How to Discern a Physical Effect from Background Noise: The Discovery of Weak Neutral Currents

Samuel Schindler
Department of Philosophy
Division of History and Philosophy of Science
University of Leeds
Woodhouse Lane
Leeds, LS2 9JT, UK
s.schindler04@leeds.ac.uk

Abstract

In this paper I try to shed some light on how one discerns a physical effect or phenomenon from experimental background 'noise'. To this end I revisit the discovery of Weak Neutral Currents (WNC), which has been right at the centre of discussion of some of the most influential available literature on this issue. Bogen and Woodward (1988) have claimed that the phenomenon of WNC was inferred from the data without higher level physical theory explaining this phenomenon (here: the Weinberg-Salam model of electroweak interactions) being involved in this process. Mayo (1994, 1996), in a similar vein, holds that the discovery of WNC was made on the basis of some piecemeal statistical techniques—again without the Salam-Weinberg model (predicting and explaining WNC) being involved in the process. Both Bogen & Woodward and Mayo have tried to back up their claims by referring to the historical work about the discovery of WNC by Galison (1983, 1987). Galison's presentation of the historical facts, which can be described as realist, has however been challenged by Pickering (1984, 1988, 1989), who has drawn sociological-relativist conclusions from this historical case. Pickering's conclusions, in turn, have recently come under attack by Miller and Bullock (1994), who delivered a defence of Galison's realist account. In this paper I consider all of these historical studies in order to evaluate the philosophical claims that have been made on the basis of them. I conclude that—contrary to Bogen & Woodward (1988) and Mayo (1994)—statistical methods and other experimental inference procedures from the "bottom-up" (i.e. from the data to the phenomena) were insufficient for discerning WNC from their background noise. I also challenge Galison's notion of the "end of experiments" and shall take the wind out of the sail of Miller and Bullock's attack on some of Pickering's claims, whilst rejecting Pickering's sociological-relativist conclusions. Instead, I claim that an epistemic warrant from the 'top down' in the form of a theoretical postulate of the Weinberg-Salam model was necessary for "ending the experiments", i.e. for the acceptance of WNC as a genuine phenomenon in the scientific community.

Keywords: Weak Neutral Currents, experimental noise, discovery, phenomena, data, statistics

> What do we learn from studying the past? Answer: *If we don't have the right theoretical prejudice*, progress in experimental physics is likely to be very slow.—Sakurai (1978) on the discovery of weak neutral currents.

1 INTRODUCTION

How do scientists discern a physical effect or phenomena from noise? Which sorts of techniques and what sort of arguments do they use in this process? Under which conditions can scientists be certain that a particular effect or phenomenon is not an artefact but rather genuine? Trying to find answers to these questions should be a prior concern for philosophers of science. After all, we usually think that science is privileged as an epistemological enterprise in discovering the furniture of the world. Distinguishing effects or phenomena[1] from 'unimportant' background noise and 'intervening factors', which potentially mask 'genuine' effects, should be pivotal for any discovery claim. Therefore, clarifying this problem should have hugely important ramifications for philosophical notions of theory testing, inference procedures like the

[1] In this paper shall use the terms "physical effect" and "phenomena" interchangeably.

Inference to the Best Explanation (IBE), and last but not least for the all prevailing realism/antirealism debate. If we cannot be sure about the genuineness of a particular effect, how should we be able to test a theory that predicts that effect? How can we infer the causes of those effects, if we cannot be sure that the effects at hand are not genuine ones but rather vitiated by other effects? How can science be approximating the truth if there are no objective procedures for discerning and establishing genuine effects that form the backbone for scientific progress?

Despite the importance of finding out how scientists discern genuine physical effects from noise this problem has only fairly recently received attention from philosophers of science. The emergence of so-called *New Experimentalism*[2] in the 1980s has been very formative in this respect. Although many of the claims I am going to make in this paper may be read as a critique of New Experimentalism, I want to concentrate on those philosophical and historical accounts of the establishment of physical effects that have made the discovery of WNC the subject of their discussions (which happen to be show great sympathy towards New Experimentalism)[3]. Galison (1983, 1987) has tried to address this problem in the form of historical case studies, most notably, in his study of the discovery of weak neutral currents (WNC). Being a historian, Galison is often not very explicit on the philosophical views which underlie his histories. All the more explicit are Bogen and Woodward (1988) and Mayo (1994, 1996), who have used the discovery of WNC in order to support their philosophical accounts of effect-noise discrimination. Bogen and Woodward have argued for what one may call a "bottom-up" construction of phenomena from data, whereas Mayo

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[2] The term *New Experimentalism* was coined by Ackermann (1989) and refers to a movement that puts strong emphasis on the experimental aspects of science as being to some degree autonomous from theoretical science and, amongst other things, capable of establishing the reality of phenomena and even theoretical entities. Ian Hacking, Peter Galison, Allan Franklin, and Nancy Cartwright are often mentioned as the main proponents of New Experimentalism. Hacking (1983) is usually said to have originated this movement, the upshot of which is often illustrated with Hacking's dictum "Experiments have a life of their own independent of theory". See Mayo (1994) for three interpretations of this slogan.

[3] This (and the fact that I do not want to blow this paper out of proportion) is why I shall not explicitly discuss Allan Franklin's "epistemological strategies" in this paper, which he claims are vital for establishing physical effects and for ruling out artefacts. However, since I shall argue against the "bottom-up" construction of scientific phenomena (see main text), and since all of Franklin's epistemological strategies are experimental in nature, I take it that Franklin's

tried to give credence to her “error statistical” (also: “severe testing”) approach. Bogen and Woodward, and Mayo have all largely relied on Galison’s account which however has not remained unchallenged. Pickering (1983, 1984, 1989) has criticised Galison’s realist construal of the discovery of WNC and has argued for sociological-relativist conclusions. Pickering, in turn, has been attacked by Miller and Bullock (1994) who have defended Galison’s realist account of the discovery. In this paper I want to reconsider the historical details of the discovery of WNC in order to critically assess the philosophical claims that have been made with them. To this end, I will first of all introduce Bogen and Woodward’s, and Mayo’s general philosophical accounts of effect-noise discrimination in Section 2 and Mayo’s particular construal of the discovery of WNC in Section 3 in terms of statistical arguments. Based on the historical material, I shall then present a critique of all those philosophical accounts (in particular Mayo’s and to some extent Bogen and Woodward’s) which have assigned an essential role to statistical arguments in the discovery of WNC (Section 3). This I shall do by drawing attention to aspects of the experimental methods and evidence which are inherently unsusceptible to statistical arguments but which were nevertheless crucial in the discovery of WNC. In Section 4 shall also support a point made by Pickering according to which (i) the experimental apparatus in the 1960s was already capable of detecting WNC and (ii) the data obtained with this apparatus already indicated a WNC signal but was not recognised or assigned major importance to by the experimentalists. In Section 5 I shall discuss an argument which dealt on the expected spatial distribution of WNC events in so-called “bubble chambers”. This argument has been cited by Miller and Bullock (1994) as a reason why the 1960s chambers could not in principle have discovered WNC. Based on the historical evidence and the problem of neutron cascades in particular, I shall refute this claim and work out the all the more critical role of the neutron background estimates provided by the Monte Carlo simulations. In Section 6 I shall finally criticise Galison’s notion of the “end of experiments” and I shall argue instead for the establishment of WNC through an epistemic warrant from “top-down”, which I take to be incompatible with Bogen and Woodward’s construal of the establishment of

strategies can be subjected to similar criticism. For descent explicit criticism, see Pickering (1990), Rasmussen (1993) and the various responses it has provoked (see Rasmussen 2001).

phenomena in particular. I shall conclude this paper by making an argument of the generalisabilty of the present case.

2 Philosophical Accounts of Effect-Noise Discrimination

2.1 Bogen and Woodward's Bottom-up Construction of Phenomena

Where philosophers had long been talking about the relationship between theories and observations, Bogen and Woodward (1988) notoriously introduced the "third level" of phenomena and characterised them by contrasting them to traditional observable data. Whereas data 'cannot be predicted or systematically explained by theory', phenomena, being 'inferred from the data', can be predicted and explained, 'but in most cases are not observable in any interesting sense of the term' (pp. 305-6). The inference of phenomena from data is a one-way road:

> The direction of inference in such cases is "upwards" from the data to some feature of the phenomenon, rather than "downwards" from the phenomenon to features of the data" (Woodward 2000, p. S164)

In other words, the phenomena are constructed from the bottom-up from the data without the theory having any word in this construction process (see Fig. 1).

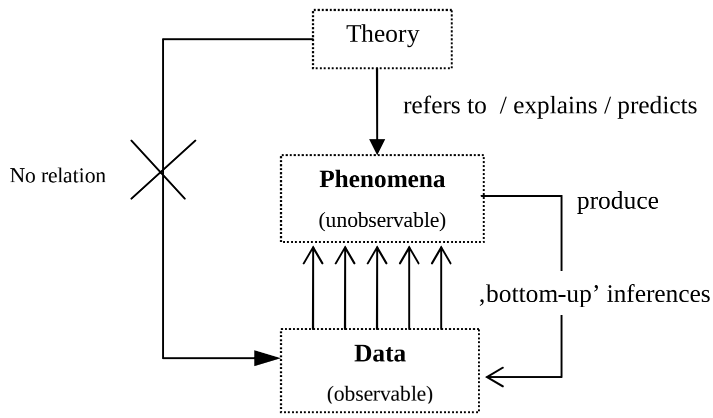

Fig. 1 Bogen and Woodward's data/phenomena/theory trichotomy. From Schindler (2007), modified.

Furthermore, whereas data are 'idiosyncratic to particular experimental contexts, and typically cannot occur outside of those contexts', '[p]henomena, by contrast, are not idiosyncratic to specific experimental contexts. We expect phenomena to have stable, repeatable characteristics which will be detectable by means of a variety of different procedures' (p. 317). These procedures have to make sure that the data, which serve as the basis for the inference of phenomena, are reliable.

> [T]he question of whether data constitute reliable evidence for some phenomena turns (among other things) on such considerations as whether the data are replicable, whether various confounding factors and other sources of possible systematic error have been adequately controlled, on statistical arguments of various kinds, and on one's procedures for the analysis and reduction of data. (p. 327)

A point which Bogen and Woodward emphasise very strongly is that the reliability of the data can be established *without* higher order theory explaining why particular data happen to pop out of one's experimental apparatus. On the contrary,

> Explanations of the data, to the extent they can be given at all, will lack generality and will be closely tailored to individual cases; they often will be enormously complex, and will rely heavily on ad hoc or after the fact assumptions. (Woodward 1989, p. 401)

Although Bogen and Woodward are less explicit about this point, it is also clear from their discussion that the piecemeal procedures for establishing the reliability of the data (reduction of data, data analysis, ruling out confounding factors, control of errors, etc.) are entirely independent of the predictions and explanations the tested theory makes about the *phenomena*. It is here where I disagree with Bogen and Woodward. Before I shall articulate my disagreement in the case of the discovery of WNC, I want to consider another philosophical account that has made heavy use of the discovery of WNC.

2.2 Mayo's statistical error account

Mayo has argued that although the *New Experimentalists* 'are right to insist on the tasks of distinguishing and subtracting out backgrounds, quite apart from the aim of testing high-level theories' (which I take to be very much consistent with Bogen and Woodward's account discussed above), 'something more general is needed to understand how experimental practices accomplish these tasks' (Mayo 1994, p. 277). Mayo contends that her so-called "error statistical account" does the trick and that it is standard error statistical tools that are capable of 'discriminating signals from noise, ruling out artifacts, distinguishing backgrounds, and so on' (ibid., p . 273).

