On the prediction of the Omega minus baryon

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Abstract

This paper analyses the prediction of the Omega minus baryon. Confirmation of the particle's existence was a turning point for the acceptance of Gell-Mann and Ne'eman's Eightfold Way model, affirming its empirical success. As a result, it laid the groundwork for the quark model, ensuring confidence in the viability of Eightfold Way and turning physicists towards a deeper investigation of the scheme's SU(3) structure.

The prediction of the Omega minus particle has been analysed by philosophers as a non-standard form of inference, primarily due to the involvement of group theory in making an existence claim. Prominent accounts include Bangu (2008), Ginammi (2016), Bueno and French (2018), and Tricard (2023). These authors present a variety of interpretations, but tend to treat the inference either as a heuristic in the context of discovery, or as relying on premises that lack justification.

I will argue that the features of the Eightfold Way mean that the inference featured heuristic arguments, but that they were justified precisely because of the group theory involved, alongside the explicitly tentative nature of Eightfold Way. In particular, I will propose a form of inference that I call possibility counting from symmetries (PCS), where the possibility space of certain theories are constructed based on group-theoretic tools, thereby justifying the prediction of new entities. Further cases of PCS in particle physics suggest the need for a strong empirical basis justifying the introduction of new group-theoretic structure into models and theories.

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1 Introduction

The quark model is a core part of the Standard Model, our best theory accounting for the subatomic structure of matter. Because of the experimental difficulty and complex theoretical background, the development of the quark model contains episodes that serve as illuminating case studies for the philosophy of science. A significant predecessor model to the quarks is the Eightfold Way, based on an approximate SU(3) symmetry of the strong interaction, as a way of classifying the increasing number of new particles detected in collision experiments.

The model itself underwent an extended period of confirmation due to its novel theoretical ideas and an initial lack of empirical evidence, culminating in the prediction and discovery of the Ω^- baryon. The confirmation of the particle's existence was a turning point for the acceptance of the Eightfold Way, affirming its empirical success. As a result, it lay the groundwork for the quark model, ensuring confidence in the viability of Eightfold Way and turning physicists towards a deeper investigation of the scheme's SU(3) structure—and thereby the quark model.

The prediction of the Omega minus particle has been analysed by philosophers as a non-standard form of inference, primarily due to the involvement of group theory in making an existence claim. Prominent accounts include Bangu (2008), Ginammi (2016), Bueno and French (2018), and Tricard (2023). These authors present a variety of interpretations, but tend to treat the inference either as a heuristic in the context of discovery, or as relying on premises that lack justification.

I will provide a new analysis, arguing that features of the Eightfold Way mean that the inference was heuristic, but was justified because of group theoretic features and the explicitly tentative nature of the model. I propose a novel form of inference I call possibility counting from symmetries (PCS), where the possibility space of certain theories are constructed based on group-theoretic tools such as representation theory—thereby justifying the prediction of new entities.

It is important to analyse the prediction of the Ω^-, because the episode not only demonstrates how predictive inferences based on preliminary models can be empirically adequate as a result of the representational choices made, but also shows how the success of preliminary models can lead the concepts and methods involved to become entrenched during the construction of more mature theories. I will argue that theoretical developments in high energy physics have been made on a similar basis to the Ω^- prediction, whether successfully in the case of the Higgs boson, or less so in the case of supersymmetry—with the differentiating factor being the amount of empirical evidence motivating the introduction of new group-theoretic structures to models and theories.

The structure of the paper is as follows: §2 covers the historical background behind the Ω^- prediction; §§3-5 analyses existing accounts of the prediction. §6 considers three neglected but relevant aspects of the episode, and §7 presents the inferential pattern of possibility counting from symmetries (PCS). §8 applies PCS to other predictions in particle physics, as a way of investigating its efficacy in situations beyond the Eightfold Way, and finally §9 concludes.

2 Why was it significant?

I begin with a historical overview of developments leading to the Ω^-. Throughout the 1950s, unknown particles were detected in cosmic ray experiments with longer lifespans and higher rates of incidence than expected. New quantum numbers of strangeness and hypercharge, modelled on isospin, were introduced to account for these properties; the Gell-Mann-Nishijima formula related them to established quantum numbers of spin, charge, and nucleon number[1].

It became clear that the strong interaction played a key role. While there was success in classifying the new particles, their phenomenological nature failed to provide a deeper explanation, sparking further theoretical speculation. Based on the conservation of strangeness and hypercharge, physicists began to investigate symmetry-based solutions to characterise the strong interaction, prompting the utilisation of group theory. Various accounts were proposed—Pais (1953), Sakata (1956), and Schwinger (1957) among others—with the impact of Yang and Mills (1954) resulting in shift towards *local* rather than global gauge symmetries.

Success came with two 1961 papers from Murray Gell-Mann and Yuval Ne'eman, proposing an approximate SU(3) symmetry: the Eightfold Way model. Assigning different group representations to families of subatomic particles, Gell-Mann and Ne'eman categorised the known baryons, pseudoscalar mesons, and vector mesons. Insufficient empirical evidence meant

[1]See Borrelli (2021) for an excellent overview of this history.

that the model was not widely accepted however, with competing models of the strong interaction remaining viable: the Sakata model, for example, was *also* constructed using SU(3).

By 1962, a critical amount of experimental evidence regarding the family of baryons with angular momentum $J = \frac{3}{2}$ had accumulated. Four Δ baryons (Δ^-, Δ^0, Δ^+, and Δ^{++}) of strangeness $S = 0$ were discovered in 1952 by Fermi and his collaborators as proton-pion collision resonances (Anderson et al. 1952). This was followed by three Σ^* baryons (Σ^{*-}, Σ^{*0}, and Σ^{*+}) of $S = -1$ in 1961. Finally, crucial evidence was reported at the 1962 International Conference on High Energy Physics in Geneva. A pair of Ξ^* resonances (Ξ^{*0} and Ξ^{*-}) of $S = -2$ were found. These baryons could fit into either the **10**- or **27**-representation of SU(3); however, experimental evidence suggested the absence of a $S = +1$ resonance from nucleon-kaon collisions, an expected result if the **27**-representation was correct (Ne'eman 2010, p.369).

The **10**-representation became the obvious choice (Figure 1). Gell-Mann (1962a) details his prediction of the Ω^- made at the conference[2]. Assuming the existence of a $S = -3$, $I = 0$ particle that he labelled Ω^-, Gell-Mann derived an expected mass of 1685 MeV from a generalisation of the Gell-Mann-Okubo formula. Furthermore, he postulated the interaction most likely to create the Ω^-—$K^- + p \to K^+ + K^0 + \Omega^-$—along with the possible decay modes: $K^- + \Lambda$, $\pi^- + \Xi^0$, and $\pi^0 + \Xi^-$.

The empirical confirmation of the Ω^- took longer, though as Gell-Mann mentioned during the conference, anomalous observations such as Eisenberg (1954) had suggested the existence of the particles. Definitive detection of the particle arrived in Barnes et al. (1964), featuring an experiment conducted by bombarding a hydrogen bubble chamber with K^- mesons, following the procedure Gell-Mann had suggested. With a decay through the $\Xi^0 + \pi^-$ channel, and a mass of 1686 ± 12 MeV, the experiment presented the Eightfold Way with a striking empirical success.

