Symmetries as grounds for induction: the case of the Ω– baryon

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1. Introduction

The roles and uses of symmetries in physics are now widely studied. Since Van Fraassen (1989), it is customary to distinguish two non-exclusive uses of symmetries: (1) as *properties*, either of physical situations with symmetric structures, or of laws obeying symmetry principles; (2) as grounds for *arguments*, to simplify and solve a problem on the basis of its symmetries, or by relying on the rule that the symmetries of a situation cannot be broken without a reason. Symmetries can also have different status and play different roles in a theoretical framework: classificatory, normative, unifying and explanatory (Brading & Castellani, 2003). Although a large variety of historical cases naturally fit into these categories (Hon & Goldstein, 2006), the prediction of the omega minus particle (Ω^-) requires further examination, as it reveals a role of symmetries that has not been emphasized before. Based on this case, the aim of this paper is to show that, under certain conditions, mathematical symmetries offer a ground for inductive-nomological reasoning, as they make it possible to formulate laws that are held to be confirmed by empirical data, and to make the subsequent predictions.

The successful prediction of Ω^- by Gell-Mann and Ne'eman in 1962 is a paradigmatic episode where mathematical symmetries played an astonishing role indeed. Since the pioneer work of Wigner (1939), mathematical groups had been used to classify already known particles into families by capturing their internal symmetries. Symmetries were used as *properties* of physical objects for *classificatory* purposes. But in the 1962 episode, symmetries have also been used to predict the existence of unknown particles. At the CERN conference in Geneva, nine spin-3/2 baryons had already been detected and fitted into a 10-place diagram of the SU(3) group (a pyramid decuplet), leaving only one spot unoccupied. According to Ne'eman's report, Gell-Mann predicted that a tenth particle Ω^- existed, with the right characteristics, to fill the last spot in the mathematical representation, and physicists "saw the pyramid being completed before their very eyes" (Ne'eman & Kirsh, 1996, p. 203). Two years later, Ω^- was detected in the Brookhaven's synchrotron (Samios & Fowler, 1964). How to account for this predictive use of symmetries, and what is the form of the argument here?

These questions interact with a problem which Bangu (2008) raises about the justification of this prediction. It does not seem legitimate to predict the very existence of a particle just to fill a diagram, only guided by a sense of mathematical beauty or completion. So, is there a methodological principle which scientists can legitimately rely on to make such predictions? This problem divides into two questions. First, which methodological principle is best assumed at the basis of these predictions, which allows to reconstruct them in the most adequate way?[2]

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[2] This is not an historical nor psychological question, as I won't inquire about what went on the scientists' minds. I am only asking for a strong enough principle to support the predictions that physicists actually made, but not too strong either, so as

Second, once the adequate principle has been brought to light, a *de jure* question must be raised: is it an acceptable principle in scientific methodology, i.e. is this kind of prediction legitimate or not?

Bangu (2008) argued that Ω^-'s existence can be predicted following standard (and rationally acceptable) *abductive* lines, but that its characteristics require a non-standard and highly problematic "Reification Principle" (RP), which allows to "jump" from mathematics to reality. Ginammi (2016) claimed that no general principle was at work in such predictions, but only a "heuristic strategy". In the same vein, Bueno and French (2018) argue that the episode is adequately analyzed as a pragmatic use of a mathematical "surplus structure" (Redhead, 1975), which can be either left hanging or physically interpreted, depending on contextual physical considerations.

I think that none of these accounts adequately model what happened in 1962. In section 2, I recount the episode in greater detail, outlining the precise inferences that scientists made and that need to be accounted for. I then argue against Bangu that the existence and characteristics of Ω^- were not predicted along abductive lines (section 3), nor grounded on a dubious reification principle (section 4). In section 5, I argue against the heuristic accounts, which overestimate the degree of freedom of predictive practices in the episode, which actually follow a more constraining methodological principle.

My positive claim is that the Ω^- episode reveals an uncharted role of symmetries as grounds for an inductive-nomological reasoning, corresponding to a new kind of symmetry arguments. From apparent symmetries between detected objects of a same family, and assuming specific structural constraints on the relations among its members, a rule of induction which I call (PSE) (for "Principle of Symmetric Extension") states that an object of this family cannot *alone* exist and fit into a symmetry scheme, but only in symmetry relations with a definite number of symmetric counterparts. This principle thus allows to *relationally* predict the existence of other members in the family, not as separate entities, but as *relata* of already detected members (section 6). I then show that the Ω^- episode can be adequately accounted for with this principle (section 7). Turning to the *de jure* aspect of the problem, I then claim that (PSE) is perfectly legitimate because it is nothing but an inductive principle, in which symmetries merely play the role of a criterion for the nomological (section 8). In the last two sections, I defend (PSE) against two possible objections, which accuse it of being Pythagorean-inspired (section 9) or some sort of "Reification" principle in disguise (section 10).

2. The 1962 episode: multiplet rejections and omega's prediction

Let me to start by relating the 1962 episode in greater detail, to underline precisely the inferences that the scientists actually made, and which will have to be accounted for. At that time, the rotations of the SU(3) group were already used to group baryons into families (multiplets), which members are equivalent with respect to the strong nuclear interaction. Mathematically speaking, baryons which are classified into a same family are described as all transforming into each other according to the symmetries of the group, while preserving a quantum number (Unitary spin). They can be represented by a multiplet diagram which is an "irreducible representation" of this group. As already said, nine spin-3/2 baryons had already been detected and it was accepted at the conference that they were already too many to fit

not to ground predictions that they haven't made.

into an octet diagram of SU(3) (as previously known baryons and mesons had done) (Ne'eman & Kirsh, 1996, p. 199).

These nine detected Δs, ∑s and Ξs baryons fitted equally well into both remaining diagrams of SU(3), a pyramid decuplet and a 27-place multiplet, and at the beginning of the conference it was unclear which diagram to choose between the two:

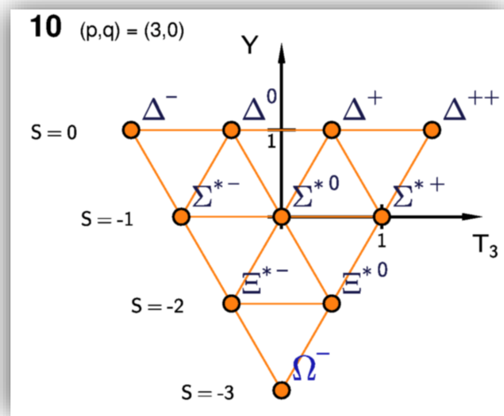

Diagram 1: The spin-3/2 baryons decuplet
Source: http://backreaction.blogspot.com

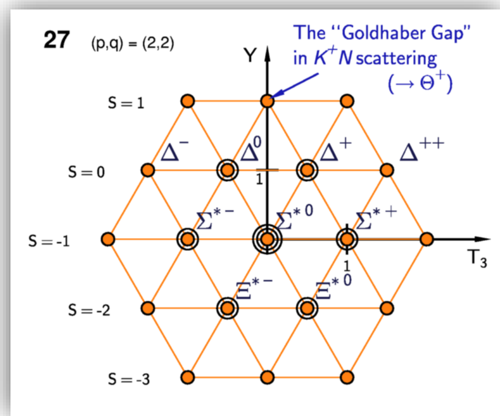

Diagram 2: The spin-3/2 baryons 27-plet
Source: http://backreaction.blogspot.com

However, an experiment had been conducted at Berkeley right before the conference to detect a baryon (Θ^+) which would fit only into the 27-plet and not the decuplet. At the conference, Gerson and Shoulamit Goldhaber reported it as missing, and from this "Goldhabers' gap" scientists inferred that the 27-plet was to be rejected as a whole. They gave up predicting any particle that would belong only to the 27-plet, and assumed now that no such particles exist. This inference has not been sufficiently emphasized and will prove crucial to my argument below.

From this failure, scientists turned to the decuplet as the only adequate representation of the baryons family and thus predicted the existence of the Ω^- particle. As Ne'eman tells it: "the lack of a positive strangeness (…) resonance in the Berkeley experiment was now pointing unambiguously to a decuplet" (Hargittai & Hargittai, 2004, p. 48). Two important features of Ω^-'s prediction must be underlined. First, it is a consequence of the more general prediction that all ten particles described in the decuplet exist. As nine were already known, this is equivalent to the prediction that the last one, Ω^-, exists. But this should not obscure the fact that the number of particles was essential to the prediction: if the spin-3/2 baryons are not 27, then there are exactly 10. Second, Ω^-'s characteristics are predicted according to the purported symmetries of the system: its strangeness, charge and mass need to differ from those of the other nine in some precise way, such that it keeps the same Unitary Spin as them.

To sum up, three inferences were made at the conference:

(1) From the nine detected Δs, ∑s and Ξs baryons, the octet diagram was rejected.
(2) From the "Goldhabers' gap", the 27-plet was rejected as a whole, which implies in particular that no particle exists which would belong exclusively to the 27-plet.
(3) From the nine detected Δs, ∑s and Ξs baryons and the "Goldhabers' gap", the existence and characteristics of a tenth baryon (Ω^-) was predicted.

