To envision a new particle or change an existing law?

Hypothesis Formation and Anomaly Resolution for the Curious Case of the β Decay Spectrum

Abstract

This paper addresses the question of how scientists determine which type of hypothesis is most suitable for tackling a particular problem by examining the historical case of the anomalous β spectrum in early nuclear physics (1927-1934), a puzzle that occasioned the most diverse hypotheses amongst physicists at the time. It is shown that such determinations are most often implicitly informed by scientists' individual perspectives on the hierarchical relations between various elements of the theory and the problem at hand. In addition to this main result, it is suggested that Wolfgang Pauli's neutrino idea may well have been an adaptation of Ernst Rutherford's original and older neutron idea, which would provide evidence that the adaptation of older ideas is a more common practice than is often thought.

Keywords

Hypothesis Formation – β spectrum – Early Nuclear Physics – Neutrino – Energy Conservation

1. Introduction

In physics, as in other scientific disciplines, an anomalous experimental result can occasion the formation of formally quite different hypotheses. Confronted with such a result, a scientist has no strict guidelines to help her determine whether she should explain the result by withdrawing or adapting a constraint of the current theory (e.g. a law), or else by presupposing the existence of a hitherto unobserved entity that makes the anomaly fit within that theory (e.g. a particle). But she has more options than this: she can also suggest a new structural model, blame the anomaly on an overlooked feature of the experimental setting, or stretch and modify the theoretical classes that label the observables, among other possibilities.

If a scientist knows in advance which kind of hypothesis would best explain the anomaly, she can employ more efficient heuristics. For instance, when Max Planck was studying the experimental anomalies of Rayleigh-James' and Wien's laws for the spectrum of black bodies, he sought a new law that fitted the data. Similarly, when Ernst Rutherford was confronted with the backwards scattering of α particles, he knew he had to construct a new structural model for the atom.

As the case study examined in this paper illustrates, however, the situation is not always so clear: when an experimental anomaly proves perseverant, even the greatest minds in the field can differ strongly in opinion about which kind of hypothesis would lead to a satisfying explanation. Suggested hypotheses can vary so widely primarily because the determination as to which formal kind of hypothesis is needed is in itself an abductive and, hence, defeasible inference. Although often inferred implicitly, this choice is hugely important, as it determines what direction the initial search will take.

A lack of heuristics for this initial choice of hypothesis type presents itself as a problem especially when formal representations are utilized, such as in logic or AI. Because such representations determine in advance what formal types of hypotheses can be inferred, the choice of hypothesis type is (often implicitly) made when the premises are translated into the formal language: there are different ways to describe a (realistic) anomaly in natural language, any of which can lead to a different formal representation.

My aim in this paper is not to suggest a normative heuristics for this choice, for given the lack of research in this area we lack sufficient knowledge about how scientists in the field decide on this matter (see Section 2 for an overview of the philosophical literature on discovery and hypothesis formation). Instead, my more modest goal is to examine how this choice was made in one notable instance, by examining a concrete case study with various diverging hypotheses. It will be shown that this choice is almost always made implicitly in a manner determined by the scientist's previous experiences and specific way of perceiving the problem, and that, moreover, scientists in general are sometimes, due to their particular perspective and the strong ontological commitments it often entails, very unwilling to accept other kinds of hypotheses.

Between 1927 and 1934, a manifest and persistent anomaly mystified the physics community: while α and γ decay behaved perfectly according to the new quantum mechanics, the energy of electrons emitted in β decay displayed a broad continuous spectrum. This puzzle intrigued the most established and famous physicists of the time, including Bohr, Heisenberg, Pauli, Rutherford, Chadwick, Ellis, and G.P. Thomson, and incited a lively debate among them. Curiously, all suggested hypotheses were of very different formal types: **Ellis and Wooster were willing to give up the universality of the quantum postulate,** Rutherford and Chadwick thought of varying internal energies, Bohr suggested a restriction of the energy conservation principle, Heisenberg tinkered with a second quantization of space at the scale of the nucleus, and Pauli suggested the existence of a new elementary particle – all these hypotheses being, as we will see, quite radical and highly controversial.

By focusing in detail on how these scientists arrived at their hypotheses, this paper challenges the somewhat mythical proportions this passage has received in more popular histories of science, which, with its focus on genius and success, typically traces great discoveries back to a single man who enlightens his community by a kind of epiphany. New ideas do not come out of nowhere, but are related to older suggestions. This debate also cannot, as is often done, be narrowed down to Pauli's and Bohr's stances alone: many more ideas were around at the time, and all of them influenced each other.

I begin, in Section 2, by situating this paper's research question in the philosophical literature on scientific discovery and hypothesis formation, and then, in Section 3, introduce the specific case to be examined historically. In Section 4, the main part of this paper, I analyze in detail the reasoning processes of six prominent physicists (or couples) who tried to address the β puzzle. Finally, in Section 5, I summarize these results and connect them back to the questions raised in this introduction.

But before we continue, some reservations about the methodology and scope of this paper are in order.

First, I will not discuss this case in a purely historical or descriptive fashion, as this has been done sufficiently and extensively in other places such as Jensen 2000, Pais 1986, Bromberg 1971, Brown 1978, Hughes 1993, Navarro 2010, Cassini 2012, and Guerra et al. 2012. Instead, I will try to reconstruct how the various protagonists could have reached the hypotheses they suggested and show how the choices they made along the way are related to their personal perspectives – a project I was able to perform only because of the excellent scholarship on this period by historians of science, as their extensive coverage made that the, given the temporal and spatial scope of this episode, nearly impossible task of a full reassessment of the archival record could only marginally benefit this project.

Second, in principle, there are three ways to study human reasoning processes such as hypothesis formation: from an internal perspective by

analyzing direct feedback from the agents (e.g. psychological experiments), from an external perspective by linking the agents' recorded ideas to the historical and scientific context (e.g. historical case studies), or via simulation by trying to reproduce the agent's ideas (e.g. computational or logical approaches). As I do not assume that scientists make a conscious "metachoice" concerning which pattern of hypothesis formation is most appropriate for a particular problem, I believe that the examination of historical cases is the best method to gain some initial insight into how and why different patterns might have been employed in response to a single problem, as we can, by virtue of hindsight, situate these suggestions in their context. Having said this, of course, one should immediately note the drawback that we have no means to gather direct feedback from the agents themselves; we have only our interpretations of their scattered remarks, which are always based on assumptions and might be erroneous. This same problem occurs even when agents are alive and approachable, as agents tend to rationalize and reconstruct their thoughts afterwards.[1]

Therefore, this article does not claim to offer a factual representation of the agents' thought processes. It aims, rather, to offer a coherent interpretation of how the protagonists' recorded ideas could have originated by making reasonable assumptions and specifying the historical surroundings. As Darden (1990, p. 4) has already acknowledged, this kind of research necessarily has a speculative flavor, as it can merely reconstruct "how" the agents "possibly" arrived to their hypotheses. Still, as it is generally assumed in the literature on discovery (see Section 2), this kind of research offers us insights into the process of hypothesis formation that cannot be obtained by exploring logical principles or by psychological experiments alone. There is certainly value in trying to provide the best possible explanation of how actual agents in actual historically important debates arrived at their ideas, and such will be my aim in these pages.

2. Hypothesis Formation and Philosophy of Science

In this section, I summarize the status quo of the research on hypothesis formation and discovery in the philosophy of science by reflecting on how the main questions of today were shaped over the course of the twentieth century. This will allow us to situate the analysis and conclusions of this article in a broader perspective.

[1] As Allan Franklin (1993, n. 110) reported in his study of the rise and fall of the fifth force hypothesis, protagonists might fail to give an accurate view of their own ideas and positions, even though the interviews were conducted only a few years later. Sometimes, these reconstructions become apparent if they are confronted with external historical evidence, as for instance in Brown (1978), who showed that Pauli's recollections concerning whether the neutrino is a nuclear constituent were incorrect (see Section 4.6).

At the beginning of the twentieth century, at the height of logical positivism, philosophers generally held that the mind's ability to generate new hypotheses is situated outside the realm of rational thinking.[2] This idea was crystalized in Reichenbach's very influential distinction between a context of discovery and a context of justification (Schickore and Steinle, 2006; Laudan, 1980; Kitcher, 2013), which underlies and gives expression to the longstanding prejudice that scientific discovery (and hypothesis formation) were not proper matters of interest for the philosophy of science (e.g. Popper, 1959; Laudan, 1980, p. 182).

Opposing this strong bias of the early twentieth century, Charles Sanders Peirce was the first to advocate the idea that explanatory hypothesis formation is a distinctive and important form of rational inference, for which he used the notion of "abduction" (Peirce 1958, CP 5.172).[3] This suggestion was picked up by Hanson, the pioneer of a generation of philosophers that based their reflections on a thorough familiarity with the history of science (e.g. Hanson 1958, p. 3). By discussing how Kepler inferred the hypothesis of elliptical planetary motion from the observations made by Tycho Brahe, Hanson argued that Kepler's "keen logical sense" is shown in the sound reasons he cited for every step he made, steps whose explanatory character prevents us from classifying them as purely inductive generalizations from the available data (1958, p. 84-85). Although Hanson has been rightly criticized for underestimating the role of theoretical and other constraints in scientific discovery (Nickles 1980, p. 23; Darden 1991, p. 10) and confusing the actual generation of hypotheses with their preliminary evaluation (Schaffner 1980, p. 179), his observation that scientific hypothesis formation is a reasonable affair has been confirmed over and over again by philosophers of science who have extensively studied real historical discoveries (e.g. Franklin 1993, p. 124; Darden 1991, p. 3; Nersessian 2008, p. 5).

Despite this widespread consensus, however, concerning the rationality of scientific discovery processes and the acknowledgement that hypothesis formation can be addressed rationally, two things remain clear: (1) that the

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[2] It was not that these early philosophers of science claimed that full-fledged theories could originate from bold leaps of the imagination. Rather, as Meheus (1999) shows, they generally acknowledged and even sometimes discussed the use of rational search methods in scientific discovery. Only, for them, these methods relied essentially on the input of early hypotheses and particular interpretations, which could not themselves be derived by rational processes.

[3] Peirce distinguished abduction, the formation of explanatory hypotheses, not only from deduction but also from induction, the inference from cases to generalized statements. Although the distinction between abduction and induction can be put into question in its specifics (Aliseda 2006, p. 33-34), Peirce was certainly the first to argue for the rationality of explanatory reasoning that is not based on generalizations from cases.

notion of "abduction" or "abductive reasoning" has not been broadly accepted, and, more importantly, (2) that the study of this particular type of inference has not been assigned a central place in research on scientific processes and discoveries. I distinguish three main reasons for this turn of events.

First, Peirce's original ideas on abduction, which were recorded over a period of decades, underwent several changes and lack a coherent interpretation (Anderson, 1986; Kapitan, 1992, p.15; Plutynski, 2011). As a result, the concept of abduction refers in the present literature to at least three different types of inference: the generation of new (explanatory) hypotheses (e.g. Gabbay and Woods, 2006; Campos, 2011); the inference to the (truth of the) best explanation (e.g. Harman, 1965; Lipton, 1991; Douven, 2011); and the selection of the hypothesis that is most worthy of pursuit (McKaughan, 2008). Such a situation clearly leads to mutual misunderstanding if one neglects to specify the type of reasoning one is discussing.

Second, even if one does agree on the type of inference in question, there are even more interpretations concerning how broadly abductive reasoning should be conceived. Is it a particular and rather constrained formal reasoning pattern, as it is generally conceived in logics and AI (e.g. Flach and Kakas 2000; Gabbay and Kruse, 2000)? Or is it an all-encompassing scientific method, as Hanson (1958, 1961) and some proponents of the IBE view (e.g. McMullin 1992) held it to be? In such a climate, it is also difficult to clearly draw the line between abduction and induction (Aliseda 2006, p. 33-34) and hardly any effort has been put into clarifying the relation between abductive reasoning and other well-studied practices in scientific discovery, such as the construction and refinement of models and the formation of new concepts. Aliseda, in her monograph on abduction, summarizes the situation as follows:

> Many authors write as if there were pre-ordained, reasonably clear notions of abduction and its rivals, which we only have to analyze to get a clear picture. But these technical terms may be irretrievably confused in their full generality, burdened with the debris of defunct philosophical theories. (Aliseda 2006, p. 34)

Third, Hanson presented the rationality of discovery as if there existed a unified method of discovery, i.e. abduction, which could be linked to the old notion of a "logic of discovery" (Hanson, 1958, 1961). But for the next generation of scholars on discovery, it became clear that the old Baconian dream of a single subject-independent algorithmic procedure of discovery had been debunked by the complex subtlety of present-day theories, the long and arduous processes of discovery that led to them, and the important role of (previous) theories in scientific reasoning. Therefore, they distanced themselves from the Hansonian views in favor of a multitude of methods of discovery; some even argued against the formal logical treatment of any particular pattern (Nickles 1980, p. 23-28). This strong criticism led many to regard the literature on abduction as a more logically and epistemologically oriented side branch, which has a somewhat exegetical nature (e.g. Niniiluoto, 1999; Hintikka, 1998) only loosely connected to the mainly historically-oriented stem of research on scientific discovery in the philosophy of science. Still, the literature on abduction has in recent years bridged this gap, to a certain extent, by acknowledging the multitude of "patterns of abduction" and attempting to provide a classification of formal patterns that can be used to address real historical cases (Schurz, 2008).

