Avoiding Reification
Heuristic Effectiveness of Mathematics and the Prediction of the Ω^- Particle

Michele Ginammi
mginammi@gmail.com
Ph.D. Scuola Normale Superiore (Pisa, Italy)

Abstract

According to Steiner (1998), in contemporary physics new important discoveries are often obtained by means of strategies which rely on *purely formal* mathematical considerations. In such discoveries, mathematics seems to have a peculiar and controversial role, which apparently cannot be accounted for by means of standard methodological criteria. M. Gell-Mann and Y. Ne'eman's prediction of the Ω^- particle is usually considered a typical example of application of this kind of strategy. According to Bangu (2008), this prediction is apparently based on the employment of a highly controversial principle—what he calls the "reification principle". Bangu himself takes this principle to be methodologically unjustifiable, but still indispensable to make the prediction logically sound. In the present paper I will offer a new reconstruction of the reasoning that led to this prediction. By means of this reconstruction, I will show that we do not need to postulate any "reificatory" role of mathematics in contemporary physics and I will contextually clarify the representative and heuristic role of mathematics in science.

Keywords: Applicability of Mathematics, Mathematical Model, Particle Physics, Prediction, Representation, Symmetry

Contents

1. Introduction

In his 1998 famous book, Mark Steiner argues that the role of mathematics in contemporary physics is peculiar. According to him, very often contemporary physicists draw important consequences about the physical world by relying on *purely formal* mathematical considerations, or 'analogies', which seem not to be in any sense rooted in the *content* of the mathematical representations. In this sense, the applicability of mathematics turns out to be 'magic' or—as Wigner (1960) would have put it—'miraculous'.[1]

Among the examples offered by Steiner in support of his thesis, Gell-Mann and Ne'eman's discovery of the Ω^- particle in 1962 is one of the most interesting. What is relevant in this discovery is the fact that the prediction of this new physical entity seems to be motivated *only* by the mathematics employed (the theory of irreducible group representations).[2] According to Steiner, this is an interesting example of analogy reasoning in physics, but he does not enter into the details of the prediction. A detailed analysis of this example is offered by Bangu, first in his 2008 article and then in his 2012 book. Bangu argues that the prediction of the Ω^- particle relies on a methodological principle, which he calls the "reification principle".[3] This principle is not justifiable by means of our standard methodological criteria, and Bangu himself takes this principle to be highly problematic.

I will offer a new logical reconstruction of the prediction of the Ω^- particle that *does not rely*, in any sense, on the reification principle (neither explicitly, nor in disguise). This alternative reconstruction will be based on some considerations about the representative role of mathematics and on the heuristic effectiveness that representing mathematical structures may exhibit under some conditions. I will firstly present Bangu's reconstruction of the reasoning that led Gell-Mann and Ne'eman to their prediction (sec. 2), and I will underline some difficulties in it (sec. 3.1 and 3.2). In order to solve these difficulties, I will introduce a general account for mathematical representativeness (sec. 3.3), and then I will discuss under which conditions mathematics can play a heuristic role in science (sec. 3.4). Within this framework, I will offer a way to account for Gell-Mann and Ne'eman's reasoning *without relying on Bangu's reification principle.*

[1] Steiner himself justifies the appropriateness of the word "magic" in this context: «Expecting the forms of our notation to mirror those of (even) the atomic world is like expecting the rules of chess to reflect those of the solar system. I shall argue, though, that some of the greatest discoveries of our century were made by studying the symmetries of notation. Expecting this to be any use is like expecting magic to work» (Steiner, 1998, p. 72).

[2] As it will become clearer later, this does not amount to saying that *no empirical fact* played a role in shaping the prediction. What Steiner is stressing here, is that the *justification* for the prediction seems to be purely mathematical—namely, purely based on the mathematical formalism employed.

[3] In Bangu (2012) the principle is called "identity principle", but the shift of terminology does not change the substance of his argument. Since no argument is offered to motivate the latter terminology over the first, I will refer to this principle by means of the first terminology, since I think is less ambiguous.