Now, let me set the broad structuralist framework in which the episode has traditionally been analyzed.[3] The rotations of the SU(3) group are used to (at least) *classify* the known particles, and this classificatory use of symmetries is well analyzed in terms of a mathematical representation *M* of a physical system *S*. Under which condition a mathematical structure *M* can be considered a *good* representation of a physical system *S*? According to the structural or "mapping account", representativeness is properly captured by a structure-preserving morphism ϕ from *S* to *M*, by which the relations in *M* "mirror" corresponding relations in *S*, in the sense that both sets of relations form the same structure.

Ginammi (2016) convincingly argues that a *minimal* condition for a *good* representation is that ϕ be an injective morphism (a *monomorphism*), so that every element of *S* is represented by a distinct element in *M*. This monomorphism condition well captures the first inference which scientists made at the 1962 conference: as already nine spin-3/2 baryons were known to exist in the family *S*, no monomorphism could take place from *S* to *M* if *M* is an octet diagram of SU(3). The rejection of an 8-place representation is thus already well accounted for and I will consider it unproblematic.

When *M* is used for classificatory purpose only, it may possess "excess" or "surplus" structure, i.e. redundant elements and relations which don't represent anything in *S*. This doesn't prevent *M* to be a good representation of *S*, as the latter is effectively "immersed" in the former. If it turns out that ϕ is bijective, then it is a structure-preserving one-to-one correspondence (an *isomorphism*) with no redundant content in *M*.

Returning to inferences (2) and (3) above, it's apparent that they both follow the form: "if ϕ is a monomorphism, then ϕ is an isomorphism". Indeed, Ω^-'s prediction (3) runs like this: all particles detected so far fit monomorphically into the decuplet *M*, so all places in the decuplet are occupied by an actual particle of *S* (isomorphism). Similarly, the rejection (2) of the 27-plet from the Goldhabers' gap follows the line: there is one element of *M* having no physical referent (and thus being pure excess structure), i.e. not all spots of *M* represent actual particles of *S*, so no particle of *S* fits well into the 27-plet - which is equivalent, by contraposition, to the form of Ω^-'s prediction. So, let me turn now to the existing accounts, to see on which principle they attempt to base these inferences.

3. An abductive inference?

We find in (Bangu, 2008) two distinct attempts that reconstruct Ω^-'s prediction as an abduction, i.e. an inference whose premises state observed facts and whose conclusion explains these facts and contains the existence of Ω^-.

First, Bangu claims that Ω^-'s existence (though not its characteristics) can be predicted along abductive lines, following Neptune's example. When an observed fact appears as an anomaly, because it differs from what predicts a well-accepted law, it is legitimate to change the initial conditions rather than to reject the law, and to postulate an unknown physical object in interaction with which the anomalous phenomenon is "explained away". But this abductive pattern does not apply here, because there is simply no *observed* anomaly that the postulation of Ω^- could be called to explain. Bangu says that the anomaly is "the lack of a physical referent" for the apex of the decuplet (2008, p. 247), but as Ginammi judiciously notices, this is a "ghost anomaly" (2016, p. 23), because when Gell-Mann predicted Ω^-, it had

[3] This framework is explicit in (Ginammi, 2016) and (Bueno & French, 2018) – not in (Bangu, 2008). But as from now on I will refer to the SU(3) multiplet as a mathematical representation *M* of the physical system *S* (the baryons family), it is useful to set this framework now.

not even begun to be sought in laboratory. The "hunt" only began in 1963 in the new Brookhaven synchrotron.[4]

Second, Bangu also suggests another and quite different abductive scheme: starting from an observed regularity, one considers that it would be a highly improbable coincidence if this regularity did not correspond to a law, and thus infers a law-like generalization as its explanation. Here, the observed regularity would be that (R) "All already detected spin-3/2 baryons fit into the decuplet", from which it is inferred that (H) "All spin-3/2 baryons fit into the decuplet" (Bangu, 2008, p. 247).[5] Hon and Goldstein (2006) suggest a similar account, with a more explicit emphasis on the role of symmetries: all detected particles have characteristics with apparent symmetries, as they all transform according to the rotations of the SU(3) group. This regularity would be an implausible coincidence if these symmetries were not the correct classification scheme, so it is reasonable to infer that (H) all spin-3/2 baryons respect this scheme (Hon & Goldstein, 2006).[6] Once (H) is abductively inferred, the prediction of Ω^- should follow deductively.

But it doesn't. Even granting that this kind of abductive inference has a strong appeal, it does not allow to predict Ω^- at all. From (H), it only follows that *if* a new spin-3/2 baryon exists or is detected, *then* it will fit into the scheme, but not that a tenth baryon exists *simpliciter*. To predict the existence of Ω^- and to "read off" its characteristics directly from *M*, one needs to rely on a generalization like (H'): "For every place *Γ* in the multiplet *M*, there exists a corresponding particle in *S* with the appropriate characteristics". (H') differs from (H) in one crucial respect: it ranges not over all the particles of *S*, but over all the places in *M*. In structuralist terms, while (H) states that the whole baryon family *S* is monomorphically embedded *in M*, (H') asserts that there is a monomorphism *from M* to *S*. Without (H') and from (H) alone, it doesn't seem possible to predict Ω^- from an empty spot in *M*.

So, if (H') is needed, the next question is: could it be legitimately inferred as an explanation of the observed regularity (R)? There are reasons to doubt that. First, (R) cannot even be explained by (H') alone. Even admitting that for each place in *M* there exists a corresponding baryon in *S*, it doesn't follow that if *x* is an observed spin-3/2 baryon belonging to *S*, then *x* has its place in *M*. This obstacle can be overcome, as the regularity (R) can be inferred from the *conjunction* of (H) and (H'). Together (H) and (H') assert that there is an invertible one-to-one correspondence (an isomorphism) between *S* and *M*.[7] Yet, a second problem arises: (H) alone may be accepted as a nomological explanation of (R), but *not the conjunction* of (H) and (H'), because (H') adds no explanatory content. How can a "fact" about the elements of a representation *M* offhand explain anything about particles in *S*? For *M* to acquire such an

[4] What may be troubling here is the fact that in 1962, Ω^- was already perceived as "missing", i.e. virtually occupying the apex of the pyramid but not detected yet. However, this cannot be regarded as a *fact* to be explained, because Ω^- wasn't missing in the sense of an experimental failure, since it was not yet hunted. Ω^- was "missing" only in the sense that scientists believed that Ω^- should exist, which is precisely not a fact to be explained, but the hypothesis to be justified.

[5] Bangu brings up the objection that "fitting into the decuplet" is most likely not a "natural" property, so (H) doesn't express a relation between natural properties or kinds, and therefore doesn't qualify as a law-like generalization (p. 248). Yet (and for now), "fitting into the decuplet" need not mean more than the fact that baryons "have determinate masses, charges and strangenesses" such that they form a mathematically describable regularity. They all "fit into the decuplet" very much like different gases all "fit the mathematical relation: PV= αT", by having their volumes, temperatures and pressures, which (presumably) are natural properties.

[6] Hon and Goldstein label this abductive use of symmetries as "heuristic", but the disagreement here is largely a question of vocabulary. They construe Ω^-'s prediction as a symmetry *argument*, which is based on a rule of inference and is not a simple method of discovery; and what they call a "heuristic uses of symmetries" may very well be called "symmetry-based abductions": it is a matter of inferring "backward from the effect to the cause" (Hon & Goldstein, 2006, p. 430).

[7] That when there are monomorphisms (or injective functions) in both directions between *S* and *M*, there is an isomorphism (a bijective function) between them, is true if *S* and *M* are sets (Schröder–Bernstein theorem), but is not always true in Category Theory.

explanatory power, it is necessary to assume, beforehand, that these mathematical "facts" in *M* objectively reflect *physical facts* about particles in *S*, which is precisely what is at issue here.

Thus, an abductive reconstruction of Ω^{-}'s prediction seems inadequate, because there is no *previously* observed anomaly that Ω^{-}'s postulate helps explain away, and because the hypothesis (H') cannot legitimately be inferred as an explanation of some observed regularity. Regarding the second attempt, it may be valid to abductively infer from experimental data that a symmetry scheme is *correct* to classify a family of given particles, but abduction is not enough to infer that the symmetry scheme must be *complete*. Thus, the methodological principle behind Ω^{-}'s prediction is not that of abductive reasoning.

4. A Pythagorean, analogical principle?

From the previous section, it appears clearly that the main jump leading to Ω^{-}'s prediction is from (R) to (H'). Since (R) implies that (R') "Some places *Γ* in *M* are actually occupied by existing particles", then the inferential step that must be justified is from (R') to (H'). It is a sort of generalization *within* the representation, from the fact that *some* places in *M* are occupied to the hypothesis that *all* are. Perhaps scientists relied here on what Bangu calls a "Reification Principle":

> **(**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" (Bangu, 2008, p. 248).

From such a principle and (R'), it is surely possible to infer that (H') all spots in *M* have corresponding particles as physical referents, and thus to predict the existence and characteristics of Ω^{-}. To borrow Steiner's term, (RP) is a "Pythagorean" principle, which expresses the belief that our mathematical formalisms are not just computational or classificatory tools, but also reliable means of tracking truth about physical reality.[8] (RP) is also a "reification" principle, because it allows to "read off" the existence and characteristics of a physical entity directly from the representation. Finally, (RP) is "analogical", for it relies on the "formal similarity" (Steiner) between mathematical elements. In this context, two elements are regarded as "formally similar" if they are both solutions to the same equation[9] or both belong to the same mathematical structure. In Ω^{-}'s case, the apex of the pyramid is formally similar to elements with already known referents, as they all are elements of the same mathematical structure, which leads to predict the new particle.