In the 1960s and '70s, the rise of historicism in the philosophy of science showed that scientific confirmation is not the neat logical process it was once taken to be, and hence not so easily separable from the process of discovery (Nickles 1980, p. 2). About the same time, research in psychology and AI showed that there are better and worse heuristics for problem solving, which opened the way, at least in principle, for the construction of normative theories for problem solving activities such as scientific discovery (Simon, 1973). These new understandings led to the emergence of a group of philosophers whose research focused primarily on the process of scientific discovery, the so-called "friends of discovery". By around 1990, this loosely-knit group agreed on the following ideas, which still stand today: (1) that scientific research is a gradual step-by-step process of constant refinement (Darden, 1991, p. 11; Langley et al., 1987, p. 57-59; Shah, 2007) and that, as such, there is no strict distinction between the context of discovery and the context of justification (Nickles, 1980, p. 8-18; Hoyningen-Huene, 2006); (2) that discovery can be seen as a problem solving activity, and thus can be addressed rationally (Simon et al., 1981; Nickles, 1978); (3) that there are no definite algorithms or logics of discovery, but only a plethora of heuristics, strategies and methods that are context- and subject-matter-dependent (Achinstein, 1980; Nickles, 1990; Darden, 1991, p. 11); (4) that both the hypothetico-deductive and inductive views of the scientific method are obsolete, as the first retains the old distinction between discovery and justification (Darden 1991, p. 9-17) while the second neglects the importance of theoretical constraints in scientific problem solving (Nickles, 1980, p. 35).

Where does this leave research on discovery, hypothesis formation and abduction nowadays? Although agreeing on the central insights listed above, research has led in many different directions, each pursuing a particular methodology. (1) Some researchers have tried to further naturalize the insights about discovery processes by linking the patterns found in studying historical discoveries to the psychology literature (e.g. Nersessian, 2008; Thagard, 2012; Magnani, 2001). (2) Others have attempted to specify and classify the various particular patterns or strategies employed in scientific discovery (e.g. Darden, 1991; Schurz, 2008; Hoffman, 2010), which has led to in-depth studies of lesser known patterns or strategies (e.g. Darden and Craver, 2002; Gauderis and Vande Putte, 2012). (3) Given the general understanding of science as a step-by-step process (e.g. Blachowicz, 1998), some others have attended closely to the construction and refinement of models in science, which they hold to be key instruments of investigation (Morgan and Morrison, 1999). Due to the heterogeneity of the class of models, this research varies wildly and is often subject-dependent, as the literature on mechanistic models in biology illustrates; it is also partially linked to the psychology literature via the research on model-based reasoning (e.g. Nersessian, 2008). Finally, (4) computational philosophers of science have continued to refine artificially intelligent discovery systems in an effort to determine, in the spirit of Simon, which heuristics and weak problem-solvers might be efficient instruments of discovery (for an overview, see Darden 1997).

Although the field has become disciplinarily fragmented in recent years, the research challenges that bind these different strands are very similar:[4] namely (1) to explicate the various heuristic and often context-dependent "patterns of discovery"[5] and (2) the relation between them, (3) to provide rational and normative guidance on them, and (4) to pursue the possibilities of computational discovery and its relation to human discovery. All four of these challenges are daunting in scope, yet, given the results obtained in past decades, should not be considered unaddressable.

This paper aims to contribute to the second of these four challenges, i.e. the explication of the relation between the various patterns,[6] by providing

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[4] Several scholars, such as Thagard and Darden, have contributed to more than one of these lines of research. In general, the various strands give each other's results a sympathetic reading.

[5] Although not exactly referring to the same thing, Hanson's phrase (1958) fits remarkably well here (Hanson himself was actually more concerned with the "discovery of (conceptual) patterns" than with the various "patterns of discovery"). Yet my own usage is intended in the same sense as Schurz' "patterns of abduction" (2008), though in a somewhat more generalized way.

[6] This question can be considered as a part of the second of the four challenges Nickles set for research on discovery (1980, p. 44): "How is the overall progress of scientific inquiry

(a) a historical case study with different and contrasting "patterns of discovery" for a single scientific problem and (b) some initial descriptive insights concerning the question of why scientists employ a particular pattern of discovery rather than some other one.

3. The β puzzle in 1927

This introductory historical section provides the necessary background for the analyses in Section 4, but contains no novel results in itself, aside from making a case for the self-evidence of the *p-e* model. It first summarizes the relevant experiments that led to the β anomaly as it was perceived in 1927 (based on Franklin, 2001; Jensen, 2000; Pais, 1986; and Malley, 2011), and completes this background picture with an overview of nuclear theory around 1927 and the various problems it faced (based on **Stuewer, 1983;** Brown, **2004**; Pais, 1986; **Hughes, 1993, 1998, 2003;** Jensen, 2000**; and Bernandez and Ripka, 2013**).

3.1 Experimental History of the β spectrum

The story of the β puzzle goes back to 1896, when Henri Becquerel discovered the phenomenon of radioactivity: some particular substances radiate spontaneously and independently of any interaction with the environment. The discovery of this curious form of radiation was made by mere luck; it revealed itself for the first time in the imprints left by uranium on some photographic plates that Becquerel had stored in a dark cupboard, deprived of all incoming sunlight.

From that moment forward, experimental discoveries unfolded at a steady pace. In 1899, Ernst Rutherford showed that the radiation emitted by uranium consisted of at least two different kinds of radiation, which he labeled α and β radiation. Even though α radiation was identified by Rutherford as helium ions only in 1907, it was already established in 1904 by William Bragg that it had a mono-energetic spectrum, i.e. α particles of a particular radioactive element are always emitted with the same characteristic amount of energy. For β radiation, it was already suggested shortly after Rutherford's discovery that it consisted of electrons (the elementary particles **then** recently discovered in cathode rays by J.J. Thomson). By 1902, this thesis was confirmed by experimental evidence provided by Becquerel and Walter Kaufman. Their experiments even hinted at a possible continuity of the β spectrum, though this idea was not accepted by the community at the time. According to Franklin (2001, p. 30), this was a justified call given that

possible, given the wide diversity of frequently incompatible methods employed during various historical periods and across scientific disciplines and given the clear failure of all accounts purporting to describe the (one and only, permanent) scientific method?" Also, this research contributes to the issues raised by computational philosophers of science, who understood that their various method should be integrated (Langley et al., 1987, ch. 9).

their experimental setup was too inaccurate to draw such a conclusion. The main reason why this idea was not taken seriously at the time, however, was that one expected β decay to be analogous to α decay, and so to produce a mono-energetic spectrum.

In 1909, William Wilson showed that β rays could not be a homogenous stream of mono-energetic electrons, given that, in matter, β particles had an exponential absorption curve, while homogenous electron streams (such as cathode rays) had a linear curve. Hence, the electrons found in β rays must have a range of energies, a variety that could not be explained by analogy to α decay.

Shortly after this, improved energy spectra for β radiation showed the occurrence of multiple lines, which suggested that there existed a discrete set of possible emission energies. As such, one suspected that β sources consisted of multiple unstable elements, still all decaying with a characteristic energy and, therefore, resulting in a single line in the spectrum. But, as line spectra grew more detailed due to the improving quality of spectral photography, more and more lines appeared, and it came to be understood that it was "impossible to assume a separate substance for each beta line" (Otto Hahn, as cited in Franklin, 2001, p. 43). β radiation appeared to be truly heterogeneous.

In 1914, while theoretical explanations for these line spectra were still lacking, James Chadwick and Hans Geiger tried to count the distribution of electrons in these lines **with an improved particle counter. To their great surprise, they found hardly any line. For the first time, they established the continuous spectrum of β radiation on a solid experimental basis. The earlier observed complex line spectra proved to be just a secondary effect of the process of spectral photography.**

Experimentalists were left perplexed. The idea that β decay was emitted with a continuous spectrum seemed impossible. Many, the most prominent among them Lise Meitner and Otto Hahn, but also Chadwick himself, put forward a long list of hypotheses to explain this surprising result, such as secondary radiation of the electrons, the production of recoil electrons, influence by γ rays, etc. What all these hypotheses had in common was that all supported the initially mono-energetic emission of β particles, and ascribed the continuity to subsequent secondary processes, somewhere between the radioactive source and the measurement of the spectrum.

This speculation came to an end in 1927, when Charles D. Ellis and W. A. Wooster **from the Cavendish laboratory in Cambridge (which had been led by Rutherford since 1919)** constructed a direct test to determine whether energy was lost between the β ray source and the location of measurement. By measuring the average heat increase per β particle emission, they found that the energy needed for this increase was the average and not the maximum of the β spectrum. This means that no energy was lost after the emission, and that, hence, the β particles were emitted from the source with a continuous spectrum.

This experiment did not immediately settle the discussion, however. Although starting to question her own secondary origin hypotheses, Lise Meitner came to doubt whether Ellis and Wooster had controlled for all these possible secondary effects in their experiments, as a result of which certain continental physicists, by contrast with their colleagues in England, did not have much confidence in the Cavendish results. Until 1929, Pauli, for example, thought that non-detected γ rays were the cause (Jensen, 2000, p. 137-143; Rueger, 1992, p. 315). Only after Meitner and Orthmann replicated, improved and confirmed the Ellis and Wooster experiments in the late spring of 1929 did a general consensus arise concerning the continuity of the β spectrum. In a famous letter to Ellis (in July 1929), Meitner admitted that:

> It seems to me now that there can be absolutely no doubt that you were completely correct in assuming that beta radiations are primarily inhomogeneous. But I do not understand this result at all. (Meitner, as cited in Franklin, 2001, p. 59).

Before examining the various proposals to explain this counterintuitive result (in Section 4), I will complete the background picture by sketching the contours of nuclear theory in 1927. More particularly, we must consider the prevailing nuclear model at the time and its difficulties.

3.2 Nuclear theory in 1927

In 1927, the prevailing model for the constitution of atomic nuclei was the so-called *p-e* model. In this model, the nucleus of an atom with mass number *A* and charge number *Z* consists of *A* protons (*p*) en *A-Z* electrons (*e*), kept together by the electromagnetic force. For example, according to the *p-e* model, the α particle, identified as the nucleus of ${}^{4}_{2}He$, consisted of four protons and two electrons. By comparison, in our current understanding, this particle consists of two protons and two neutrons, held together by the residual strong force.

While this *p-e* model became hugely problematic around 1927, it was the core assumption of virtually all nuclear models proposed since the famous Rutherford-Geiger-Marsden experiments led to the discovery of the nucleus in 1911.[7] The tenacity with which the problematic p-e model was

[7] For an overview of this exotic assembly of often quite speculative models, see Stuewer, 1983, pp. 22ff.; Hughes 1998, n. 17; Pais, pp. 230ff. Despite their wide variety, these models all had in common that they presupposed the existence of electrons in the nucleus, and

adhered to is related to the inevitability of its adoption. Before we consider the details of the various problems related to this model, something must be said as to why its adoption appeared so self-evident to so many at the time (Pais, 1986, p. 231) and why this model was so deeply entrenched in the minds of physicists of that era (Stuewer, 1983, p. 32); or, as Brown (2004, p. 309) has put it, why electrons in the nucleus were taken for granted until the discovery of the neutron in 1932. This reconstruction will help us understand the mind-set of the physicists discussed in Section 4.

A constitution model is expected to specify in a reductionist fashion the various elements and the relations among them. In the case of a model for the nucleus, a specification of the various elementary particles and forces must explain the observed properties: that these particles stick together inside the nucleus, that they allow for radioactive radiation[8], and that each element has a specific mass and charge number[9]. At the time, few elementary particles were known. Since J.J. Thomson's discovery, the negatively charged electron was best-known, and its charge was considered the elementary unit of electrical charge. Regarding positively charged particles, until Rutherford's discovery of the nucleus there was no need to presuppose the existence of "corpuscules", because in Thomson's old "plum pudding" model the positively charged matter was spread uniformly within the atom. It was the insight that most of the atom is void, except for a dense material nucleus that concentrates the positive charge, that led naturally to the idea that nuclei are a kind of positively charged (composite) particles. In particular, two types of nuclei were well-known: the nucleus of the lightest element, hydrogen, and of the second lightest, helium, which had been identified as the constituent of α radiation. Finally only two forces were known at the time, electromagnetism and gravity, though the effects of the latter are negligible on an atomic scale.

