neutrons entering through the side produce a flat distribution [caused by the radial distribution of the neutrino beam], just as neutral current events would do (Fry and Haidt 1975, 12). The second point, which was considered the "more dangerous" one (Haidt 2004, 28), concerned the fact that CC events could produce *neutron cascades* in the shielding of the chamber. That is, the penetration

length of the neutrons would be considerably elongated by the neutrons being knocked-off by the knocked off by the incoming neutrinos/antineutrinos setting free further neutrons. Neutrons would therefore penetrate much further into the chamber than without cascades. The argument from the different spatial distributions of neutrinos and neutrons inside the chamber was thus undermined. Hence,

> There is then a priori no longer a distinctive feature between n-[i.e. neutron-induced] and ν-[i.e. neutrino] induced interactions, unless by a *quantitative calculation* the proof is given that the number of n-induced interactions is a small fraction of the NC candidates *despite* the cascade effect (Haidt 1994, 192, altered emphasis).

In other words, the qualitative spatial distribution argument for neutral currents was only as good as the quantitative estimates of neutron background.

Given the critical importance of background estimates for discovery claims about NC outlined above (particularly, but not exclusively for hadronic NC), it is interesting to note that the background estimates were initially received with a fair amount of scepticism within the physics community. For instance, a review of the Gargamelle and HPWF results in the November 1973 issue of *Physics Today* concluded*:*

> Although both groups [Gargamelle and HPWF] suggest that they may be seeing neutral currents, they also offer alternative explanations. *And many experimenters are sceptical that either group has demonstrated the existence of neutral currents* … because the CERN group [viz. Gargamelle] had to employ a Monte Carlo calculation to obtain this result. (Lubkin 1973, 17-9)

Similarly, a review article in *Science* noted that the Monte Carlo calculations "represent the *least certain link in the chain of evidence supporting the CERN findings*" (Hammond 1973, 374). Consequently, the Gargamelle collaboration went to some length to somehow prove the validity of their estimates. One crucial measure they took was a set of test runs with protons, whose tracks, contrary to the ones of neutrons, are observable in the chamber, but which behave similarly enough to emulate the reactions triggered by neutrons. On the basis of their cascade programme, Fry and Haidt (1975), in an unpublished internal report, predicted in advance the proton-induced cascade length versus initial momentum (cf. Haidt 2004). The programme appeared to be validated rather well. Haidt presented these results at the *American Physics Society (APS)* conference in Washington in April 1974. The in Fry and Haidt (1975, 12)'s own words "*a posteriori* justification" of the cascade programme was then incorporated in a publication many years after the NC was accepted as real (Blietschau et al. 1977). Nonetheless, as it should turn out in due course the early results of the Gargamelle collaboration were significantly too low. Already in 1975, the CERN

collaboration concluded that their originally published R ratio of 0.22±0.03 had to be corrected to 0.34 $^{+0.17}_{-0.09}$ (Deden et al. 1975). We shall take up this issue again in Section 3.3, but let us first consider the HPWF experiments.

3.2 HPWF: mysterious ending and neglected negative result

In their search for the NC, the HPWF group at Fermilab used spark chamber experiments. Spark chambers consist of parallel metal plates with helium or neon or a mixture of those between the plates. The discharge, which develops along the ionised paths left by charged particles after applying a high voltage to the plates, allows one to visualise the events in the chamber. Spark chambers are equipped with counters that would register only those events specified by the experimenter in advance.