3 Reification

With this background in mind, the next three sections will consider existing accounts of the Ω^- prediction. The idiosyncratic prediction of the Ω^- has attracted attention, with philosophers attempting to provide accounts that demonstrate novel inferential patterns in science. Hon and Goldstein (2006), for example, identify the argument as depending on a heuristic rule: "...starting from the model (i.e., the group, SU(3), taken as a constraint on the premise), [Gell-Mann] sought its conclusion (the existence of these

[2] According to Ne'eman's recollections, he had the same realisation as Gell-Mann, and raised his hand to report this; the more senior Gell-Mann was chosen instead (Ne'eman 2010, p.369).

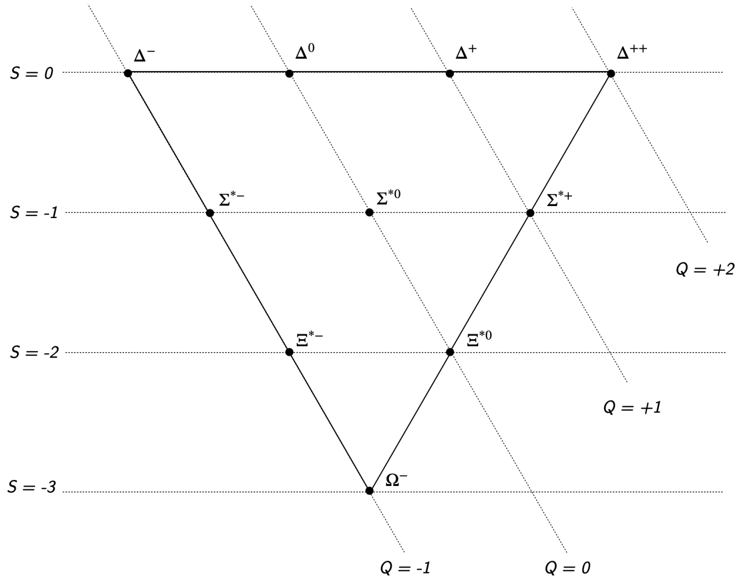

Figure 1: The $J^P = \frac{3}{2}^+$ baryon resonance decuplet.

particles)..." (Hon and Goldstein 2006, p.436). I believe the Eightfold Way model provides more structure and motivation to the prediction than these authors grant. As such, I will analyse accounts from Bangu (2008), Ginammi (2016), Bueno and French (2018), and Tricard (2023), before presenting my own reading in §6.

Bangu (2008) features the first comprehensive philosophical account. Contrasting the inference with the deductive-nomological account of prediction, Bangu claims that a key distinction is the absence of reference to physical interactions with other elements of the physical system in the prediction of Ω^-'s properties. Instead, the inference relies on what he calls the Reification Principle, which I state below. Bangu emphasises that the principle lacks justification unless one adopts a Pythagorean reification of mathematical structure.

The inferential argument that he extracts from Gell-Mann and Ne'eman's recollections is as follows (Bangu 2008, p.244):

> (P1) Each of the upper nine positions in the symmetry scheme has a physical interpretation.
> (H) Spin-$\frac{3}{2}$ baryons fit the symmetry scheme.
> (P2) The apex is formally/mathematically similar to the other nine positions.
> (P3) The physical existence of a baryon having the predicted characteristics is not forbidden (can occur in nature).
> (C) The apex position has a physical interpretation. (That is,

the coordinates of this position describe a 10th spin-$\frac{3}{2}$ baryon.)

According to Bangu, deductive-nomological accounts of prediction require that the new entity play some explanatory role, with its physical characteristics accounting for interactions with other elements of the physical system. He claims that the introduction of the Ω^- satisfies this criterion, helping explain the anomaly that the absence of the particle would create. A second criterion, however, demands that the physical properties of the new entity should be determined through a quantitative analysis of interactions with other physical entities. Bangu argues that the Ω^- prediction fails this test: "No interaction between the known baryons (or other elementary particles) and a hypothetical Y was mentioned by Gell-Mann to the effect that Y should have certain physical features..." (Bangu 2008, p.248). Instead, the Reification Principle was used to derive the properties of the Ω^- (Bangu 2008, p.248):

> (RP) If Γ and Γ' are elements of the mathematical formalism describing a physical context, and Γ' is formally similar to Γ, then, if Γ has a physical referent, Γ' has a physical referent as well.

But justifying the RP is problematic: "...there is no naturalistic explanation why RP could work, while the very idea of a higher-level correspondence between mathematical objects and reality reminds us of numerology, astrology, and other dubious practices involving reifying mathematics..." (Bangu 2008, p.250). He believes that the RP is particularly difficult to sustain from the position of a naturalist or empiricist, given that it suggests a direct correspondence between mathematical structure and physical entities in a way that requires the reification of said structures.

Bangu further rejects a heuristic reading of the RP. Such a view states that the principle was used only to produce a hypothesis rather than fully justify it, rendering it sufficient in the context of discovery. Bangu argues that this is untenable, because the situation inverts the relationship between the context of discovery and justification: the Ω^- was first postulated through theory, and was only later confirmed in empirical experiments. In particular, he believes a "...heuristics typically helps us discover the equation of X, not X itself—getting to X itself is possible only via a very problematic reification step..." (Bangu 2008, p.252). Bangu claims that the prediction of the Ω^- and application of the RP serve a justificatory rather than heuristic role in the argument. Because of the non-standard inference made, he advocates for a position of methodological opportunism in scientific practice, according to which no epistemological system needs to be followed in developing scientific theories (Bangu 2008, p.256).

Overall, Bangu's account of the prediction strikes me as somewhat inaccurate. One minor point is that there *were* in fact anomalous observations that required explanation: in his recollections of the 1962 conference, Ne'eman stated that, "Returning to his seat, Gell-Mann passed near us, recognized Yehuda [Eisenberg] and exclaimed ⟨⟨this might also fit your cosmic-rays event!⟩⟩ Eisenberg had indeed probably observed an Ω^- back in 1954..." (Ne'eman 2010, p.369). However, given that this anomaly was not a driving factor in Gell-Mann's prediction of the Ω^-, Bangu's argument is not significantly affected.

More importantly, I agree with criticisms from Bueno and French (2018), who take issue with Bangu's rejection of a heuristic characterisation. Firstly, it is clear that scientific discovery does not always begin with an anomalous entity or phenomenon. Take, for example, the prediction of the positron, which was based on negative energy solutions to the Dirac equation, presenting another instance of scientific discovery based on mathematical features of the theory, though involving group theory in a less direct way than the Ω^- prediction. Furthermore, I agree with Bueno and French's claim that a sharp distinction between contexts of discovery and justification cannot be maintained in this case, with the development of the Eightfold Way intertwining these contexts in an inseparable way. I believe both of these issues are highlighted by scrutinising Bangu's claim that the Ω^-'s properties were not inferred through physical interactions. This was his motivation for introducing the Reification Principle, since the physical properties of the Ω^- were only deduced through mathematical means based on its position in the baryon decuplet.

Firstly, many contemporary scientific discoveries are made *without* the initial observation of anomalous phenomena. High energy physics (and much of fundamental physics in general) no longer involves macroscopic phenomena that are easily observable. To even *locate* anomalous phenomena, contemporary experiments are usually dependent on an extensive theoretical basis to suggest a relevant domain of experimentation—consider the search for the Higgs boson at the LHC, or the discovery of gravitational waves at LIGO. Neither of these cases can be attributed to a single anomalous phenomenon. So it seems problematic to define heuristics and scientific discovery under such narrow terms, and undermines the sense in which the Ω^- case is uniquely problematic.