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except for a few exceptions (e.g. van den Broek's 1913 model, which took α particles and electrons as the fundamental constituents), most of these models conjectured, already well before the experimental liberation of H-nuclei (protons) from heavier nuclei (Rutherford, 1919), the existence of some kind of particle with positive elementary charge in the nucleus. These fundamental constituents were generally combined, however, into larger stable substructures such as α particles and other speculative entities (e.g. Rutherford's X-particle (1920, pp. 392ff.)). Although Rutherford complained of the excess in speculative models, he seems to have taken some part in it too (Hughes, 1999, p. 346).

[8] Around 1911, the peripheral electrons orbiting the nucleus were sufficient to explain most thermal, optical, elastic, magnetic, and chemical properties of atoms; the only exception to this idea appeared to be the phenomenon of radioactivity:

> Radioactive phenomena form a world apart, without any connection with the preceding phenomena. It seems therefore that radioactive phenomena originate from a deeper region of the atom. (Marie Curie, as cited in Pais, 1986, p. 223)

[9] These numbers were summarized in the table of Mendeleev, originally in a table in which the elements were ranked according to increasing atomic weight. In 1913, Van Den Broeck conjectured that the rank in the table actually matches the nuclear charge Z.

As both α and β particles were observed in radioactivity, it was natural to assume that both were present in the nucleus. Yet as the internal mass of all elements was always an integer multiple – the atomic weight number – of the mass of the H-nucleus, and not always of the He-nucleus (which is four times heavier), it made more sense to take the H-nucleus (which Rutherford called the "positive electron") as the fundamental nuclear constituent, a hypothesis first conjectured by Rutherford (1914) and later confirmed by his discovery of artificial disintegration and liberation of H-nuclei from nitrogen nuclei (1919). As electrons do not add up to the atomic weight number (their mass is only about $1/1000^{th}$ the mass of a proton), assuming the presence of that number of H-particles was the only way to account for the weight of a nucleus. Yet as the nuclear charge is in general about half the nuclear weight, the presence of electrons in the nucleus seemed the only logical explanation to compensate for this positive charge. After all, did one not observe their emission in radioactive β decay? Moreover, the electromagnetic attraction between negative and "positive" electrons explained the stability of nuclei.

Seen in retrospect, this model is the simplest possible given one important ontological commitment. One had to consider elementary particles as truly fundamental, i.e. indestructible and permanent, much in the way that the ancient Greeks had regarded atoms as the smallest building blocks of the universe: they are never created and never destroyed, nor can they transform into each other. This ontological assumption seemed natural at the time (see Brillouin's testimony, quoted in Navarro, 2004, p. 451). The idea that the electrons found in β decay were created in the process itself seems not to have crossed these physicists' minds. While already in 1924 Eddington had spoken of particle creation (in the context of cosmic ray research), and Dirac, in 1928, became the first to adopt it in mathematical quantum mechanics (Bromberg, 1976), only in 1933 did it come to be understood that elementary particles could be created from and transformed into radiation, that they were unstable and decayed into other particles, and that they were not only the building blocks of matter but also the vehicles for nuclear interaction (Navarro, 2004, p. 451-455).

In other words, the *p-e* model appeared self-evident: it was a simple, elegant and visual model that explained all the observed data (as the father of this model, Rutherford, liked them to be (Hughes, 1999, p.343)); no extra-existential assumptions were needed about unobserved particles; and the ontological commitment on which it was based was fully in line with the conception of elementary particles prevalent at the time. Any other explanation would have had to introduce radically new categories of particles or forces or drastically change existing concepts, which would require extensive theoretical elaboration or, at least, some experimental evidence to be considered a challenge to the elegant and straightforward *p-e* model.

This apparent self-evidence of the *p*-e model explains scientists' **relatively** long adherence to this model, even as new discoveries gradually changed the theoretical framework, such as the first glimpses of the strong nuclear force in 1921 or the concept of wave-particle duality. Only by 1932, when the neutron was discovered, one finally **started to** understand that the *p-e* model, by that time hugely problematic, was obsolete. **Yet still, although Heisenberg was able to complete a new nuclear model constituted of neutrons and protons within just four months (Bromberg, 1971), it took several years for the neutron to be truly accepted as an elementary particle and not merely as a close proton-electron combination (Stuewer 1983, p. 46-56; Navarro, 2004, p. 442-443; Fernandez, 2013, p. 263-270).**

3.3 Problems for nuclear theory around 1927

Around 1927, the *p-e* model started to face various difficulties. Apart from the discovery of a continuous β spectrum, at least three other important difficulties were pointed out. These difficulties differed slightly from the β puzzle, as they consisted of problems specific to the *p-e* model, whereas, in the case of the β puzzle, it was not clear where the problem was situated.

Two problems were pointed out by Ralph Kronig, an American physicist who suggested the electron spin before George Uhlenbeck and Samuel Goldstein (Stuewer, 1983, pp. 34-35). In 1926, he showed that, unless the spin of the various nuclear electrons exactly cancelled each other out, the magnetic moment of the nucleus would be much larger than the observed effects in spectral photography. Nuclear electrons should produce splitting levels of the same size as peripheral electrons (the so-called fine structure), whereas experimentally the magnetic moment of the nucleus produces effects at a smaller scale (the so-called hyperfine structure).

While some, like Owen Richardson, tried to explain this anomaly by assuming that nuclear electrons radiate part of their spin, Kronig, in 1928, found another, even harder anomaly. Observing the spectra of nitrogen nuclei, he discovered that these nuclei obeyed Bose-Einstein statistics, an indication that they have an integer spin. But both electrons and protons were known to have a spin of ½. Therefore, nitrogen nuclei, which according to the *p-e* model consist of 14 protons and 7 electrons, should have in total a half-integer spin and, therefore, obey Fermi-Dirac statistics – a contradiction. Kronig concluded that "in the nucleus protons and electrons do not maintain their identity in the same way as in the case when they are outside the nucleus" (as cited in Stuewer, 1983, p. 35).

The final problem that troubled the nuclear electron hypothesis was the so-called Klein paradox. This paradox, formulated by one of Niels Bohr's close collaborators at the end of 1928, was intended to attack the Dirac equation and its negative energy solutions. According to the Dirac equation, electrons confined to a region the size of the nucleus would have a high enough probability of escaping through the potential barrier (with negative energy) that they could not be confined to the nucleus at all (Stuewer, 1983, p. 39). It is significant that this paradox was at the time considered a paradox for the Dirac equation, while according to our present understanding it is a problem for the *p-e* model, more particularly, for the presence of electrons in the nucleus.

4. Six different hypotheses to solve the β puzzle

In this section, we consider six hypotheses meant to account for the continuous emission spectrum of β decay, as experimentally demonstrated by Ellis and Wooster (1927) and verified by Meitner (1929). Each suggestion was in its own right an original idea that could provide the explanatory link to the β anomaly; all sketch in a more or less programmatic way how the initial idea might lead to a full explanation, as well as how the intended explanation should be understood in relation to the theoretical framework the researchers had in mind. Although three of the six hypotheses seem very similar, i.e. seem all to restrict the energy conservation principle, I still consider them as distinct hypotheses because they are formed differently, and so connect their basic idea differently to the theoretical framework.

4.1 Ellis and Wooster: Non-Universality of the Quantum Postulate:

At the end of their seminal paper, in which they experimentally demonstrated the continuous β spectrum, Ellis and Wooster offered "a simple hypothesis by which these facts can be reconciled" (1927, p. 122-123). But, while their experimental results are today part of the canon of nuclear physics, these last pages have gained little, if any, traction in the physics community, mainly because – even if the Cavendish laboratory in Cambridge was not noted in the 1920s for its openness to developments in mathematical physics (Hughes, 1998) – it shows an almost surprising misunderstanding of the basic quantum postulate as a consequence of undisturbed classical particle motions. Although this idea did not leave any mark on the further course of events, it has some interest for our specific purposes.

Ellis and Wooster's hypothesis is based on Rutherford's satellite model of the nucleus. This was Rutherford's version of the *p-e* model, which, by 1927, he had developed from an early explanation of his discovery of artificial disintegration (1919, p. 589-590) into a highly sophisticated and

structured visual model that enabled him to explain both the artificial disintegration of light elements and the radioactivity of heavy elements (Stuewer, 1986).

In the final version of this semi-classical model (Rutherford, 1926, p. 370-371), which is the version Ellis and Wooster refer to (1927, p. 122), the nucleus is said to be composed of three distinct regions. Surrounding a positively charged inner nucleus, one could first distinguish, at a distance, a number of electrons, and then, at a further distance, a number of neutral satellites circulating the system. These neutral satellites were α particles (He-nuclei) that had gained two electrons in a close bond, kept in stable orbits by polarization or magnetic forces.

Based on this model, Ellis and Wooster put forward the following hypothesis:

> There is no reason why the outer satellite region should not be quantised, and so give the possibility of ejection of alpha particles of definite energy, but yet the electronic region unquantised in the sense that the electrons have energies varying continuously over a wide range. (Ellis and Wooster, 1927, p. 122)

The pattern according to which this hypothesis is formed is very straightforward. On the one hand, it is observed that there is a qualitative difference between the discrete α spectrum and the continuous β spectrum; on the other hand, the employed nuclear model is cited to establish that the particles emitted in α and β decay first reside in different regions of the nucleus. Hence, via a simple instance of abductive reasoning, it becomes reasonable to suggest that the same qualitative difference applies to these two regions. As Ellis and Wooster (1927, p. 123) took the essence of quantum theory to be the quantized orbits model (nowadays generally referred to as the Bohr-Sommerfeld atomic model or the old quantum theory), they regarded the discrete emission spectrum of α decay as an indication that the neutral satellites (containing the α particles) orbit the inner nucleus in quantized orbits (analogous to the electrons in the old Bohr-Sommerfeld model). Based on these assumptions, Ellis and Wooster were able, quite straightforwardly, to conceive the inner orbit containing the β particles as not quantized or continuous.

It was not that Ellis and Wooster were unaware that the quantum postulate, i.e. that there is a quantum of action, was to be universally applied on the atomic and subatomic level. They simply did not regard this universality as a necessity, but rather as a contingent fact, though one which had to that point been consistently confirmed by experiment. This can be seen in the following passage, where Ellis and Wooster anticipate some suspicious frowns as they continue their discussion:

> It is interesting to enquire whether this picture of the free electrons in the nucleus existing in unquantised states is contrary to modern views. At first sight it would certainly appear to be so, but this is not necessarily the case. (Ellis and Wooster, 1927, p. 122-123)

They explained this by stating that, for a particle to occupy a quantized orbit, it must be able to "describe many complete orbits without disturbance" (an analogy with the classical quantization of, for example, standing waves might have played a part here). While this condition might be fulfilled for the outer shell of neutral satellites, one can "scarcely expect undisturbed electronic orbits" so close to the positively charged nucleus.

In short, Ellis and Wooster did not regard the quantum postulate as a genuine postulate; for them, it was an emergent phenomenon that arises from particles describing stable orbits, which could be described by classical mechanics and electromagnetism (although they gave no account of how this emergence might happen). Given this perspective, though they did not question the applicability of this postulate to existing atomic theory or to the outer nuclear layer, they did not feel the need to retain its apparent universality, which allowed them a straightforward solution for the β spectrum within the contours of Rutherford's nuclear satellite model.

As might be suspected, this idea had a very brief history. By 1927, quantum mechanics and the dominant Copenhagen interpretation had been developed and quickly started to spread (Kojevnikov, 2011; Heilbron, 1985). Gamov (1928) applied these new ideas to the nucleus and α decay, and succeeded in providing a quantitative explanation of the Geiger-Nutall relation between the decay constant and the energy of the emitted particle, something Rutherford's semi-classical qualitative satellite model was totally unable to do (Stuewer, 1985; see Section 4.4). Yet as Gamov's calculations confirmed Rutherford and Chadwick's experimental results in the Cambridge-Vienna controversy, Rutherford realized he had to accept Gamov's model over his own, even if this realization took him some time (Hughes, 1999; Stuewer, 1986, p. 349-352). Also, Ellis appears to have shifted only slowly, as, in 1929, he was still defending his early view in a letter to Meitner (Jensen 2000, p. 134).

4.2 Rutherford and Chadwick: Identical Nuclei with Varying Internal Energies

After Ellis and Wooster's paper, experimentalists generally shied away from advancing much speculation concerning what might explain the β spectrum. An exception can be found, however, in some remarks made by Ernst Rutherford and James Chadwick (1929) in an article on artificial **disintegration**, published in early 1929, in which they suggested that it might be possible that not every nucleus of a given element has the same internal energy.