Bangu claims that (RP) is at work in Ω^{-}'s prediction, but also, against Steiner, that (RP) is not methodologically sound, as it appeals to "quasi-mystical correspondences between mathematical objects and reality" which contradicts "the typical naturalist/empiricist" methodology and amounts to a modern form of superstition.[10] Moreover, as a general principle, (RP) in simply false, since there are numerous cases where it obviously fails (Bangu, 2012, p. 87) (Ginammi, 2016, p. 23). Ginammi also points out that its application relies on the all too vague notion of "formal similarity", which according to Ginammi "we cannot specify it in a *non-ad hoc* way" (2016, p. 23). I agree with all these reasons to reject (RP), and if such a

[8] "Prediction today, particularly in fundamental physics, refers to the assumption that a phenomenon which is mathematically possible exists in reality—or can be realized physically. (…) In short, the concept of 'prediction' has itself become thoroughly Pythagoreanized" (Steiner, 1998, pp. 161-162).

[9] This is the case with Dirac's prediction of the positron, see (Dirac, 1931, p. 60), (Dirac, 1930, p. 361) and (Bangu, 2012, p. 95).

[10] See (Bangu, 2008, p. 250) and (Bangu, 2012, chap. 5 and 6).

principle were indeed at work or needed in Ω^-'s kind of predictions, they "should be regarded as (mathematical) prophecies rather than (scientifically respectable physical) predictions" (Bangu, 2008, p. 250).

It is not easy to determine whether scientists like Gell-Mann did have such a principle in mind.[11] Regardless, the question that I am asking is rather whether an (RP)-based reconstruction of the episode is consistent with what physicists actually did. As I said, (RP) is strong enough to predict Ω^-'s existence, but it also raises serious problems.

First, (RP) doesn't account for the rejection of the 27-plet from the "Goldhabers' gap". Suppose that (R') "Nine places *Γ* in the 27-plet are occupied by existing particles". The conjunction of (R') and (RP) implies that (H') "For every place *Γ* in the 27-plet, there exists a corresponding particle in *S* with the appropriate characteristics", which in turn implies that the particle Θ^+ exists. Then, from the experimental proof that Θ^+ didn't exist, it only follows by *modus tollens* that "*Not all* places in the 27-plet correspond to existing particles", and not that "*No* place in the 27-plet corresponds to an existing particle"; the 27-plet wouldn't be rejected as a whole, as it actually was. Moreover, from non-(H') scientists should also have concluded by *modus tollens* that either (R') or (RP) is false. But (R') cannot be considered falsified, because the nine detected particles still have the appropriate masses, charges and strangenesses to fit into the 27-plet.[12] And (RP) cannot be abandoned either, because once the 27-plet was rejected, scientists turned to the decuplet to predict Ω^-, for which by hypothesis they still needed (RP). Thus, a (RP)-based reconstruction fails to give a clear picture of how and why the 27-plet was rejected in favor of the decuplet.

Second, when scientists formulated predictions from a *n*-plet, they explicitly predict the *number* of baryons in the spin-3/2 family. That there are 10 or 27 particles is an *essential* part of the prediction, whereas (RP) only *accidentally* predicts the number of particles. Suppose that, *before* the Goldhabers' failure was reported, Ω^- had been detected along with the first nine spin-3/2 baryons, all then filling the decuplet. Then, right at the beginning of the conference, the scientists would have found themselves in front of a cross-road: either the spin-3/2 baryons family is now complete and contains no other member, or there are still unknown members, which are *exactly 17* (to fill the 27-plet). In this situation, which differs from what actually happened only in the chronological order of the initial events, the prediction and hunt of the Θ^+ particle would have had the value of a *crucial experience*, able to decide between two mutually exclusive and collectively exhaustive hypotheses. And the Goldhabers' failure would have sealed the victory of the decuplet over the 27-plet. The problem is that we do not see how (RP) alone could justify this reasoning in a crucial experiment. Although (RP) is strong enough to predict that *each* of the *n* empty spots in a multiplet has a physical referent, and thus that there exists *n* more particles, it does not allow to predict that *none* of these particles exist if the *n-1* others don't. There is something in the inferences that were made that (RP) is unable to capture here.

Third, whereas Bangu claims that (RP) "plays the crucial role of giving precise indications about the characteristics of the new particle" (2008, p. 248), this principle allows to predict

[11] Note that (RP) is very close to the famous "Totalitarian Principle": "Anything that can happen does happen" (Ford, 1963), which is often attributed to Gell-Mann, although its origin is unclear (Kragh, 2019). One finds in (Gell-Mann, 1956) the following claim: "Among baryons, antibaryons, and mesons, any process which is not forbidden by a conservation law actually does take place with appreciable probability." Following Kragh (2019), the "Totalitarian Principle" is best understood as some kind of "plenitude principle", which has been used to predict the existence of physical objects or effects, which theories contained as mathematical possibilities (and with varying degrees of success: black holes, pulsars, tachyons, etc.).

[12] I will consider later a sense of "fitting into" in which (R') can be understood not as a fact but as a falsifiable hypothesis. But it will also appear that on this condition, (RP) becomes useless.

only that *Γ′* has a physical referent with the adequate physical characteristics, without telling what those characteristics must be. These must then be read from the coordinates of *Γ′* in the Isospin∗Strangeness plane. However, Ω^-'s characteristics are *essential* to the prediction for symmetry reasons. In their "Eightfold way" model, Gell-Mann and Ne'eman posited that the spin-3/2 baryons could be classified with the transformations of the SU(3) group and conceived of as different states in which a spin-3/2 baryon can be, while preserving its Unitary Spin. So, the particles' characteristics in a same multiplet have to differ in such a way that their Unitary Spin is preserved, and these symmetries determine Ω^-'s predicted characteristics. But the notion of "formal similarity" in (RP) is too poor to capture this symmetry argument: it only expresses the fact that *Γ* and *Γ′* belong to *the same* multiplet, but not that they have precisely defined *differences*. Therefore (RP) also fails to capture *how* Ω^-'s characteristics are predicted.

So far, I have shown that the rejection of the 27-plet and the prediction of Ω^- don't seem two follow abductive lines nor a Reification Principle. The existing accounts fail to provide with the required justificatory principle. So, perhaps no such principle was really at play, but simply a heuristic strategy?

5. Heuristic accounts: is there a general principle at play?

Let me turn to the heuristic accounts, according to which predictions from a multiplet structure rely on nothing more than a heuristic rule, which provides a guideline for discovery and theory construction, but plays no strong justificatory role. If it is the case, then Bangu's problem loses much of its gravity, for such a rule, however questionable, represents no challenge to scientific rationality. On the contrary, I shall argue that these predictions are indeed based on a general principle, which *justifies* all the predictions from a same multiplet at once, and confers to their empirical verification (or failure) a confirmatory (or refutational) force.

5.1. Ginammi's "heuristic strategy"

Ginammi (2016) embraces an explicit structuralist approach of how a multiplet diagram can be used to represent a family of particles. He reconstructs the prediction of a new particle as a sort of "representativity test" which can be performed on each empty spot of the multiplet, on a case-by-case basis.

At the 1962 conference, the nine detected Δs, ∑s and Ξs baryons fitted equally well into a decuplet and a 27-plet. In both cases, with *S* being the family of spin-3/2 baryons, there was a monomorphism taking place between *S* and both diagrams, which both contain excess structure. According to Ginammi's monomorphism condition, and although they differ in the number of empty spots, both diagrams were *good* representations of *S*, and scientists had no reason to believe that these unoccupied places *should* have physical referents or that particles were "missing". This situation does not prevent scientists from trying to discover new particles, following a three-step strategy.

First, one hypothesizes that some uninterpreted element *Γ′* in *M* plays a representative role. Second, one defines a physical interpretation for *Γ′* by "reading out" its characteristics from *M* and third, one predicts that there is a particle in *S* which fits this description. This strategy can be applied on a case-by-case basis, and doesn't rely on any general principle which would imply to fill *all* the empty spots in *M*. Ginammi then distinguishes three possible experimental outcomes and conclusions:

- **Success**: the particle exists with the predicted characteristics. One only concludes that this particle exists and that *Γ′* plays a defined representative role.
- **Type-A failure**: no particle is found. One only concludes that *Γ′* doesn't represent anything, but still holds *M* as a good representation of *S*.
- **Type-B failure**: a particle is found but with different characteristics, and doesn't fit into *M*. Since the condition of monomorphism is violated, one rejects the whole representation.

In his account, only a type-B failure has an implication on the multiplet as a whole (its rejection), whereas a success and a type-A failure are only significant for the place *Γ′* being tested. Yet, as already noted by Bangu (2008, p. 253), the successful detection of Ω^- has been interpreted by scientists not only as establishing the fact that a new particle existed, but also as confirming the whole model: "the accomplishment most remembered and acclaimed was the discovery of the omega minus", which gave "an irrefutable proof of the validity of the theory" (Ne'eman & Kirsh, 1996, p. 204). This does not prove that the scientists *legitimately* draw this conclusion, but certainly that Ginammi's reconstruction doesn't match their actual inferences.