An aspect unique to the Eightfold Way also motivates me to reject the RP as essential to the prediction. It is important to remember that the model was in a stage of relative theoretical immaturity: seen mainly as a classification scheme for families of hadrons, it was not widely accepted as an adequate description of actual physics. There were competing models such as the Sakata model making different empirical predictions. Heuristic approaches in this context seem perfectly natural; the prediction of a new entity in this context, to make the theory amenable to empirical testing, does not strike me as requiring commitment to the actual existence of certain mathematical structures. As I will elaborate on in §6, the tentative nature of the Eightfold Way means that such a prediction was a way of testing the viability of the scheme rather than fully committing to a reification of mathematics.

This point is further emphasised by the complexity of the scenario, involving interactions between theory and data. The Eightfold Way was constructed with constant reference to incoming empirical data—recall that Gell-Mann's prediction of the Ω^- occurred at the same conference where the discovery of the Ξ^* baryons and absence of the $S = +1$ resonance were announced. Particularly because the Eightfold Way was not a comprehensive theory and more of a preliminary model, it appears incorrect to label the prediction of the Ω^- as an inference purely in the context of justification. Its prediction was at least partially aimed at generating more empirical data, rather than solely justifying the Eightfold Way as a theoretical model, which in any case was not the kind of model which physicists were seeking to justify in a comprehensive way[3].

Gell-Mann's prediction was also more comprehensive than Bangu describes. It was not merely an existence claim about the Ω^- based on properties derived from the **10**-representation. Gell-Mann characterised the Ω^- through its mass, derived from an adapted Gell-Mann-Okubo formula, and lists possible interactions with particles from other multiplets: "At 1685 MeV, it would be metastable and should decay by the weak interactions into $K^- + \Lambda$, $\pi^- + \Xi^0$, or $\pi^0 + \Xi^-$...A beam of K^- with momentum $\lesssim$ 3.5 GeV/c could yield Ω^- means of $K^- + p \rightarrow K^+ + K^0 + \Omega^-$..." (Gell-Mann 1962a, p.1). Note that decays occurred through *weak* interactions, underlining the application of theoretical tools beyond that of the Eightfold Way.

So interactions *were* in fact an integral part of the prediction, though perhaps not the initial motivation for suggesting the existence of the Ω^-. In a high energy setting, the description of an $S = -3$, $I = 0$ particle would have given very little for experimentalists to rely on; rather, it was the mass formula along with the particle's decay modes that provided the information necessary for the experiments conducted in Barnes et al. (1964).

This feature emphasises two points: firstly, that prediction is not a straightforward process, with existence claims feeding back into the model to generate further descriptions of the particle based on theoretical considerations, leading to predictions based on theories beyond the model itself. Secondly, it highlights the Eightfold Way's status as a preliminary model that was simultaneously being empirically tested and theoretically extended.

I argue that Bangu has mischaracterised the prediction of the Ω^-, failing to account for the theoretical structure the Eightfold Way is contained in, along with the prediction's role in verifying the model. The Ω^- was not

[3]This process is strongly reminiscent of the interplay between theoretical models and data in Newton's theory of gravitation. For more on this, see the accounts of Newton's developmental process in Harper (2011) and Smith (2014).

just derived from the bare decuplet structure for the $J^P = \frac{3}{2}^+$ baryons, but rather involved inferences grounded in the theoretical structure of the Eightfold Way. Furthermore, it is unclear that his characterisation of heuristics is relevant to the scenario at hand, undermining his rejection of the view.

4 Mathematics as heuristic

Next, I consider Ginammi (2016), who rejects Bangu's reading by emphasising the representational role that mathematics play in the theory, and the heuristic effectiveness of such mathematical structures. To begin, he claims that there is an effective mathematical representation of "...a physical 'structure' or 'system' S if and only if there exists a monomorphism ϕ from S to M, where S includes only the elements of the physical system at issue that are relevant for the physical phenomenon itself..." (Ginammi 2016, p.24). This is because "...we want that the mathematical structure grasps all the relevant elements and all the relevant facts and relations in the physical system, without loss of relevant information..." (Ginammi 2016, p.24).

In the context of the Ω^- prediction, he scrutinises Bangu's claim that there is an anomaly to be explained: "...Bangu evokes a ghost anomaly as if it were real, but in effect we do not have to do here with an actual anomaly...", because there is no real threat to the claim that spin-$\frac{3}{2}$ baryons fit the symmetry scheme until the prediction has been made (Ginammi 2016, p.23). Thus, the prediction seems to be made to *prevent* the anomaly rather than explain one away.

More importantly, Ginammi argues that the problem could take two forms: "...either (A) we could find no particle at all; or (B) we could find a particle not having the predicted characteristics..." (Ginammi 2016, p.24). Ginammi believes that case (A) does not constitute an anomaly, because the mathematical formalism can still play a representative role without being *fully* representative. Only case (B) is a true anomaly, which would create a problem as it directly contradicts the model's theoretical predictions.

But even when a model *does* satisfy the monomorphism condition, we can have still have two issues: which mathematical elements do or do not represent physical entities, and whether we have physical interpretations for *all* elements of the mathematical structure. But in this case, "...we can still use this model (because this is, after all, a model, since it satisfies the monomorphic condition) as a 'hypothesis generator'. In other words, we can still rely on the heuristic effectiveness of this model..." (Ginammi 2016, p.25). This is possible as long as one is able to claim that at least *some* of the mathematical elements such as functions or relations do in fact play a representational role, and that there is a physical interpretation available for these representational elements.

So Ginammi sees the prediction of the Ω^- as deriving from three logical steps: firstly, whether the element Γ of the mathematical structure M plays a representative role at all; that a particular interpretation of Γ is specified; and finally, whether the physical system has a feature corresponding to Γ via the interpretation (Ginammi 2016, p.25). Thus he bypasses the need for the problematic Reification Principle, and instead focuses on the heuristic role of the model to generate new hypotheses. In particular: "What permits us to move from the mathematical level to the physical level is not the mathematical formalism itself, but rather the interpretation we offer of it..." (Ginammi 2016, p.26).

Bueno and French (2018) subsume the case within their framework of partial structures, which describes scientific representation as morphisms between partial mathematical structures and physical systems, leaving the possibility of surplus structure in the mathematical formalism which is uninterpreted (Bueno and French 2018, p.41). Bueno and French's argument is similar to Ginammi's, and focuses on the part played by heuristics in scientific inference: "...one that allows a role for mathematical structure but only in the context of the relevant physical reasoning..." (Bueno and French 2018, p.225).

While surplus mathematical structure in the decuplet led to a heuristic argument for making the prediction of the Ω^-, "...it was not simply a case of observing that the nine known spin 3/2 baryons fit into the scheme and hence there should be a tenth, but of drawing on indirect evidence provided by the success of a major programme in physics..." (Bueno and French 2018, p.226). Thus, Bueno and French argue that there is no need to invoke the RP in making the prediction of the Ω^-, because there is sufficient justification from historical precedent for the heuristic application of mathematical structure to physical systems, thus predicting the existence of the Ω^-.