2. The discovery of the Ω^- particle: existential predictions and the reification principle

2.1. Bangu's reconstruction

Bangu's reconstruction of the Gell-Mann and Ne'eman predictive reasoning (hereafter, GMNPR) is based on the detailed account given by Ne'eman. Here is the whole passage to which Bangu referred:

> In 1961 four baryons of spin $\frac{3}{2}$ were known. These were the four resonances Δ^-, Δ^0, Δ^+, Δ^{++} which had been discovered by Fermi in 1952. It was not clear that they could not be fitted into an octet, and the eightfold way predicted that they were part of a decuplet or of a family of 27 particles. A decuplet would form a triangle in the $S - I3$ [strangeness-isospin] plane, while the 27 particles would be arranged in a large hexagon. (According to the formalism of SU(3), supermultiplets of 1, 8, 10 and 27 particles were allowed.) In the same year (1961) the three resonances $\Sigma(1385)$ were discovered, with strangeness -1 and probable spin $\frac{3}{2}$, which could fit well either into the decuplet or the 27-member family.
>
> At a conference of particle physics held at CERN, Geneva, in 1962, two new resonances were reported, with strangeness -2, and the electric charge -1 and 0 (today known as the $\Xi(1530)$). They fitted well into the third course of both schemes (and could thus be predicted to have spin $\frac{3}{2}$). On the other hand, Gerson and Shoulamit Goldhaber reported a 'failure': in collisions of K^+ or K^0 with protons and neutrons, one did not find resonances. Such resonances would indeed be expected if the family had 27 members. The creators of the eightfold way, who attended the conference, felt that this failure clearly pointed out that the solution lay in the decuplet. They saw the pyramid [in fig. 1] being completed before their very eyes. Only the apex was missing, and with the aid of the model they had conceived, it was possible to describe exactly what the properties of the missing particle should be! Before the conclusion of the conference Gell-Mann went up to the blackboard and spelled out the anticipated characteristics of the missing particle, which he called 'omega minus' (because of its negative charge and because omega is the last letter of the Greek alphabet). He also advised the experimentalists to look for that particle in their accelerators. Yuval Ne'eman had spoken in a similar vein to the Goldhabers the previous evening and had presented them in a written form with an explanation of the theory and the prediction. (Ne'eman and Kirsh, 1996, pp. 202-203)

When a few years later, in 1964, experimentalist physicists looked for the Ω^- particle in their accelerators, they found out exactly what Gell-Mann and Ne'eman predicted: the particle exists and it has exactly the predicted characteristics (see fig. 2).[4]

Now, one might ask: why did the experimentalist physicists trust Gell-Mann and Ne'eman's prediction? On which ground did they believe in the existence of the new particle, and why did they think that this (supposed) new entity would have exactly the same characteristics as guessed by Gell-Mann and Ne'eman?

Here are the logical steps that, according to Bangu, underly the previous historical report:[5]

[4] Actually, the story is not so simple. They could prove the existence of the Ω^- particle, along with its characteristics, *except* for its spin. Although this hyperon was discovered more than 40 years ago, a conclusive measurement of its spin has only recently been obtained by Aubert et al. (2006).

[5] See (Bangu, 2008, pp. 243-248).

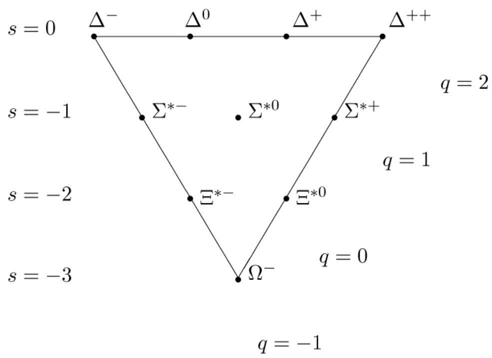

Figure 1: spin-$\frac{3}{2}$ baryon decuplet.
Credits: http://math.ucr.edu/home/baez/diary/march_2007.html.