According to Mayo's statistical error account, or approach of "severe testing", a *severe* test is a test with a low error probability, i.e. a low probability of passing a test in spite of being false. Mayo's programme is primarily directed against Bayesianism, which she calls a "theory-dominated philosophy of confirmation" (Mayo 1994, p. 272). Bayesians try to establish quantitative measures for the appraisal of theories. Given a particular initial probabilistic value of credence assigned to a hypothesis *H*, Bayesians ask how much some evidence *e* increases or decreases this value. This is measured by the conditional probability of *H*, given *e*, using Bayes' theorem. Although for Mayo, too, probabilities are the main tool in her account "probability is associated with the test procedure, not with the hypothesis" and thus "error probabilities are not degrees of credibility", as for the Bayesians (Carrier 2001, p. 94; cf. Mayo 1996, p. 11, p. 72). Rather,

> Error probabilities are not probabilities of hypotheses [like in Bayesianism], but the probabilities that certain experimental results would occur, were one or another hypothesis true about the experimental system (Mayo 1996, p. 367).

That is, probabilities in Mayo's account do not measure the degree of credence of a hypothesis (as in Bayesianism) but rather represent the probabilities of "how mistaken" one could be when holding a particular hypothesis given the state of the world. It is the significance level (set arbitrarily and usually to the value of 5%), i.e. the probability that *H* has been accepted erroneously, which decides whether we should adopt a hypothesis or not. Another important difference between Mayo's account and Bayesianism is her rejection of "white gloves" epistemology (see Carrier 2002, p. 94). Mayo refrains from simply assuming that the data are "already at hand", as she accuses Bayesians of doing. Rather, she is interested in the *process* of how trustworthy data are obtained which includes the task of ruling out artefacts, noise etc. Mayo claims that her account manages to accomplish these tasks. In the following I want to discuss whether this claim can really be justified by drawing on the discovery of WNC, as Mayo (1994, 1996) has done.

3 Weak Neutral Currents and Statistical Inferences Towards Them

There are three fundamental forces in particle physics: weak, electromagnetic, and strong forces. In the early 1970s the so-called Salam-Weinberg-Glashow model unified the electromagnetic forces and weak forces into ‘electroweak’ forces. A postulate of this model, however, was the existence of Weak Neutral Currents (WNC). WNC are said to have been discovered in 1973/74 by the so-called Gargamelle group at CERN and were confirmed in about the same year by its U.S. counterpart consisting of researchers from Harvard, Pennsylvania, and Wisconsin at the National Accelerator Laboratory (dubbed Fermilab in 1974) (henceforth: HPWF)[4].

WNC are a form of weak interaction between subatomic particles. In contrast to their charged current counterparts, WNC are not mediated by charged bosons (either W^+ or W^-), but rather by electrically neutral Z^0 bosons. In neutrino-nucleon scattering, charged currents produce muons[5], whereas neutral currents do not (see Fig. 2). Because WNC cannot directly be detected experimentally, the *non*-production of muons became the main identifier of WNC events.

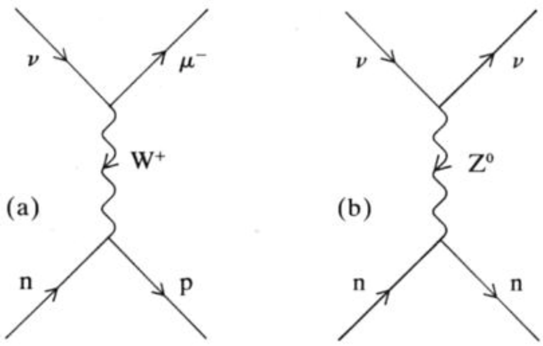

Fig. 2: Neutrino-nucleon scattering. a) charged current event, mediated by a W+ boson, which carries a positive charge from the reaction v → μ- to the reaction n → p (where v = neutrino, n = nucleon, p = proton, μ- = muon); b) neutral current event, mediated by an electrically neutral Z^0 boson (also characterised as massive analogue of the photon in electron scattering). Neutral current events produced in scattering are characterised by the charge remaining the same for incoming and outcoming particles (upper and lower parts of the diagrams respectively). From Pickering (1984).

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[4] The caveat about the discovery claim I shall justify below.

[5] Like electrons, muons are electrically charged, and subject to both electromagnetic and weak but not strong forces (i.e. the defining characteristic of leptons, as opposed to hadrons).

Quoting Galison, Mayo (1994) asks “how did the experimentalists themselves come to believe that neutral currents existed? What persuaded them that they were looking at a real effect and not at an artefact of the machine or the environment?”[6]. Mayo’s answer, as we shall see in this section, boils down to statistical arguments. Given that WNC are characterised by an absence of muonless events, counting muonless events should be sufficient in principle establishing whether WNC were present in the experiement or not. There is however a very problematic complication. The number of muonless events represents not only neutral current candidates, but crucially also those muonless events which are due to charged currents with wide angle muons that are not detected in the chamber (see Fig. 3).

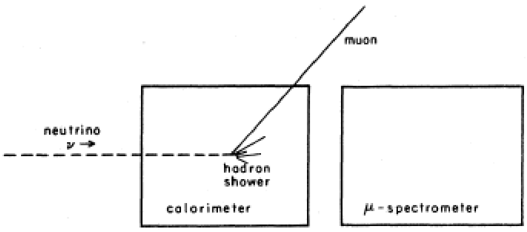

Fig. 3: Wide angle muons in the HWPF experiments.
Figure shows a schematic diagram of the HWPF detector (including a calorimeter and a μ-spectrometer) and wide-angle escaping muons. From Galison (1983).

Mayo calls this the artefact explanation. Mayo goes on to set up the question of whether the effects obtained by the HPWF group in 1972 had to be considered an artefact or a “true” effect, as an instance of classical hypothesis testing. Allegedly, reconstructing the reasoning of the HPWF group, Mayo presents us with two alternative hypotheses of the observed “muonless” neutrino induced scattering events:

> H: neutral currents are responsible for (at least some of) the results;
>
> H is false (the artefact explanation): recorded muonless events are due, not to neutral currents, but to wide-angle muons escaping detection.

Non-H is an artefact explanation because it says that although it might *appear* that H is true (i.e. that muonless events are due to neutral currents), the results are in fact due to another cause, namely wide-angle muons escaping detection, generating the *appearance* of neutral currents, when in fact only negative currents are present. Given

[6] Cf. Galison 1987, p. 136.

the experimental findings about muonless and muonful events in the spark chamber, the significant question, according to Mayo, is:

> What is the probability of a ratio (of muonless to muonful events) as great as 54/56, given that H is false? (Mayo 1994, p. 275)

That is, what is the probability that non-H, i.e. the artefact explanation claiming the absence of neutral currents and the exclusive presence of charged currents, is true? First of all, it should be noted that the way Mayo sets up the "severe test" for neutral current does not allow for genuine alternatives: either the results are explained by the existence of neutral currents (H) or they are not explained by H and the results are due to an artefact. There is no alternative between neutral currents existing and neutral currents *not* existing. So contrary to her own demands, Mayo's test scenario for neutral currents is not at all severe for the claim that neutral currents exist. But I want to leave that aside and follow Mayo further in her argument instead. As I said above, probability for Mayo is not a measure for the credibility of hypotheses (as for the Bayesians) but rather it is a measure of the significance level of the results. The significance level, in turn, is the probability that the null hypothesis (in our case H) will be rejected even though it is the correct explanation (also called a "false positive"). A high level of significance then, of course, means a low reliability of the test. The level must be as low as possible for a test to be severe[7]. Thus, if the significance level is very low (say .01 or .001) it would be "extremely improbable for so many muonless events [i.e. 54] to result, if H were false [i.e. if the artefact explanation were true] […] since escaping muons could practically never be responsible for so many muonless events, their occurrence in the experiment is taken as good ground for rejecting the artefact explanation" (Mayo 1994, p. 275). In the words of Galison,

> By comparing the number of muons expected not to reach the muon spectrometer with the number of measured muonless events, [the researchers] could determine if there was a statistically significant excess of neutral candidates. (Galison 1987, p. 217)

Crucially, muonless events due to wide angle muons (i.e. those not reaching the muon detector) cannot be measured but must be estimated. This was done with the so-called

[7] However, the smaller the significance level, the higher the likelihood of accepting a false null hypothesis. This is called a type II error (also false negative) and has to be weighed up against a type I error.

Monte-Carlo simulations. Monte Carlo simulations are probabilistic. According to Mayo,

> Probabilistic considerations are deliberately introduced into the data analysis because they offer a way *to model the expected effect of the artifact* (escaping muons). Statistical considerations, we might call them "manipulations on paper" (or on computer) [here: Monte Carlo simulations], *afford a way to subtract out background factors that cannot literally be controlled for*. (p. 276; altered emphasis)

Mayo then follows Galison who she quotes as saying about the HPWF group: "they wanted to know how likely it was that the observed ratio of muonless to muon-ful events (54/56) would fall within the statistical spread of the calculated ratio (24/56), due entirely to wide-angle muons" (Galison 1987, p. 220). The difference between the observed and the calculated ratio of neutral current candidate events (NC) to charged current events (CC) ratio (NC/CC) to be expected is therefore 54/56 – 24/56 = .536. According to Mayo, the "significant question" is whether this difference was improbable on the assumption that non-H, i.e. that the artefact explanation, was true. Since this difference is higher than five standard deviations[8], Mayo concludes "it is practically impossible for so many muonless events to have been recorded, were they due to the artefact of wide angle muons" (Mayo 1994, p. 277). In other words, the existence of neutral currents, and its discrimination from "noise" or artefacts, was warranted by the *statistical* methods the HPWF researchers used. Mayo concedes that her account is "[a]bstracted from the whole story" and its theoretical and sociological context, but nevertheless Mayo claims that her focus on – what she thinks – the "bare bones of the experimental analysis" is sufficient for establishing whether the observed experimental effects were neutral currents or mere artefacts (cf. Mayo 1994, p. 274). I strongly disagree. In fact, Mayo's very restricted focus quite severely distorts how neutral currents were actually discerned from background noise. Widening our focus in the analysis of the historical case will make clear that statistical analysis was insufficient for establishing neutral currents as the cause of the data.

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[8] The standard deviation is estimated by inserting the measured values above into a "standard statistical model" (see Mayo 1994, note 7).

3.1 It's Not All about Statistics

First of all, it is worth mentioning that Mayo is only considering one particular kind of experiment that was performed in the hunt for WNC. The experiments on neutral currents were of two kinds: bubble and spark chambers. A bubble chamber consists of a tank of superheated liquid (usually Freon) held under pressure to prevent boiling. When particles are "shot" into the tank, bubbles form along the tracks of *electrically charged* particles. Non-charged particles like neutrons, neutrinos, and Z^0 bosons which mediate WNC (see Fig. 2) cannot be observed. One needs to infer those neutral particles from the products that remain after scattering—those products must not change charge (in comparison to the incoming particles). A typical picture of a neutral current event taken in a bubble chamber is depicted in Fig. 4a.

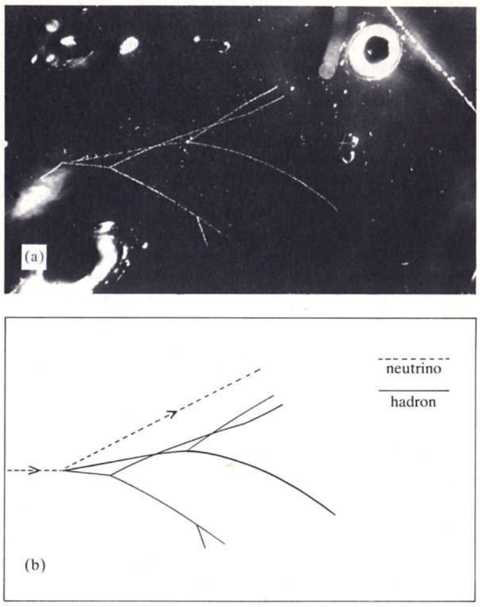

Fig. 4: Neutral current event.
a): Bubble chamber picture of a neutral current event, and (b): schematic representation of a). Neutrinos leave no track. Only the hadron events do. From Pickering (1983).