This is even more apparent with the rejection of the 27-plet. The Goldhabers' gap is a type-A failure. Following Ginammi's strategy, the scientists were only bound to conclude that the place Θ^+ of the 27-plet had no physical referent and played no representative role. Because the monomorphism condition was still met, and in the absence of other contextual reasons to reject it, the 27-plet should have remained a *good* representation, and scientists could have further applied the predictive strategy to other unexplored spots on the diagram (other than Θ^+). Yet, as it happens, the experimental failure did provide sufficient grounds for rejecting the 27-plet as a whole, and scientists assumed that no particle that would belong only to the 27-plet existed, which Ginammi's strategy fails to make sense of.

So, Ginammi's reconstruction fails to account for the actual inferences that scientists made. Let me examine another, recent heuristic account, developed by Bueno and French (2018).

5.2. Bueno and French's "partial structure" account

Their "partial structure" approach leads to results very similar to those of Ginammi's strategy in Ω^-'s case. A physical system *S* is immersed through an appropriate morphism in a mathematical structure *M*, leaving some elements unoccupied and some relations uninterpreted in *M*. The gist of their approach is to say that the precise extension of the relations within *S* is only *partially* known, which captures well the state of knowledge at the opening of the 1962 conference, where it was not known whether the apex of the pyramid (or the 18 empty places of the 27-plet) was "surplus structure" devoid of any physical significance, or possessed a physical interpretation in *S*. Just like Ginammi's strategy, their approach provides a guide for navigating in very "open" situations.

What's really special about their work is the claim, backed up by historical analysis, that the interpretation of this surplus structure is mainly decided by context and pragmatics. Here, the decision to predict new particles stems from the successful application of SU groups to the classification of other particles in the near past. Against Bangu, they claim that it is physical, not Pythagorean, considerations that prompt scientists to fully interpret the decuplet. I very much agree with this, and will come back to it in sections 8 and 9.

What I disagree with is their claim that the distinction between the context of discovery and the context of justification is blurred in this specific case (2018, p. 225), and that Bangu's originating problem (Is Ω^-'s prediction more than a mere heuristic matter? Is it *justified*, and if so, by which methodological principle?) is irrelevant to actual scientific practices. Surely, they are right against Steiner in claiming that "heuristics" should not be reduced to Pythagorean guessing (*ibid*., p. 3), because even methods of discovery that rely on mathematical structures are "intertwined" with "the relevant physical reasoning" (*ibid.*, p. 225). But this does not mean that the problem of justifying inferences dissolves into some sort of contextual liberalism.

In particular there is an aspect of the 1962 episode that their approach fails to properly account for: the 27-plet's rejection, again. Bueno and French write: "Certain observations ruled out the [the 27-plet] (basically it should have allowed for certain particles which were not detected), leaving the decuplet as the only contender" (*ibid*., p. 226). But they don't explain why the lack of a Θ^+ particle (the "Goldhabers' gap") disqualifies the 27-plet as a whole, and result in the conclusion that no other particle (say, Θ^0), that would belong exclusively to the 27-plet, exists, i.e. that the decuplet is "the only contender". In their approach and after the detection failure, the element Θ^+ in the 27-plet would again be considered an idle piece of surplus structure, but "the openness and partial nature" of information (*ibid*., p. 37) should have prevented the scientists to conclude that the particle Θ^0 doesn't exist either. Yet, the discovery of another such particle would surely have been a major anomaly for the whole "Eightfold way" model!

To sum up, there is one crucial aspect of the episode that the two heuristic approaches fail to account for, because the interpretation of surplus structure is not as free as they say. The scientists worked under the assumption that if all detected particles fit into *M*, then all other places of the multiplet are occupied by existing particles; or by contraposition, if one particle is missing, then none actually fits in *M* and the representation is to be rejected as a whole. 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*. However, if this principle is not that of abductive reasoning (section 3), nor a Pythagorean Reification Principle (section 4), then what is it? Let me now turn to the constructive part of this work, where I try to reconstruct predictions from the multiplet structure as *inductive inferences*.

6. A "Principle of Symmetrical Extension" (PSE)

My positive claim is that symmetries among particles of a same multiplet not only played a classificatory role, but were also used as a ground for inductive reasoning, to derive lawlike generalizations and predictions. In fact, symmetries are incorporated into a principle according to which an entity of a given family cannot exist and fit into a symmetry scheme *alone* but only together with the definite number of its symmetrical counterparts inside a structure, *because* they are all symmetrical. Thus, Ω^- is being *relationally* predicted, as an unknown *relatum*, from the holding of symmetry relations with already known particles. To work out this intuition, let's start with a simpler example with no considerations of symmetry.

Imagine that an amateur bibliophile attends an auction to acquire a book collection, which is said to contain the complete 2^{nd} edition of the *Encyclopaedia Britannica* (1777-1784). Suppose that eight arbitrarily chosen volumes are on public display. As she is only an amateur, she ignores that there are exactly ten volumes in the series. Yet, she has a scientific mind, and decides to list the volumes available for consultation in a table, where she assigns to each one a number representing its rank in the series, which she can read on the edge, and where she records first and last entries.

List of volumes of the *Encyclopaedia Britannica, 2nd* ed. 1777-1784		
Volume Number	**First entry**	**Last entry**
1	"A"	"Astronomy"
2	"Astrop-Wells"	"Bzovius"
3	"C"	"Czongrodt"
4	"D"	"Fuzileers"
5		
6	"K"	"Medicine"
7	"Medicines"	"Optics"
8	"Optimates"	"Poetry"
9	"Poggius"	"Scudery"
10		
11		
12		

If her aim is only to classify the known volumes, she can stop there. Her table contains some objective information: the relation of order "$<^n$" between volumes *i* and *i+n* reflects an actual order of publication "$<_p^n$" ("being published *n* ranks before"), and also empty spots, which may play no representative role. Especially, the table has twelve rows only because she needed more than eight and found the number "12" mathematically beautiful. Now, consider the two following predictions:

P_1: "There exists a 5th volume in the collection, whose first entry begins with the letter 'G' and the last with 'J'".
P_2: "There exists a 12th volume in the collection, whose first entry begins with the letter 'T' and the last with 'Z'".

Both are predictions, as she ignores the exact numbers of volumes in the series. However, while she has no reason to legitimately infer P_2, the gathered information *plus* the background assumption that the collection is *complete* allows her to infer P_1 from the table. Indeed, if the collection is complete, then there must be a distinct volume to occupy each empty spot that lies between two occupied spots.

Here I must carefully distinguish three meanings of "complete". In the simple, *empirical* sense, the collection is complete when all different, existing volumes are in it. But this is not enough, as such, to infer the existence of the 5th volume. In a *Pythagorean* sense, the collection is complete if it fills all the cells of a mathematically beautiful twelve-row table. But this is a too strong and absurd assumption. In a third, *structural* sense, the collection is complete when it contains all volumes issued one immediately after the other during the publication process, i.e. the full extension of the relation "$<_p^1$". This is the key assumption for the inference: that the publishing process is subject to a *structural constraint* such that the relation "$<_p^1$" is defined on the entire series, and that if any two volumes are in the relation "$<_p^n$", then there are *n-1* intermediate volumes and *n* intercalated instances of the relation "$<_p^1$".

This structural assumption about the represented collection is reflected by a *constraint of joint interpretation* in the representation. If two spots *i* and *i+n*, which are in the relation "$<^n$", are interpreted and occupied by two (distinct) volumes in the collection, then (a) one must open *n-1* intermediate spots such that the relation "$<^1$" is instantiated *n* times from *i* to *i+n*, and (b) one can predict that there are *n-1* volumes in the collection to occupy these spots. As it happens, this rule of prediction applies only to the 5th spot, the only one between two occupied places, and especially not to the 12th, which is not in the required relations. In that way, the existence of a 5th volume along with some of its characteristics regarding its first and last entries are *relationally* predicted, because it may correspond not to any spot in the table, but to a determinate spot, which must be *jointly* interpreted with its relational counterparts, by virtue of a structural assumption on the represented collection.

I think that something very similar goes with the baryons and their symmetry relations.[13] So, let us transpose this idea, *mutatis mutandis*, to the baryon case, in order to derive a precise predictive principle which will allow for an adequate reconstruction of Ω^-'s prediction. Just like in the previous case, predictions from a multiplet structure follow a rule of joint interpretation, which allows to predict the existence of particles in a given family and to fill spots in a representation, from the fact that these spots stand in certain symmetry relations with already occupied ones, and based on a background, structural assumption on the represented family. To work this out, I first need to dig up this assumption.

In the context of the 1962 conference, the focus is on the family *S* of the spin-3/2 baryons, from which nine members were already detected. Although Gell-mann and Ne'eman didn't explicitly endorse a representational view, they clearly assumed that the rotations of the SU(3) group were able to play at least a classificatory role and capture how the existing baryons transform into one another while preserving their unitary spin. Their model "was based on the division of the hadrons into "families" or supermultiplets, and the finding of *connections between the various members of each family*, using the mathematical notion of groups" (Ne'eman & Kirsh, 1996, p. 198), and each "'rotation' of the unitary spin in the imaginary space in which it is defined *changes one particle into another*" (*ibid*., p. 201, my emphasis). Their hypothesis was that a multiplet of SU(3) could be found to represent adequately the complete family of spin-3/2 baryons and capture their physical "connections" through the symmetries of the group.