The main error source the HPWF group had to deal with were wide angle muons escaping the part of the chamber in which muons were detected (see Fig. 4). Just like the
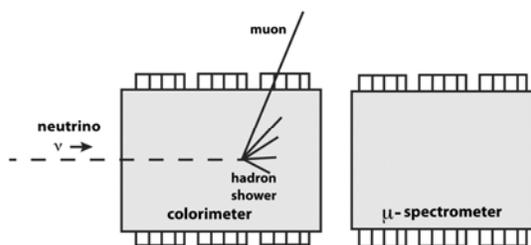

Fig. 4: Wide angle muons in the HPWF experiments. The figure shows a schematic diagram of the HPWF detector (including a calorimeter and a μ-spectrometer) and wide-angle escaping muons, which escape detection.

physicists at CERN, the HPWF group had to estimate the number of escaping muons with Monte Carlo simulations in order to determine the number of genuine NC events. And as we saw earlier, the trustworthiness of Monte Carlo simulations was a big issue in the physics community at the time of the discovery of the neutral current (see Section 3.1.2). In fact, although the HPWF group had already submitted an article announcing a positive NC result to *Physical Review Letters*, they sought to reduce their reliance on Monte Carlo estimates by minimising the distance between the calorimeter and the muon spectrometer in order to decrease the number of wide angle muons, which mimicked a NC signal. However, the reduction of the distance between the front and the rear of the chamber from four feet of iron to mere 13 inches of steel caused another unwanted effect: hadrons, which were produced by the neutrino-neutron scattering, might "punch through" the reduced separation into the muon spectrometer where they would automatically be identified as muons, therefore increasing the number of muon-ful events (and thereby reducing the NC/CC ratio). The punchthrough rate, like the number of neutron stars in the Gargamelle

experiments, had to be estimated by means of Monte Carlo simulations. Like in the Gargamelle experiments, the Monte Carlo simulations were calibrated against experimental data. And the punchthrough estimates that the HPWF group came up with seemed to fully account for *all* of the detected muon-ful events detected in the rear of the detector. Hence, the HPWF group came to the conclusion that the remaining NC signal was "statistically indistinguishable from zero" and drafted a paper with that result Galison (1983, 501). Eventually, however, the HPWF group was going to publish *positive* finding.

Although this shift in the reports of the HPWF group from a null result to the positive result is well-documented (leading to the physicists' joke of the HPWF group discovering "alternating NC"), the *reasons* for this shift remain entirely obscure (Galison 1983, 502ff; cf.Galison 1987, 235ff). *Why* did the HPWF group not publish the null result? *Why* did they not stay content with a result that they were convinced "definitely failed to give evidence of neutral currents" (ibid.)? Galison merely states that as a "result both of pressure from outside the collaboration [i.e., due to competition with the Gargamelle group] and of new evidence from within the group, *opinions were changing*" (my emphasis). Galison cites "three pieces of evidence": new Monte Carlo calculations, the spatial distribution of the events, and the finding that "among twenty neutral current candidates, five 'had no hint of wide-angle tracks'" (ibid.). However the first of those "pieces of evidence" does not constitute any evidence, let alone new evidence. Of course, the evidence for the NC depended on the Monte Carlo calculations. So a new Monte Carlo calculation could produce a different NC signal. And the trustworthiness of that evidence depended on the reliability of the Monte Carlo calculation, which, as mentioned above, depended on the accuracy of a whole set of background assumptions. But again, it would simply be a mistake to regard the calculations as evidence, let alone "new" evidence, as Galison seems to suggest. The second piece of evidence, as we saw above, was highly problematic. The spatial distribution of events could at best be indicative of the NC. The third 'piece' has got nothing to do with the problem in question, i.e., the problem of hadron punchthroughs. Rather, it has got to do with the first problem the HPWF group was struggling with, namely the problem of wide angle muons. But, as pointed out above, the attempt to control the first problem did raise worries about the second problem in the first place! So controlling the first problem did not imply a better control the second problem; if anything, the contrary was the case. It therefore remains highly obscure in Galison's account what the legitimate reasons were for the HPWF group to end their search

positively. There are further issues with the NC discovery that are not even mentioned by standard accounts of the NC discovery.