Rutherford and Chadwick's article was certainly not an attempt to solve the β puzzle. Their goal was simply to report some unexpected results **from their** artificial **disintegration experiments**. They had discovered that, after inducing the artificial emission of protons from aluminum nuclei by shooting them with α particles, the energy of these emitted protons varied widely and continuously,[10] a surprising result that could not be ascribed to inaccuracies in the measurements. After verifying that this result was not caused by hitherto unobserved particles,[11] they declared that:

> The process of disintegration of an aluminium nucleus by an α particle of given energy is not exactly the same for each individual nucleus. […] The variation in energy change must be due to variations in the internal energy either of the initial aluminium nucleus or of the final nucleus. (Rutherford and Chadwick, 1929, p. 190)

After expressing the need for further evidence and experiments, they repeated this hypothesis in their conclusion, adding that:

> This suggestion, […], is supported by evidence from the natural disintegration of the radioactive elements. The disintegration electrons from β ray bodies are emitted with energies varying over a relatively wide range and in some cases at least, e.g. radium E, the energy balance is not restored by the emission of an appropriate amount of γ-radiation. (Rutherford and Chadwick, 1929, p. 192)

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[10] Variation in the range of emitted protons had been observed earlier in artificial proton emission from nitrogen nuclei. But in their 1929 article, Rutherford and Chadwick showed that this earlier variation can be ascribed to the variation in momentum of the incident α particles. The variation for aluminum nuclei, however, is considerably larger than the variation for nitrogen nuclei.

[11] Before coming to their conclusion Rutherford and Chadwick verified that no other particles such as "neutrons" were present. **As Rutherford and Chadwick were trying to unravel the nucleus, they were prepared to find some hypothesized composite substructures, such as "neutrons", which Rutherford (1920, pp. 396-397) had conjectured to be close proton-electron combinations (see Section 4.6).**

In other words, by ascribing their results to the same cause as the similar continuous β spectrum, they suggested that the β puzzle might be explained by assuming that the internal mass or energy of an element can vary.

This idea is very radical: it infringes the principle of identity for chemical elements, which states that two atoms of the same element have identical properties. In their conceptual framework, however, the idea can be formed in a very straightforward way. Given the most basic assumptions about the process of disintegration – it is a nuclear process resulting in the emission of an observable particle – the observed continuity of emission energies can only have originated in a limited range of places: (1) either it was already present at the start of the process; (2) or it was created at the moment of disintegration; (3) or it entered somewhere between the disintegration and the place of measurement. Before I explain in more detail why Rutherford and Chadwick preferred the first option, let us take a closer look at how this disjunction is formed.

At first sight, the disjunction seems to be the result of an elementary abductive reasoning step, which can be modeled by existing logics for abduction.[12] But the disjunction does not mention just a couple of possibilities: anyone considering this disjunction (see e.g. also G.P. Thomson (1929, p. 405) or Pauli (Brown 1978, pp. 22-24)) was convinced that it covered all (initial) possibilities. It was, in other words, an exhaustive disjunction. This relates to how their knowledge of disintegration processes is structured: not as a set of propositions (the building blocks for logics), but as a coherent spatiotemporal model of the process, which synthesizes their knowledge.[13] The basic assumptions mentioned above constitute the outline of such a model, which can be represented visually by drawing the experimental setup or by a more abstract sketch. In this type of process model, one can derive a purportedly exhaustive disjunction of possible origins for a property observed at the end of the process by covering the possibility of each spatiotemporal region in the model.

This is a pattern of abduction which draws on our intuitions about causal processes. In Salmon's terminology (1984), a characteristic observed at the end of a causal process must either have been uniformly present throughout the process, or else introduced into the process as a mark by means of a single local interaction

[12] See, for instance, Gauderis 2012 for an example of such a logic, in the framework of adaptive logics. These logics have the advantage that they incorporate a dynamic proof theory, which allows step-by-step models of defeasible reasoning processes such as hypothesis formation.
[13] I understand models as they are commonly understood in the philosophical literature on the use of models in science, as "a representation of a system with interactive parts and with representations of those interactions" (Nersessian, 2008, p. 12) . These imaginary functional or structural analogues of the target phenomena allow for determining future states by mentally simulating the model by means of the interactive parts. In the case of the model for disintegration, the visual picture is the representation of the system, the various variables (which can be adjusted) the interactive parts, and the mathematical formulae (that specify the relations among the variables) the representations of the interactions.

at a certain space-time point, and the characteristic must have remained present at all subsequent stages until the space-time point of the observation. Exactly because the disintegration process is considered to be a causal one, these physicists assumed the exhaustiveness of the disjunction.

Of the three possible options**,** it is the third, i.e. that the continuous variety in energies appears in the model after the particle has left the nucleus, that the experimentalists had been investigating fifteen years before (see Section 3.1), a quest that ended with the Ellis and Wooster's caloric experiment and the consensus (among Ellis, Wooster, Meitner and Geiger) that the electron leaves the nucleus with a continuous spectrum.

This left two options open: either the variety in energy is present from the start, or it is introduced into the process at the exact space-time point of the disintegration. The first option is interchangeable with the thesis that nuclei of the same element can vary in internal energy. The second option is (in this model) equivalent to the assumption that energy is not conserved in a single disintegration (otherwise nuclei with fixed internal energies before and after cannot emit electrons with varying energies).[14] Hence, physicists were left between the Scylla and Charybdis of giving up either the principle of identity for chemical elements, or the principle of energy conservation – both of them a cornerstone of the physicist's worldview. This dilemma also explains the arduous focus of Meitner and others in earlier years to find a hypothesis that would fit the third option, and their perplexity when the caloric experiments of Ellis and Wooster excluded this possibility.

This leaves us with the question of why Rutherford and Chadwick preferred to give up the principle of atomic identity, a route taken by no other protagonist in this history. In my opinion, the answer should be sought in the fact that Rutherford and Chadwick were in the first place experimentalists.[15] Experimentalists tend to have, as Franklin (1999, p. 97) has put it, an instrumental loyalty. While their ideal might be to look for "the best physics experiment in their field that can be done", and consequently build the appropriate apparatus, in reality they tend to look for the best experiment that can be done with their existing equipment (or with a minor modification). In that way, they recycle their expertise time after time, and become more and more experienced in employing the existing apparatus and its underlying models.

[14] Of course, this equivalence pertains only to this particular model of disintegration, which assumes that the nucleus emits a single particle. This was the model that most physicists had in mind at the time.

[15] See Hughes (1993,1998) for a thorough discussion of the relation between the experimentalists at the Cavendish laboratory and theoretical physics.

In the case of these Cavendish researchers, nuclear reactions were typically elicited by smashing small particles (mostly α particles) into nuclei, and then an effort was made to determine the properties of the remnants by observing them in electromagnetic fields – experiments and calculations that crucially depend on the conservation theorems. By performing this type of experiment over and over again, the theoretical models on which these experiments were based were ever more deeply ingrained in their minds. As Franklin (1999, p. 149) has claimed that there are probably no antirealists in the laboratory[16], it is perhaps unavoidable that experimentalists form a deep belief in the veracity of their underlying models, i.e. that these models, which they manipulate mentally each time they perform physical experiments, have a true functional or structural analogy to reality. The only time Rutherford and Chadwick mention the conservation theorem in their paper of 1929 is when they explain the model of artificial disintegration on which their calculations and experiments are based. Also in their book on radioactivity (Rutherford, Chadwick and Ellis, 1930), though published at a time when they must already have heard of Bohr's proposals to limit energy conservation (Jensen, 2000, p. 160), they hardly mention the conservation theorem, except when they use it to explain their models. Clearly, due to their experimental bias, they were unable even to question the validity of the conservation theorem, for it was an inherent part of the underlying models for the experiments they performed every day.

On the other hand, the internal or rest mass of an element can be measured. From an experimental point of view, it is perfectly conceivable that what was thought to be identical turns out to allow for small variations. **In fact, it was at the Cavendish laboratory that precisely a decade earlier Aston had discovered, with his newly devised mass-spectrograph, the isotopes suggested by Soddy: identical chemical elements with varying masses (Hughes 2009, Fernandez & Ripka, p. 166-171, Soddy, 1921, p. 369). Due to this long history of research on isotopes at the Cavendish, it must have appeared quite reasonable to Rutherford and Chadwick to expect that further variations at the level of individual nuclei might be measured. As a result,** they supported the thesis of non-identity until 1932 despite the lack of any experimental proof, such as a varying lifetime for radioactive elements (Jensen, 2000, p. 161).[17]

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[16] Franklin's claim is a strong version of the *entity realism* proposed by Hacking (1983), which takes manipulability of an entity as the criterion for belief in its existence. Franklin claims that experiments can also give us reasons to believe in the truth of some laws between these entities. In this article, I only use the descriptive part of his claim, i.e. that experimentalists tend to form such beliefs.

[17] They did try, however, to minimize this radicalism by situating the variation in the binding energy between the disintegration electrons and the nucleus, thus leaving the stable part of the nucleus identical (Rutherford, Chadwick and Ellis, 1930, p. 410). Opposition to their proposals

4.3 G.P. Thomson: Energy Non-Conservation as a consequence of Quantum Mechanics

As mentioned in Section 3.1, theoretical physicists, certainly on the continent, did not immediately appreciate the seriousness of the problem (Jensen, 2000, p. 137-143). According to Pais (1986, p. 309), only one reference to the Ellis and Wooster paper can be found in all the literature of 1928: a short note from George P. Thomson in *Nature.* In this first article (1928) and the more substantial article he published a year later (1929), Thomson explained the β spectrum in terms of the non-conservation of energy for the emitted electrons.

The single most interesting feature of this account, which will turn out to be incoherent, is that Thomson described the non-conservation of energy in β decay not as an anomaly but as a result that was "to be expected on the new wave mechanics" (1928, p. 615).

Shortly before the fifth Solvay conference in 1927, a consensus had emerged concerning the mathematical equivalence of the formalisms of Born-Heisenberg and Schrödinger,[18] offering the field a new and versatile set of formal tools that could be applied to many open problems in theoretical physics and leading, in subsequent years, to a substantial list of successful explanations. Thomson regarded the experimental β anomaly as just one of the many puzzles to be solved by means of this new formalism.[19] Given the

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was, given the lack of experimental evidence, very fierce; consider the following quote from Bohr's Faraday Lecture*:*

> Although the corresponding variations in mass would be far too small to be detected by the present experimental methods, such definite energy differences between the individual atoms would be very difficult to reconcile with other atomic properties. (Bohr, 1932, p. 382)

[18] **See for instance the discussion between Heisenberg and Schrödinger after the latter's talk at the 1927 Solvay conference (Bacciagaluppi and Valentini, 2009, pp. 471-472; for a detailed history of the reconciliation of the two formalisms, see Longair, 2013, ch. 15; for some recent discussion about the actual equivalence of the original formalisms, see Muller, 1997; Perovic, 2007). Of course, the agreement on the formalisms' equivalence was only a footnote to the real discussion at the Solvay conference concerning the interpretation of quantum formalism (for an introduction, see Heilbron, 1985; Bacciagaluppi and Valentini, 2009; Mehra, 1975). For this reason and due to the heterogeneity of the group of physicists involved, it would be premature to label this episode as the installment of a new Kuhnian (1962) paradigm (see also Beller, 1999, ch. 14; Bokulich, 2006, for an interesting analysis of Kuhn's notion of a "paradigm" as reminiscent of Heisenberg's notion of a "closed theory", a concept Heisenberg used to mount his rather dogmatic defense of the Copenhagen interpretation of quantum mechanics). Apart from this discussion (see e.g. Massimi 2005 for a reading of this period that is more sympathetic towards Kuhn's ideas), I do think that Kuhn's description of "normal science" fits the period from 1927 onward, as it should not be forgotten that, while senior professors continued their interpretational debates, most contributions to the field were made by a large group of younger researchers that employed the new mathematical formalism to address a wide variety of problems in the field (Kojevnikov, 2011, pp. 346-348).**

[19] **G.P. Thomson, following C.G. Darwin, was attracted by Schrödinger's wave formalism mostly because it allowed, as both Thomson and Darwin believed, for more "mechanical"**

broad consensus and the quick succession of solved puzzles, this expectation is understandable. It might even explain, to a certain extent, Pais' observation that the severity of the β puzzle was not directly appreciated**: in these first years of quantum mechanics, the number of puzzles that could be addressed was still large indeed and the frontiers of formalism's application were still vague. As such, it was not immediately clear which puzzles would turn out to be a challenge for the new theory.** In the case of the β spectrum, this took at least a year.