To put it more formally, let us treat a SU(3) *n*-plet as a mathematical structure $M = \langle dom(M),\{R_i\}\rangle$, where $dom(M)$ is a non-empty set of elements $\{\Gamma_1,\ldots,\Gamma_n\}$ and $\{R_i\}$ a *group* of binary symmetry relations[14] defined on $dom(M)$. As *M* is supposed to represent *S*, it is assumed that there is a structure preserving monomorphism ϕ from *S* to *M* such that any $s_i \in dom(S)$ is represented by an element $\Gamma_i = \phi(s_i) \in dom(M)$, and by which relevant physical relations $\{r_i\}$ in *S* are "mirrored" by (some of) the symmetries $\{R_i\}$ of *M*. This is Ginammi's *minimal* condition for *M* to be a *good* representation of *S*: some of the $\{R_i\}$ have a physical meaning, through which they allow to classify the particles of *S* by mirroring their structure. All this is compatible with a mere classificatory purpose, where symmetries are instrumental to map what is known or supposed to exist, not to predict anything.

But there is more. Nine spin-3/2 baryons had been found, which could fit both into a decuplet or a 27-plet of SU(3). Why not accept both? On the contrary, the physicists clearly assumed that one, *and only one* of the two SU(3) *n*-plets had to be chosen, otherwise they

[13] Rigorously speaking, "symmetries" are relations or transformations belonging to the mathematical level, and should be distinguished from the physical relations or connections that they mirror. In what follows, I stick to this distinction as much as possible, but sometimes indulge in speaking of "symmetries between particles".

[14] In the general case, there is no reason to consider binary relations only, and the structure *M* can also be supplemented with *functions* or *operators*; this simple version will be sufficient to account for the role of the SU(3) rotations in Ω^-'s prediction, as rotations are easier to picture as binary relations.

would have had no motive to reject the 27-plet once the “Goldhabers’ gap” was reported. That the two representations are taken to be mutually exclusive is surprising and must be reflected on, as it reveals a stronger use of the symmetries. Indeed, one might think that if the decuplet is a good representation of the baryon family, then *ipso facto* the 27-plet also is, because the former seems *embedded* in the latter. The 27-plet seems to be nothing but the decuplet *plus* more spots and relations, such that any particle which fits into the decuplet also fits into the 27-plet. Here, a particle is said to “fit well” into a multiplet when its observable characteristics (of spin, charge, mass, strangeness, etc.) locate it adequately in a point of the diagram, i.e. by virtue of its monadic characteristics only and, which is crucial, independently of the relations it may or may not have with others. Thus, the 10-place representation seems “nested” in the 27-place one.

However, this is not right from a group-theoretic point of view, and to make it clear this I need to say a little more about group representations. A representation of a group *G* is a *mapping* from *G* to a linear group *GL(V)* operating on a vector space *V*. It means intuitively that the transformations of *GL(V)*, which operate on elements of *V*, mirror the actions of a mathematical group G – where “mirror” means “generate the same symmetries”. So, a “representation” is a mathematical description of how different states of a vector space transform under the action of a group, and it can be put in the form of a multiplet diagram whose elements are symmetrical vectors of characteristics. A *n*-plet of SU(3) is a *n*-dimension representation of SU(3) acting on an imaginary space *V*, whose elements are vectors of eight characteristics (hence the name of the model: “Eightfold way”). When Gell-Mann suspected that spin-3/2 baryons arrange into a multiplet of SU(3), he generated possible multiplets by taking the Tensor product of two octet representations: $8 \otimes 8$, which gives a representation of dimension 64 (Gell-Mann, 1964 (2018), p. 28). But Gell-Mann was looking for an *irreducible* representation, i.e. one which contains no invariant subsets (other than itself and the identity relation). The $8 \otimes 8$ representation is *reducible* and can be decomposed in a direct sum of irreducible representations:

$$8 \otimes 8 \ = \ 1 \oplus 8 \oplus 8 \oplus 10 \oplus \overline{10} \oplus 27$$

As already nine heavy baryons were found, only the decuplet (and its anti-decuplet) and the 27-plet remained as possible candidates. The key point here is that, as decomposition products, the two multiplets corresponds to two *distinct* because non-overlapping subsets of transformations. Thus, the decuplet *is not* embedded in the 27-plet, as both correspond to *distinct* sets of transformations and describe two *different* ways that *n* states transform one into another. In other words, the way in which these states transform into each other is codependent with the cardinality of the class: 10 states do not transform like 27, and each *n*-plet diagram can be seen as being generated from a distinct *n*-dimension symmetry group.

From a mere classificatory viewpoint, this wouldn’t prevent both *n*-plets to be good representations of *S*. As long as no more than 10 particles are detected, both ways of modelling their transformations are empirically adequate and need not be exclusive. However, as physicists assumed that *only one* was correct, this has crucial consequences regarding their use of symmetries. Their goal was not merely classificatory, but also to find which of the two linear symmetry-groups, the 10-dimensional or the 27-dimensional (henceforth: “10-SU(3)” and “27-SU(3)”), was the *correct one*: the symmetries were used not only to capture relations among existing baryons, but also to identify whether the baryons transform like a ten-member or like a 27-member family. Thus, it was assumed that each baryon actually doesn't transform in the same way and doesn't have the same relations to the others, if it transforms in 8 or in 26 symmetrical possible others.

This changes the sense in which a particle can be said to "fit well" into a *n*-plet. Now it means that the particle not only has the adequate characteristics to occupy a determinate spot in the diagram, but also that it stands in the exact physical relations which are mirrored by the symmetries of the *n*-dimensional linear group. For instance, to fit well into the decuplet, a spin-3/2 baryon not only has to be of determinate mass, charge, strangeness, etc., but also to stand in the required relations with exactly nine others, those relations which are mirrored by the transformations of 10-SU(3). In other words, 10-SU(3) defines the bundle of *relational* characteristics that the particle must have as a necessary condition to be said to "fit into" the decuplet. The competing *n*-plets thus behave like two exclusive hypotheses on how the baryon family is structured, which reveals the following background assumption:

> **Structural Assumption (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 of 27-SU(3), are "jointly" instantiated.

Some remarks are in order. First, (SA) is a very strong assumption, which is crucial to the upcoming predictions. But it is not Pythagorean as such, because it is *not* assuming that *S* completely fills the mathematical representation. (SA) is rather an assumption about the *physical* relations $\{r_i\}$ themselves, according to which they are subject to a structural constraint: any object s_i of *S* cannot stand in a relation r_i without also standing in all other relations of $\{r_i\}$ with other objects of S.[15]

Second, it is an empirically defeasible assumption, which would have been rejected if, for example, 14 spin-3/2 baryons had been found. This number would not have been significant from a group theoretic point of view, thus proving that the way in which they transform would not be ruled by a complete set of transformations.

Third, and this is where I agree with one of Buenos and French's claims, this Structural Assumption is rooted in past successes in applying the SU(3) group to shape families of particles (spin-1/2 baryons and mesons) into *complete* octets. (SA) is not an assumption that must be made every time a mathematical structure is applied to a physical system, but only when there are good, *physical* reasons for doing so, which here have to do with the way particles related to each other into families and seemed to satisfy such structural constraints. What I add, against Bueno and French, is that this physical assumption is not simply a heuristic incentive to explore and further interpret a mathematical surplus structure, but a sufficient condition for applying a logically binding principle of prediction to that structure.

This principle is a rule of "joint interpretation" of the elements in *M*. Suppose that two elements, Γ_1 and Γ_2, are occupied by two particles s_1 and s_2, such that their relation $R_{1,2}(\Gamma_1,\Gamma_2)$ mirrors a relation $r_{1,2}(s_1,s_2)$ in *S*. If a particle, say s_1, cannot stand in this relation without also standing in every other relation of $\{r_i\}$, it means that every other relatum of Γ_1 through all the relations $\{R_i\}$ must also be interpreted. They must exist as its symmetrical counterparts. To work this out more formally, let us define the "Symmetrical Extension" of Γ_i through the relations $\{R_i\}$ (or "$SE(\Gamma_i,\{R_i\})$"): for any Γ_i, Γ_j of *M*, $\Gamma_j \in SE(\Gamma_i,\{R_i\})$ if and only if there is a symmetry

[15] In the previous example, the structural constraint on the collected volumes of the *Britannica* was simpler and only "downward": the holding of a relation "$<_p^n$" implied that of *n* relations "$<_p^1$". Here the constraint on *S* is richer, as the relations $\{r_i\}$ are mathematically conceived of as forming a group.

$R_i \in \{R_i\}$ such that $R_i(\Gamma_i,\Gamma_j)$. $SE(\Gamma_i,\{R_i\})$ is nothing but the set of all elements of *dom(M)* which Γ_i bears a relation R_i with. Note that, if Γ_i bears a relation R_i with every other element of *dom(M)*, then $SE(\Gamma_i,\{R_i\}) = dom(M)$. This allows us to formulate the following "Principle of Symmetrical Extension" (PSE):

> **(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 $\Gamma_j \in SE(\Gamma_j,\{R_i\})$ there is an object $s_j \in dom(S)$ such that $\Gamma_j = \phi(s_j)$".