Tricard (2023) criticises both accounts on the grounds that they are unable to make sense of the *rejection* of the **27**-representation. Because they focus on providing a positive heuristic account of the Ω^-'s prediction, Ginammi, Bueno, and French place little emphasis on the failure of the **27**-representation in predicting a $S = +1$ particle. For example, Ginammi's account is unable to make sense of the wholesale rejection of the **27**-representation based on this failure, because implies that the representation is still valid even though it is incomplete. Nor can Bueno and French's partial structure approach explain why the surplus structure of the **27**-representation led to its rejection. Because these conclusions run counter to the heuristic approaches proposed above, Tricard claims that a "...general, justificatory but constraining principle was at play, allowing them to assume that—in structuralist terms—if a multiplet M is a monomorphic representation of the particle family S, then M is also an isomorphic representation of S..." (Tricard 2023, p.12).

These heuristic accounts undersell the importance of the group-theoretic structure in an additional way: an inherently modal aspect arises given that the possibilities of the model are dictated by the group representations. The interpretation of these representations is less of a choice than these heuristic accounts portray, something I will return to in §7. These accounts are nonetheless valuable for giving a more structural role to the mathematical framework in question. In particular, Bueno and French's attention to the past empirical successes of the Eightfold Way is valuable, underlining a pragmatic reason for physicists to make the prediction. I will seek to further emphasise this point in §6.

5 Structures as inductive

One final account is that of Tricard (2023), who provides a structuralist interpretation of the inference: elements of a mathematical representation related by symmetries should be interpreted *jointly*, granting symmetries a nomological role that justifies inductive inferences. Tricard claims that this view overcomes the difficulties faced by the heuristic accounts due to the more stringent role played by mathematical structures, explaining both the failure of the **27**-representation *and* the prediction of the Ω^-.

For group representations to play a role beyond classification, Tricard claims that the following Structural Assumption (SA) is made (Tricard 2023, p.17):

> **(SA)**: the family of particles S is complete, not only in the empirical sense that it contains all (different) spin-3/2 baryons, but also in the structural sense that the relations $\{r_i\}$, which are mirrored by the transformations of either 10-SU(3) or 27-SU(3), are "jointly" instantiated.

So particles within a family must not only *exist*, but also satisfy a set of relations with other particles in the family. Drawing on Bueno and French, Tricard emphasises that this is grounded in the historical success of the Eightfold Way representing complete families of particles.

Tricard aims to extract an even more fundamental principle which constrains the inference in a non-heuristic way. Define $M = \langle dom(M), \{R_i\} \rangle$ as a mathematical structure containing elements $\Gamma_i \in dom(M)$, and $\{R_i\}$ the symmetry relations defined on $dom(M)$. The symmetric extension of Γ_i through $\{R_i\}$, written $SE(\Gamma_i, \{R_i\})$, contains any elements Γ_j that are in a symmetry relation $\{R_j\}$ with Γ_i.

For a physical system S with features s_i and physical relations $\{r_i\}$, the minimal condition for a good representation of S is that there exists a structure preserving monomorphism ϕ from S to M (essentially Ginammi's criterion). Tricard imposes what he calls the Principle of Symmetric Extension (PSE) as a further constraint (Tricard 2023, p.18):

(PSE) "If there is a $s_i \in dom(S)$ that fits into M such that $\Gamma_i = \phi(s_i)$, then for any other element $\Gamma_j \in SE(\Gamma_j, \{R_j\})$ there is an object $s_j \in dom(S)$ such that $\Gamma_j = \phi(s_j)$".

He claims that this allows for the prediction of new particles within a structured family based on reasons of symmetry, thus justifying the prediction of the Ω^- in the baryon decuplet.

Tricard argues that this is an inductive principle, as it quantitatively generalises physical properties of a system, based on the structural properties of the mathematical elements, to new elements of the physical system. In particular, symmetries are "...not only used as a descriptive tool to capture the structural *properties* of a system. They play the crucial role of a criterion for the nomological and legitimate the induction of a hypothesis, as they capture how different objects *differ* from one another *while all being instances of a same law*..." (Tricard 2023, p.22).

Tricard is careful to avoid the charge of reification, according to which the inference of physical objects' existence from the mathematical formalism of a symmetry scheme is illegitimate. He argues that the "...prediction is not that of an *actual* object, but of a *physical possibility*: particles of a new kind *can physically* be brought to existence in certain conditions—at energy levels that can only be reached in large colliders, through specific collisions and disintegrations, e.g. when 5-GeV kaons collide with protons in a bubble chamber..." (Tricard 2023, p.25).

In particular, symmetries in this context are used to "...bring together different physical phenomena or objects as *instances of the same law*, to formulate precise nomological hypotheses and predictions...", rather than as a genuine reification of mathematical structures, where the symmetries are a result of the Pythagorean existence of group structure (Tricard 2023, p.25). I believe that this is a perceptive claim: the very nature of high energy physics means that the 'existence' of a particle is only deduced from transitory events—the prediction of the Ω^- is shorthand for a series of interactions in particle collisions from which its existence can be deduced.

However, Tricard's account does not refute the view that the prediction is in some sense heuristic, even though he *is* successful in imposing more structure on the inference in a way that he claims is logically binding (Tricard 2023, p.17). The problem is that the application of the PSE itself is dependent on the Structural Assumption, which is justified mainly through the historical success of the Eightfold Way. Whether the PSE can be deployed in the first place, which I take to be the more relevant question, remains something that can only be answered case by case. The overall chain of reasoning leading to the prediction of the Ω^- remains heuristically justified, even though it does obey the helpful constraint of the PSE, along with further rules laid down by the theoretical background.

It seems unnecessary to postulate the PSE once a group representation is taken to generate the model's possibilities. The prediction of the Ω^- simply becomes a case of considering all the states provided by the group representation. Furthermore, Tricard does not account for the Eightfold Way's status as a preliminary model. While it does provide a *good* reason for the SA and PSE to be applied, it was also done so in an exploratory fashion, in order to generate new empirical evidence and to differentiate it from rival models. While the presence of symmetries provides a useful inductive step in the PSE, it ultimately comes down to the Eightfold Way's tentative nature that the prediction of the Ω^- was made.

6 Is this the whole story?

Having evaluated these three existing accounts of the episode, I now present three essential features of the Ω^- prediction not raised by previous authors. Firstly, they undersell the validity of the Totalitarian Principle, even as a background assumption; secondly, these accounts neglect the theoretical framework the Eightfold Way offers, along with its *already* established empirical successes; thirdly, they make little comment of the tentative nature of the Eightfold Way, which provided a motivation for making new predictions even in the absence of deeper justification.

One may want to account for why the possibilities dictated by group representations are expected to occur in experiments. This idea leads to what is known as the Totalitarian Principle (TP): the claim that anything that is not forbidden by nature will necessarily happen[4]. Attributed to Gell-Mann because of a comment in a footnote, its sincerity is difficult to judge (Gell-Mann 1956, p.859):

> Among baryons, antibaryons, and mesons, any process which is not forbidden by a conservation law actually does take place with appreciable probability. We have made liberal and tacit use of this assumption, which is related to the state of affairs that is said to prevail in a perfect totalitarian state. Anything that is not compulsory is forbidden. Use of this principle is somewhat dangerous, since it may be that while the laws proposed in this communication are correct, there are others, yet to be discussed, which forbid some of the processes that we suppose to be allowed.

Both Bangu and Tricard reject the principle.Bangu (2008) notes the Totalitarian Principle has a family resemblance to the Reification Principle: "The historical truth is that Gell-Mann appealed to such a principle consciously...Gell-Mann alluded on various occasions to another virtually

[4]For examples of papers in physics explicitly referring to the principle, see Cabello (2019) and Guo et al. (2023).

identical principle, a contemporary version of Lovejoy's 'Principle of Plenitude' (any genuine possibility actualizes at some moment in an infinite time) called by him the 'Totalitarian Principle': anything which is not (theoretically) prohibited is compulsory..." (Bangu 2008, fn.26). Similarly, Tricard contrasts the PSE with the Totalitarian Principle and Bangu's Reification Principle, seeing both as inadequate explanations of the prediction.