After the HPWF group had managed to produce a positive NC-result under obscure circumstances, which then nevertheless became widely known as the evidence that together with the evidence from the CERN group constituted the 'discovery' of the NC, the HPWF group carried out further experiments on the NC. A problem of the experiments by the HPWF group was that *mixed* neutrino/antineutrino beams were used. What was needed for determining the Weinberg angle of the GWS model, however, were *separate* NC/CC ratios to be obtained on the basis of *separate* neutrino (R) and antineutrino (R_{bar}) beams. Indeed, the HPWF published the results for those separate ratios in the same volume of *Physical Review Letters* in which their 'discovery' paper (with a mixed beam) appeared (Aubert et al. 1974a). This result by the HPWF group, contrary to the their (positive) mixed-beam-result and contrary to Gargamelle's result, was in fact *inconsistent* with the predictions of the GWS model (cf. Perkins 1997). However, this negative published result was largely ignored. Rousset (1974), for instance, in an early review of the experimental NC results in which he mentioned also the above 'negative' HPWF results, concluded that "*all* [!] the present experimental results *are compatible* with the existence of neutral currents, *as described in the Weinberg theory*" (163; added emphasis). That the negative result was indeed problematic is indicated by the fact that the HPWF collaboration in two of their later publications (Benvenuti et al. 1976; Wanderer et al. 1978) sought to undermine it. Essentially, they tried to explain their early low R ratios by supposing that (i) the neutrino/antineutrino beams had not been sufficiently pure and that (ii) the earlier results had been "statistically limited". But then again, their positive NC finding, which came to be quoted as evidence for the GWS model (as for example in the above quote by Rousset), was accepted as reliable *despite* these limitations. So it seems that physicists applied different standards for experiments producing GWS-supporting NC evidence on the one hand, and experiments producing evidence not supporting the GSW model on the other hand.

3.3 Fake convergence

Besides Gargamelle and the early HPWF experiment, there was a third experiment giving evidence for NC in the critical period of 1973-75 by a collaboration at Caltech (Barish et al. 1975). The results by all three groups were in good agreement within errors. As J. J.

Sakurai remarked a few years after the discovery, apparently also simply leaving aside the negative HPWF result:

> To me the most convincing thing about the three inclusive experiments was the following. The three groups used very different detection devices and very different neutrino beams; the background problems they had to face were very dissimilar […] Yet the final numbers obtained by the three groups more or less agreed with each other within errors. (Sakurai 1978, 51)

Here we probably have one of the clearest statements by a scientist of what has come to be known as the *robustness* argument in philosophical discussions about experiment (Hacking 1983).[10] Robustness arguments are no-coincidence-arguments: it would be an unlikely coincidence if the experimental results E were not reliable despite different experiments *making different background assumptions* all producing E. The robustness argument is regarded to be one of the most powerful experimental strategies for ensuring data reliability (Stegenga 2009). Interestingly, however, despite the convergence of NC results, particularly the value for the NC/CC ratio for neutrino beams (R) was much lower: it was only two thirds of today's value (0.2 vs. 0.3, approximately). Furthermore, the Weinberg mixing angle that was determined from R and R_{bar} in the early 1970s was 0.38; it now is 0.23 (Perkins 1997, 2003). *Prima facie*, this sheds significant doubt on the accuracy of the results in 1973/4. Although the reason for this discrepancy in the Weinberg angle may lay in the limited knowledge of the correct quark model at the time, which is required for fixing the Weinberg angle, the convergence of the R ratios is a completely different matter, for they determined independently of any quark model. So how does one explain this convergence on a value that was too low? Although too low results in a *single* experiment about a new phenomenon might be explained by over-cautious energy cuts (reducing the NC/CC rate), the *precise convergence* of results in *different* experiments, making *different* background assumptions, and operating in *different* energy ranges (1 GeV at Gargamelle vs. 6 GeV at HPWF and Caltech) is much harder to account for. The explanation I was given for this by physicists involved in the original experiments was that, firstly, in order to be on the safe side, Gargamelle applied big energy cuts, thereby reducing the number of NC candidates and bringing down the R ratio (Haidt personal communication). Since, secondly, the HPWF collaboration was struggling with their experimental apparatus, they *might* have been happy to receive some "numerical guidance" for the kind of result they should be aiming for (Perkins personal communication). Although I was not able to find