In contrast to bubble chambers, in spark chambers the passage of electrically charged particles is recorded by electronic means rather than by physical ones (bubbles). Both methods have their merits and shortcomings. There is a trade-off between quantity and quality. Whereas bubble chambers provide more detail of the individual tracks, spark chambers record a much higher rate of interactions but less detail. Mayo, in her analysis, discusses only the latter experiments, which of course are a priori more susceptible to statistical analysis. There is even one type of event which is utterly unsusceptible to statistical analysis: neutral *lepton* current events. These events are very rare. The first event of this sort, depicted in Fig. 5, was found in a total of 700,000 pictures[9].

Fig. 5: The first ever recorded neutral lepton current event.
Neutral lepton current ($\bar{\nu}_\mu + e^- \rightarrow \bar{\nu}_\mu + e^-$) recorded in Gargamelle/CERN, found in Aachen/Germany. The picture shows an electron's trajectory going from the left to the right of the picture. The white circles are just lights illuminating the bubble-chamber liquid and thus have neither evidential nor theoretical import. From Hasert et al. (1973).

How was this first lepton current event found? Bogen and Woodward (1988), who, it should be said, view bubble chamber photographs as data and WNC as phenomena, assert that generally

> the extent to which such methods of data-reduction are independent of any concern with explanation is illustrated by the fact that the person or machine performing these tasks can carry them out *without understanding either the theory which explains* the interactions for which the photographs are evidence, or the physical principles by which the equipment works. Data-reduction aimed at isolating and analyzing relevant data *does not require explanation of the data*, even though it may be essential to establishing reliability. (ibid., p. 333; my emphasis)

That is, according to Bogen and Woodward (1988) for data analysis and reduction, one need not have any understanding whatsoever of the theory that seeks to explain the phenomena, which the data are supposed to support. It is interesting to note, however, that the so-called Aachen picture of Fig. 5 was first misidentified by 'the women scanning the bubble-chamber negatives' as a charged current event and it took the research student Franz Hasert (i.e. someone with a good knowledge of the theory

[9] Miller and Bullock (1994) refer to Pickering's number of 700,000 events as being "trivially incorrect because the single picture was found after scanning 100,000 pictures" (p. 925). Miller and Bullock (1994) do not provide any sources, but appear to rely on Perkins (1997) as they do in the rest of their article quite heavily. And yet, it is not Pickering but them who did not do their homework properly. The original article by Hasert et al. (1973) proves Pickering correct: "A total of 375 000 ν and 360 000 $\bar{\nu}$ pictures were scanned twice and one single electron event satisfying the selection criteria was found in the $\bar{\nu}$ film" (p. 122).

at stake) to identify this picture correctly (cf. Galison 1983). More importantly, this single picture was enough to persuade some of the reality of neutral currents. Miller and Bullock quote Perkins (1997) as saying that "after this memorable episode, everything that subsequently happened in the neutral current story was for me something of an anticlimax" (p. 5). Also Galison (1983, p. 486) quotes Helmut Faissner (a later member of the Gargamelle group at CERN) as saying that "the event has excited us a great deal" (p. 487)[10]. Statements like these stand in stark contrast with Bogen and Woodward's assessment of the epistemic import of the Aachen picture:

> […] matters must be arranged so that data is [sic] produced *sufficiently frequently* and in *sufficiently large amounts* […] to support conclusions about the existence of phenomena […] In the neutral current experiments, for example, some investigators initially favored the use of interactions involving the scattering of an electron off a neutrino [i.e. potential lepton neutral current events] […] It had the very serious disadvantage that it *occurs very rarely*, and thus is *unlikely to yield enough data to support statistically reliable conclusions*. For this reason, the investigators focused on interactions involving neutrinos and nucleons [i.e. potential hadron neutral currents], which produce *much more data* […] (Bogen and Woodward 1988, p. 320; my emphasis)

Quite obviously Bogen and Woodward seem to have misjudged the situation here. If they were right that statistics plays the role in experiments that they think it does, the Aachen picture would have not had much value for the scientists involved in the WNC experiments. After all, they had obtained just a *single* picture! And still, it was enough for some of the researchers to convince themselves of the reality of WNC to such an extent that everything afterwards was "something of an anticlimax"[11]. But back to Mayo.

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[10] Pickering (1984) has claimed that "it is a common place of particle physics that a single event cannot prove the existence of a new phenomenon" (p. 93, fn. 16), and has been harshly criticised for that (Miller and Bullock 1994; Rousset 1994). If it really was the case that this single event had such persuasive power, it harms Pickering's opponents more than himself, as we shall see below.

[11] Gallison (1983 and 1987) makes out two different research traditions within high energy physics: a visual and a statistical tradition. Whereas some experimenters, in the "image" tradition, seek to discover "golden events", i.e. "single picture[s] of such clarity and distinctness that it commands acceptance" (ibid., p. 22), experimenters of the logic tradition are interested in data amenable to statistics. Clearly, the Aachen picture was most satisfactory to the visual tradition. For a criticism of Galison's distinction, see Staley (1999). Staley does

4 WEAK NEUTRAL CURRENT EXPERIMENTS IN THE 1960S

Mayo focuses not only on the HPWF experiments (rather than the Gargamelle bubble chamber experiments) but she also focuses on experiments which were performed in the early 1970s and disregards those experiments that were performed already in the 1960s[12]. Rather curiously, the experimental techniques and designs of the 1960s—apart from refinements as to their size, for instance—were essentially the same that were used in the 1970s. Pickering (1989) therefore asks quite justifiably:

> How can experiments at one time demonstrate the non-existence of some empirical phenomenon, and then later its existence? (ibid., pp. 224-5)

Galison's account does not answer this question but rather glosses over it. As Pickering notes, Galison's account "handles this problem with the language of 'mistakes', 'misfortunes' and so on" (Pickering 1989, p. 225), whereby

> Galison nowhere documents and analyzes the nature of the earlier mistakes; he *asserts* that whatever was done in the 1960s was mistaken. (ibid.; original emphasis)

According to Pickering, Galison's account "edits out" the 1960s experiments at the expense of his account being "prone to self-destruct", because one may justly want to ask *why* the 1960s experiments were mistaken. What was wrong with them if the 1970s experiments "took place in the domain of *established* physics, meaning within *established* techniques and theoretical ideas" (Galison 1983, p. 505; emphasis added), which were already part of practice in the 1960s.[13] Mayo does not even mention this problem. And yet, as we shall see in the next section, the consideration of the 1960s

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not question the existence of the two sorts of practices but he does claim that they were not as clear cut as Galison suggests they are.

[12] According to Pickering five spark chamber experiments were performed in the 1960s. Two in Brookhaven, two at CERN and one at the Argonne National Laboratoy near Chicago. See Pickering (1984, p. 101; fn 101).

[13] It is striking that two other detailed accounts of the discovery of neutral currents either gloss over or completely ignore the 1960s experiments (see Sciulli 1979, Miller and Bullock 1994). Miller and Bullock (1994) are harshly critical of Pickering (1984), who provides the only account that addresses this shift aptly. Miller and Bullock say for instance that "Consequently, to assert, as Pickering does […], that neutral currents were in the data is meaningless because they were all but swamped by background" (p. 924). This represents a gross misunderstanding of Pickering by Miller and Bullock because Pickering *does* in fact assert that neutral currents were *subsumed* (!) under the neutron background. See main text below for details.

experiments is absolutely fundamental for a proper understanding of how the effect of neutral currents was discerned from its background.

4.1 The 1960s Spark Chamber Experiments by the HPWF Group

Statistics would have not been of much use in discovering WNC in the 1960s spark chamber experiments[14]. Working in the paradigm of the then accepted V–A theory of electroweak interactions (see Section 7 for details), which presumed that only charged currents existed, the HPWF group built their electronic detectors in such a way that they would register only those neutrino-induced events in which muons were produced. This was done in order to filter out all the uninteresting neutron background. But of course, it effectively meant that neutral currents could not possibly have been observed with this trigger. Statistics would have been utterly useless for discerning WNC because the apparatus was built in such a way as to simply eliminate any sort of neutral current 'signal'. Before neutral currents could therefore be recorded, the muon trigger had to be modified accordingly. As Rubbia recalled, this modification was carried out "not because I had decided it [beforehand], but because Steve Weinberg gave me good reason for it" (Rubbia 1980, interviewed by Galison (1983))[15]. Quickly the HPWF group realised that the signal they were immediately recording might be induced by wide-angle muons and devised a Monte Carlo program for estimating the number of wide-angle muons (already discussed in Section 3). On the basis of these estimates ($R=NC/CC=0.20\pm0.09$)[16], the HPWF group submitted a draft announcing the discovery in the *Physical Review Letters*. However, the HPWF group did not stop there. In order to keep the wide-angle muons to a minimum, they decreased the distance between the calorimeter and the muon spectrometer (Fig. 6), making the capturing of wide-angle muons by the spectrometer more likely. This, however, posed a new problem. The decrease of the separation between the front and the rear of the chamber from 4 feet of iron to mere 13 inches of steel could cause another unwanted effect: hadrons might "punch through" the

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[14] See footnote 12.

[15] The "good reason" had to do with the fact that WNC were a consequence of Weinberg-Salam model, which I shall have more to say about below.

[16] Compare this with $R=54/56=0.96$, which Mayo quotes above in Section 3.4.1.1.

reduced separation into the muon spectrometer (see Fig. 6) where they would automatically be identified as muons, increasing the number of muonful events and thus cancelling a potential neutral current signal.

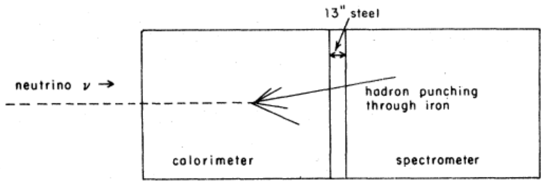

Fig. 6: Hadron punch-through.
Picture shows a schematic diagram of the HWPF detector and hadron punch-through. From Galison (1983). Cf. Fig. 3.

However, after estimating the penetration rate of hadrons at about 13% of all the hadron events in the chamber, the HPWF group concluded that this would be insufficient to account for the muon-ful events detected in the rear of the detector. The HPWF group noted that the "neutral-current signal was very much smaller than [previously assumed] […] possibly being zero" and thus "no significant evidence for the existence of the weak neutral current" could be provided (Pickering 1983, p. 103; cf. Galison 1983, p. 501). At this point, it seemed, the HWPF group had established the non-existence of neutral currents. A draft paper was prepared to replace the earlier version (discussed above), where they reversed their own conclusion from a discovery claim to a "non-discovery" claim[17].

So how did the HPWF group then get from a claim about the absence of neutral currents to a discovery claim? This crucial step, quite astoundingly, remains unexplained in Galison's account. He almost handwavingly states that as a "result both of pressure from outside the collaboration and of new evidence from within the group, *opinions were changing*". The hadron punch through estimate doubled, and the muon-ful/muon-less ratio was re-calculated at 12-15%[18]. Notice that this value lies

[17] The fact that the HPWF group discovered, then withdrew their discovery claim and then withdrew from their withdrawal resulted in the joke that the group had discovered "alternating neutral currents".