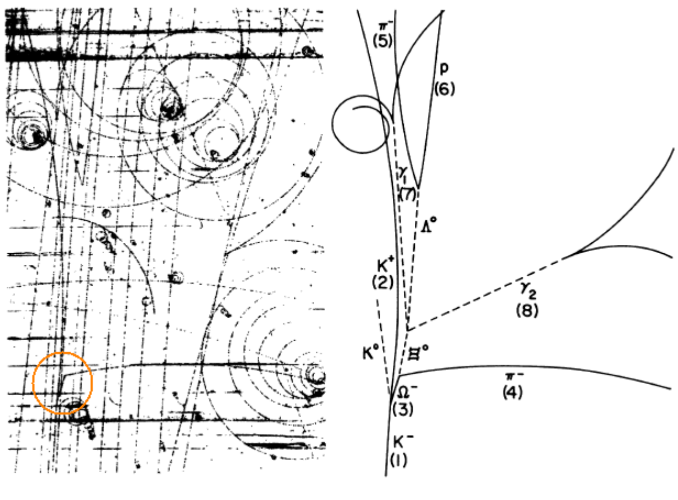

Figure 2: Photograph (left side) and line diagram (right side) of the decay of an Ω^- particle in a bubble chamber. The short track of the Ω^- particle is highlighted by the circle in the low left corner.
Credits: Barnes et al. (1964) (Brookhaven National Laboratories).

(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. (It is similar in so far as it is, like them, an element of the scheme).*
(P3) – *The physical existence of a baryon having the predicted characteristics is not forbidden (can occur in nature).*
(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.*
(C) – *The apex position has a physical interpretation. (That is, the coordinates of this position describe a 10th spin-*$\frac{3}{2}$ *baryon.)*

«This line of reasoning—Bangu glosses—is supposed to answer the question asked by the experimentalist physicist ready to perform the detections, namely 'What are the grounds to believe that (a) there is an entity (b) having the predicted physical characteristics?'». (Bangu, 2008, p. 245)

2.2. GMNPR and the DN model

The need to introduce RP is not *prima facie* evident and in order to justify its employment Bangu compares the Ω^- prediction with other two cases of 'standard' prediction (standard in respect to the DN model)[6]: Urbain Leverrier and John Adam's prediction of the planet Neptune and Wolfgang Pauli's prediction of the neutrino. In both these cases, the existential predictions are accounted for within this model with no particular difficulties: when accurate measurements reveal that there is an anomaly in a physical system (which is supposed to be properly described by a set of laws), then we have two possibilities to explain away this anomaly: either (A) we try to correct the laws, keeping the initial conditions unchanged; or (B) we keep the laws unchanged and we hypothesize that the initial conditions are different from what we have previously supposed to be the case. Clearly, (A) is a very costly option to follow. So, preferably we opt for (B), and thus we suppose that something exists that was not previously accounted for, we make a prediction, and then we test it empirically.

In the two standard cases the new entity is postulated for *explanatory* reasons: the neutrino and the planet Neptune were postulated in order to *explain away a physical anomaly*. Moreover, the physical characteristics of the postulated entity are inferred from an analysis of the physical *interactions* between the hypothetical entity and the rest of the system. For example, the characteristics of Neptune (mass, position, velocity, etc) were inferred from the analysis of how the new entity should be in order to explain (away) the anomaly. Thus, the criteria on which Bangu bases his comparison are the following two: (a) *explanation* and (b) *interaction*. If the DN model could account for the Ω^- prediction, then this should satisfy conditions (a) and (b); but, according to Bangu, only condition (a) is satisfied:

[6]According to the DN (deductive-nomological) model (see for example Hempel (1965)), to explain a scientific fact is to show how that fact can be derived from (i) a set of laws of nature and (ii) some initial conditions—which is the same as to predict it by applying the laws to the appropriate circumstances.