Kragh (2019) is doubtful that the principle played an explicit role in the Ω^- prediction: "Not only does the term TP not appear in the 1956 paper in Nuovo Cimento, it also does not appear in any of Gell-Mann's publications whether scientific or popular...." (Kragh 2019, p.3). Furthermore, "...Gell-Mann initially emphasized that the quark model was a formal scheme and that real quarks did not exist. For several years he thought that quarks were "fictitious" and not real detectable particles..." (Kragh 2019, p.5).

I believe that the principle is less problematic than these authors have portrayed. Because quantum mechanics is a probabilistic theory[5], outcomes that are not explicitly forbidden by the theory have a non-zero probability of occurring. This seems to be the most reasonable justification of the Totalitarian Principle: as long as the theory does not explicitly forbid a process, the probabilistic nature of quantum mechanics implies that with enough interactions, any permissible process will occur.

As noted by Gell-Mann, in an incomplete theory there may exist further restrictions on outcomes which have not been found. However, in the absence of such a restriction, the prediction of the Ω^- seems to arise straightforwardly once the possibility space of the model is defined by group representations of SU(3); the Totalitarian Principle then implies that such a process *will* occur in experiments. I will return to the Totalitarian Principle in §7, where it will play a more general role beyond this particular prediction.

Next, I will consider what the Eightfold Way specifically contributed to the prediction. To their credit, Bueno and French (2018) acknowledge the importance of the scheme, arguing that it was because of the historical success of the Eightfold Way in classifying complete families of hadrons that led to the prediction of the Ω^-. However, there are further details about the scheme's impact worth discussing.

There was precedent for the prediction of new particles based on the Eightfold Way. The initial success of the Eightfold Way was due to its classification of the eight known baryons. This provided the first sign that hadrons could be organised in *complete* families according to the scheme. However, only *seven* mesons were known at the time: the three π, and the pairs of K and $\bar{K}$. Recall that Gell-Mann had deduced the representation of

[5] I will bypass arguments about the exact nature of this probability, which involves interpretations of quantum mechanics.

the pseudoscalar mesons from the following tensor product representation:

$$\mathbf{8} \times \mathbf{8} = \mathbf{1} + \mathbf{8} + \mathbf{8} + \mathbf{10} + \overline{\mathbf{10}} + \mathbf{27}. \tag{1}$$

The higher dimensional representations of **10**, $\overline{\mathbf{10}}$ and **27** did not resemble the known properties of the mesons; Gell-Mann therefore attributed the **8**-representation to the mesons, and predicted the existence of a $I = 0$, $S = 0$ pseudoscalar meson.

Now known as the η, Gell-Mann labelled it the χ^0, arguing: "The **8**-representation, occurring twice, looks just the same for mesons as for baryons and is very suggestive of the known π, K, and $\bar{K}$ mesons plus one more neutral pseudoscalar meson with $I = 0$, $Y = 0$, which corresponds to λ in the baryon case. Let us call this meson χ^0 and suppose it exists, with a fairly low mass..." (Gell-Mann 1961, p.18). Furthermore, the particle "...should decay into 2γ like the π^0, unless it is heavy enough to yield $\pi^+ + \pi^- + \gamma$ with appreciable probability..." (Gell-Mann 1961, p.31).

Experimental evidence for the η was found by Pevsner et al. (1961) through a three-pion decay, but not identified with the η meson until Bastien et al. (1962). As Rosenfeld (1975) states: "This completed the first meson octet, but by later standards it attracted little attention (no press conference, no flurry of theoretical papers, and no 1962 edition of UCRL-8030)..." (Rosenfeld 1975, p.559). Note that essentially the same inferences as with the case of the Ω^- had been drawn—but with the drawback that the decay modes were different from expected and required further theoretical work to justify its identification with the η.

Set in this context, the Ω^- prediction appears less exceptional. Three families of hadrons were already successfully classified by the Eightfold Way by 1962: the baryons, pseudoscalar mesons, and vector mesons. The completion of the pseudoscalar meson octet had required the prediction and confirmation of the η meson. Once the nine known $J^P = \frac{3}{2}^+$ baryons had been discovered, Gell-Mann could simply follow the reasoning used previously to predict the existence of the Ω^-. What made this prediction particularly notable then, perhaps, was the precise characterisation of the Ω^- through the mass formula, as well as the decay states—all of these only possible because of the theoretical framework contributed by the Eightfold Way.

The Eightfold Way provided a theoretical framework within which the prediction of the Ω^- could be proposed. This emerged through multiple avenues: the group-theoretic properties of the baryon decuplet; the physical properties of the Ω^- such as its mass and decay pathways; and the previous success of SU(3) in classifying known particles *and* predicting the existence of new ones. The introduction of the 10-representation was simply a continuation of this theoretical framework.

This leads to my final point: both Gell-Mann and Ne'eman emphasised that the Eightfold Way was a tentative model that should be subject to further empirical verification. The prediction of the Ω^- was thus aimed principally at generating new empirical results that differentiated the model from its theoretical rivals, with relatively little ontological commitment as some authors have claimed.

First, the group representations of the Eightfold Way were initially defined through relatively informal methods, there was no attachment to any underlying physical picture. For example, baryons in Gell-Mann's account were described by a fictitious particle-antiparticle model, with the **8**-representation derived from a $\mathbf{3} \times \bar{\mathbf{3}} = \mathbf{8} + \mathbf{1}$ product (Gell-Mann 1961, p.3). The **10**-representation was therefore not present as a possible representation of baryons. But because Gell-Mann and Ne'eman only committed to viewing the SU(3) symmetry abstractly, they could consider more general representations of the SU(3) group such as the **10**-representation.

Furthermore, empirical results from the period presented distinctly mixed signals. One particularly problematic result was the odd relative parity of Σ and Λ, which seemed to directly undermine the baryon octet—the *main* piece of empirical evidence that the Eightfold Way was correct. Similarly, the Gell-Mann-Okubo mass formula, while providing some empirical confirmation of the Eightfold Way, was being questioned by authors such as Oakes and Yang (1963).

As a result, there was a constant competition from alternatives—the most notable being the Sakata model, which proposed that the p, n, and Λ were fundamental particles which composed the rest of the hadrons. The benefit of this model was its clear ontological picture when compared to the Eightfold Way, which took a structural approach that the authors explicitly rejected any physical interpretation of. Most notably, the Sakata model *also* yielded an SU(3) symmetry, though because the fundamental particles were fixed, the resulting representations of composite particles differed from the Eightfold Way.

A clear piece of evidence for this conflict is Gell-Mann (1962b), which summarised relevant results from his 1961 Caltech Report for a public journal. It was written up *mainly* in the framework of the Sakata model, because of the doubts Gell-mann had about his own Eightfold Way: "...for a while I embraced what was by then the Sakata model, and submitted to the Physical Review a version of my work on flavor SU(3) in which the octet assignment of baryons was almost entirely suppressed..." (Gell-Mann 2010, p.339). So the Eightfold Way was in no way settled as a theoretical model of the strong interaction.