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[10] Interestingly, Hacking himself did not think it likely that one would find scientists articulating robustness arguments as explicitly as this (Hacking 1983, 201).

out why Caltech's result also coincided with Gargamelle's and HPWF's, one might speculate that the Caltech group, under the impression of the converging results of Gargamelle and HPWF, was compelled to calibrate their results against these previous results. These speculations of course presuppose that it was desirable for the HPWF collaboration to produce a positive result in accordance with the GWS model in the first place; otherwise HPWF might have looked for guidance elsewhere. One might be tempted to give a sociological explanation here along the lines suggested by Feynman (1974) for the replication of the measurement value for electron charge by Millikan, which also turned out to be much too low. Feynman suggests that this had to do with the authority of Millikan. But a wilful replication of the results of an authoritative source is implausible as an explanation of the replication of the low Gargamelle results. On all accounts, the authority in those days did not lie with CERN but rather with their American counterpart (e.g. Perkins 1997). Rather, I believe that the desirability of a positive NC result had to do with the attractions of the GWS model, which we shall consider in the next section. But before we can enter this discussion, there is a further aspect of the NC experiments which we need to draw attention to.

3.4 Alternative Interpretations

As both the Gargamelle and the HPWF group pointed out, their results admitted different interpretations; they did not *have* to be interpreted in terms of the NC as predicted by the GWS model (Hasert et al. 1973a; Benvenuti et al. 1974). Both groups in fact explicitly listed these alternative interpretations in their 'discovery' articles, which could not be excluded as alternative interpretations. They chose the NC interpretation, however, because it allegedly was "the simplest explanation of this result" (Benvenuti et al. 1974). As Andre Rousset, a physicist involved in the original experiments, remarked several years later:

> What is finally more surprising is the fact that nobody asked a more basic question on the Gargamelle collaboration results. Which proof can we give that the only interpretation of the NC selected events is the existence of the weak neutral currents? Even that the NC candidates are definitively not neutron interactions, is it demonstrated that there are neutrino reactions? … The interpretation as a weak neutral current interactions of neutrino is the most plausible, but it results mainly from a theoretical prejudice. (Rousset 1994, 349)

Crucially, the above list of alternative interpretations of the Gargamelle and HPWF results also contained heavy leptons decaying into hadrons. And it was heavy leptons that were

predicted by some of the competitors of the GWS model. Clearly then the theoretical prejudice Rousset mentioned had to do with the GWS model and its attractions, which we shall discuss in detail in the next section. Curiously, there was only a *single* experiment that systematically tested the prediction of the GWS's most attractive alternative, a model by Georgi and Glashow (GG), which we shall consider in more detail below. This experiment did not find any heavy lepton particles in the energy range predicted by the GG model, which was cited as a reason for rejecting the GG model (Barish et al. 1974; cf. Goldhaber and Smith 1975, 750f).

4   ON THE ATTRACTIONS OF THE GWS MODEL

The two previous sections undermined the view that there was any unequivocal evidence for the GWS's main empirical prediction in the early 1970s of the NC. Neither was there any "argument beyond doubt", any evidence that 'sealed the deal' (Section 3.1), nor did all NC evidence speak in favour of the GWS model. Although there was *some* evidence for the NC as predicted by the GWS model, there was also evidence against that prediction and evidence that was ambiguous. Instead of despairing in the face of the evidential uncertainties they were confronted with, they turned elsewhere for guidance, namely to the virtues of the GWS model such as unification, simplicity, beauty, external consistency, and fertility. The consideration of these properties in detail will be our concern for the rest of this section.