Thomson's account is clearly in contradiction with the orthodox or Copenhagen interpretation of the quantum formalism,[20] as he did not accept two of its central theses: the completeness of the wave function and the complementarity principle (which, as originally formulated by Bohr (1928),[21] states that the principles of conservation of energy and momentum are complementary to the space-time description of elementary particles). Thomson claimed that "the conception of a particle in motion is almost meaningless unless it can be supposed to have a definite velocity at a definite time" (1929, p. 413), meaning that he disbelieves that the wave function – inherently probabilistic in nature – provides a complete description of the electron. In fact, he adhered to the pilot-wave interpretation of quantum mechanics: wave functions exist physically as accompanying or guiding pilot-waves of particles, while the particles themselves have a definite but "hidden" trajectory. The standard formulation of this interpretation, also known as the de Broglie-Bohm interpretation, manages to be empirically equivalent with the Copenhagen interpretation by restricting the epistemic access to this definite trajectory to what is known in a statistical way via the wave formalism – hence the hidden character of this trajectory. But Thomson was misled by this idea of definiteness, and tried to gain knowledge about the electrons' trajectories by other means: by stating that "the equality of the particles emitted and atoms transformed is exact and not statistical" (1929, p. 406), he took the principle of identity (in which he firmly believed, unlike Rutherford and Chadwick) to require that the properties of the hidden trajectories must be exactly the same for all emitted electrons. This is in clear contradiction with the (empirically adequate) orthodox interpretation, which take this principle only to require that identical systems can be described by the same wave function; measurements of identical

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explanations (Navarro, 2010). In the present case, this preference for mechanical explanations would lead to his faulty assumptions.

[20] Recent scholarship has shown that the Copenhagen interpretation is a far less coherent (Beller, 1996) and unified (Howard, 2004) view than has been traditionally thought, as Bohr's and Heisenberg's views on complementarity diverged quite seriously. This does not need to concern us here as, Thomson rejected any notion of complementarity.

[21] As Camilleri (2007) demonstrates, Bohr originally conceived this concept as the complementarity between the description of the stationary unmeasured state (for which conservation of energy and momentum applied) and the description of this state in terms of position measurements (a space-time description). It was only in the wake of his dispute with Einstein that he extended this view around 1935 to our current understanding of the complementarity principle in terms of mutually exclusive experimental arrangements.

systems are still uncertain and distributed according to the probabilities specified by the wave function. Thomson, however, fallaciously inferred that "the initial velocity is the same in all cases." (1929, p. 415) – apparently unaware that the attribution of a definite speed would prevent any knowledge about the position of these particles.[22] At the same time, he employed the Gaussian model for a free particle (a well-known exemplar of the quantum formalism that describes position probabilities in terms of moving Gaussian curves, also called wave packets) to describe the evolution of the electrons' wave function in time.[23] As the variance of moving Gaussians grows over time, this model predicts that the uncertainty for position measurements will rise proportionally. At this point, Thomson had no other option than to accept that the electrons (which are spread according to this Gaussian distribution inside the wave packet, on his pilot-wave view) can speed up or down from their initial velocity to move to the front or back of the wave train. Hence, the energy of a single emitted electron is not conserved in free space.[24]

Despite his confused assumptions, it is for our purposes still interesting to investigate the pattern of discovery by which Thomson arrived at these ideas. The initial step in his reasoning is rather easy to retract: he found that by taking a Gaussian with appropriate parameters, one could get a "fair fit to Ellis and Wooster's result" (1928, p. 616). The visual resemblance between the somewhat skewed experimental curve of the β spectrum (Figure 1) and the mathematical shape of a Gaussian, which figures prominently in the quantum model for a free particle,[25] caused him to assume that the emitted electrons in β decay behaved as free particles with the same wave function. **Once he had formed this initial hypothesis via visual identification, he could then apply this model to the data and calculate the properties of this wave function: it should be in a large superposition of momenta, and have a rather small initial wave length that increased with time.** Combined with his faulty assumption of the exact and equal initial velocity, this led him to his thesis of "straggling" electrons. The

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[22] Heisenberg's uncertainty relations state that the uncertainty in position is inversely proportional to the uncertainty in momentum. Physically realistic quantum models allow, therefore, for both uncertainty in momentum and position, treating a system as in a superposition of both momenta and positions.
[23] Thomson appears to make a peculiar categorical difference between velocity, a property of particles which has a definite nature, and momentum, a property related to the wave formalism which can be superposed. As such, he takes the emitted electrons to have initially a definite velocity, while at the same time allowing them to be in a superposition of momenta (as prescribed by the Gaussian model of a free particle).
[24] The assumption that the electrons had at first a definite and equal velocity is the real problem in Thomson's reasoning. Pais' remark that "his conclusions resulted from inappropriate manipulations with phase velocities and group velocities" (1986, p. 312) refers only to the consequences of this initial confusion. Bromberg traces Thomson's mistake to the fact that he employs a pilot-wave model (1971, p. 311), but this assumption would not be problematic if he adhered to, for instance, the de Broglie-Bohm interpretation.
[25] Thomson refers to a presentation of this model by Darwin in 1927, but the Gaussian model for a free particle is still a textbook exemplar of quantum mechanics.

calculated small initial wave length – he compared it to the sound pulse produced by a firing gun – allowed him to explain why non-conservation was observed only for β decay, because it was much smaller than other observed wave lengths in those days.

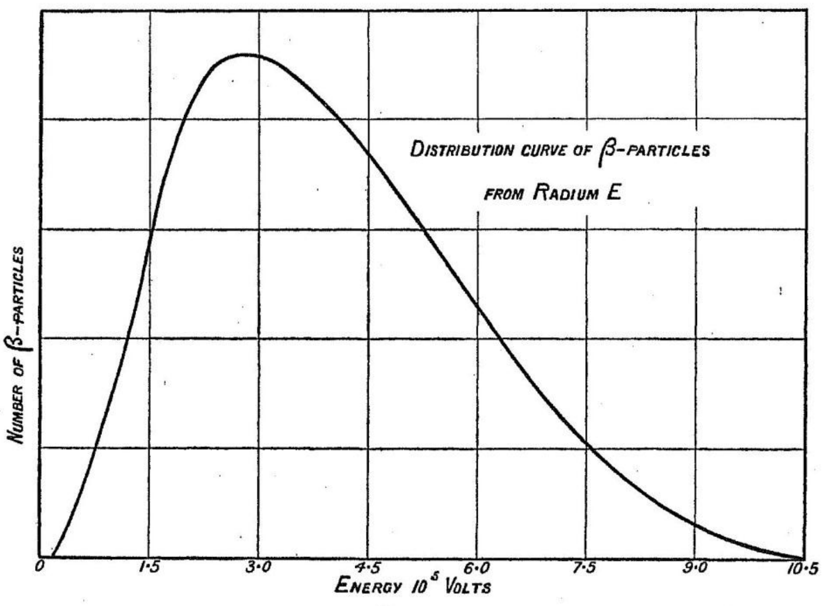

Figure 1: The experimental β-spectrum visually resembles a Gaussian distribution. (reproduced from Ellis and Wooster, 1927, p. 111, permission granted by Royal Society Publishing)

Thomson's account is a clear example of how the counterintuitive aspects of quantum mechanics misled even renowned physicists in the early years.[26] **Willing to embrace the new formalism and maybe blinded by its attractive fruitfulness, Thomson thought it also contained the key to the β puzzle. In order to solve this puzzle, he actually used a very straightforward pattern of discovery: the visual recognition of a well-known mathematical model in the experimental data. Despite the soundness of his reasoning, he unfortunately relied heavily on an assumption based on classical intuitions, which made his contribution inconsistent.**[27] However, the idea that energy is not conserved in β decay would prove persistent.

4.4 Bohr: Non-Conservation of Energy as part of a theory for Elementary Particle Constitution

[26] G.P. Thomson received a shared Nobel prize for physics in 1937 precisely for his work on the wave character of the electron.

[27] **According to Navarro (2008, 2010), part of the trouble for his transition to quantum mechanics can be related to the old continuous aetherial worldview of his father J. J. Thomson and his classical Cambridge training, influences he struggled considerably to get rid of.** It took him until 1930 to come to "understand that the new physics was totally alien to the old notion of explanation by way of mechanical models." (Navarro, 2008, p. 250)

The essence of Niels Bohr's stance on the matter is generally reduced to his embracement of the idea of energy non-conservation (e.g. Franklin, 2001, p. 68; Pais, 1986, p. 309). Yet scholarly work by Bromberg (1971, p. 309) and Jensen (2000, chapter 6) shows that we cannot evaluate Bohr's suggestion independently of the much broader scope he had in mind. By 1929, Bohr (and Heisenberg) became convinced that nuclear and atomic systems differ profoundly, and that a new theory must be constructed to address the various problems at the nuclear scale – a theory without energy conservation. By the time Bohr felt assured enough to first publish his ideas, however (Bohr, 1932), the β puzzle and the other nuclear problems had already polarized the field between his supporters and opponents. As the criticisms of the latter were aimed particularly at his views about energy non-conservation, Bohr seemed to stress this thesis in a more autonomous way beginning around 1932.

To understand why Bohr was on the outlook for a new physical theory, we have to trace his views to his first attempts to solve the β puzzle. These can be found in an unpublished, programmatic note written in June 1929,[28] which is particularly interesting for our purposes because, taken together with some short remarks in his correspondence, it displays the formation of his ideas. He starts this note with a reference to Thomson, whom he credits with bringing the idea of a limitation of the energy principle into connection with the β puzzle. But, in his well-known gentle way,[29] he informs us that Thomson's view is wrong and resulted from a misunderstanding of the complementarity principle. Bohr considered this principle to be a crucial insight of quantum theory, a view he saw confirmed by the successful explanation of α decay by Ronald Gurney and Edward Condon (1928) and, independently, by George Gamov (1928) (Stuewer, 1985).[30]

The reason why Bohr deemed the α decay explanation so "striking"– it was the first explication of a quantum tunneling phenomenon – is that it used the quantum formalism and the complementarity principle to explain a curious aspect of radioactivity, i.e. that the disintegration time of a nucleus is independent of its previous history, and subject only to a fixed chance. On a classical account, this is

[28] This note is included in Vol. 9 of the Bohr Scientific papers (Bohr, 1929/1986). In the introduction to this volume, Peierls (1986) states that this note must be "in substance" the same that he sent to Pauli on 1 July, accompanied by the words: "The other is a little piece about the beta-ray spectra, which I have had in mind for a long time, and which has been typed in the last few days, but I have not yet made up my mind to send it off, since it yields so few positive results and has been written so sketchily."
[29] Bohr's contemporaries did not always understand his well-known hesitation to use confrontational language; consider the start of Pauli's answer to this note: "It already starts so depressingly with a reference to the nonsensical remarks by G.P. Thomson, and from this the people in England will only draw the erroneous conclusion that you regard these remarks as important." (as cited in Peierls, 1986, p. 5)
[30] Gamov stayed at the Bohr Institute in Copenhagen during the academic year 1928-1929, where he unsuccessfully tried to apply the same style of reasoning to the β spectrum; this failure is one of the factors that led Bohr to the β puzzle.

inexplicable: either the energy necessary to overcome the nuclear binding energy remains constant (in which case there would be either instant disintegration or no disintegration at all) or else it changes in time (in which case disintegration would be dependent on the previous history of the nucleus). But quantum mechanics allows for the description of another type of behavior. To obtain this, Gurney, Condon and Gamov modeled the nucleus by a constant potential well (Figure 2; the well is formed by the peaks G and C), in which an α particle is, classically, captured (in an orbit between F and D), i.e. its energy is lower than the potential energy peaks (i.e. the needed energy to overcome the nuclear binding energy). Classically, this particle cannot escape without external energy, but in quantum mechanics the (measurement of) the position of a particle is subject to a certain uncertainty. This means that there is a non-zero probability that the particle, confined between F and D, is actually located past the potential energy barrier (e.g. between B and M) while maintaining the same energy level. In other words, the particle has "tunneled through" the potential peaks raised by the nuclear binding energy. As Bohr noted, this is a "particularly instructive example" (1929/1986, p. 87) of the complementarity between the principle of energy conservation and the space-time description in terms of position measurements. In fact, it will be exactly this satisfying explanation that led Bohr to think that β decay could not be explained by quantum mechanics, but that a new theory was needed.

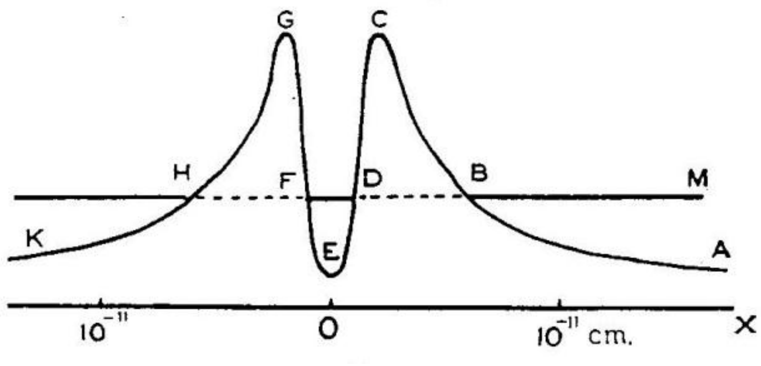

Figure 2: Electrons captured between FD can (quantum mechanically) "tunnel through" the nuclear potential barriers HGG and DCB remaining at the same level of energy. (reproduced from Gurney and Condon 1928, p. 439, permission granted by Nature Publishing Group)

This α decay explanation also changed the predominant conception of radioactivity. As we have seen in the earlier work of Thomson (1929, p. 412) and Ellis and Wooster (1927, p. 123), radioactivity was often considered to be a violent explosion in which a particle is hurled away. But Gurney and Condon say that it is better to change this in light of the present explanation and speak rather of particles that are "slipping away" (1928, p. 439), while Gamov speaks of "leaking" particles (1928, p. 805). **This new metaphor for radioactivity led to**

drastic changes in the predominant conception: while radioactivity had long been regarded as an abrupt change in the history of the involved particles, including possible deformations or alterations, the new image of leaking drops suggests that it is a purely mechanical process and so must be described as such.