I believe that the prediction of the Ω^- was meant in large part as a *differentiating* factor between the two models. While the Sakata model *also* resulted in an SU(3) symmetry, it made subtly different predictions because of the fundamental particles involved: the possible representations were those of $\mathbf{3} \times \mathbf{3} \times \bar{\mathbf{3}} = \mathbf{3} + \mathbf{3} + \bar{\mathbf{6}} + \mathbf{15}$. The Ω^-, as a result of the **10**-representation, could not arise from the Sakata model; as Ne'eman remarked, "There was no such state in the relevant Sakata assignments or in other models, so the test was a really good one..." (Ne'eman 2010, p.369).

Thus the prediction of the Ω^- was not just a product of heuristic arguments about the symmetry properties of known particles. It was also a result of the Eightfold Way being a preliminary model in need of empirical evidence to differentiate it from a rival model, so that the underdetermination by empirical data could be broken. I believe that this is an aspect which other philosophers have paid little attention to, thereby missing an important motivation for making the prediction of the Ω^- *beyond* mere symmetry properties.

I believe that the heuristic picture presented by Bueno and French (2018), alongside the more stringent rules imposed by Tricard (2023), provide the closest to what I consider an adequate account of the Ω^- prediction. However, I have attempted to flesh this account out with further details of how the Eightfold Way impacted the prediction. Not only did the Totalitarian Principle serve as a reasonable background assumption, the Eightfold Way model itself provided guidance as to *how* the prediction should be made, with the prediction of the η meson an underappreciated forerunner of the Ω^-. The tentative nature of the Eightfold Way further motivated the search for new empirical evidence, with the goal of differentiating it from rival models.

7 Possibility counting from symmetries

With both the development of the Eightfold Way and the prediction of the Ω^- in mind, the following question arises: does the application of group theory imply a certain approach to understanding possibilities within a theory? This explicitly modal aspect to the application of group theory is left largely unexplored in the literature, with the exception of Tricard (2023), who suggests that the prediction of the Ω^- postulates the physical *possibility* of the particle rather than a claim about the existence of a single entity. However, the application of group theory in the context of the Eightfold Way appears to introduce modality more generally, in the sense that symmetries arising from the group *generate* the possibility space of the model. This is the inferential pattern I call *possibility counting from symmetries* (PCS).

First, I register that the kind of possibility in question is ambiguous. It is of a stronger variety than logical possibility, given the constraints introduced by the theory through empirical considerations. On the other hand, because no dynamics have been introduced, and no physical laws have been postulated, it cannot be identified with nomological possibility. The notion of possibility rests between logical and nomological possibility, constraining the possible states of the model through group representations without invoking dynamical laws. The closest established notion is that of kinematically possible models (KPMs), which describe possibilities which contain the right objects in a physical theory, but are not constrained by dynamical laws[6].

The application of group theory suggests a particular approach to possibilities, given significant representational assumptions must be made. First, a vector space that describes the possibilities of the physical system in question is identified. A linear group representation mapping group elements onto automorphisms of the vector space then relates vectors in the space to each other through relevant transformations[7]. Because the theories in question are quantum theories, the vector spaces furthermore have the structure of a Hilbert space, with the dimension of the representation defining the dimension of the Hilbert space.

Representation theory provides a decomposition of the possibility space in terms of the fundamental representation of a group. The fundamental representation, which is usually the smallest dimensional representation that is faithful, can be used to build higher dimensional irreducible representations, since any higher dimensional irreducible representation will be contained within the direct sum decomposition of an n-fold tensor product of the fundamental representation with itself. The theorem that provides the mathematical basis for this claim is the theorem of the highest weight, due to Élie Cartan, which classifies the irreducible representations of complex semisimple Lie algebras. I will not go through a detailed explanation here[8]; however, the upshot is that irreducible representations of a complex semisimple Lie group can be classified up to isomorphism by their highest weight.

To define the notion of weights, note that the maximal set of commuting Hermitian generators H of a given representation of a Lie algebra, such that $H_i = H_i^\dagger$ and $[H_i, H_j] = 0$, is known as a Cartan subalgebra, and its elements the Cartan generators. The eigenvalues associated with the Cartan generators are the *weights* of a representation (Georgi 1999, §6). An ordering for these weights can be defined in an arbitrary basis of the Cartan subalgebra, for which there will be a *highest* weight of the representation invariant across any basis (Georgi 1999, p.104).

Because the highest weight of any representation can constructed from the fundamental weights of a Lie algebra, any irreducible representation of a complex semisimple Lie algebra can be defined in terms of a tensor product of the fundamental representation. Crucially, any irreducible representation of a semisimple Lie algebra is contained within a n-fold tensor product of the fundamental representation, with a similar theorem holding for representations of connected compact Lie groups, presenting a general method

[6]For an amenable proposal describing such possibilities, see the discussion of KPMs in March (forthcoming).

[7]In physics, these are usually *unitary*.

[8]See Hall (2015, Chp. 9) for a full proof of the theorem.

for finding higher dimensional representations of Lie groups.

In the context of the Eightfold Way, the presence of higher dimensional representations of SU(3) can be interpreted as suggesting at an underlying composite model with three degrees of freedom, as a result of the three-dimensional fundamental representation. This also makes sense of the prediction of the Ω^-: given these three degrees of freedom, the possibilities dictated by the **10**-representation include an $S = -3$ particle. One then invokes the Totalitarian Principle, which implies that the possibility will occur given a sufficient number of events—thus, a prediction can be made.

Physicists certainly could have taken the higher-dimensional representations of SU(3) out of context, and used them to represent families of particles without considering the construction of representations through a tensor product of the fundamental representation. However, most physicists chose to at least *introduce* the higher-dimensional representations on the basis of these product representations, even in the absence of a specific composite model. What warrants the prediction of the Ω^- is a form of possibility counting: since the **10**-representation of SU(3), which was already able to account for nine of the baryon resonances, the inherently modal nature of the group-theoretic structure meant that it was justified to suggest the existence of the Ω^-.

How does historical evidence fit with this interpretation? For physicists working on the Eightfold Way, there was a reticence towards attributing a physical interpretation to their mathematical methods. Gell-Mann explicitly rejected a physical interpretation of the baryon model used to derive the SU(3) symmetry: "We shall attach no physical significance to the l and $\bar{L}$ "particles" out of which we have constructed the baryons. The discussion up to this point is really just a mathematical introduction to the properties of unitary spin..." (Gell-Mann 1961, p.17). This attitude can be found in other authors such as Sakurai: "...one should not ask which elementary particles are "more elementary than others," and which compound model is right, but rather characterize each particle only by its internal properties such as total hypercharge and mean-square baryonic radius..." (Sakurai 1960, p.3).

Similarly, in the context of expanding to further families of particles, Gell-Mann explicitly stated: "If some states are lacking in a given supermultiplet, it does not necessarily prove that the broken symmetry is wrong, but only that it is badly violated..." (Gell-Mann 1962b, p.1081). This statement appears to vindicate Ginammi's claim that mathematics can be representative while retaining surplus structure. However, I believe that the approximate nature of the SU(3) symmetry provides a more comprehensive explanation of this stance, preventing a full embrace of the possibility space postulated by the group-theoretic structure. Gell-Mann in particular "took the position that SU(3) of flavor could be abstracted from something like the p, n, and Λ model and used as an approximate symmetry with whatever representations were needed..." (Gell-Mann 2010, p.342).