Unification. According to Margaret Morrison, who provides a comprehensive discussion of the electroweak unification in her book *Unifying Scientific Theories* (Morrison 2000), "[u]nity was not the goal or even the motivating factor" in the development of electroweak gauge theories by Glashow and Weinberg, and "[u]nity was simply not mentioned as a factor in the theory's acceptance, either by its founders or by their colleagues" (ibid., 125), and "[t]he fact that a particular model had succeeded, to some extent, in producing a unification was not compelling in any sense of the word" (134). I do not agree with any part of this assessment. Morrison bases her claim that the unification was not a motivating factor for the founding fathers of the GWS model on a personal email correspondence with Glashow and Weinberg. However at least from Weinberg's ground-breaking article (Weinberg 1967), which he considers as yet another attempt in the "very long" "history of attempts to unify weak and electromagnetic

interactions" dating back to the 1930s (1266), it is glaringly obvious that unification was indeed a central driving force:

> Leptons interact only with photons, and with the intermediate bosons that presumably mediate weak interactions. What could be more natural than to *unite* these spin-one bosons into a multiplet of gauge fields? *Standing in the way of this synthesis* are the obvious differences in the masses of the photon and interrnediate meson, and in their couplings. *We might hope to understand these differences* by imagining that the symmetries relating the weak and electromagnetic interactions are exact symmetries of the Lagrangian but are broken by the vacuum (1264; emphasis added).

Glashow opened his contribution to the GWS model six years earlier just as unmistakenly:

> At first sight there may be little or no similarity between electromagnetic effects and the phenomena associated with weak interactions. *Yet certain remarkable parallels emerge* with the supposition that the weak interactions are mediated by unstable bosons. […] *The purpose of this note* is to seek such symmetries among the interactions of leptons *in order to make less fanciful the unification of electromagnetism and weak interactions*. (Glashow 1961, 579-80 added emphasis)

Furthermore the *vast majority of proposed gauge theories* that were proposed in the 1970s sought to unify the two kinds of interactions. In fact, there was just a single (!) relevant theoretical approach at the time that would *not* seek to unify weak and electromagnetic interactions. And as we shall see in a moment, this approach had very few adherents. So whether or not they *explicitly* mentioned unification as a factor in the theory's acceptance, it strikes me as highly implausible that it wasn't such a factor.

Beauty. It is striking that *all* electroweak gauge theories presupposed that the relevant symmetries of nature were exact despite the appearances and therefore incorporated a Higgs mechanism (see Section 3). But what are the grounds for positing such exact symmetries? For Weinberg, they are of an aesthetical nature. Calling upon Plato's parable of the cave, Weinberg writes that although "nature does not *appear* very simple or unified … by looking long and hard at the shadows on the cave wall, we can at least make out the shapes of symmetries, which though broken, are *exact principles governing all phenomena, expressions of the beauty* of the world outside" (Weinberg 1979, 556, added emphasis). But are exact symmetries not reflections of nature, i.e., of what is really out there in the world? Perhaps, but we have only indirect evidence for such symmetries. And even though we *now* do have such indirect grounds for assuming those symmetries (cf. Kosso 2000), at the time of interest of this paper, such indirect grounds for the symmetries presumed by the GWS model were not available. At any rate, what really

matters for our purposes is that exact symmetries were a highly attractive property to the physics community.