These observations allow us to understand why Bohr put forward the idea of a new theory. If we follow the new metaphor and take radioactivity to be a purely mechanical process, β decay should be modeled analogously to α decay, because the mechanical constraints are similar: a decay ratio which is fixed, a single observed and identified particle, and a change in nuclear constitution that conserved total atomic mass and charge. But as Gamov (1928) showed, the quantum tunneling model, which provided a successful explanation of α decay, also allows for a calculation of the rate of radioactive decay as a function of the velocity of the emitted particles (the so-called Geiger-Nutall relation). In other words, the decay rate and the energy of the emitted particles are directly related. If this is so, the continuous β spectrum would also force the rate of β decay to vary continuously amongst the different nuclei, which would contradict the observed fixed decay rate. Hence, Bohr concluded that:

> The existence of a well-defined rate of decay of β ray disintegrations would exclude any simple explanation of the continuous β ray spectra based on the ordinary ideas of wave mechanics. (Bohr, 1929/1986, p. 87)

Bohr regarded the quantum mechanical framework as inapt to connect the β decay observations. Another theory was needed, and in fact a conceptual niche presented itself quite directly: if β particles do not "leak" from the nucleus, then they cannot be present inside the nucleus beforehand (otherwise quantum mechanics and the quantum tunneling model would apply); and if they are not present in the nucleus beforehand, this means that the β particles are created in the process of decay itself – a type of behavior for which no theory was yet formulated. But before Bohr could substantiate this new theory for the constitution of elementary particles, he realized that his conclusions – apparently logical consequences drawn from quantum mechanics and the observational data – presented a severe challenge to the *p-e* model, for which the presence of electrons in the nucleus is a basic assumption. First, then, he had to address this theoretical conflict.

It is well-known that Bohr approached this kind of theoretical conflict by scrutinizing the concepts involved and specifying how they apply to the experimental observations.[31] It is this style of reasoning that Heisenberg hints at when he describes that Bohr's insight was not so much

> a result of a mathematical analysis of the basic assumptions, but rather of an intense occupation with the actual phenomena, such that it was possible for him to sense the relationship intuitively rather than derive them [sic] formally. (Heisenberg, 1967, p. 95)

More particularly, we can describe Bohr's pattern of reasoning as follows: by analyzing the meaning, preconditions and implications of the concepts involved, he could identify which minimal conceptual assumptions were needed to describe the experimental data. As such, he could, by restricting the meaning of these concepts, carve out the necessary conceptual space to resolve the contradicting elements – if his attempt was successful. Of course, this pattern was not a straightforward algorithm that Bohr could easily execute: explicating the many, often merely intuitive aspects of and assumptions related to the meaning of a concept was a painstaking process, in which Bohr succeeded only through numerous discussions with his collaborators, friends and visitors to his center. The most important result he arrived at in this process is his famous complementarity principle **(Camilleri, 2007, p. 520-524; Shomar, 2008, p. 329-334)**: faced with the apparent contradiction between energy conservation and uncertainty of position, he realized that the idea of energy conservation makes sense only for isolated states (which, as such, are not observed), while the meaning of the concept of position inevitably involved measurement, i.e. the observation of this position. Hence, we obtain complementary descriptions for the same phenomenon by limiting the contradictory concepts in such a way that they can co-exist. As Heisenberg notices:

> This concept of complementarity fitted well the fundamental philosophical attitude which he had always had, and in which the limitations of our means of expressing ourselves entered as a central philosophical problem. (Heisenberg, 1967, p. 106)

This preoccupation with the meaning of concepts is indeed a constant in Bohr's writing, such as when he described the task of physicists as:

> not to penetrate in the essence of things, the meaning of which we don't know anyway, but rather to develop concepts which allow us

[31] There exists a vast literature on Bohr's work and philosophy. For an introduction to the role of (classical) concepts and language in his reasoning, see Howard, 1994; Favrholdt 1994; Bokulich & Bokulich, 2005; on scrutinizing the conditions of the various concepts, see Favrhodlt, 1994, p. 83, 94-95; Folse, 1994, pp. 134-137; Camilleri, 2007, pp. 520-524; on the importance of experimental observations, see Shomar, 2008, Tanona, 2004, p. 685.

> to talk in a productive way about phenomena in nature. (Bohr in a letter to H. Hansen, as cited in Pais, 2000, p. 23)

Bohr employed this same pattern of reasoning in the present case. Scrutinizing the role of electrons in the *p-e* model, he realized that they are needed only to ensure the correct atomic charge and the electromagnetic attraction that keeps the nucleus together.[32] For these roles, the only property of (nuclear) electrons that one has to assume is that they have a negative elementary charge – a non-mechanical aspect. On the other hand, the many problems related to the idea of nuclear electrons (the β spectrum, the spin of the N-nucleus, the absent total magnetic moment, the Klein paradox) all have to do with the mechanical properties of these electrons: their momentum, energy and spin. These problems led Bohr to conclude that :

> The behavior of electrons bound within an atomic nucleus would seem to fall entirely outside the field of consistent application of the ordinary mechanical concepts, even in their quantum theoretical modification. (Bohr, 1929/1986, p. 88)

By separating the mechanical and electric properties of (nuclear) electrons, Bohr was able to resolve this conflict. On the one hand, electrons do not exist as individual particles inside the nucleus; only their total charge (a multiple of the elementary charge) exists and is somehow distributed inside the nucleus to ensure the *p-e* model. On the other hand, it is only in the process of β decay that an electron is created as a "dynamical individuality" (Bohr 1929/1986, p. 88), while a negative unit charge attaches itself somehow to this newly formed particle. By restricting the assumptions about electrons in both cases, Bohr carved out conceptual space between the two existing theories for a yet to be constructed theory of "the constitution of elementary electric particles" (1929/1986, p. 87). For him, it was clear that this new theory could only account for the various energetic puzzles surrounding the electron concept – Bohr was at the time also puzzled by the classical problem of the infinite self-energy of electrons, and connected the energy production in stars to this new theory – if it was not subjected to the principle of energy conservation.

To this analysis, we can add a further observation. As Pais (1986, p. 312, note 20) and Jensen (2000, p. 149) have already remarked, this proposal is not related to the earlier BKS-theory (Bohr, Kramers and Slater, 1924), which had already proposed the idea of energy non-conservation in order to remedy the old

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[32] From 1921 on, serious doubts arose concerning whether it was the electromagnetic force that kept the nucleus together. Still by 1928, no valid alternative had been proposed (Pais, 1986, p. 240).

quantum theory.[33] In fact, as Darrigol (1992, p. 214) has noted, Bohr had come upon the idea to limit the conservation of energy and momentum even earlier. But it has not been sufficiently stressed in the literature that at each of these three times there was a different reason to consider the non-conservation of energy. Previous to the BKS-paper, Bohr privately held the opinion that the idea of momentum conservation will be impossible to reconcile at a micro level with the fact that momentum changes by both discontinuous jumps and continuous waves (Darrigol, 1992, p. 214). In the BKS-paper, which presented a probabilistic theory for the first time, the energy conservation theorem had to be sacrificed to ensure the statistical independence of the atoms:

> It may be emphasized that the degree of independence of the transition processes assumed here would seem the only consistent way of describing the interaction between radiation and atoms by a theory involving probability considerations. This independence reduces not only conservation of energy to a statistical law, but also conservation of momentum. (Bohr, Kramers and Slater, 1924, p. 792-793)

Energy conservation would imply that each time an atom emitted a quantum of energy, another atom would absorb this quantum – a contradiction with the assumption that these processes are statistically independent. Bohr acknowledged that this theory was mistaken following the 1925 experiments by Compton and Simon (Franklin 2001, p. 65-68). The reason why he proposed energy non-conservation for the third time in our case study in 1929 is clearly different, and this time, as Gamov noticed, "he now goes even further and stresses that the energy need not be conserved even in the mean" (as cited in Jensen, 2000, p. 149).

However, there is a parallel to be observed. **Clearly, maintaining the energy theorem was not high on Bohr's list of priorities. He was willing to sacrifice it if necessary, and, although his previous experiences with abandoning it had been unsuccessful, he still thought the idea could help him solve the many puzzles of nuclear theory.**

This brings us to our main questions: **why was Bohr so willing to withdraw the energy conservation theorem? And why did he not take seriously Pauli's suggestion to acknowledge a new particle (see Section 4.5) when the debate about the β spectrum narrowed to these two suggestions around 1932?** In specifying his method of reasoning, we saw that Bohr gave absolute

[33] **For a historical introduction to the BKS theory, see Darrigol, 1992, ch. 9; Longair, 2013, pp. 194-197.** It is common in the literature to suggest that the relation between Bohr's ideas of 1924 and those of 1929 (e.g. Franklin, 2001, p. 68). Lakatos (1970, p. 168-173) perceives non-conservation of energy even as the central thesis of a research program that ran from 1924 (the Bohr-Kramer-Slater paper) until 1936 (the Shankland experiments).

priority to the observational data, which he tried to account for using the physical concepts at his disposal. **He had always been rigorous in this. In the BKS-paper he stated, concerning photons, that:**

> Although the great heuristic value of this hypothesis […], the theory of light-quanta can obviously not be considered as a satisfactory solution of the problem of light propagation. (Bohr, Kramers and Slater, 1924, p. 787)

At a moment when many physicists were starting to accept the physical existence of Einstein's photons (certainly from the 1922 Compton experiments onwards), Bohr continued to disbelieve in their existence until the mid-1920s (Stachel, 2009); due, in his words, to a lack of experimental observation. It is then no surprise that Bohr was also reluctant to assume the existence of another new particle, of which, by 1929, there was not the slightest experimental trace.

This analysis concurs with Shomar's (2008) characterization of Bohr as a "phenomenological realist", i.e. someone who has a realist position about low-level phenomenological models, which are a kind of theoretical descriptions of reality, but who has an instrumentalist position about high-level theories, which have merely the status of conceptual tools. This explains in the same way why Bohr was hesitant to accept new particles (phenomenological models that link very closely to experimental observations), but willing to sacrifice the energy conservation principle (a high-level theoretical principle). This leaves us with the question of why the energy conservation theorem should have been the law that was sacrificed, and not another law or principle. In my opinion, the main reason was that Bohr projected that the new theory he had in mind could address all energetic problems at once, and that therefore it would be better at first not to include such a strong theorem about which earlier he had already had doubts.

This summarizes his views on the matter. **Unlike Thomson, Bohr realized that this problem was beyond the borders of the new quantum formalism. This did not, however, lead him to doubt the new quantum mechanics;** rather he thought that, epistemically, the best one could do in describing phenomena was to assemble a patchwork of various theories, joined by correspondence principles and common concepts and stripped from contradictions by specifying and restricting these concepts. Still, one should also keep in mind that Bohr had hardly any "positive results" to support this new theory, and his hesitation to put these ideas in print demonstrates that he was aware of the radical nature of his suggestion.

4.5 Heisenberg: Non-conservation of Energy as part of a Second Quantization at the Scale of the Nucleus

We will touch only briefly on Werner Heisenberg's ideas, mainly because he had an independent stance on the matter, which was never published and which he held only for a few months. **Also, his idea, however short-lived, must be understood in relation to his broader research project at the time: to cope with the many infinities and paradoxes associated with the establishment of a relativistic quantum electrodynamics, especially at distances on the order of the size of the electron (Cassidy, 1981; Rueger, 1992).**

Heisenberg's suggestions concerning the β spectrum were brought to light by Bromberg (1971), and recorded in two letters to Bohr, dated in February and March 1930 and available at the Niels Bohr Archive. In these letters, as Bromberg tells us, Heisenberg proposed to construct a mathematical lattice world with grid cells of nuclear dimensions. If the scale of the system was large with respect to these cells, normal quantum mechanics would apply; but within these cells, phenomena obeyed new laws. He obtained these by turning the Klein-Gordon differential equation into a difference equation tailored to these new cell dimensions. This had as a consequence that the energy of particles became periodically dependent on the wave number. By further supposing that particles near the maxima behaved as electrons and those near the minima as protons, Heisenberg constructed a first picture of the nucleus without "real" nuclear electrons. The price of this idea was high: within the cells, neither charge, energy nor momentum was conserved, which made him ask Bohr whether he regarded "this radical attempt as completely crazy" (as cited in Bromberg, 1971, p. 325). Yet Bohr was on much the same track, as we have seen, except for charge conservation, and also on the outlook for a new nuclear theory. However, Heisenberg's idea was short-lived: already in April 1930, after a meeting with Bohr, Pauli and Gamov in Copenhagen, he jettisoned it because he realized that the introduction of a fixed cell grid length could not be relativistically invariant.