[18] Cf. Galison (1983, p. 502). Galison does not give reasons for this change of the HPWF group's attitude towards neutral currents other than three "pieces of evidence", whereby the "most convincing for Cline" (a member of the HPWF group) Galison takes to be the finding that "among twenty neutral current candidates, five 'had no hint of wide-angle tracks'" constituting an argument with "a small selection of events, clean of possible edge effects, and with an analysis that *did not require resorting to Monte Carlo techniques*" (pp. 502-3; my emphasis; for details on the Monte Carlo calculations see Section 5.1). This, of course,

well below the one Mayo quotes (cf. Section 3 of this paper). Sometime before August 1973 a decision was finally taken to proceed with the publication of the original paper (confirming the existence of WNC), supplemented with the refining work of the experimental design that had taken place in the meantime.

4.2 Pre-Gargamelle Bubble Chamber Experiments at CERN

In support of his claim that the experimental machinery already used in the 1960s was apt for making a discovery claim about WNC, Pickering—among many other things—refers to the following quote by Frank Sciulli, a member of the Gargamelle group at CERN in the 1960s, who later recalled that

> In retrospect, it is likely that events due to neutral currents had been seen as early as 1967. Data from the CERN heavy-liquid bubble chamber […] *showed a surprisingly large number of events with hadrons in the final state, but with no visible muon* [i.e. neutral current candidates]. In 1967 there was little pressure to rectify these uncertainties. Five years later the *theoretical climate* had changed dramatically, so there were persistent but cautious efforts to conclusively resolve whether such events were actually anomalous. (Sciulli 1979, p. 45)

I shall return to Sciulli's statement that the "theoretical climate" had changed in Section 7. But let us concentrate on Sciulli's claim that already the 1960s experiments had detected neutral current candidates. Miller and Bullock (1994) have questioned the trustworthiness of Sciulli's recollection, claiming that the "CERN bubble chamber was 1.2m long [in contrast to Gargamelle's size of 4.8m], *too short to disentangle neutral currents from charged ones*" (*ibid*., p. 906; cf. p. 911). We shall see in a moment that this claim by Miller and Bullock is incorrect. But it is worth mentioning first of all that Sciulli is not alone with his recollection. Faissner, for instance, very much like Sciulli, recalls that

> Having seen these photos myself, and too lightly dismissed them as neutron stars, I cannot blame this slip on unfit instrumentation: *Our old sparkchambers were well suited for the interaction analysis*. That we did not believe what we saw was *an unfortunate conspiration of mental blocking, by*

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although it might explain why a particular individual (here: Cline) was convinced of the existence of WNC, by no means establishes why WNC became to be accepted in the physics *community*.

> *theoretical prejudice*, and experimental mischief. (Faissner 1979, partially quoted in Galison 1983, p. 483) [19]

Also Dieter Haidt, another member of the Gargamelle group, recalls about the CERN bubble chamber experiments prior to Gargamelle that "such *events [neutral currents] were just waiting among the already scanned events*" (Haidt 2004, p. 27; my emphasis). Surely, all these recollections cannot be blamed on bad memory! A. Rousset, a researcher of NPA (Nuclear Physics Apparatus, the precursor of Gargamelle), retrospectively even stated that he could *prove* that a neutral current signal was already present in the 1960s:

> It was particularly perverse [sic] to *demonstrate* in retrospect that *neutral currents were already standing in the picture taken in 1967 in the CERN/NPA chamber*. The *proof* was given in 1974 [Rousset quotes his contribution to the High Energy Physics Conference in London (1974)] using the print out of those neutrino events from the 1967 experiment at CERN. (Rousset 1994, p. 347)

Before I shall go into some details about some of the data indicating the existence of WNC which were already available in the 1960s, one needs to notice that—as in spark chambers—WNC have to be discerned from artefacts. As in the spark chambers, the relevant artefacts in bubble chambers are caused by charged events whose muons are not detected. More specifically, in bubble chambers artefact WNC-type events are caused by charged current events occurring in the shielding of the chamber, where they produce muons and neutrons. Whereas the muons (because negatively charged) propagate to the periphery of the chamber within the shielding, the neutrons shoot into the chamber where they cause hadron events. Because neutrons—like WNC—do not leave any tracks in the chamber (remember, only charged currents do), and because their associated muons do not make it to the visible chamber (where they would leave tracks) they will be identified as WNC proper (Fig. 7). Therefore, one needs to take extra care to rule out neutron stars as potential explanans for the observed WNC-type events.

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[19] Although Faissner explicitly mentions sparkchambers in this quote, he refers to the following PhD theses: E.C. Young, Oxford Thesis (1966) and Yellow Report CERN 67-12; M. Paty, Paris Thesis (1965), and CERN 65-12; M. Holder, Aachen Thesis (1967) and Aachen Report PITHA-19. The first two theses discussed bubble chamber experiments.

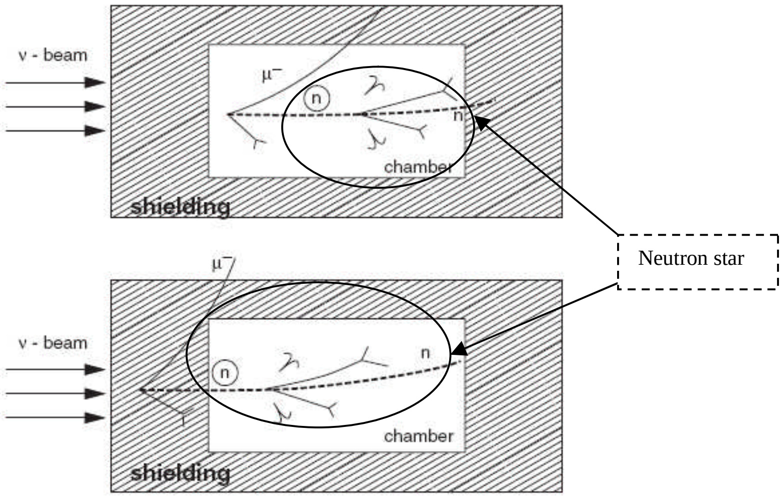

Fig. 7: Neutron stars in bubble chamber.
The figure shows two forms of neutron stars triggered by a neutrino beam.
Above: a neutrino hits a nucleus, producing a muon (μ^-), hadrons, and a neutron (*n*). The neutron, again, hits another nucleus, producing even more hadrons, but without producing a muon. The event caused by the neutron can unproblematically be associated with the neutrino beam (and hence be identified as a charged current event), which is why these events are called "associated events" (AS) (also called neutron stars (n*)). All this happens within the visible chamber. *Below*: starting in the invisible shielding, making the muon event (μ^-) undetectable. The latter gives the *appearance* of a neutral current event (non-associated or "background event" (B)). The interaction length of the AS events within the chamber serves as the basis for estimating the number of unobservable B events by means of Monte Carlo programmes. Diagram from Haidt (2004); adapted. Also compare with the problem of „escaping muons" in the HPWF experiment (Fig. 3).

4.2.1 Disregarded Evidence for Neutral Current Events in the 1960s

As Pickering notes, Young—a graduate student compiling his PhD thesis—who made "the most detailed analysis of the neutrino runs from 1963 to 1965" including the use of Monte Carlo calculations for cascades taking place in the shielding (see Section 5.1), found that 150 neutral-current type events could *not* be attributed to neutron background, compared with around 570 charged current events, making for a ratio of 1:4, the ratio later to be found in Gargamelle (Pickering 1983, p. 99)[20]. Sakurai (1978) concurs that

[20] Pickering, in a footnote (1983, p. 205, fn. 12), concedes that Young did not explicitly state those 150 neutral current type events, but claims that "it follows directly from the background estimate (Young 1967, p. 58) and the counts of different types of events (ibid., p. 39, Table

> In [Young's] thesis he is reputed to have concluded that the number of muonless events was about three times the neutron background he could estimate […] *if these excess muonless events had been attributed to neutral currents, we would have obtained a neutral-to-charged current ratio of 17± 6%, roughly the currently accepted value.* (Sakurai 1978, p. 44)

According to Pickering the later estimates by the Gargamelle group were therefore „*essentially only refinements of those made earlier by E.C.M Young* […]" (Pickering 1983, p. 192; see also Pickering 1984, p. 99). However, for Young and his supervisors this was not enough for inferring support for neutral currents. On the contrary, even though Young recognized neutral currents as a possible explanans, neutral current type candidates were subsumed under "neutron background":

> Neutral events without lepton candidates [i.e. possible neutral-current events] *are taken as neutron-induced background*. This is true only when neutrino interactions of the neutral current type are neglected. (Young 1966, p. 41)

So for some reason, Young decided that WNC should indeed be neglected as a possible explanation of the data and instead preferred to explain his data by "leakage effects from the primary proton beam" (Young 1967, p. 58), i.e. effectively by neutron background (see below for details). In striking parallel, G. Myatt in "the most extensive" discussion of neutron background in a CERN experiment carried out in 1967 noted that "a considerable number of neutral events were found which had no μ^- [muon] candidate" (i.e. WNC type events) and went on to say that "[i]t is interesting to speculate on the origin of these *neutrons* [!]", without even *considering* whether neutral currents might be responsible for the observed muonless events (quoted in Pickering 1984, p. 98). Myatt and his group compared the muonless events (Fig. 8, left) with the associated neutron stars they had observed (Fig. 8, right).

2.3)". Pickering portays Young as dissenting from the general 1960s paradigm, in which the existence of WNC remained unconsidered. This is not the case. See main text.

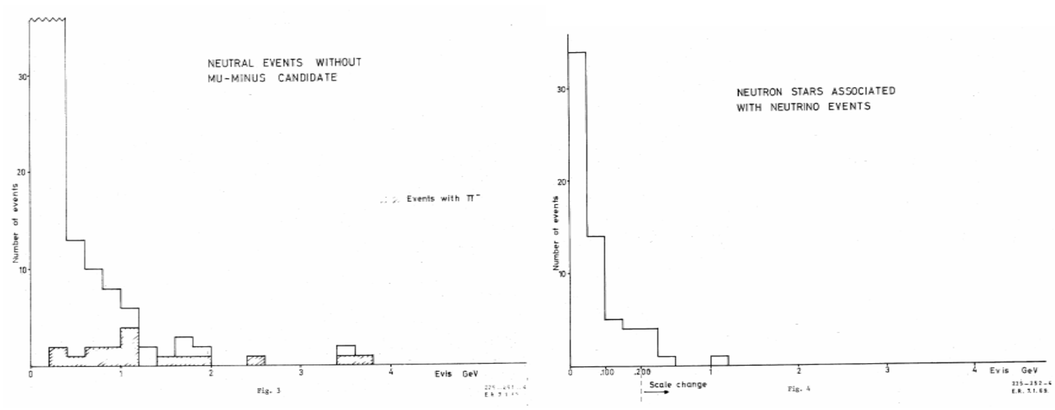

Fig. 8: Early, neglected, recordings of Neutral Current candidates.
Left: Neutral current type events. Right: Observed neutron stars in the NPA chamber (the pre-Gargamelle chamber at CERN). From Myatt (1974).

Myatt concluded that the "isolated neutron stars" in Fig. 8 (left) (i.e., in fact, the observed neutral current candidates) were "more numerous" than the observed neutron stars in the chamber (Fig. 8, right). Comparing the neutral current candidates of Fig. 8 (left) with the neutron stars of Fig. 8 (right) (for E_{vis} > 50 MeV), they found a ratio of about 20:1 (!) in favour of WNC candidates. But instead of making a discovery claim or at least exploring the possibility of neutral currents, they simply blamed all neutral current type events on neutron stars by fiat. Without even attempting a quantitative analysis, as was later going to be carried out (see Section 5.1), they blamed the production of neutron stars in the shielding for all WNC-type events.