The attraction of unification and exact symmetries was strong. So strong, in fact, that other empirically adequate approaches that did without electroweak unification and exact symmetries were completely ignored by the vast majority of the physics community. According to Sakurai (1978, 66), there were "only three people in the world engaged in the heresy of contemplating 'alternatives' [to electroweak models]—Bjorken at SLAC, my collaborator, Pham Quang Hung, and myself".[11] The paucity of response to these alternative approaches led to some frustration among its proponents. Bjorken (1979), for instance, after setting out his phenomenological approach which preserved "all the predictions of the standard model for neutrino-induced neutral currents [...] *without* assuming weak-electromagnetic unification, existence of intermediate bosons, or existence of a spontaneously broken local gauge symmetry" (335; original emphasis), concluded that "it is unlikely that either the arguments or the alternatives we have sketched have enough force to induce many theorists to abandon gauge theories. There are strong, albeit *mostly subjective, reasons* favoring the gauge-theory approach" (*ibid.*, 345; added emphasis). In another publication, Bjorken clarified that with "subjective reasons" he meant "those which persuade even in the absence of data" (Bjorken 1977, 702). Bjorken listed six such reasons: electromagnetic unification, the "intermediate-vector-boson hypothesis" that was used to effect unification in most models,[12] "the origin of gauge boson- and fermion mass", which in the gauge models was accomplished by SSB, i.e., "an underlying local gauge principle", renormalisability, and universality (1979, 345-6). At one point Bjorken found a rather drastic way to air his frustration about the lack of attention to his alternative approach:

> [...] it is hard to find *any* theorist (including myself) working actively and continuously on theories which lead in a direction contradictory to that of gauge theories [...] To be sure, the absence of criticism of gauge-theory ideology these days is quite understandable. To work on something else is to become a bit of a social outcast, and that is something the younger (untenured) generation may choose not to face. (Bjorken 1977, 701)

The preference for gauge theories and, in particular, the preference for the GWS model went deep. So deep, in fact, that it even affected data analysis.

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[11] For a systematic overview of those alternatives see Sakurai (1974).
[12] The only exception was a model by Georgi and Glashow (see below).

Sakurai (1974) had suggested that muonless events be interpreted in terms of baryonic current, rather than in terms of the NC mediated by the Z^0 particle, as envisaged by the GWS model. In this paper, Sakurai complained with—for publications in the *Physical Review* rather untypical—candour that experimentalists "by default" interpreted their data (i.e. muonless events) in terms of the GWS model and urged them in future to approach their data "*unhampered by theoretical prejudices*" (Sakurai 1974, 9, original emphasis). In the practices "followed by most experimental rapporteurs in major international conferences", however, physicists assumed that "half of the theory is correct", before comparing for consistency of the Weinberg mixing angle between NC/CC ratios for neutrinos and antineutrinos (Sakurai 1974). It was therefore "difficult to see whether models with qualitatively different sets of coupling parameters [than the ones of the GWS model] also fit the same data" (51-2). But what were the causes for this preference of the GWS model over its electroweak gauge theory alternatives?

Simplicity. There are three aspects in which the GWS model was simple. First, the fact that the GWS model was renormalizable, in Weinberg's words, meant that it satisfied a constraint which allowed "only a few *simple* types of interaction" (Weinberg 1979, 547). Second, it comes equipped with "the simplest possible" Higgs mechanism for the class of SU(2) x U(1) gauge models (cf. Weinberg 1979, 547 257), a number of which were proposed after the GWS model. Third, the GWS model posited no more heavy leptons than were known at the time of its proposal. With regard to the two last aspects, the GWS model had a clear edge over its competitors. Let us consider these aspects in turn.

In his Nobel Prize lecture Weinberg explained that renormalisability was a very important constraint on his theorizing about electroweak interactions. Taking inspiration from Dirac's use of simplicity in devising quantum electrodynamics, but being sceptical about "purely formal ideas of simplicity" in the context of "theories of phenomena which have not been so well studied experimentally", Weinberg wrote

> I thought that renormalizability might be the key criterion, which also in a more general context would *impose a precise kind of simplicity on our theories* and help us to pick out the one true physical theory out of the infinite variety of conceivable quantum field theories. (Weinberg 1979, 547)

Renormalizability imposes simplicity on theories because

> Roughly speaking, in renormalizable theories no coupling constants can have the dimensions of negative powers of mass. But every time we add a field or a space-time derivative to an interaction, we reduce the dimensionality of the associated