But there is evidence that Gell-Mann's attitude was not as rigid as the papers suggest. Zweig (1980) recalls an earlier version of Gell-Mann's report, in which the following passage occurs:

> What physical significance do we attach to the $\bar{L}$ and l when we say that the eight baryons N_i transform like $\frac{\bar{L}\lambda_i l}{\sqrt{6}}$ with respect to unitary spin? Certainly we are not claiming that the baryons must be bound states of leptons l and heavy bosons $\bar{L}$ under the influence of some very strong interaction. The leptons show no signs of having any strong couplings, and the heavy bosons $\bar{L}$, carrying baryon number 1 and lepton number -1, have made no appearance. For the time being, we cannot be sure of the physical significance of the analogy we have drawn, except possibly insofar as it concerns the weak interactions (see Section VI). But the physical consequences for the baryons and mesons of assuming the eight representations of unitary spin for the baryons are clear and precise.

This presents a more ambivalent attitude towards the possibility of a composite baryon. While Gell-Mann does not embrace the model that he used to derive the SU(3) symmetry, he does not reject the possibility that there is an underlying composite model either.

Furthermore, composite models for the hadron were prevalent in the theoretical community. Fermi and Yang (1949), for example, postulated that mesons were composed of baryon-antibaryon bound state. The Sakata model, on the other hand, constructed the other hadrons out of the proton, neutron and Λ baryon. Gell-Mann (1962b) was explicit in entertaining the Sakata model as a legitimate option, because of experimental evidence that conflicted with the Eightfold Way.

So my conclusion is twofold: applying group theory to families of baryon was uniquely useful for the prediction of the Ω^-, because it allowed the construction of a possibility space in which the **10**-representation of SU(3) implied the existence of the Ω^-. Though physicists attempted to approach the Eightfold Way from an abstract perspective which did not rely on any composite model of the hadrons, underlying considerations meant that it is likely they had the composition of hadrons in mind.

I now identify this pattern of inference as *possibility counting from symmetries* (PCS). By accounting for the symmetries of a model, group theory provides a notion of possibility counting that can be used to generate predictions about the existence of new states or entities. The inference has a two-step structure: first, a categorisation of existing empirical data is completed through group-theoretic means, just as the **8**-representation categorised the known baryons and pseudoscalar mesons. Second, because the group-theoretic structure generates a well-defined possibility space for the model, predictions regarding possibilities not yet observed are made.

This creates a contrast with the association of symmetries with redundant structure in the interpretation of physical theories. Symmetry-related models of a theory are usually taken to represent the same physical content, leading to the interpretation of symmetry-invariant features of a theory as representing elements of reality. Dasgupta (2016), somewhat ironically, used the term 'symmetries-to-reality reasoning' to describe this process, despite the fact that symmetries are used to argue against taking symmetry-variant features as real. On the other hand, symmetries in PCS inferences *generate* the distinct possibilities in a model, allowing for prediction that lead to further empirical confirmation of the scheme using the form of PCS inferences.

Predictably, there are issues with applying the PCS procedure too generally. Consider an arbitrary spin-1 particle. Because the SU(2) group is identified with spin, it is technically valid to describe the spin-1 representation as resulting from the following tensor product of fundamental two-dimensional representations: $\mathbf{2} \times \mathbf{2} = \mathbf{3} + \mathbf{1}$. While composite particles such as a hydrogen atom can indeed be understood in this way, this does not warrant the inference that *any* spin-1 particle, such as photons and other vector bosons, is composed of two spin-$\frac{1}{2}$ particles.

One distinguishing factor is that the SU(2) symmetry is an exact symmetry, directly connected to representations of the Poincaré group describing spacetime symmetries. While spin is an important quantum number used to classify different families of particles, representations of SU(2) play less of a role categorising particles, the crucial first step in PCS reasoning. More importantly, quantum electrodynamics defines the photon field as an elementary entity; the same applies to vector bosons in other Yang-Mills theories. Unless we find a compelling reason to refine these theories, the possibility space has already been prescribed, and requires no further elaboration. Conversely, the approximate nature of the Eightfold Way motivates the application of PCS: we *know* the SU(3) symmetry is explicitly violated, meaning the model cannot be a fundamental description of the strong interaction. The group-theoretic structure of SU(3) populates the possibilities of the model, in a way which hints at a more fundamental structure with three degrees of freedom—quarks.

So the quark model is arguably a direct result of PCS, enforced by the empirical discovery of the Ω^-. While a preliminary proposal of quarks was made already by Goldberg and Ne'eman (1963), this paper did not find a wide audience due to uncertainty about the Eightfold Way, and its somewhat abstract notation. Rather, Gell-Mann (1964) and Zweig (1964), published in the same year as the confirmation of the Ω^-, would result in the adoption of the quark model. As Gell-Mann comments: "If we assume that the strong interactions of baryons and mesons are correctly described in terms of the broken " eightfold way" [*footnote suppressed*], we are tempted to look for some fundamental explanation of the situation..." (Gell-Mann 1964, p.151).

Because the fundamental representation is implied by the existence of higher-dimensional representations through the theorem of the highest weight, a more fundamental explanation involves the three-dimensional fundamental representation of SU(3). The **8**-representation and **10**-representation classifying the baryons were further evidence of a composite baryon structure, given the decomposition:

$$\mathbf{3} \times \mathbf{3} \times \mathbf{3} = \mathbf{10} + \mathbf{8} + \mathbf{8} + \mathbf{1}. \tag{2}$$

The result was the identification of the degrees of freedom with the up, down, and strange quarks—a theoretical development which would lead to the fully fledged quark model.

8 Other cases of PCS

Having identified the inferential pattern of PCS in the previous section, an instructive question to ask is whether it appears in other contexts. I suggest two successful cases of the PCS inference: the prediction of the positron and the Higgs boson. On the other hand, I believe the cases of supersymmetry, as well as Grand Unified Theories such as Georgi-Glashow SU(5) theory and SO(10) theory, are situations where the PCS inference is problematic.

A form of PCS was used in the prediction of the positron from the negative energy solution to the Dirac equation. While I will not provide a comprehensive account of the inference here, I highlight aspects which demonstrate the application of PCS. Dirac's postulation of his eponymous equation was an attempt to unify the theories of quantum mechanics and special relativity. What Dirac needed was a representation of the Lorentz group SO(1,3) which could describe the behaviour of spin-$\frac{1}{2}$ particles, resulting in application of spinors, whose dynamics are then described by the Dirac equation. A result of the equation is a negative energy solution, based on which the prediction of the positron was made. Group theory, in the form of representations of the Lorentz group, resulted in the prediction of a new entities, demonstrating a case of the PCS inference.

With the success of symmetry principles in predicting the Ω^-, deriving further predictions based on group-theoretic considerations became a prevalent inference for developing the rest of the Standard Model, such as the prediction of the Higgs boson. Postulated as a way of explaining the mass of gauge bosons in Yang-Mills theories, it resulted in the introduction of the Higgs field, a spin-0 boson which leads to spontaneous symmetry breaking of the electroweak interaction, endowing the $W^\pm$ and Z gauge bosons of the weak interaction with mass. So symmetry considerations, alongside the framework of Yang-Mills theory, led to the introduction of the Higgs field. Once again, I believe that this is a case of PCS reasoning, because of the symmetry considerations at play.