Given not only the anecdotal but also the quantitative evidence discussed in this section, there can hardly be any doubt that had the experimentalists been willing to discover weak neutral currents already in the 1960s, they would have done so!

4.3 Upper Limits, Inclusive vs. Exclusive Currents, and Energy Cuts

Rather than citing the neutral current signal they observed as such, experimentalists preferred to set *upper limits* for neutral currents, which did not discriminate between neutron background and neutral currents. Continuing the quote above, Rousset says that

> In conclusion, the neutral currents were already present in the pictures taken in 1967 at CERN. Why have they not been found then? These events were only used to set an upper limit [quotes Cundy (1970)]. (Rousset 1994, p. 347)

In Block et al. (1964) a limit of 3% was set on the ratio of elastic neutral current (NC) to charged current (CC) cross-sections[21]:

$$\frac{\sigma(\nu_\mu + p \rightarrow \nu + p)}{\sigma(\nu_\mu + n \rightarrow \mu^- + p)} < 0.03$$

This limit existed in the literature as long as until 1970, when Cundy et al. (1970) pushed the upper limit upwards to 12 ± 6%.[22] However, Weinberg (1972) showed that this "limit" was within the predictions of his model of electroweak forces (15-25%) arguing that "the proposed theory is neither confirmed nor refuted" by the data, and that "a more detailed analysis of the data is needed (ibid., p. 1415). Palmer (1973), too, concluded that "although no experimental claims to the observation of neutral currents have yet been made, nevertheless the published data is consistent with the simple Weinberg model […]"[23]. Recently, Weinberg said about these limits that

> A 1970 experiment [by Cundy et al.] had given a value of 0.12 plus or minus 0.06 for this ratio, *but the experimenters didn't believe that they were actually seeing neutral currents*, so they didn't claim to have observed a neutral current reaction at a level of roughly 12% of the charged current reaction, *and instead quoted this result as an upper bound.* The minimum theoretical value 0.15 of this ratio applies for sine-squared theta equal to 0.25, *which is not far from what we now know, is the correct value. I suspect that this 1970 experiment had actually observed neutral currents*, but you get credit for making discoveries only when you claim that you have made the discovery. (Weinberg 2004, p. 10; my emphasis)

Even though the data for a discovery claim were available, the will was not.

Apart from the fact that experimentalists in the 1960s spoke of limits rather than signals, there were two more procedures used by the experimentalists that added to the diminishment of neutral currents. First, as Pickering (1984, p. 99, fn. 33) notes,

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[21] Cross-sections refer to the effective area of interaction between beam and target particles. *Elastic* cross sections refer to those events where no new particles are produced (e.g. AB → AB), whereas *inelastic* events refer to particle producing events (e.g. AB → ABC). Cf. Pickering (1984, p. 28).

[22] According to Perkins (1997) this was due to a "stupid book-keeping error". The original article by Cundy et al. reads that "the previous limit [NC/CC] << 0.03 given by us is wrong, since single unidentified positive tracks were incorrectly assumed to be pions" (Cundy et al. 1970).

[23] Publication of this paper—for some reason—was delayed until after the Gargamelle report was published. See Pickering (1984, p. 205. fn. 15)

attention focused on *exclusive* (e.g. $\nu + p \rightarrow \nu + \pi^+ + n$), rather than *inclusive* events ($\nu + N \rightarrow \nu + X$, where N = target nucleon (neutron or proton) and X = *any* hadron), with the latter having a much larger cross section and therefore a much bigger chance to be detected[24]. Second, all bubble chamber experiments (the ones in the 1960s as much as the ones in the 1970s) imposed an energy cut of 1 GeV on the data in order to reduce the neutron background. However, this cut in the 1960s was performed on the basis of the "visible" energy, i.e. the energy of the events on the film. In consequence, whereas the energy of the charged current events comprised hadron energies *plus* the energy of their associated muon (cf. Fig. 7 below), the energy of the neutral current events consisted of hadron energies only. This automatically resulted in more charged current than WNC-type events. In the context of the presuppositions of the 1960s this made sense because it was presumed that all neutral candidates were due to neutron background. This procedure was changed in the 1970s in the Gargamelle experiments, when experimentalists—induced by theorists—started to consider the existence of neutral currents. Under this new presupposition, the cutting procedure had become "unfair". The Gargamelle experimenters abandoned the old cutting procedure and calculated only the total energy of the hadron events, leaving out the energies of the muon events to establish equality in calculating the energies and consequently the ratio of WNC-type to charged current events. Considering this point in his book even Galison (1987) writes:

> *Data analysis could make that radical difference.* How events were analyzed and what resources were devoted to the task of sorting and structuring the flood of information available from the large experiment *could often give an experiment an enormous advantage, or even determine if and when a discovery was made*. (p. 174; original plus added emphasis)

So, quite obviously, there was a change in the experimentalists' attitudes towards their evidence that cannot be explained by the data themselves. Reasons for this change will be considered in Section 7. In the face of the positive evidence for WNC that quite obviously were available already in the 1960s experiments, one may wonder how Miller and Bullock came to the conclusion that the 1960s Gargamelle bubble chamber experiments were inappropriate for discovering neutral currents (see Section 4.2 on page 18).

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[24] Notice that the upper limit given by Block et al. (1964) and Cundy et al. (1970), quoted above, is for an exclusive current.

5 THE ARGUMENT FROM SPATIAL DISTRIBUTIONS

Why do Miller and Bullock claim that the pre-Gargamelle chamber at CERN was *too small* for the neutral current effects to be observed? Miller and Bullock's claim stems from the following argument made by the CERN physicists in their discovery paper (Hasert et al. 1973). Hasert et al.'s argument tried to take advantage of the *spatial distribution* of neutron background events and neutral current events respectively within the chamber. Diagrams a), b), d), e) of Fig. 9 display the spatial distributions of neutral current type events (NC) and charged current events (CC) for both neutrino (v) and antineutrino induced events ($\bar{v}$). Diagrams c) and f) display the ratio NC/CC, showing that the distribution of both NC and CC is almost isomorph.

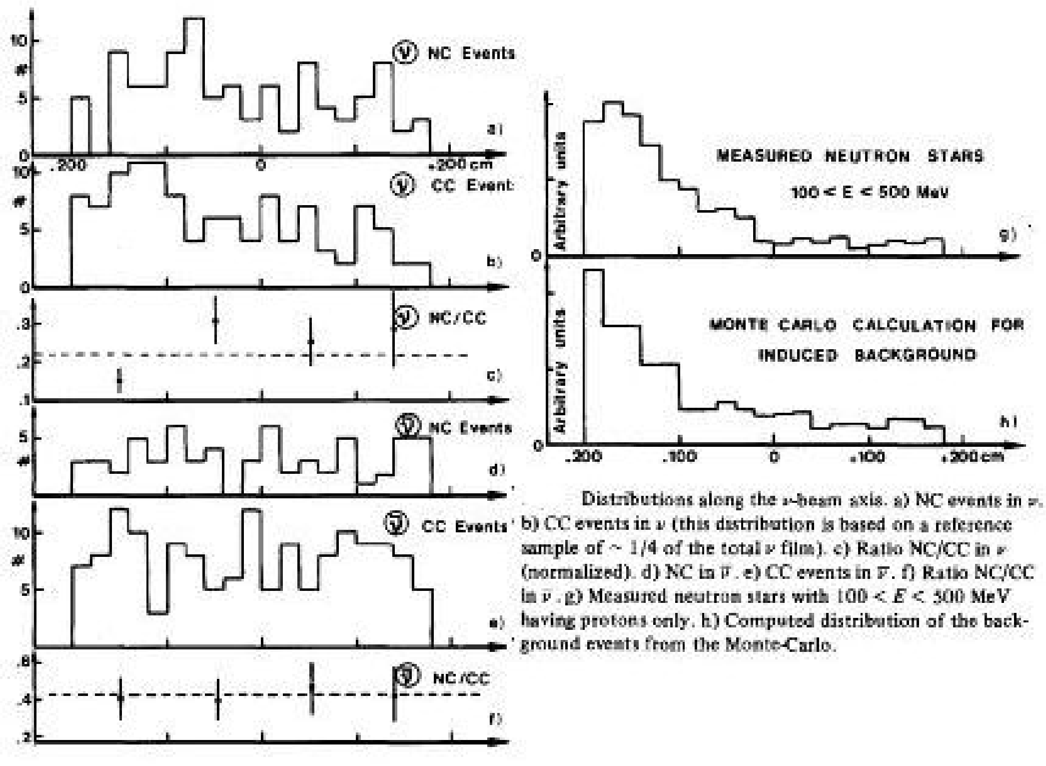

Fig. 9: The argument from spatial distributions.

See main text for details. From Hasert et al. (1973).

Both the distribution of NC and CC is uniform along the length of the chamber. And this is the crucial point now. Hasert et al. argued that *if* the neutral current type events were caused by neutrons induced by the neutrino beam at the front of the chamber, one would expect an exponential decrease of neutron reaction along the chamber length. This is depicted in diagram g) of Fig. 9, where the observed neutron stars within the chamber are plotted. If one now subtracts the latter diagram from diagrams a) and d) one *should* – according to this argument – obtain all the *genuine* neutral current events. This is the argument from the spatial distribution of the events, and it is this argument which a large part of the CERN group wanted to publish as evidence for neutral currents. Notice that this argument crucially depends on the so-called interaction length of neutrons: if the interaction length of neutrons is not much shorter than the length of the chamber, the argument from spatial distribution simply cannot be made. And this is what Miller and Bullock imply for the CERN chambers, which were used before Gargamelle. They quote Galison as saying "the very possibility of using the spatial distribution of neutral candidates as a test of the neutron background *hinged on the fact that Gargamelle was much larger than any previous bubble chamber*" (Galison 1987, p. 169). According to Miller and Bullock in the Gargamelle chamber therefore "*there was effectively a built-in calibration of the neutron background"* (Miller and Bullock 1994, p. 912; original emphasis). However, this claim is simply incorrect. The interaction length of neutrons, i.e. the length before they hit a nucleus, is only 70cm, much less than the 1.2 metre of the 1960s CERN bubble chamber[25]. An argument from spatial distribution was possible *in principle* in the old chambers already. However, even if Miller and Bullock's claim was right, the argument from spatial distribution was simply insufficient for proving the existence of neutral currents, as was pointed out by Haidt and Fry (1973, 1975), both members of the Gargamelle group. Haidt and Fry drew attention to two points "which damped the euphoria" that had sprung from the spatial distribution argument (Haidt 2004, p. 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 (anti-)neutrino beam only. This, however, was a gross oversimplification.

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[25] Miller and Bullock (1994) do not even bother mentioning the interaction length. The interaction length of 70cm can however be found in Haidt (2004), and in Fry and Haidt (1975). Although Galison (1983) states that "neutrons had an interaction length in the bubble-chamber liquid *longer than the dimensions of the older bubble chambers" (*p. 484), in Galison (1987) he notes that "in the basement of the CERN EP building even a decade after the experiment terminated, cartons still sat stuffed full of printouts by Camerini, Osculati, Pullia, and others from throughout this period; each is marked on the cover "Λ= 70" (centimetres); "Λ = 50"; and so on" (p. 190).

> Genuine neutral current events should be distributed uniformly along the chamber assuming uniform detection efficiency […] Neutrons entering at the front produce indeed an exponential fall-off along the neutrino beam axis. This can be seen in [Fig. 9]. *But neutrons entering through the side produce a flat distribution, just as neutral current events would do*. (Fry and Haidt 1975, p. 12; added emphasis)

In other words, the neutrino beams were not perfectly straight but also had a radial distribution, knocking off neutrons in the side of the chamber, mimicking neutral currents. Those neutrons entering through the sides would simply destroy the idea of the spatial distribution being informative as to the nature of the neutral-current type events (cf. Fig. 10)[26]. Therefore, "the spatial distribution *can only give a hint* to the nature of the neutral current candidates […]" (Fry and Haidt 1975, p. 12; added emphasis)[27].