23

coupling constant. *So only a few simple types of interaction can be renormalizable.* (Weinberg 1979, 546-7)[13]

In particular, the renormalisability constraint ruled out SU(2) x SU(2) symmetry, which Weinberg had first considered for describing electroweak interactions in leptonic currents. Although Weinberg was not able to prove renormalizability of the GWS model when he proposed it (Weinberg 1967), he clearly hoped that it would turn out to be (Weinberg 1967). And as mentioned above, 't Hooft later did manage to prove the renormalizability of the GWS model (t Hooft 1971). Renormalizability, however, was no unique feature of the GWS model. On the contrary any forthcoming electroweak alternative model was going to have to satisfy this constraint. The second aspect in which the GWS model was simple, however, was indeed unique.

When Weinberg devised the mechanism for spontaneous symmetry breaking in the GWS model, he assumed that symmetry is broken in the "simplest possible" way for this kind of model (Weinberg 1974, 1979). Even several years after the GWS model was put forward and after a number of other electroweak gauge models had been developed (mostly SU(2)xU(1) alternatives; cf. Bernabeu (1977)), it was, according to a comprehensive overview article, "among all the gauge models proposed so far […] [still] *by far the simplest model incorporating unification*", for it contained the "simplest Higgs mechanism" (Sakurai 1978, 64, added emphasis).

Before the neutral current search in neutrino scattering experiments were announced at CERN and Fermilab (Section 3), the probably most attractive competitor to the GWS model in the early 1970s, as indicated by citation analyses (Koester et al. 1982), was the model proposed by Georgi and Glashow (1972) (not to be confused with the Grand Unified Theory (GUT) by the same authors (Georgi and Glashow 1974)). And interestingly, it is the *only* competitor model mentioned by *both* Glashow and Weinberg in their respective Nobel Prize lectures. There was indeed a sense in which the Georgi-Glashow (GG) model, which based on a SU(2) gauge symmetry group only, was *more* attractive than the GWS model. It provided a "more profound unification" than the GWS model, for it classed the neutral photon and the two charged massive vector bosons (W^+, W^-) in the *same* family of particles (cf. Weinberg 1974; t Hooft 1980), and it accomplished the electroweak unification without the postulation of a new neutral IVB (i.e., the Z^0 in the GWS model). But the GG model had several weaknesses. First, contrary to the GWS

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[13] As an example for a non-renormalizable theory Weinberg mentions the Fermi theory of weak interactions, in which "the coupling constant has the dimensions of $[mass]^{-2}$".

model, the electron mass was presumed to be the difference between two mass terms, one being bare mass and the other being generated by spontaneous symmetry breakdown. Bjorken pointed out that "[n]o rationale for the miraculous cancellation is given", rendering the model "utterly unbelievable". In order to render the plausibility of this model "at least non-vanishing", one had to introduce another Higgs particle into the model (Bjorken and Llewellyn Smith 1973, 33-4). In the GWS model, in contrast, electron mass (together with masses for the Z^0 and for the $W^\pm$ particles, given the mixing angle) simply dropped out of the derivations (Weinberg 1967). Furthermore, several authors considered the model "artificial" (Bjorken 1972) or even "ugly" (Glashow 1980) which probably had to do with its generalization to hadrons, which, in Georgi and Glashow's own words, contained "considerable arbitrariness" (Georgi and Glashow 1972). Third, and perhaps most importantly, the GG model was epistemically much more costly than the GWS model in terms of the number of particles it invoked: it required the existence of altogether five quarks and not less than four new heavy leptons. In comparison, the GWS model made do with four quarks and no new leptons.