This principle allows for a use of the symmetries between elements in a structure *M* which is not merely classificatory, but genuinely predictive. Any object s_j can now be predicted as being represented by a place Γ_j which belongs to the "Symmetrical Extension" of already occupied places. The prediction is entirely relational: one infers that Γ_j is occupied by an object of *S* because Γ_i is and the relation $R_{i,j}(\Gamma_i,\Gamma_j)$ is supposed to mirror a relation between s_i and another object in *S*. That way, s_j is *relationally* predicted through its symmetry relations with already detected objects.

Note that, like Bangu's "Reification Principle", (PSE) bridges the gap between the mathematical and physical levels, allowing to physically interpret certain elements of the mathematical structure. But (PSE) presupposes (SA), because (SA) determines the sense in which a particle s_i is said to "fit well" into *M*: by being subject to a structural constraint at the physical level. So, if (PSE) crosses the gap between the mathematical and the physical, it's only thanks to a prior assumption about how the physical level is structured.

But note also that, unlike (RP), (PSE) doesn't allow to indiscriminately predict that every element of the representation *M* has a physical referent. Because it is a principle of *relational* prediction, it has a screening virtue: it only extends to a substructure of *M* which is defined as the "Symmetrical Extension" of an already interpreted element Γ_i of *M*. Of course, in the special case where $SE(\Gamma_i,\{R_i\}) = dom(M)$, then it can be predicted that every other element Γ_j of *M* corresponds to a distinct object in *S*.

It is also important to note that, in this latter case, (PSE) differs from the blunt postulate that *M* isomorphically represents *S*,[16] because (PSE) is a conditional principle. It only states that *if* there are objects in *S* whose relations are mirrored through ϕ by relations of $\{R_i\}$ in *M*, then all elements in a substructure of *M* also represent objects in *S*. That is: if ϕ is a monomorphism from *S* to *M*, then it is an isomorphism from *S* to a special substructure of *M* – which is exactly the type of inference that was found in the Ω^- episode (section 2).

Now, let me check that (PSE) provides an adequate reconstruction of the 27-plet's rejection and of Ω^-'s prediction. At the 1962 conference, nine spin-3/2 baryons had been found, which could fit both into a decuplet or a 27-plet of SU(3). Assuming that they actually fit into the 27-plet, i.e. that the known spin-3/2 baryons actually transform according to 27-SU(3), then one predicts that there exists a particle to occupy the place Θ^+ in the 27-plet:

(P1) There are nine particles in *S* which fit into the 27-plet and occupy the places Δ^-, Δ^0, Δ^+, Δ^{++}, Σ^{*-}, Σ^{*0}, Σ^{*+}, Ξ^{*-} and Ξ^{*0}
(P2) $\Theta^+ \in SE(\Delta^-,\Delta^0,\Delta^+,\Delta^{++},\Sigma^{*-},\Sigma^{*0},\Sigma^{*+},\Xi^{*-},\Xi^{*0},\{27\text{-}SU(3)(V)\})$
(C) There is a particle in *S* which occupies the place Θ^+ in *M*

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[16] Otherwise, the prediction would be circular: one would be able to predict that an element Γ_j of *M* represents an unknown object in *S* only because she has first postulated that all elements of *M* do.

The first premise postulates that the nine detected spin-3/2 baryons fit well into the nine places in the 27-plet and transform according to 27-SU(3). The second premise asserts that Θ^+ belongs to the symmetrical extension of each of the nine occupied spots. Then, by applying (PSE), one infers the conclusion (C). With the Goldhabers' experiment proving (C) false, then by *modus tollens* either (P1) or (P2) is false. But (P2) only states a "fact" about the structure in *M*. So, (P1) is the faulty premise. As the Goldhabers' gap constitutes a violation of the symmetry, it is inferred that the existing baryons actually *don't fit* into the 27-plet, as they don't have the required relations, which is reason enough to reject this multiplet as the correct representation of the baryon family. As a rule of rejection, (PSE) implies in this context that if the 27-plet is not an *isomorphic* representation of *S*, then it is not even a *monomorphic* representation of *S*.

Second, the prediction of Ω^-'s existence and characteristics can be accounted for, along the following lines:

(P1') There are nine particles in *S* which fit into the decuplet and occupy the places Δ^-, Δ^0, Δ^+, Δ^{++}, Σ^{*-}, Σ^{*0}, Σ^{*+}, Ξ^{*-} and Ξ^{*0}

(P2') $\Omega^- \in SE(\Delta^-,\Delta^0,\Delta^+,\Delta^{++},\Sigma^{*-},\Sigma^{*0},\Sigma^{*+},\Xi^{*-},\Xi^{*0},\{10\text{-}SU(3)(V)\})$

(C') There is a tenth particle in *S* which occupies the place Ω^-

That way, the existence of Ω^- is *relationally* predicted through its symmetry relations with already detected particles of the baryon family. Note that now, unlike with (RP), Ω^-'s characteristics are now an *essential* part of the prediction, because the symmetries determine the only bundle of characteristics that the particle can have in the decuplet while preserving the Unitary Spin. In other words, Ω^-'s characteristics result from the symmetry relations which the particle bears with the other particles of the same family, and which determine how they all transform one into another.

It is important to note that, in my account, the two premises (P1) and (P1') are now mutually exclusive. Because "fit into" means "have the adequate characteristics *and relations*", a detected particle cannot be said to fit into *both* the decuplet and the 27-plet. This means, perhaps counterintuitively, that although they are premises of two predictive reasoning starting from the same data, (P1) and (P1') are not mere statements of observable facts, but already *hypotheses*. That the particles belong to the decuplet or that they belong to the 27-plet are two distinct, mutually exclusive hypotheses, which are both consistent with the observable characteristics but lead to distinct predictions. As a consequence, and unlike with (RP), the exact number of baryons in the spin-3/2 family is now also an *essential* part of the inference. Especially, the fact that the two premises (P1) and (P1') are mutually exclusive (and collectively exhaustive) hypotheses explains why the hunt of the Θ^+ particle had the value of a *crucial experiment*, able to decide between two possible representations. As each describes how *n* states symmetrically transform one into another, each premises also states that a baryon cannot exist if precisely *n-1* others don't.

(PSE) also avoids some of (RP)'s serious problems. First, it doesn't involve any vague notion of "formal similarity", but the well-defined fact of belonging to the extension of a set of relations. Thus, (PSE) is in no way a mysterious, Steiner-like "analogical" principle. Second, (PSE) can be justified in a non-*ad hoc* way, for reasons "other than its effectiveness in this case" (Ginammi, 2016, p. 23), for its application primarily relies on a background assumption (SA) about the descriptive power of the SU(3) symmetries, which were already successfully applied to spin-1/2 baryons and mesons. The application of (PSE) here is justified in the scientific context and with reference to background assumptions.

I have worked out a principle which allows to predict the existence and characteristics of particles for symmetry reasons, once some particles of a structured family have already been detected. Yet, is (PSE) an acceptable methodological principle at all? Let's turn to the epistemological justification of the principle.

7. Is (PSE) really an *inductive* principle?

I claim that (PSE) is actually nothing but an *inductive* principle, and that it is therefore acceptable, or at least as acceptable as any inductive principle can be. This can be challenged on two grounds. Admittedly, (PSE) is an ampliative rule, which allows to draw a prediction about unobserved entities from experimental data. But, for one thing, it is difficult to see how it is a genuine inductive, *generalization* rule, and for another, what is inferred doesn't look like a *law-like* generalization. It seems therefore not correct to make it a genuine inductive principle, and to claim that it is as such legitimate. Let's meet these two objections.

I must admit that reconstructing Ω^-'s prediction as an induction raises a specific problem, because it is obviously not a standard, enumerative form of induction. The observed particles exhibit a regularity (R), as they all seem to "fit into *M*". Yet, if one were to merely project this last predicate to all particles of the family, one would only get the generalization (H): "All spin-3/2 baryons fit into *M*", from which the required predictions do not follow. Thus, one needs a rule that allows to "jump" from some known objects to unknown others, such that the jump is grounded on some fact or assumption about the observed objects themselves. However, *this is* (a specific version of) the problem of induction itself: to find a "connection" between distinct facts or entities on which an ampliative rule can be based. My claim is that, under certain conditions, symmetries can be these connections.[17]

Next, I must also admit that (PSE) is not a rule of *qualitative* generalization. In a qualitative generalization, one first detects a *resemblance* among observed objects ("all observed Fs are G"), and one projects the same predicate ("being G") onto the unobserved objects. The present case differs in these two respects. What is detected among observed particles is not a mere resemblance, but *quantitative variations* in charge, mass, strangeness, etc. All the detected baryons that fit well into *M* differ in these quantities and are similar only in that they are spin-3/2 baryons. But one also notices that these variations preserve a same complex quantity (the unitary spin), and may thus be treated as transformations of a *n*-dimensional representation of SU(3). Then, one extends these regulated variations to predict unknown cases. Thus, the operation is not that of qualitative, but of *quantitative* generalization, for what is first detected and then projected is not some qualitative similarity but quantitative variations that hold a specific mathematical property constant. This does not prevent the inference from being a generalization. And by answering the first objection, an original, properly epistemological role of symmetries is brought to light: symmetry relations here take the place of the resemblance relation as a criterion of projection.