The key to understanding how Heisenberg arrived at such a radical theory can be found in the following passage from a letter he sent to Bohr in December 1929, in which he commented on Ellis' findings that protons emitted in the artificial disintegration of N also showed a continuous spectrum (this turned later out to be mistaken):

> I find Ellis' claim that also the H particles from the disintegration of N show a continuous spectrum dreadful; for how shall one then understand the sharp α ray spectra? (Heisenberg, as cited in Jensen, 2000, p. 148)

As he later acknowledged in a personal interview with Bromberg (1971, p. 328), the many problems associated with nuclear electrons in particular and the nucleus in general made him wonder why the α spectrum was the lucky exception. **In other words, instead of β decay, he started to consider that α decay might be the anomaly; but in doing so, he presupposed at the same time the existence of a nuclear theory that explained the other nuclear problems, especially the infinite self-energy of a point electron, which proved such a hurdle for QED (Cassidy 1981, p.8)**. By thus inverting the anomaly, he reached the same conclusion as Bohr in a much more straightforward way, i.e. the need for a new (nuclear) theory. But unlike Bohr, who expected this to be a theory of elementary particle constitution, Heisenberg foresaw a more general theory of all nuclear phenomena (which would reveal that α decay was the real exception).

Heisenberg observed that the main difference between quantum mechanics and the nuclear problems was one of scale. **At the same time, he realized that the scale of the nucleus was of the same dimension both as the classical electron radius (which proved in QED to be the scale of the electron, below which the theory diverged to infinity)** and as the Compton wavelength of the proton (as the proton was the heaviest particle known at the time, this was the smallest length in which the uncertainty relations allowed a particle to be localized). These coincidences must have led him to hardcode this dimension as the dimension of grid cells, of which he had the freedom to alter laws within their boundaries.

This construction of a quantum-nuclear divide is clearly set up in a way analogous to the classical-quantum divide: processes of large scale with respect to the pivotal distance can be described by the former theory, while phenomena at the scale of this distance obeyed new laws.

At this point Heisenberg needed some formal tool to start exploring this new level. He did this using the relativistic Klein-Gordon equation for spinless particles[34] and hardcoding his cell axiom directly into this equation by changing the differential equation into a difference equation. It was this formal "point of attack" – as he called it himself (Bromberg, 1971, p. 328) – that led him to deduce the various results he proposed.

Interestingly, Heisenberg's strategy**, which is part of what Cassidy (1979) had called his "professional style",** had proven fruitful before: he obtained the results of his first paper in 1925 on matrix mechanics via a similar procedure, i.e. using the correspondence relations and hardcoding in these formulae the model of virtual oscillators (MacKinnon, 1977; Miller, 1984/1986, pp. 135-38). This

[34] Heisenberg at the time, just like Bohr, had issues with Dirac's infinite sea interpretation of the relativistic Dirac equation for particles with spin, which might be why he returned to the older Klein-Gordon equation for this theory.

time, his results were less lasting, and after realizing that his theory could not stand the test of relativity, he turned his focus away from nuclear physics, because of the lack of a new formal point of attack, until the discovery of the neutron in 1932 (Bromberg, 1971, p. 329).

4.6 Pauli: a new elementary particle as a nuclear constituent

Wolfgang Pauli's suggestion to solve the β puzzle is quite famous because the canonical history of modern physics has equated the new particle he envisioned with our neutrino, making of Pauli's idea a highly original epiphany that provided the key to the β puzzle – a story spiced up by the anecdotal details from his original letter addressed to the "*liebe radioaktive Damen und Herren*" from Tübingen (Pauli, 1957/1964, p. 1316). This outsiders' perception has, however, been successfully challenged: the particle Pauli first had in mind was a different particle than what is currently understood as the neutrino emitted in β decay (Brown, 1978; Pais, 1986). Furthermore, while it is correct to credit him for hypothesizing an as-yet unobserved particle to solve the β puzzle, **the idea he had in mind was not necessarily so new as is commonly thought. As I will show in this section, it could well have been an adaptation or variant of Rutherford's original neutron idea.**

Pauli's famous letter,[35] dated December 4th 1930, in which he presented his "desperate remedy", was written to a group of experimentalists – the most important among them being Meitner and Geiger – that held a seminar in Tübingen three days later which he was unable to attend.[36] In this letter, Pauli hypothesized an electrically neutral particle, named the "neutron", which is a permanent constituent of the nucleus with spin of ½. Its velocity, he said, was somewhat below the speed of light and its mass was relatively small, on the order of the mass of an electron. This particle was to be kept in the nucleus by electromagnetic forces, and so must have a magnetic moment.

In the introduction to this letter, Pauli expressed his hope that the discovery of this particle would solve both the β puzzle and the anomalous spin of the nitrogen nucleus. In other words, just like Bohr, he tried to solve several nuclear

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[35] Pauli himself made this letter public in a lecture on the history of the neutrino (Pauli, 1957/1964, p. 1316-17). The idea must have come to his mind only shortly before this letter: the first known written reference to it is in the form of a letter from Heisenberg to Pauli three days earlier (Jensen, 2000, p. 153; Pais, 1986, p. 315).

[36] As the letter states, he was expected to attend a ball the night before – according to Pais (1986, p. 315), the Italian student ball – at which his presence was "indispensable". Pais spices this story further by revealing that Pauli wrote this letter only a week after his first wife, to whom he had been married for less than a year, left him. Pauli seems to have referred once to the neutrino as that "foolish child of the crisis of my life", which caused Pais to stress the importance of this anecdotal evidence as follows:

> "I tend to regard Pauli's association between his time of personal turmoil and the moment at which he stated his new postulate as highly significant. Revolutionary steps were out of line with his general character." (1986, p. 314)

puzzles at once. This might be the reason why his proposal had characteristics in common with both our present neutron (a neutral spin-½ constituent of stable nuclei) and our present neutrino (a very light spin-½ particle that carries away the remnant energy in β decay). It was not until the experimental discovery of the 'heavy' neutron by Chadwick in 1932 (which explained the spin of the N nucleus) that Pauli would finally consider his proposal – renamed the neutrino by Fermi – no longer as a nuclear constituent but solely as the key to the β puzzle (Brown, 1978, pp. 24-28).

Pauli was at first particularly hesitant about his ideas, and he was well aware that the lack of experimental evidence could be held strongly against him. In a letter to Klein one week after his original letter, he wrote that:

> So, if the neutrons really existed, it would scarcely be understandable that they have not yet been observed. Therefore, I also do not myself believe very much in the neutrons, have published nothing about the matter, and have merely induced some experimental physicists to search in particular for this sort of penetrating particles (Pauli, as cited in Jensen, 2000, p. 154).

However, as he received a "positive and encouraging" answer to his original letter from Geiger – Pauli puts great emphasis on this support in his recollections (Pauli, 1957/1964, p. 1317) – and given the severity of the problems, he kept toying with his idea and started lecturing about it on a trip across America the next summer. In October 1931, while attending the first nuclear physics conference in Rome, he must have sparked Fermi to develop his own β decay theory (Brown, 1978, p. 27). However, until the discovery of Chadwick's neutron, Pauli's proposal remained a minority position, the majority of physicists being convinced by Bohr's ideas (Jensen, 2000, p. 155). Only after the 1933 experimental results of Ellis and Mott did Pauli finally allow the first printed publication of his – since the discovery of the neutron evolved – idea (in the report of the 7th Solvay Conference in 1933; reprinted in Brown, 1978, p. 28). Ellis and Mott's experiments favored Pauli's suggestion because they found that the β spectrum had a sharp upper limit, indicating that something (a neutrino) carried away the difference in energy rather than that the electron energies were distributed randomly around an average emission energy (in the case of non-conservation of energy).

Let us now try to understand how Pauli originally came to his idea. Clearly, his motivation stems from a serious discontent with Bohr's thoughts about the non-conservation of energy. In the letter to Klein, quoted earlier, he also made a more elaborate argument against Bohr's proposal in the form of a thought experiment:

> Imagine a closed box in which there is radioactive β decay; the β rays would then somehow be absorbed in the wall and would not

> be able to leave the box. […] If the energy law thus would not be valid for β decay, the total weight of the closed box would consequently change. (This conclusion seems quite compelling to me.) This is in utter opposition to my sense of physics! For then it has to be assumed that even the gravitational field – which is itself generated by the entire box (including the radioactive content) – might change, whereas the electrostatic field, which is measured from the outside, should remain unchanged because of the conservation of charge. (Yet both fields seem analogous to me; that, incidentally you will recall from your five-dimensional past.) (Pauli, as cited in Jensen, 2000, p. 153)

The five-dimensional past to which Pauli refers is his own early physics career in relativity. At the age of 21, Pauli wrote a state of the art overview of general relativity, which impressed even Einstein (Pais, 2000, p. 215). As such, although he does not mention it explicitly in this article, he must have been aware of Noether's theorems, which state the correspondence between conservation laws and (differentiable) symmetries of fields. In fact, for this article Pauli made use of Klein's notes, which called attention to these theorems several times (Kosmann-Schwarzbach, 2010, p. 93). Seen from this perspective, Pauli found it unacceptable that the analogical treatment of the various field symmetries was broken. But this perspective differed significantly from that of the average quantum physicist at the time. As Heisenberg recalls in an interview with Thomas Kuhn in the 1960s:

> Much later, of course, the physicists recognized that the conservation laws and the group theoretical properties were the same. And therefore, if you touch the energy conservation, then it means that you touch the translation in time. […] But at the time, this connection was not so clear. Well, it was apparently clear to Noether, but not for the average physicist. (Heisenberg, as cited in Kosmann-Schwarzbach, 2010, p. 85)

Pauli was clearly ahead of his time. As one of the few protagonists in quantum physics, he adhered already to a modern ontology that considered particles and fields (with their symmetries) as the unifying ontological entities.[37] For him, conservation laws were not just empirical laws, but structural relations grounded in his ontology, and as such could not be refuted by simple empirical observation.

This analysis concurs with De Regt's (1999) analysis of the heuristic methodologies of Pauli and Heisenberg. Based on their earlier work in the mid-1920s (on matrix mechanics and the Zeeman effect), De Regt interpreted the difference in methods between them at the level of their

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[37] Steven Weinberg ascribes to Feynman the present-day day view that even those two basic ontological entities coincide (1999, p 241).

personal philosophies: Pauli was an ontological realist whose operationalist methodology placed consistency with other theories and simplifying unity above empirical adequacy, while Heisenberg would be best described as a kind of pragmatist (although not fully anti-realist), whose principal aim was to forge mathematical theories that were empirically adequate, even if to do so he had to employ ad hoc strategies (see, for example, his suggestions concerning the lattice world in Section 4.5).

This ontological necessity of the conservation laws must have triggered Pauli to think over the nuclear problems himself:

> I tried to connect this problem of the nuclear spin and statistics with the other problem of the continuous beta-spectrum by the idea of a new neutral particle without abandoning the energy theorem. (Pauli, 1957/1964, p. 1316, my translation)

In essence, the energy conservation theorem is an equation in which the measurements before must be balanced with those taken after. The abnormal statistics for the N-nucleus, too, are basically an unbalanced spin equation with the sum for the theoretically predicted particles on one side and the observed total spin on the other. Such unbalanced equations cannot be balanced in so many ways: if one is certain of the terms and their values present in the equation, the only way to balance it is by adding something with appropriate values to the picture. In Pauli's case, as charge was already conserved, this meant an electrically neutral particle with spin of ½ and appropriate momentum and energy.

However, the idea of a new neutral particle was not new: Rutherford had already in his Bakerian Lecture (1920) mentioned the possibility of "an electron to combine much more closely with the H-nucleus, forming a kind of neutral doublet". **Such a neutral particle, which was just one of the many speculative nuclear composite particles he suggested (Hughes, 2003, p. 362), would have "very novel properties":** "it should be able to move freely through matter, and its presence would be difficult to detect" (1920, p. 396). Rutherford, at the time unaware of the strong nuclear force, thought that such particles were necessary to explain nuclear constitution:

> The existence of such atoms seems almost necessary to explain the building up of the nuclei of heavy elements; for unless we suppose the production of charged particles of very high velocities it is difficult to see how any positively charged particle can reach the nucleus of a heavy atom against its intense repulsive field. (Rutherford, 1920, pp. 396-97)

It is my thesis that it is well possible that Pauli, realizing that the presence of a neutral particle could restore the conservation laws, thought of

Rutherford's idea and understood that it could, with slightly modified properties, offer a solution for the problems he was working on. In the remainder of this section, I will develop several arguments for this speculative thesis.