The case of the Higgs presents a more complicated relationship with empirical evidence: rather than a straightforward categorisation of experimentally detected particles, it was an inconsistency with evidence regarding certain theoretical features that prompted the introduction of group-theoretic structures. Yang-Mills theory appeared to be a promising way of accounting for fundamental interactions, with the Eightfold Way its most recent success. Features of the U(1) electromagnetic and SU(2) weak interactions further suggested a possible unification into a SU(2) $\times$ U(1) electroweak interaction at high energies. However, symmetry breaking of the electroweak interaction into two separate forces faced the issue of Goldstone's theorem, which states that massless gauge bosons arise from broken continuous symmetries—but no such particles were observed. Inspired by spontaneous symmetry breaking mechanisms in condensed matter physics, the Higgs mechanism was proposed as a way of fixing the issue, achieved through the introduction of a new entity: the Higgs boson.

As a case of PCS, I believe the following symmetry considerations were applied: first, the unification of U(1) and SU(2) groups, which created a shared possibility space in which electroweak theory operates—this drive to unify symmetry groups plays a further role when I consider Grand Unified Theories shortly. Secondly, the Higgs mechanism and spontaneous symmetry breaking were proposed to eliminate theoretical issues arising from unification, constraining and refining the possibility space—thus prompting the prediction of the Higgs boson, which would finally be confirmed in 2012. While the relationship with empirical evidence is less direct than in the case of the Eightfold Way, the PCS inference was still grounded in some form of empirical justification.

On the other hand, a case that demonstrates the limitations of the PCS inference is that of supersymmetry, a category of theories proposing a symmetry between fermions and bosons, such that each existing particle has a corresponding particle partner of the opposite type. The introduction of supersymmetric Lie algebras was directly related to flavour symmetry: with the success of SU(3), physicists tried to connect internal degrees of freedom to external spacetime ones. In particular, many focused on the SU(6) group, which hopefully be able to incorporate both the internal flavour SU(3) group and external spin SU(2) group. However, the Coleman-Mandula theorem constrains the symmetry group of an interacting quantum field theory to factor into the product of the Poincaré group and the internal symmetry group, meaning that it is not possible to use more complicated groups to describe the symmetries of quantum field theories (Dardashti 2021, p.48).

However, the theorem assumes that the symmetries are represented by Lie algebras. One way of evading this is to use *graded* Lie algebras, whose vector spaces can be decomposed into indexed subspaces. The result is an additional symmetry between fermions and bosons—thus, supersymmetry. While supersymmetric gauge theories have been popular as a solution to conceptual problems in high energy physics, such as the naturalness problem, there has so far been no empirical evidence for the existence of particle superpartners. The most prominent supersymmetric theory, the Minimal Supersymmetric Standard Model is widely considered a failure due to the absence of new particles in experiments at the Large Hadron Collider, which were designed to test the possibility of low-mass supersymmetric partner particles.

To what extent is the reasoning contiguous with that used in the Eightfold Way? While both involve the application of arguments based on symmetry to suggest the existence of particles, I believe that there is a significant difference in the inferences, resulting in a failure of PCS. Unlike both the Higgs boson and the Eightfold Way, the postulated symmetry was not derived from existing physical phenomena—there is no evidence of any supersymmetric partner particle—but primarily as a way to avoid a no-go theorem. As a result, the application of PCS seems to be problematic in the absence of a basis of concrete empirical evidence.

I will now look at a slightly more ambiguous case. An alternative (and sometimes complimentary) approach to Beyond Standard Model physics are Grand Unified Theories (GUTs), which unify or simplify the $\mathrm{SU}(3)\times\mathrm{SU}(2)\times\mathrm{U}(1)$ Lie group of the current Standard Model. Examples include SU(5) Georgi-Glashow theory, SO(10) theory, and E_6 theory. These were primarily developed in the 1970s, in the wake of the success of $\mathrm{SU}(2)\times\mathrm{U}(1)$ electroweak unification and the consolidation of quantum chromodynamics—prompting theorists to look for ways to *further* unify the Standard Model's groups into a simple Lie group.

There were several motivation for formulating GUTs. Most immediately, the success of electroweak unification prompted theorists to speculate that the SU(3) group of the strong interaction could be integrated. Other features of the theory were also dissatisfactory, such as the number of coupling constants and parameters: "The $\mathrm{SU}(2)\otimes\mathrm{U}(1)$ gauge couplings describe two interactions with two independent coupling constants; a true unification would involve only one" (Georgi and Glashow 1974, p.438). The arbitrariness of the group structure was another issue; GUTs provided an opportunity to simplify the Standard Model's semisimple Lie group into a simple group. These reasons for unification appear to be essentially *aesthetic*, appealing to the potential simplicity and elegance of GUTs.

Experimental results relating to GUTs have been somewhat mixed. Georgi-Glashow theory is unfortunately empirically inadequate: while most GUTs feature the phenomenon of proton decay, the lifetime predicted by Georgi-Glashow theory is far too short; coupling constants in the theory also unify at far too low energies (Abe et al. 2017). SO(10) and E_6, alongside supersymmetric extensions of many GUTs, remain viable theories, but no positive empirical evidence has been found—mainly because the energy scales involved are beyond our current experiments. I argue that GUTs present an intermediate case for the PCS: one where the empirical evidence is more compelling than the case of supersymmetry, and motivations grounded in historical success, but where the inference still fails to bear out.

The relationship between PCS and empirical evidence is less clearcut; the case of GUTs seems less fraught compared to supersymmetry, given that the strategy of unification had historical success; however, no empirical evidence suggested the necessity of a single simple Lie group. There are many open questions the Standard Model is unable to answer, such as the problem of neutrino oscillations and the presence of dark matter in cosmological observations; but these problems did not *directly* motivate the introduction of new group-theoretic structures for GUTs in the same way as the Eightfold Way, or even the Higgs boson. Many of these problems had not been explicitly identified when GUTs were formulated in the 1970s. We are of course fully aware of these problems now, which perhaps renders the exploration of GUTs more pertinent: for example, because the right-handed neutrino may help address the problem of neutrino oscillation, there is more viability to applying PCS in the context of SO(10) theory.

Thus PCS becomes a problematic form of inference in the absence of evidence justifying the introduction of new group-theoretic structure. I would propose that PCS is only fully justified in cases where the evidence *directly* relates to the introduction of new group-theoretic structure, whether this is in a positive way where the structure is imposed on existing empirical data, or in a negative way where a problem in empirical data demands the introduction of new structures and entities. I believe that paying attention to the inferences made in preliminary models such as the Eightfold Way leads to a helpful perspective on our current theories: we thereby analyse entrenched methods and inferences such as PCS that theorists use to push the field forward, and scrutinise their viability in novel contexts—hopefully leading to new perspectives in cutting-edge physics.

9 Conclusion

The prediction and discovery of the Ω^- baryon in the 1960s cemented the success of the Eightfold Way, directly leading to the development of the quark model. The inference to the existence of a new particle was based on group-theoretic concepts, with representation theory chief among them. While existing accounts tend to emphasise either the role of reification or heuristic reasoning in the context of discovery, I believe that the mathematical features of the prediction, coupled with the need to distinguish the Eightfold Way model from its competitors, justified the inference to the existence of the Ω^-.

I have argued instead that a novel inferential pattern of *possibility counting from symmetries* (PCS) was used in the prediction of the Ω^-: by imposing a group structure through linear representations on a given space, we generate the possibilities of a physical model in a way that justifies predictions about new entities. While the predictions of the positron and the Higgs boson suggest a broader utility to PCS reasoning, the failure of cases such as supersymmetry and grand unified theories demonstrate the need for a stronger empirical basis when developing extensions to the Standard Model on the basis of PCS.

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