Let me turn to the second objection. (PSE) allows to infer that there are unknown objects in *S* which fit into *M* because of the postulated symmetries. Admittedly, what is thus inferred does not look like a *law-like* generalization. However, most forms of induction can actually be

[17] Typically, however, the "connection" is between a purported cause and its effect, and the rule of induction states that "the same cause necessarily produces the same effect". Here obviously, the induction will be non-causal, as it will rely not on causality but physical relations mirrored by mathematical symmetries.

reconstructed in two ways, relying either on a non-deductive *rule of prediction* or on a *relation of confirmation* between observed facts and a law-like hypothesis. Take standard enumerative induction, and suppose that all observed crows are black. From these facts, one can either *predict* that all unobserved crows are black, following the rule: "if all observed F resemble in being G, then every non-observed F is also G", or *confirm* the law-like generalization "All crows are black", from which predictions about unobserved crows deductively follow. Both ways are equivalent, as they both rely on a criterion of qualitative resemblance, used either as a rule of prediction ("project the resemblance") or as a tool for defining which generalization holds as confirmed by the data ("that which projects resemblance onto all cases").

The same applies to Ω^-'s prediction, which can very well be reconstructed as the confirmation of a law-like hypothesis, if the symmetry criterion is used as a scheme to define which hypothesis is accepted as confirmed by observations. Experimental data show that nine spin-3/2 baryons exist, with *different* characteristics of mass, charge and strangeness; but also that their characteristics vary in a seemingly non-arbitrary way, which is nicely captured by the transformations of 10-SU(3). All known particles differ from each other in a way that fits these symmetries. Thus, they are positive instances of the following law-like hypothesis (H*):

(H*) All spin-3/2 baryons transform into each other according to 10-SU(3).

Following the standard "Nicod" rule of confirmation, (H*) is thus confirmed by the observations. Note also that (H*) is equivalent to (H): "All spin-3/2 baryons fit into the decuplet" only if by "fit into the decuplet" it is meant that a particle not only has the required characteristics of mass, charge etc., but also stands in the required relations with other particles, those which are mirrored by the symmetries of 10-SU(3). Then, it deductively follows from (H*) that there are exactly ten spin-3/2 baryons with the required characteristics, because (H*) implies that each baryon bears symmetry relations to exactly nine others. The prediction of Ω^-'s existence and characteristics can easily be accounted for, as that of the tenth instance of the law.

Thus, both ways of construing the inference are equivalent, as they both rely on a criterion not of qualitative resemblance but of symmetry, used either as a rule of prediction ("extend the symmetries") or as a means of defining which generalization is confirmed by the data ("that which extend the symmetries to all cases"). Thus, the inference is indeed an induction, as it can perfectly be construed in a way which follows a classical nomological-confirmatory path.

This also explains why the main criticism against (RP) does not work against (PSE). Remember that, as a general principle, (RP) in simply false, since there are numerous cases where it leads to physically interpret all solutions of an equation, while some are simply meaningless. One could attack (PSE) in the same way, exposing cases where it leads to aberrant predictions. Take, for instance, the (classic or relativistic) Hamiltonian equation which describes the energy state of a system. It has two symmetrical solutions, one of positive energy and one of negative energy. If one blindly applies (PSE), one aberrantly predicts the existence of a physical state with negative energy, while physicists usually discard this solution as meaningless. Hence, one would conclude, (PSE) is as false as (RP) as a general principle. I admit the problem, but reply that this problem only confirms that (PSE) is indeed a principle of induction, with its validity depending on precise conditions of application.

Indeed, this problem is typical of any inductive principle. Take the rule of enumerative induction, which allows to generalize observed qualitative regularities. Mill had already taught us that not every observed regularity is worth generalizing, for there are accidental regularities which do not correspond to any law of nature (Mill, 1846, p. 226). Thus, the rule legitimately applies only to these regularities which are first *assumed* to correspond to laws. The same goes for (PSE), whose application relies on the background assumption (SA) that baryons are particles which cannot exist without being definite transformations of each other, in a way which can be captured by a full symmetry scheme. This assumption may be justified in the context of particle physics, but there are numerous contexts where symmetries are mere artefacts which don't mirror any real relations between physical objects. Hence, the fact that (PSE) legitimately applies only when such an assumption can be made only proves further that it is indeed an inductive principle.

It may also be noted that whether (PSE) is legitimately applicable or not changes with the theoretical context and the background assumptions, just as regularities that were previously held as accidental can eventually appear to be nomological. Take the classical example of Dirac's relativistic wave function. It is a Hamiltonian equation which admits of four solutions. In 1928, Dirac deemed two of them meaningless, as they would correspond to *positively* charged electrons with *negative* energy (1928, p. 618). Two years later, he changed his mind and predicted the positron, because "in the accurate quantum theory in which the electromagnetic field also is subjected to quantum laws, transitions can take place in which the energy of the electron changes from a positive to a negative value even in the absence of any external field, the surplus energy (…) being spontaneously emitted in the form of radiation" (Dirac, 1930, p. 361). The critical motivation here is that the symmetry between positive and negative energy states, which was previously a physically meaningless, can now be interpreted as a *physical change*, subject to some physical law. Again, what proves crucial here is whether or not the symmetry relations capture physical relations between instances of a law or a law-like interaction.

This leads to a very important conclusion. As a basis for an inductive inference, 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* (like (H*)). This shed new light on the assumption (SA), which required that, for (PSE) to apply, the objects of *S* be subject to "joint", structural existence conditions, which are described by a symmetry scheme, such that one cannot be the case without all the others being so. In Ω^-'s prediction, this assumption may have seemed like a magic trick that did all the work, but now its status appears clearly: the "necessity" by which an object of *S* cannot be the case without all its symmetrical counterparts being so is of the nomological kind. It is the sort of necessity that welds together and unites the instances of the same law, and by which one cannot be an actual instance of the law if specific others aren't the case as instances of the same law. Thus, (SA) only requires to assume that observed objects are instances of the same law of type (H*), which is the standard ground for making predictions from this law.

Now that I have worked out (PSE) to adequately reconstruct the Ω^- episode, and defend its legitimacy as a principle for quantitative induction, let me face two possible objections against (PSE). The suspicion may indeed remain that (PSE) is nothing but a Pythagorean or "reifying" principle in disguise. The accusation is twofold: first, predicting physical phenomena or entities for reasons of symmetry seems to rely on a Pythagorean faith in some oracular power of mathematics; second, (PSE) still legitimizes a "jump" from the mathematical possibilities of a formalism to actual physical objects, and thus is a "reification" principle. Let me face these two objections successively.

8. Is (PSE) a "Pythagorean" principle?

(PSE) allows to predict the existence and characteristics of unknown physical objects on the basis of relations internal to a mathematical representation *M*. Just because observed objects "fit into *M*", one readily infers that their relational companions in *M* must also exist with the adequate characteristics. Isn't this already too Pythagorean?

Let's compare Ω^-'s case with a prediction that also relies on relations in a mathematical representation, but that nobody would blame as Pythagorean. Suppose that the volume (V) and pressure (P) of some gases have been measured, and that they all "fit into" a curve representing a constant mathematical relation E: "P.V = α" (Boyle's law). As the law is confirmed by available data, one infers that it holds as a true law of nature. Then, focusing on points of the curve with values *p* and *v* that have never been measured before, one predicts that a gas can be brought to the corresponding states which will satisfy E. How is this different from the prediction of Ω^- based on (PSE)?

In both cases, a mathematical model – a simple mathematical equation or a 10-dimensional linear group acting on a vectorial state space – offers a structured representation of observables. Boyle's law was phenomenological and only with Boltzmann's kinetic theory of gases did we understand why gases exhibit such a pattern. Similarly, Gell-Mann's and Ne'eman's model was entirely phenomenological, and only quark theory could explain why hadrons exhibited such geometrical regularities (Ne'eman & Kirsh, 1996, p. 195). Boyle's law describes how two quantities vary together while keeping the equation true. Similarly, the "Eighfold way" model describes how a vector of eight quantum characteristics transform in *n* different states while preserving the Unitary spin. Thus, in both cases, a mathematical representation is used to capture the way that some physical quantities vary together while preserving a constant mathematical pattern.

Crucially, in both cases the prediction cannot rely on a mere empirical generalization. From the fact that all *observed* (perfect) gases satisfy E, one could generalize and predict that *all other* (perfect) gases will also fit into the curve. But this generalization cannot support the prediction that *another point of the curve* (or another solution of the equation), say (*p'*,*v'*), represents a physically possible state of a gas, which can be realized in laboratory conditions to provide a strong confirmation of E. Just like in the baryon's case, this simple prediction requires a detour *via* the mathematical representation and relies on a predictive rule very similar to (PSE): "if (*p*,*v*) represents a physical state of a gas, then any pair of values (*p'*,*v'*) which is a transformation of the values (*p*,*v*) which preserve the relation E also represents a physical state of a gas". Thus, the prediction of a new physical state is directly drawn from the mathematical curve. So, why would Ω^-'s prediction be deemed unacceptable as "Pythagorean", while this one is a standard and well accepted form of prediction?