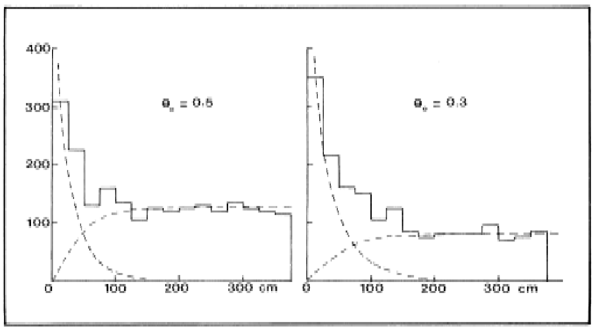

Fig. 10: Neutral current type distribution in Gargamelle.
Spatial distribution of neutral current type events along the chamber (in neutrino beam direction). Dashed lines indicate the calculated front and side contributions of neutron background. Θ = angular radius of beam in mrad.
From Fry and Haidt (1975).

The other point, which Haidt (2004) later called the "more dangerous" one, regarded the fact that AS events could produce *neutron cascades* in the shielding of the chamber (Fig. 11):

> If there was no cascade effect at all, the background neutrons would originate essentially from a 1λ thick shielding layer around the chamber. *In reality, the*

[26] Neutrons entering through the sides were induced by the fact that the radial neutrino-flux distribution "extends well beyond the chamber body and induces in the magnet coils a huge number of neutrino interactions, which in turn emit neutrons, thus generating a uniform flux entering sideways the fiducial volume. The net result is a flat *X* distribution also of *n**'s indistinguishable from neutrino-induced neutral current events" (Haidt 2004, p. 28).

[27] Although this point remained unmentioned in Hasert et al. (1973) it was taken care of in the lengthier Hasert et al. (1974).

neutron flux gets multiplied by a factor depending upon the cascade length. (Fry and Haidt 1975, p. 9)

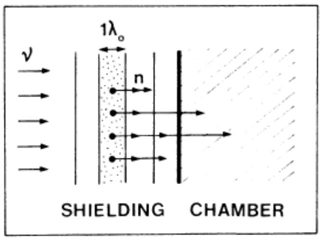

Fig. 11: Schematic depiction of a neutron cascade. Neutrino beams (*v*) entering from the front of the chamber (left side of the diagram), interacting with neutrons (*n*) in the shielding (middle; λ indicates one interaction length), and then propagating into the chamber (right). From Fry and Haidt (1975).

That is, the cascades, consisting of multiple interaction lengths, multiply the production of neutrons within the shielding, as a function of the cascade length (i.e. the number of interaction lengths). If there were no cascades, all the neutrons produced in the shielding would originate from the layer of the shielding towards the fiducial volume defined by the simple interaction lengths of neutrons. With cascades, the number of neutrons is multiplied by each cascade step (i.e. by each interaction length as part of the cascade). The neutrons reaching the chamber after the cascade, would then appear as neutral current events (see Fig. 7, lower diagram).

> There is then a priori no longer a distinctive feature between n-[i.e. neutron-induced] and v-[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, p. 192; altered emphasis)[28].

This then brings us back to the estimation of background neutron-induced events discussed in the next section: the quantitative calculations, Haidt mentions, are nothing but Monte Carlo calculations[29].

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[28] Not everybody in the collaboration agreed with Fry and Haidt about the necessity of performing neutron background calculations that included cascades. On the contrary, Fry told Galison in 1984 about Lagarrigue's reaction to his and Haidt's point about neutron cascading: 'You know, you're bogging down the collaboration. *We want to prove we've got neutral currents*' (quoted in Galison 1987, p. 193), which clearly indicates that there was a strong desire to discover neutral currents.

[29] There was another argument Haidt put forth. Haidt assumed the worst case, namely that all NC events were due to neutral background, which gave him a B/AS ratio of 1.0 ± 0.6. It turned out that the observed ratio B/AS ~ 6 (i.e. background events / associated events), was in stark contradiction with the calculated ratio B/AS = 1.0 ± 0.6. Haidt took this as a "proof" of the existence of neutral currents (see Haidt 2004, p. 29). However, it has to be noted that

5.1 The Monte Carlo Calculations of Neutron Background

As we saw above, whether one "observes" neutral currents fundamentally depends on the estimate of the neutron background, i.e. those hadron events, which are induced by neutrons rather than by neutrinos (see Fig. 7, bottom). This is because, as we also saw above, neutral currents can be "mimicked" by (in fact, they are observationally indistinguishable from) neutron induced events. In the case of the HPWF spark chamber experiments, "fake" muonless events originated when there were wide-angle muonful events, which were not detected by the apparatus, and by hadron punch-through. In the Gargamelle bubble chamber experiments, weak neutral current events were mimicked by those neutron stars, which were produced in the walls of the chamber (see Fig. 7). Both the neutron background in the HPWF experiment and in Gargamelle could not be measured. It had to be *estimated*. Since the number of *observed* neutral current *candidates* is the sum of both genuine and "fake" neutral currents (i.e. neutron induced events), the lower the estimate of the neutron background, the higher the number of estimated neutral current events. So in fact, it is not the case that there were multiple, equally valid arguments which all pointed towards the existence of neutral currents, as Galison (1984 and 1987) has suggested[30]. Rather, *everything* about the discovery claim about neutral currents hangs on the Monte Carlo estimates of the neutron background.

Mayo, Galison, Miller and Bullock treat the Monte Carlo calculations as epistemologically rather unproblematic[31]. Very much in contrast, Pickering (1983; 1984) has cautioned that the Monte Carlo estimates were far from being uncontroversial. Pickering lists four grounds ("not exhaustive") on which the Monte Carlo simulations could be legitimately challenged: details of the characteristics of the incoming neutrino beam, probabilities of the production of neutrons by neutrinos,

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the calculation of B/AS to be expected was carried out by the Monte Carlo program, which was highly contested. See next section for details.

[30] This mistake has been replicated by e.g. Bogen and Woodward (1988).

[31] In his later work, Galison seems to take a more cautious stance towards the epistemological status of Monte Carlo methods. In his book, Galison claims that "for experimenters, Monte Carlos [sic] never came to occupy a position of 'true' experimentation" (Galison 1987, p. 776). Nevertheless, Galison suggests that "Monte Carlo methods appeared to *truly* represent the deeply acausal structure of the world" (p. 778; my emphasis) which seems to indicate that he takes a realist stance towards Monte Carlo simulations.

relevant parameters for the interaction of neutrons and protons with atomic nuclei, idealised geometry of the apparatus. Pickering claims that “one can easily imagine a determined critic taking issue with some or all of these assumptions” and mentions protagonists, who were indeed voicing disagreement and lacking commitment to the conclusions reached by means of the Monte Carlo simulations (Pickering 1982, p. 96). Moreover, “even if all of the assumptions were granted, it remained the case that they were the input not to an analytic calculation, but to an extremely complex numerical simulation” (ibid.).

> The details of such simulations are enshrined in machine code and are therefore inherently unpublishable and not independently verifiable. Thus the sceptic could legitimately accept the input to the calculation but continue to doubt its output. (ibid.)

This assessment by Pickering is not as implausible, pessimistic and distortive as Galison (1983; 1987) and Miller & Bullock (1994) have claimed[32]. In a review article in *Science* Allen Hammond (1973) explained that “[t]hese [Monte Carlo] calculations are not unusual in particle physics experiments, and while they are not always accurate, *they are in this case considered unlikely to be off by more than 50 percent”*. Nevertheless, due to the smallness of the effect, apparently, “the experimenters believe that the inaccuracy does not affect their calculations”. Still, Hammond concedes that the Monte Carlo calculations “represent the *least certain link in the chain of evidence supporting the CERN findings*” (p. 374)[33]. Haidt (2004) recollects that after the “discovery” paper appeared the Gargamelle group was confronted with “a painful time of defence against unjustified attacks”[34], whereby “the opponents focused their criticism on the neutron background calculation and in particular on the

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[32] Reviewing the attacks that have been launched against Pickering (see in particular Allan Franklin’s work, in which Pickering standardly figures as the archenemy), one may feel that many of the arguments that have been made against Pickering, are more polemical than anything else. Pickering’s arguments are often much more subtle than the “self-fulfilling wish- or desire relativism” that is so often attributed to him by these authors (cf. Pickering 1995). A very nice self-defence by Pickering can be found in his (Pickering 1990).

[33] For another critical contemporary remark about Monte Carlo calculations see *Physics Today*, November 1973, where it says that “Some experimenters are still worried, however, because the CERN group had to employ a Monte Carlo calculation to obtain this result”. (p. 19)

[34] Haidt even told Galison in 1986 that “my reputation as a scientist was put in question” (Galison 1987, p. 197, fn. 153).

treatment of the neutron cascade λ_C" (*ibid.,* pp. 29-30). Even within the CERN collaboration criticism was brought forward against the discovery claims:

> The fall of 1973 was quite hot and influential people at CERN made appear Gargamelle as having made a wrong claim. The critical point was spotted immediately, namely the treatment of the cascade (Haidt undated)

Given the critical reception of the Monte Carlo model and the cascade calculations in particular, how was the weak neutral current signal established? How—to say it in the words of Galison—did the experiments on weak neutral currents end?

6 How Do Experiments End?

In his book with the same title, Galison (1987) concludes that with the 1974 paper by the HPWF group, "the first chapter of the discovery of weak neutral currents drew to an end" (Galison 1983, p. 504). He goes on to say that

> Further experiments were performed at many laboratories all over the world to determine the space-time and isotopic spin structure of the currents, *but the existence of the currents themselves seemed to be assured.* (ibid.; my emphasis)

In other words, with this paper, the experiments trying to determine the existence of neutral currents 'ended', and the discovery could be seen as having been established. Galison (1983) enforces this impression of the "end" of the experiments by quoting Cline and Mann – two members of the HPWF group – as saying "I don't see how to make these effects go away" and "the signal would not go away" respectively (pp. 502-3). Yet, I take it to be a highly questionable move to justify the discovery of WNC with personal, retrospective judgements by a few individuals. Even if we could take these judgements for granted, this would be a long way from having shown that WNC were accepted by the majority within the scientific community, which we would expect if an effect was said to be firmly established. Galison even concedes that "certainly no one moment can be pointed to either in E1A [i.e. the experiment by the HPWF group] or in Gargamelle that could be called the instant of discovery" (p. 506). In fact, it turns out that the blunt claims Galison finds in informal memorandums and recollections by some protagonists in interviews about 6 years after publication, are not at all mirrored in the original publications, which Galison presents as having effectively discovered neutral currents. The abstract of the final paper published in April 1974 by the HPWF group 1974 reads:

> A possible, but *by no means unique, interpretation of this effect* [muonless events] is the existence of a neutral weak current (Benvenuti. et al. 1974, p. 800; my emphasis).

The HPWF group concluded this paper by pointing out – rather hesitantly – that their "measurement is *not in disagreement* with the Weinberg model […]" (ibid., p 803). Still, the door was by no means shut to other interpretations, in spite of the similar findings of the Gargamelle group at CERN:

> Muonless events have also been reported in an experiment done at CERN at much lower neutrino energies. *However, other origins of the effect we observe cannot as yet be excluded* (ibid; emphasis added)

The Gargamelle group concluded in their "discovery" paper quite clearly:

> It has to be emphasized that the neutral current hypothesis is *not the only interpretation* of the observed events (Hasert et al. 1974, p. 20; added emphasis).