External consistency. The GWS model proved to be consistent with the extension from three (u, d, s) to four quarks, with the addition of a quark (c) that Glashow coined 'charm', which had been argued for in 1964 by Bjorken, Glashow and others on the basis of symmetry between those four quarks and the four known leptons. There was not going to be evidence for charm until the mid-1970s (see below). However, it so happened that also the GIM mechanism, invoked to supress strangeness-changing neutral current in the GWS model (see Section 2), *required* the existence of charm. There were thus two aesthetically pleasing models (in terms of symmetry and simplicity) in different domains, which pointed to the existence of charm. In contrast, all alternatives to the GWS model postulated new quarks for which there were no independent (theoretical) reasons for belief. But these additional quarks were "unnecessary to describe the known hadrons" and indeed would have implied new quantum numbers for strong interactions. Such additional quarks were also referred to as "fancy" (De Rújula et al. 1974, 400). Again, the principle of parsimony appears to have weighed heavily on physicists' minds.

Fertility. The GWS model quickly proved fertile in that it, in Kuhn's words "disclose[d] new phenomena or previously unnoticed relationships among those already known" (Kuhn 1977, 103). After the discovery of the J/psi-particle in 1974 (Aubert et al. 1974b; Augustin et al. 1974), which gave first indications of the existence of a new quark, it took another couple of years until physicists had convinced themselves that the GWS

model's prediction of the weakly interacting charmed quark was correct (Goldhaber et al. 1976).[14] Leading to several further discoveries of new elementary particles—including the IVBs of the weak force in 1983, for which there had been only indirect evidence in the 1970s—the GWS model and its extension to the strong interactions around 1979, culminating in our today's 'Standard Model', turned out to be an extremely fertile research programme. Although this is perhaps the most impressive virtue of the GWS model, the information that it would trigger such a fertile research programme was of course not available to the physicists who made a choice for the GWS model and against its most promising early competitor, the GG model, in the early 1970s.

5 CONCLUSION

In this paper I argued that the GWS model itself, in virtue of its theoretical virtues, helped to disambiguate the NC evidence in its own favour. Such procedure is only non-circular if the virtues the theory possesses are truth-conducive. Of course, they usually are considered not to be so, largely because nobody has ever managed to provide a principled argument for the truth-conduciveness of theoretical virtues. That is, nobody has ever argued convincingly *why* our theories are supposed to be more likely to be true when they are simple, for instance. I too regard the prospects of such a principled argument to be rather dim. Still, the lack of such a principled argument does not imply that theoretical virtues, as a matter of fact, are not truth-conducive. There are indeed several non-principled arguments, one of which I mentioned briefly at the beginning of this paper. At any rate, there is another argument worth considering in this context. As mentioned above, the GWS model was clearly more virtuous than its competitors. In particular it came with the simplest Higgs mechanism and postulated the smallest amount of additional particles. According to Kuhn, however, it is typically not the case that one theory manages to score higher on all (or even most) virtue 'dimensions' than its competitors. Instead, different theories usually exhibit different virtues, thus rendering theory-choice often a matter of personal preference. So given that the GWS model was untypical in this sense, this might provide grounds for a no-coincidence argument: it would be an unlikely coincidence if a

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[14] As Pickering (1984b, 267ff.) points out, although there was evidence for hidden charmed states before 1976 this "shed no light on the weak interactions of the constituent quark", as predicted by the GWS model, since "the decays of the ψ and ψ' to the χs, η_c, η_c' and to normal hadrons were all dominated by the strong interaction". Before the abovementioned experiment by Goldhaber et al. (1976), a number of neutrino interaction experiments were performed to generate evidence for charm, but none of them were fully convincing (cf. *ibid*, footnote 28 on page 276).

theory did possess all of the standard virtues and not be approximately true. But of course, such an argument would presuppose that theoretical virtues have at least *something* to do with the theory possessing them being true; if theoretical virtues were *completely* divorced from a theory's truth this argument will not have any persuasive power. Then again, it might be a little premature to conclude from our failure to establish this link that there is none.

Acknowledgements

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