My objector may reply that I have consciously presented the prediction from Boyle's law in an advantageous and non-standard way, which involves an unnecessary representational detour. From the fact that all measured states of P and V of a gas fit E, one could generalize and predict that all possible states of P and V of a gas will also fit into E. Then, with *p'* a possible value of pressure yet non-measured in a gas, it is predicted that this gas will also have the volume *v'* such that (*p'*,*v'*) is a solution of E. In this way, no detour *via* the mathematical representation is required, as opposed to Ω^-'s prediction.

Yet, the objector's account is partly illusionistic, because what was previously gained through the representation is now made implicit, and concealed in the expressions "all *possible* states of P and V *of a gas*" and "*p'* a *possible* value of pressure *of a gas*". Indeed, what decides that *p'* represents a *possible* pressure state for a gas (where "possible" only means "physically realizable"[18])? If, as is obviously assumed in this context, there is a natural law relating the pressure and volume of gases, then the pressure of a gas does not vary freely, but covaries with its volume. A law defines which covariations in the pressure and volume of a gas are physically possible changes. So, in predicting something about "*possible* states of P and V *of a gas*", one needs to assume that these states *can* be reached through physically possible changes. And such an assumption can only be made by relying on an available nomological statement, here, Boyle's law, which states that the admissible changes in values of P and V are those which preserve E. Hence, in her account, my objector still relies on what the mathematical representation captured of physical objects, namely their physical relations, and this element is not Pythagorean.

I conclude that Ω^-'s prediction is no more Pythagorean than the prediction from Boyle's law, for in both cases, the detour *via* the representation is legitimate only insofar as one assumes, beforehand, that the mathematical relations between the elements of the representation (points on a curve or spots on a diagram) represent physical relations. Therefore, although it involved more abstract, algebraic tools than in Boyle's case, (PSE) doesn't rely on any faith in a mysterious connection between mathematics and reality, but on a structural-phenomenological commitment: that mathematics is applicable to physical phenomena because the latter possess structural characteristics which the former is particularly efficient to describe.

Still, a huge gap separates standard, phenomenological predictions and Ω^-'s episode: in the former case, one predicts that a physical *phenomenon* (effect or state) *can* be produced, while in the latter, one predicts that a physical object *exists*. If (PSE) legitimizes this "jump" from a mathematical possibility to an *actual* physical *object*, is it not a "reification principle"?

9. Is (PSE) a "Reification" principle?

First, one may reject as illegitimate a principle that allows to predict the very *existence* of unknown objects from their position in a symmetry scheme. By contrast, a standard law only allows to predict that *if* there *exists* an object of such or such type placed under certain circumstances, then it exhibits such or such characteristics, but not the existence of an object *simpliciter*.

This fear is legitimate, but it is fueled by a rather loose use of the term "existence" in this particular scientific context. Is Ω^-'s prediction really an *existential* prediction? First, it is not clear whether scientists regard the ten spin-3/2 baryons as numerically distinct types of particles or, as Ne'eman puts it, "as different states of the same basic particle, differing only in the (…) 'directions' of the unitary spin" (Ne'eman & Kirsh, 1996, p. 201). In the latter case, the problem vanishes, for Ω^- is predicted as the last unknown *state* of an already known existing baryon, pretty much like one predicts unobserved states of a gas from Boyle's law.

Even putting these difficulties of individuation aside, it is actually not clear *what* needs to exist so that the prediction comes true, nor what "exist" means in the context. Certainly, when Gell-Mann predicted "Ω^-", he did not intend to predict the existence of one individual and particular object like Le Verrier did with Neptune; "Ω^-" is rather the name of a new *species* in

[18] The use of modal terms here will be fixed in the next section.

the particle zoo. But what is a "species" of particle, and in which sense is it an "existing" entity? One must be careful not to get into the murky waters of the realism *versus* nominalism debate, where there is no immediately satisfactory answer. If one claims that the standard model of particles quantifies on natural species construed as *universals*, the burden of proof is on her. On the other hand, if one identifies a "species of particle" with a nominalist class of *actual particulars*, this will not work out, because Ω^-'s prediction keeps all its meaning even if there are no actual Ω^- particles in the universe. Ω^- is actually so unstable that it has plausibly never occurred naturally, but only in laboratory conditions. But suppose that for financial reasons the Brookhaven experiment, which led to the actual detection of Ω^- in 1964, was never conducted and never will be in the history of the universe. Then no *particular* Ω^- actually exists. Yet, this does not deprive its prediction of its meaning! So, what does Ω^-'s prediction actually mean?

My guess is that the modality induced by the term "existence" is wrong, and that Ω^-'s 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 (Ne'eman & Kirsh, 1996, p. 204). It is however a very strong sense of "physical possibility": not only is Ω^-'s existence *not forbidden* by any known law of nature, but it is a physical possibility that is supposed to *realize*, in precise initial conditions. This looks remarkably like the kind of possibilities that are covered by a physical law, those physical possibilities which are either *realized* in actual empirical instances of a law or *unrealized but realizable* in precise counterfactual circumstances. If I am right, and if Ω^- is indeed predicted as a physically realizable instance of a law, then there is no residue of "reification" left, as the prediction is not existential *stricto sensu*.

However, the accusation of reification has a second side. It may be objected that (PSE) allows to (illegitimately) jump from a *mathematical* to a *physical* possibility, in a way very similar to the "Totalitarian Principle" (TP) (see note 10): one first assumes that symmetries define a range of mathematical possibilities that equally aspire to physical realizability, and then predicts that, if some are physically realizable, then all are, unless some unknown law forbids them. How does (PSE) differ?

The modalities are not the same. With (TP), symmetries are used in a pre-nomological context: they first dictate what *can* exist in nature and it is only in a second step, in case of asymmetric deviation, that a law or a nomic factor is posited to explain the anomaly and impose physical limits to discriminate among mathematically equal possibilities. Those two steps are sometimes inverted: the prior knowledge of nomic constraints or boundary conditions, as in the case of the Hamiltonian in classical mechanics, prevents one from reifying a mathematical possibility into a physical one. The key point is that in both cases there is a clear-cut separation between a range of mathematical possibilities and that part of it which is reified into physical possibilities.

Things are different with (PSE), in which the role of symmetries is nomological. Symmetries are used to bring together different physical phenomena or objects as *instances of the same law*, to formulate precise nomological hypotheses and predictions. Here, the possibilities in the representation are immediately construed as *physical* possibilities, for they are seen as instances of the same law whose symmetries depict physical relations. The solidarity of the "All or none" type is not that of "equal" mathematical possibilities, but that of different instances of the same putative law: one cannot be instance of that law if specific others aren't the case as instances of the same law. Here, symmetries are not an alibi for quasi-oracular inference, but capture the type of relations between different instances of the same law.

This can be verified in the episode. Recall the Goldhabers' gap: the fact that the 'Θ^{+}' particle didn't exist was not interpreted as a symmetry break, indicating that some physical limitation forbade that all 27 mathematical possibilities were realized. It was rather inferred that the whole 27-plet was refuted as a general hypothesis, and that the nine known particles actually did not transform according to the symmetries of 27-SU(3). The type of failure here proves that the symmetry-based prediction was of the nomological kind, and didn't rely on a reification principle.

10. Conclusion

Going back to Bangu's question (was Ω^{-}'s prediction a heuristic gamble, or can it be justified by a methodologically acceptable principle?), my answer is twofold. First, I have argued that Ω^{-}'s existence and characteristics, as well as the 27-plet's rejection, can be adequately inferred not along abductive or Pythagorean lines, but on symmetry grounds. Mathematical symmetries were used not only to classify the existing baryons, but also to understand how they transform into each other, and thus to theoretize not only on their characteristics, but also on their relations. In my account, scientists first assumed that particles of a same family are subject to a structural constraint, such that one cannot "fit" into a *n*-plet of SU(3) without transforming into exactly *n-1* other particles with determined characteristics, and then relied on a predictive rule of "joint interpretation" according to which, "once a member of a given multiplet is found, all the other members of the multiplet must also exist" (Lipkin, 2002, p. 53). I have argued that this "Principle of Symmetrical Extension" provides a more adequate picture of the episode than other available accounts.

Second, I have defended (PSE) against possible objections and claimed that it is a perfectly sound principle from a methodological point of view, as it is nothing but an inductive rule. In doing so, I have brought to light a new role for mathematical symmetries as a nomological criterion. In standard enumerative inductions, one uses a criterion of resemblance to bring different objects together as instances of a same law and to formulate the law. I claim that symmetries can play the same epistemological role, as they are able to capture how different objects or states covary in their characteristics while preserving a constant mathematical pattern, and thus help formulate the law in the algebraic language of Group Theory. What's crucial here is that the rule of joint interpretation between symmetrical elements of a structure – which, like Dumas's musketeers, stand together and are "One for all and all for one" – is not driven by a Pythagorean faith in the divining power of mathematics. It merely expresses the fact that all instances of a same law are united and connected, such that one cannot be the case without the others also existing (or being realizable under specific physical conditions), which gives ground to their prediction.

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