Likewise, the November 1973 issue of *Physics Today* concluded that

> 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*. (Lubkin 1973, p. 17)[35]

The scepticism of the research community, as we saw, primarily sprang from the—according to Hammond in *Nature*—weakest point of the discovery claim, i.e. the Monte Carlo programme with its numerous assumptions. But even if one wanted to take the Monte Carlo calculations as granted without questioning them, the observations of the HPWF group did not actually confirm the Salam-Weinberg model and the observations of the Gargamelle group were far below the currently accepted value, as Perkins (1997) pointed out (see also Fig. 12):

> It is interesting to not that the HPWF result *is actually inconsistent with the Salam-Weinberg theory*, while the Gargamelle result shows a value of R [i.e., NC/CC] that *is only about two-thirds of the present-day value* […]. The value deduced for $\sin^2\theta_W = 0.38 \pm 0.009$ has to be compared with the present value of 0.23. (Perkins 1997, p. 442)

In other words, whereas the Gargamelle results were not at all accurate when compared to currently accepted values, the HPWF results were even inconsistent with the latter.

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[35] Lubkin, G. B. (1973), *Physics Today*, vol. 26, November, 17, pp 19-17.

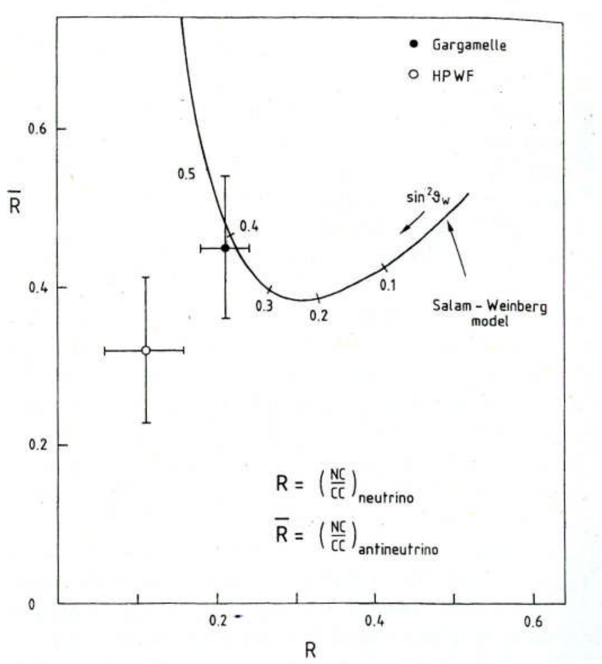

Fig. 12: Comparison of the results of the main Neutral Current experiments.
The figure shows the results obtained from Gargamelle and HPWF as compared to the predictions of the Salam-Weinberg model. From Perkins (1997).

So it appears that the experiments, which Galison in his account makes look unequivocal, are actually far from it. There also appears to be no difference *in principle* between the evidence and the arguments, which led to the "discovery event" in the 1970s and those which did not in the 1960s. Even the evidence which the 1973 and 1974 papers by the Gargamelle and the HPWF groups cited for the existence of neutral currents was subject to doubt and open to counter-arguments. So contrary to Galison, the experiments did not quite so obviously "end" with the Gargamelle 1973 and the HWPF 1974 papers in the sense of WNC being firmly established. Furthermore, we cannot even say *why* WNC experiments should have ended where Galison wants them to end. The end in Galison's story seems to be rather arbitrary. In Galison's story we encounter many arguments and a few instances of evidential support or lack of support, where Galison's story *could* have ended. At one instance Galison even explicitly says that the HPWF group "seemed to have reached the end of a difficult series of experiments" but then titles his next section with "dismanteling of an ending" and describes how the HPWF group decided that they had *not* observed neutral currents (1987, p. 223; see Section 4.1 for details). None of the arguments or any of the evidence, which Galison considers, appear to be *inherently* capable of putting a stop to the debates about the presence or absence of WNC.

Pickering shrewdly remarks that Galison appears to have "the knack of describing the last persuasive argument in a controversy as being so persuasive as to be, actually, unarguable" and points out that the "literary undoing of all persuasive arguments *except the last one* goes unjustified in the text and adds nothing to our understanding of how experiments end" (Pickering 1988, p. 472; emphasis mine). In fact there was no difference between the arguments leading to an alleged end of experiment those which did not result in the establishment of WNC: the amount of neutral current events *always* critically depended on neutron background estimate. This did not change at any point throughout the efforts made by the HPWF and the Gargamelle group for establishing WNC as genuine phenomenon. Moreover, *even if* the arguments Galison quotes as leading to the discovery had been different from those arguments that did not *and* if those arguments were capable of putting a stop to the experiments, Galison does not provide any analytical tools for detecting such arguments, let alone for locating an end of experiments.

The fact that Galison talks much about "constraints" does not change anything about this assessment. As Pickering (1995) has noted, Galison is not very explicit about what he means by constraints, with the most explicit statement being: "I want to use the notion of constraint the way historians often do – to designate obstacles that while restrictive are not absolutely rigid" (1987, p. 47). The concept of constraint allows Galison to adopt some form of middle ground: although nature did not force scientists to accept the existence of WNC, it did nevertheless somehow compel scientists to do so. In fact, the concept of constraint possesses almost magical features. If scientists discover a new phenomenon, Galison can tell a story where scientists were "constrained" in their practice towards this discovery. On the other hand, if scientists failed to discover anything, then Galison can claim that there were no constraints present or that they were simply too weak. It is then questionable what sort of work the concept of constraint does for us in terms of us gaining a better understanding of any historical episode.

Furthermore, one might want to become a bit more concrete and ask, what really were the constraints in the discovery of neutral currents? "Instruments, specific devices, theories, models (phenomenological) laws, unification, and individual runs", one reads rather puzzlingly (Galison 1987, p. 254). One then has to wonder *how* all these entities managed such constraining. But perhaps, they did not. It rather seems that Galison story could have done without any constraint-talk whatsoever without losing any of its content, with the reason being that Galison's account is just a descriptive story; not an explanation[36]. We therefore—contrary to Galison's claims—do not understand *why* the experiments ended (if they actually did)[37].

Still, despite the conclusions reached above and rather paradoxically, the 1973 experiment by the Gargamelle group and the 1974 experiment by the HPWF group are nowadays *indeed* quoted as "discoveries" (e.g. Weinberg 1980, p. 518)[38]. How can that be? There is then an interesting question as to what Pickering called the "openness" and "closure" of experimental systems (see e.g. Pickering 1983, p. 6). A closed experimental system is perfectly understood and rules out every conceivable error and background noise. An open experimental system, instead, is a system which is imperfectly understood and thus always open to criticism and different interpretations. According to Pickering closed systems are only to be found in science textbooks but not in the lab or in actual scientific practice. In actual scientific practice, closed experimental systems are never possible:

> Experimenters do their best, of course, to eliminate all possible sources of background, but it is a commonplace of experimental science that this process has to stop somewhere if results are ever to be presented. Again, a *judgement*

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[36] For a more explicit characterisation of constraints see Weinert (1999). Weinert illustrates his notion of constraints with an example from early atomic physics. J.J. Thomson's plum pudding model became untenable because of Rutherford's scattering experiments. In the language of constraints, Rutherford's model *constrained* the conceptual space of possible atomic models. And yet, it is rather unclear why we cannot talk of a clear refutation of Thomson's model. Constraints seem to be too weak for accounting for this historical episode.

[37] Galison explains that he does not see the task of historians to "produce rational rules for discovery", which – he informs us – is "a favourite philosophical pastime" (p. 277). The task of the historian is rather to "capture the building up of a persuasive argument about the world around us, even in the absence of the logician's certainty" (ibid.). Yet, as the discussion of this paper shows, the pervasiveness of the arguments Galison cites in his work obviously seem to be relative and apparently not sufficient for making a discovery claim. It should perhaps also be noted that Galison's characterisation of philosopher's "favourite pastime" is more of a caricature than anything else. On the contrary, in the "early days" philosophers of science used to ban discoveries from analytical analysis due to their complexity. Although philosophers long ago gave up on the context of discovery/justification distinction, and although they have tried to make sense of discoveries, they have certainly not given themselves to the illusion that there were "rational *rules* for discovery", as Galison contends.

[38] Rather naively Weinberg states that "as everyone knows, neutral currents were finally discovered in 1973" (Weinberg mentions the simultaneous discovery by the HPWF group in a footnote). This is a good example of distorted retrospective history produced by scientists. See the locus classicus Kuhn (1996) for details.

> is required, that *enough* has been done by the experimenters to make it probable that background effects cannot explain the reported signal, and such judgements can always, in principle, be called into question. (ibid., p. 6; original emphasis)

These judgments do not figure in textbook accounts. Rather, Pickering suggests, scientists identify their theoretical constructs with "the contents of nature" and "then use this identification retrospectively to legitimate and make unproblematic existing scientific judgements" (ibid., p. 7)[39]. In the case of the discovery of weak neutral currents that means that

> [T]he experiments which discovered the weak neutral current are now represented in the scientist's account [and in Galison's for that matter] as closed systems *just because the neutral current is seen to be real*. Conversely, other observation reports which were once taken to imply the non-existence of the neutral current *are now represented as being erroneous:* clearly, if one accepts the reality of the neutral current, this must be the case. (p. 7)

I entirely agree with this assessment. Likewise, I agree with Pickering that, yes, *real* experimental systems (not the idealised ones to be found in textbooks) are *always* open systems. And although, yes, a judgement is required *in principle* that *enough* has been done, I believe that such judgements are *never* made in actual scientific practice. More likely I hold to be the view that debates about experimental systems simply fade out, when both or either of the following two conditions is met: a) the adopted concept (here: neutral currents) proves to be viable in other experimental contexts, and b) it is necessitated by a theory. The latter we of course observed with the discovery of neutral currents.

7 EPISTEMIC WARRANT FROM TOP-DOWN

Throughout this section we saw that the *active* search for neutral currents, on a very basic level, induced experimentalists to change their experimental apparatus (HPWF trigger), and their data analysis (exclusive vs. inclusive currents, "energy cutting" procedure). In the words of A. Rousset:

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[39] This is how Pickering's paper title 'Against Putting the Phenomena First' (1984) derives. It is against the habit of 'using phenomena to explain scientific practice' in order to 'ask why one scientist *succeeded* in observing some historically-accepted phenomenon while others *failed*' (p. 86).

> If the theoreticians didn't urge the experimentalists on the search of weak neutral currents, the NC candidates would perhaps remain in the scanning books. (ibid., p. 348)

This appears to be rather incompatible with Bogen and Woodward's account of the bottom-up construction of scientific phenomena according to which the theory predicting the phenomena should neither bear on the production nor the analysis of the data (see Fig. 1 on page 5). We also saw that the neutron background could not be observed but had to be *estimated*, and that this estimate directly determined the "strength" of the neutral current signal. Those estimates, arrived at by means of the Monte Carlo simulations, are challengeable in principle and actually were challenged. So surely, contrary to Bogen and Woodward (and the New Experimentalists) experimental practice on itself was not capable of delivering enough warrant for strong belief in the phenomenon of neutral currents. At least part of the warrant must have come from somewhere else than from experimental practice. This is nicely illustrated by another quote by Rousset, who, some 20 years after the "discovery", wondered why nobody among Gargamelle's critics had asked an even more fundamental question than just putting into doubt the cascade calculations:

> 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 [sic; viz. "if"] the NC candidates are definitively not neutron interactions, is it demonstrated [with certainty] that there are neutrino reactions? […] The interpretation as weak neutral current interactions of neutrino is the most plausible, *but it results mainly from a theoretical prejudice*. […] It is astonishing that this weakness of the argumentation was not exploited to criticize the Gargamelle experiment with a stronger argument than the unfair criticisms [focussing] on the neutron background. (Rousset 1994, p. 349)

In other words, if we want to follow Rousset, the commitment to the existence of neutral currents was essential to the discovery claims about neutral currents. Where did this commitment to weak neutral currents come from? The answer is provided by the Weinberg-Salam model.