First, Rutherford's "neutron" idea remained very much alive before Chadwick's discovery and was part of the research program of the Cavendish laboratory (Stuewer, 1983, p. 27; Fernandez, 2013, p. 253; for a list of early references to the neutron, see Stuewer, 1983, n. 150); this notwithstanding that Chadwick's (1932, p. 698) claim – namely that the particle he discovered was precisely the particle discussed by Rutherford in his Bakerian lecture – was also an attempt to gain some prestige for the Cavendish laboratory in the field of theoretical nuclear physics and raise some much-needed funds (Navarro, 2004, p. 443; Hughes, 2000, p.46).

Consider for instance Rutherford and Chadwick's article of 1929, discussed in Section 4.2. There, they proposed a hypothesis for the observed continuous spectra (i.e. that identical nuclei have varying internal energies) only after

> the liberated protons were examined in order to test whether any particles other than protons were present; for example, whether the particles of very long range might possibly be 'neutrons'. (Rutherford and Chadwick, 1929, p. 189)

This particular article is mentioned by Bohr to Fowler in a letter concerning the β problem (Jensen, 2000, p. 147), and Heisenberg, too, mentioned these experiments in a letter to Bohr (Jensen, 2000, p. 148). It is very plausible that Pauli, who was in both Bohr's and Heisenberg's inner correspondence circle (e.g. Bohr's first attempt to solve the β spectrum was sent to Pauli) and who regularly visited Copenhagen to discuss the problems of the day, knew this article, written only one year earlier, **or, at the very least, was familiar with Rutherford's desire to find a neutral nuclear constituent or neutron.** This puts the following quote from his original letter in perspective:

> […] i.e. the possibility that there could exist electrically neutral particles in the nucleus, which I want to call *neutrons*, and which have a spin of ½, obey the exclusion principle, and distinguish themselves from light quanta in the fact that they do not move at the speed of light. (Pauli, 1957/1964, p. 1316, my translation)

Secondly, Pauli tells us the following about his views by the time he lectured about his idea on his American trip in the summer of 1931:

> I did not held them anymore as nuclear building blocks; *therefore*, I did not call them neutrons anymore, and used no particular name for them. (Pauli, 1957/1964, p. 1316, my translation, emphasis added)

Apparently, the reason why he called them neutrons was that he thought they were nuclear constituents. But, even more importantly, Brown (1978, p. 24-27) has demonstrated that this statement is wrong and in contradiction with the recollections of the participants at the 1931 Rome conference and the newspaper articles detailing Pauli's American travels. Brown situated the moment when Pauli changed his mind about whether it was a nuclear constituent in 1932 or 1933, yet did not draw the obvious conclusion: that Pauli thought his 'neutron' was a nuclear constituent until Chadwick discovered the (heavy) neutron in February 1932. **This would mean that, although he thought that Rutherford was mistaken in regarding the neutron as a composite particle, he was convinced of their presence in the nucleus.**

Finally, even in his recollections, Pauli links his idea to Rutherford's by describing Rutherford's suggestion in the historical paragraphs before the earlier quotes. Furthermore, Pauli criticizes Rutherford for taking the neutron as a close combination of a proton and an electron, and informs us that this was the reason why Rutherford had no experimental success in finding neutrons in hydrogen discharges (Pauli, 1957/1964, p. 1315). While this is a correct analysis from a present-day point of view, it could also reflect Pauli's thinking in 1930**: the main difference between his and Rutherford's ideas was that Pauli's neutron was an elementary particle, which allowed for a lower mass and the half-integer spin that all known elementary particles had at the time. It seems possible that Pauli adopted this idea in light of the failure of Rutherford's experiments up to 1930.**

In summary, although I do not deny its somewhat speculative nature, there is evidence for the thesis that Pauli, consciously or not, thought that Rutherford's idea, given its neutral charge and high penetrability, could be used as the fitting piece to solve the nuclear problems, on condition that it was not considered as a proton-electron combination but instead as an elementary spin ½ particle of smaller mass. The idea that there might be two different neutral particles probably occurred to him only after the discovery of the (heavy) neutron by Chadwick in 1932. This thesis, however, demystifies one of the many stories about epiphany that have entered the canonical history of science, and supports the more credible view that many new ideas originate from adapting old ideas for new purposes. **After all, the only difference between Rutherford's and Pauli's original idea was a difference of mass and of its elementary nature (being a spin ½ particle);** Pauli's idea had to undergo many more adaptations before it became our current idea of the neutrino.

5. Review and Conclusions

Let me start by summarizing the six discussed attempts to form an explanatory hypothesis for the anomalous β spectrum. This will enable us to draw some general conclusions about these processes, which will link this case study back to the questions raised in the introduction.

First, I discussed Ellis and Wooster's suggestion in their seminal paper about the β spectrum. Making use of Rutherford's nuclear satellite model, they suggested that the difference between the discrete α spectrum and continuous β spectrum could be traced back to the nuclear layer in which the particles originated. Therefore, they suggested, in addition to the quantized orbit in which the α particles resided, there was an unquantized orbit of β particles, and questioned the universality of the quantum postulate. Ellis and Wooster thought this was justified because they regarded the quantum postulate not as a genuine postulate, but rather an emergent phenomenon arising from particles describing stable orbits.

Second, we considered Rutherford and Chadwick's ideas. These experimentalists suggested that the continuous β spectrum was caused by variations in the internal energy of otherwise identical β nuclei. This suggestion seriously infringed the identity principle, which states that equal particles (or atoms) are indistinguishable. To reach this radical hypothesis, they evaluated a spatiotemporal process model of β decay, and realized that the continuous variations must either have entered in the decay itself or else been present from the start. As the former option implied (in the interpretation of their model) that energy was not conserved – something inconceivable because of their "experimental bias" – they regarded the latter option as more plausible. **Most probably, an analogy with Aston's discovery of isotopes a decade earlier at the Cavendish laboratory has played a role in Rutherford and Chadwick's reasoning.**

Next, I discussed Thomson's account, which is a clear example of how the counterintuitive aspects of quantum mechanics misled even renowned physicists in early years. Fully embracing the new formalism and maybe somewhat blinded by its attractive fruitfulness, he assumed that it also contained the key to the β puzzle. To solve it, he actually followed a very straightforward pattern of discovery: he visually recognized a Gaussian curve in the experimental data, and calculated the appropriate parameters for a maximal fit with the quantum mechanical model of a free particle (which is formulated in terms of Gaussian curves). This identification and calculation led him to the conclusion that energy was not conserved in the process (which is, in his account, a natural consequence of quantum mechanics). Unfortunately, he relied heavily on certain classical assumptions that rendered his contribution contradictory. However, the idea that energy is not conserved in β decay proved persistent.

The fourth discussed physicist was Niels Bohr. By considering the successful quantum mechanical explanation of α decay (which results in a mono-energetic spectrum) and recognizing the mechanical equivalence of α and β decay, Bohr understood that if the β electrons were present in the nucleus beforehand, then in their case quantum mechanics would also predict the occurrence of a mono-energetic spectrum. Because this was (experimentally) not the case, he concluded that the electrons must have been created in the process of decay. As no physical theory yet existed for such a process of elementary particle creation, he foresaw the development of a new theory, which, if he did not impose energy conservation on it, would allow him to solve multiple energetic problems in the nucleus at the same time. Yet as the nuclear model prevalent around 1929, the *p-e* model, required the presence of electrons in the nucleus, Bohr first had to address this apparent contradiction, which he did by means of a typical Bohr-style conceptual analysis. By this process, which was also at the heart of the formulation of his complementarity principle, he was able to reconcile apparent contradictions and link observations back to classical concepts.

In discussing Heisenberg, we noted that he turned the debate on its head, regarding solved problems as in fact anomalies and vice versa. **Overwhelmed by the many problems concerning the nucleus and the formulation of QED**, he thought that α decay (for which a sound quantum mechanical explanation existed) was the anomaly for a yet to be constructed theory of nuclear physics that will explain all problems. Inspired by the quantum-classical divide, he proposed a new divide between the nuclear and the quantum (in his view, the atomic) level, which allowed him to construct new laws for this new level via an appropriate correspondence principle – exactly the same process that led him earlier to the formulation of his matrix mechanics.

The sixth and final physicist I discussed was Pauli, who was put off by Bohr's and Heisenberg's denunciations of energy conservation. Given his personal history in relativity, he recognized the unifying power of fields as ontological entities and the consequences of Noether's theorem. Conservation laws, therefore, were at the heart of his ontology: as they seemed to be violated in experiment, he understood that the only option was to add something to the picture (something not yet observed) in order to balance them. **At the time, Rutherford's early proposal of the neutron was already in the air, and it could well be that Pauli, as has been shown, used this early idea in a slightly adapted form.**

Having thus reviewed the various attempts to solve the β puzzle, and so completed a case study of genuine variation in different patterns of hypothesis formation, I will now draw some general conclusions by answering the following two questions, based on available evidence, which reflect the questions raised in the introduction: how did the scientists in this case study determine which pattern of hypothesis formation they would employ? And do the patterns of hypothesis formation employed in this study have any common features that tend to be overlooked in the literature on hypothesis formation?

The main conclusion of this study is that, in the examined case, the scientists' choice of a type of hypothesis formation was always made implicitly and determined directly by their personal perspective on their field, as well as on the nature of the problem at hand. Even when scientists generally work with the same formalisms and theories, they sometimes have different perspectives on how the various elements structurally hang together, and it is this perspective, which is often based on their personal experiences, that implicitly determines which patterns of discovery the scientist will judge suitable. In our case study, we saw that Ellis and Wooster's idea that the quantum of action resulted from classical stable orbits led them to question the universality of the quantum postulate; that Rutherford and Chadwick's experimental bias prevented them from questioning the laws of conservation, which were at the core of their experimental models; that G.P. Thomson's continued adherence to his classical intuitions concerning electrons confused him, and led him finally to an incoherent conclusion; that Bohr's total perspective on science as describing the observational phenomena in everyday concepts informed and motivated his method of tinkering with higher-level concepts, yet led him to remain hesitant of allowing new low-level phenomenological models (such as particles); that Heisenberg's reversal of solved problems and anomalies cleared an entire field for him, for which a theory could be constructed; and that Pauli's ontological perspective made him suspicious of all proposals to limit conservation laws.

This idea concurs to a certain extent with Henk De Regt's (1996) point that physicists' philosophical remarks are not of much importance for the philosophy of science, but can be understood as the justificational grounds for their research heuristics in confrontation with their contemporaries. (See also Kojevnikov, 2011 on the role of philosophizing for the protagonists in this story)

As a second conclusion, the case study also suggests that real-life scientists apparently do not employ different patterns of hypothesis formation when approaching a single puzzle: they tend to stick to a pattern that best fits their current perspective. This adherence to a certain method has led many of the involved scientists to important results: Bohr's complementarity principle, Rutherford's idea of the neutron and Heisenberg's matrix mechanics were all obtained by the same patterns of hypothesis formation as they used in this case. Still, this tendency to adhere to a particular pattern is certainly not absolute, and none of the scientists involved had any problem acknowledging the subsequent results of others.

Finally, as different as the patterns and motives of these scientists were, two properties of hypothesis formation patterns tend to appear prominently that are not always adequately appreciated in the literature: the adaptation of old ideas and the use of visual and intuitive models.

First, none of the scientists discussed presented a completely new idea; all adapted an old idea for new purposes or drew an analogy with an existing idea. **Ellis and Wooster reinterpreted Rutherford's nuclear satellite model; Rutherford and Chadwick drew an analogy with the research on isotopes conducted in their laboratory**; G.P. Thomson employed the existing Gaussian model for a free particle; Bohr had relied already several times previously on the rejection of energy conservation; Heisenberg constructed his nuclear-quantum divide in analogy with the quantum-classical divide; and **Pauli could well have been influenced by Rutherford's older idea of the neutron**.

Second, all of the scientists discussed relied on visual or intuitive models. **Ellis and Wooster's model was clearly a visual nuclear model;** Rutherford and Chadwick used a causal process model, which allowed them to derive an exhaustive list of possibilities; G.P. Thomson started from a visual identification of the β spectrum as a Gaussian curve, yet also it was his visual pre-quantum models of particles that led him to misunderstand quantum mechanics; Heisenberg introduced grid cells as a form of lattice theory; and Pauli adhered to an ontology based on the symmetries of fields. The only exception here might be Bohr, but if we understand how he tried to apply (restricted) everyday concepts to physical phenomena, we realize that what he was doing was exactly re-introducing intuitive images and concepts in an overly mathematical and formal theory.

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