In the early and mid-1960s weak neutral *lepton* currents were known of only through analogy with charged lepton currents (cf. Fig. 5) and were regarded as virtually non-existent[40]. Neither charged nor neutral lepton currents were observed,

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[40] See Galison (1987). See also e.g. Lee and Yang (1960), "Theoretical Discussion on Possible High-Energy Neutrino Experiments", *Physical Review Letters*, 4, 307-377.

nor were they expected to be observed since their cross-sections[41] were very small. Even though the Weinberg-Salam model[42] of electroweak interactions first only made predictions about lepton currents, shortly after its proposal it was extended to neutral *hadron* currents for which it predicted the same order of magnitude as for charged hadron currents. In this respect the predictions of the W-S model differed considerably from the predictions of the then accepted V-A theory of weak interactions. Neutral currents therefore were crucial for deciding between the then accepted V-A theory[43] and the newly proposed W-S model. However, the W-S model did not attract much attention, until it was renormalized by t'Hooft in November 1972 (see Koester et al. 1982). This made the W-S model reasonably predictive, because it rendered its inherently infinite predictions finite[44]. Still, the model was in severe mismatch with the experimental data. Weak neutral currents had not been observed. Although the so-called GIM mechanism (after its inventors Glashow, Iliopoulos and Maiani) saved the W-S model against refutation from strangeness[45] changing neutral current by positing a fourth 'charmed' quark, the mismatch between theory and experiment remained for strangeness *conserving* neutral currents[46]. A whole industry of model building emerged, which tried to save the unificatory benefits of the W-S model whilst at the same time accommodating the fact that neutral currents were not observed (see Pickering 1983, p. 186; Koester et al. 1982)[47]. The problem with these theories was that the price for the annihilation of neutral currents was the introduction of new particles (neutral/charged heavy leptons). Pickering notes that "[t]heorists

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[41] Cf. footnote 21.

[42] The W-S model is now also known as the "standard model" and was independently proposed by Weinberg (1967) and Salam (1967).

[43] This theory was developed by Feynman and Gell-Mann (1958), "Theory of Fermi Interaction", *Physical Review*, p. 109, p. 193-198.

[44] The W-S model (or theory) is a gauge theory, a particular kind of quantum field theory. Solutions to Quantum field theories are not exact but need to be approximated by perturbation series. For higher order approximations these series lead to infinite results. Recognising that only a small number of distinct types of infinites occur allows one to substitute those types by measured values. This step is known as 'renormalisation'.

[45] Strangeness is a property of particles for describing decay of particles in strong and electro-magnetic reactions.

[46] Although the GIM mechanism was used to save the W-S model, it was originally devised to account for the kaon-day anomaly. For details see Pickering (1983, pp. 180-1).

[47] One here is strongly reminded of Thomas S. Kuhn's assertion of the proliferation of theories in the response to anomalies.

found themselves articulating models referring to phenomena which seemed to have no experimental counterparts" (Pickering 1983, p. 186). This mismatch was finally corrected with the discovery of weak neutral currents by the HPWF and the Gargamelle groups, clearly favouring the W-S model over its "neutral-current-free" rivals.

Pickering suggests that there exists some sort of symbiosis between theoretical and experimental practice in the sense of a *mutual* dependency. If this symbiosis is somehow broken up, i.e. if the match between theoretical and experimental practice is lost, scientists will do anything they can to regain this symbiosis. Pickering has interpreted this symbiosis in socio-economic terms. He has claimed that WNC were a 'socially desirable phenomenon' (ibid., p. 109) and that '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' (ibid., p. 87; my emphasis)[48]. However, even though I agree with many of the points Pickering makes about the discovery of WNC, I do not think that one needs to resort to any such socio-economic arguments. Even if one were to reject all sociological arguments as being irrelevant to the discovery of WNC, from the discussion of this paper it should have become very obvious that the evidence itself (despite being present already in the 1960s, as we saw) was insufficient for convincing physicists of the existence of WNC. I suggest that it was the W-S model that made the existence of WNC plausible, because it *required* the existence of WNC[49]. Apart from the available data, it was this epistemic warrant from the "top-down", as it were, which gave physicists enough confidence in the existence of WNC. Hence the W-S model provided an epistemic warrant without which WNC might never have been discovered. This claim comes in a weak and a strong version. According to the weak version, there would have been no motivation for the active search of neutral currents—involving the change of apparatus and changes in the data analysis procedure. According to the stronger version, without the W-S model the gathered evidence would itself not have been

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[48] Pickering has received much criticism for this statement (see e.g. Franklin 1986, 1990, 2002).

[49] It should be noted that the WS model was not just *any* model but one that had certain valuable features. It unified the phenomena and it was, in contrast to its competitors, a somewhat elegant theory.

strong enough to warrant belief in the existence of neutral currents. On the basis of the discussion of this paper I am prepared to embrace both claims.

8 CONCLUSION

In this paper an attempt was made to shed some light on the problem of discerning a physical effect or phenomenon from noise by considering the discovery of Weak Neutral Currents (WNC). Incidentally, it is this discovery that has frequently been cited by extant philosophical accounts of effect-noise discrimination. Bogen and Woodward (1988), for instance, have claimed in general, and in the case of the discovery of WNC in particular, that phenomena are inferred from data without the involvement of the causal theory that seeks to explain either the data or the phenomenon in question. According to them, inferring phenomena from data is entirely extraneous to any explanatory enterprises and merely requires the application of various well-established experimental procedures like data reduction, statistical arguments etc. The latter have been particularly emphasised by the other philosophical account of effect-noise discrimination, which has made heavy use of the discovery of WNC (Mayo 1994, 1996). I have argued against those two views by, on the one hand, criticising statistical arguments as being insufficient for the establishment of genuine effects (particularly *pace* Mayo and to some extent *pace* Bogen and Woodward) and on the other hand by showing that, contrary to Bogen and Woodward, the higher level physical theory (here: the Weinberg-Salam model of electroweak interaction) explaining the phenomena (here: WNC) *did* bear on the data analysis and production (here: bubble and spark chamber photographs). In Section 3.1 this paper I argued for the limited role of statistical arguments by referring to those experiments (i.e. bubble chamber experiments) which are less susceptible to statistical arguments than spark chamber experiments (which Mayo quotes exclusively) simply because they produce a smaller amount of data (but data of a higher “resolution”). In particular, it was a *single* bubble chamber picture (the so-called “Aachen picture”), which “sealed the deal” for many physicists who were involved in the WNC experiments, i.e. this single picture gave them enough epistemic warrant for believing in the existence of WNC.

More severely, none of these philosophical accounts of effect-noise discrimination can accommodate Pickering’s observation that the data analysis by the

HPWF and the Gargamelle group changed considerably from the 1960s to the 1970s, from when WNC were taken to be practically inexistent to when discovery claims about WNCs were made. Pickering has argued that the 1960s experiments were already capable of discovering WNC *in principle*. This assessment has been confirmed in the present paper (Section 4) against claims to the contrary (Miller and Bullock 1994). In the 1960s the HPWF experiments used a trigger, which recorded muon-ful events *only*. This changed in the 1970s when Weinberg had given the experimenters some theoretical motivation (in the form of the WS model postulating WNC) for taking into consideration muon-less events (i.e. WNC candidates) by allowing the trigger to record such events. Quite obviously, the experimental apparatus of the 1960s itself was well capable of detecting a WNC signal. The only thing that did change was the *willingness* of the experimenters to detect such a signal. This change is not explicable by any statistical arguments. And quite clearly the theoretical predictions of the phenomena *did* bear on the data production—very much contrary to Bogen and Woodward. In the case of the bubble chamber experiments, Miller and Bullock (1994) have alleged that the argument from spatial distribution, discussed in Section 5, could have not been possibly made in the pre-Gargamelle bubble chambers. This is simply incorrect. Moreover, even if Miller and Bullock were right, the argument from spatial distribution was rendered invalid by neutron cascades, as Haidt and Fry pointed out. In *none* of the historical and philosophical accounts of the discovery of WNC, this point has received due recognition. This was corrected in the current paper. In this paper, we even saw evidence that data indicating the existence of WNC were already present in the 1960s and that those data obviously had been overlooked by the researches involved in the experiments at the time.

Contrary to Galison in particular, who has given the impression that the discovery of WNC was the result of many different lines of arguments, I have emphasised in Section 5.1 that any discovery claim critically depended on the Monte Carlo estimates. Since WNC cannot be observed but only *estimated* in relation to neutron background from which they are observationally indistinguishable, a lower estimate of neutron background *automatically* entails a higher number of WNC events. As a consequence of the vulnerability of the Monte Carlo estimates, no real end of the WNC experiments can be located in the original literature—very much contrary to Galison (Section 6). Even if there was such an end, I have argued, Galison does not provide any analytical tools for identifying such an end. More seriously—

again contrary to Galison—there seems to be no sufficiently compelling epistemic appeal to originate from either any arguments devised in the experimental chase for WNC, nor from the evidence itself. Still, we take WNC to have been established and discovered. In contrast to Pickering, I have argued that the reasons for the discovery claim are not to be sought in sociological factors but rather in reasons to do with the theory at issue (Section 7). Neutral currents were discovered not only because there was sufficient evidence for them (and as I have shown there was sufficient evidence for WNC long before a discovery claim was made), but also because the Salam-Weinberg model required them. I called this the epistemic warrant from top-down.

What are the more general lessons to be learned from this case? Is it reasonable to think that the strength of a particular physical effect is always going to be as critically determined by the estimation of background noise as in the case of the discovery of WNC? Should we be prepared to accept that experimental techniques and the data produced by them are never going to be sufficient for effect discrimination and (occasionally) the corresponding discovery claim? May the discovery of WNC not just be an extreme case without much import for our notion of effect discrimination? Of course, this might indeed be so. It is an empirical question as to how general the conclusions of this paper are really going to be, and it will require further studies of signal discrimination in other domains of science. However, there are indications that the conclusions of this paper will stand for a number of cases. Thus far, the discovery of WNC was taken to be (particularly by Bogen & Woodward, Mayo, and Galison) the paradigm case for low level, experimentally-driven discrimination of noise and physical effect. It is then rather telling that precisely this paradigm case obviously does not fit the scheme of the 'bottom up' construction of phenomena, which has most clearly been articulated by Bogen and Woodward but which has much in common with the central tenets of New Experimentalism. Furthermore, I do think that it is reasonable to assume that physical effects will never occur in isolation and on their own (as much as we desire this and as much as we try to achieve this[50]) but rather will always be accompanied by other 'unwanted', 'intervening' or 'background' effects[51]. Therefore a claim about the amount and constitution of a

[50] See Cartwright (1983), Chapter 3, for an illuminating discussion of this point.

[51] There is also a story to be told about what sorts of effects are going to count as 'interesting' or 'uninteresting', 'wanted' or 'unwanted', 'relevant' or 'irrelevant', 'genuine' or 'false'. It is

particular effect will naturally also have to say something about those other effects, which are held to be different from the effect in question. There will thus most likely be a critical dependency relationship between claims about a particular effect and claims about the extent of 'noise', very much in accord with the conclusions reached about WNC in this paper.

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interesting to note that in the case of WNC it was the theory (Salam-Weinberg model) that assigned those positive attributes to WNC and those negative ones to neutron-induced events. Also this situation should be generalisable.

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