(a) *Explanation*: In order to say that the Ω^- particle prediction was made for explanatory reasons, Bangu has to show what the anomalous phenomenon to be explained away is. According to him, the regularity mentioned in the P1 premise (*i.e.*, the fact that *nine* positions *out of ten* had a physical referent) can be explained only if we assume that, in fact, the law-like generalization H is a true law of nature. But, according to Bangu, if we assume the law-like generalization H, it would be an anomaly *not* to have a referent for the apex position. The anomaly to be explained away is, in this case, the presence of an empty place in the SU(3) scheme. Thus, according to Bangu, in this respect (*explanation*) there is no difference between the case under examination and the two standard cases of existential prediction.

(b) *Interaction*: It is here that RP comes in. The properties of Ω^- are obtained on the basis of the analogy between the elements of the formalism (by means of P1 and, mainly, P2). But to conclude C from P1 and P2—Bangu argues—we need RP. More precisely, RP is indispensable in 'spelling out' the properties of Ω^- from the formalism: «[...] Gell-Mann did just that, indicating that the new baryon should have the characteristics arrived at via Γ', the unoccupied spot in the scheme. Thus, this principle plays the crucial role of giving precise indication about the characteristics of the new particle». (Bangu, 2008, p. 248)

In sum, according to Bangu the crucial role played by RP is just what makes the Ω^- different from the other typical DN predictions. The point at issue is that its physical characteristics «[are] *not* based on the calculation of the parameters of its interactions with other elements of the system. These characteristics are postulated *directly* from the formalism, by relying on RP». (Bangu, 2008, p. 249)

2.3. Bangu's conclusions

Apparently there is no way, for a standard methodologist, to solve the above problem: the DN model *cannot* account for the prediction of the Ω^- particle.[7] Bangu takes it that this particular case of existential prediction suggests that a novel, non-standard, pluralistic stance on methodology should be taken. On the other hand, he argues, RP cannot be assumed as a generalized methodological principle, simply because there are situations in physics in which RP does not work. Therefore, we are not justified in taking RP as a valid methodological principle, even if we accept a non-naturalistic explanation of why RP could work—simply because it *does not always work*.

These considerations seem to justify Steiner's conclusion about the fact that contemporary physicists rely on non-naturalistic strategies in order to predict and discover new entities and new laws. In fact, to a certain extent, they *do* justify such a conclusion. However, Bangu does not seem to be really well-disposed towards this outstanding conclusion, and prefers to opt for a weaker, middle-way approach, that he calls

[7] Bangu takes into consideration, and rejects, three possible attempts that a standard empiricist/naturalist methodologist could make to solve the problem raised in connection to the DN model: (1) saying that RP just served a heuristic (not justificatory) role; (2) emphasizing the 'physical reasons' underlying the prediction to the detriment of the formal ones (RP); (3) observing that it is not the formalism itself that predicts, but our *interpretation* of it. Bangu discards all these three attempts as unsatisfying. I agree with him that the Ω^- case cannot be accounted for by the DN model, and that these three attempts fail in solving the problems *in connection to the DN model*. However, I will show in the following that the third attempt is particularly promising *when it is pursued within a proper account for mathematical representativeness.*

"methodological opportunism". Since the problem dealt with until now concerns more the scientist-qua-methodologist than the working scientist, Bangu suggests that the working scientist should not be inhibited by epistemological considerations:

> This kind of puzzle, the scientist-qua-methodological opportunist stresses, should not be allowed to *interfere* with the practice of science, as no attempt to offer a principled, systematic account (either standard or non-standard) of those episodes should be followed too far or taken too seriously. (Bangu, 2008, p. 256)[8]

I perfectly agree with Bangu on this consideration. The problems we are dealing with are philosophical problems and there is no particular reason for working scientists to let them hinder them in their work. However, as philosophers, we cannot accept this conclusion, and we are asked to clarify the methodology at play in existential predictions like the one of the Ω^- particle. In the next sections I will suggest a possible solution to these methodological worries.

3. Avoiding reification

3.1. Problematicity of RP

It is clear that RP is the main source of our problems. Indeed, if we offered an alternative reconstruction of the GMNPR which does not resort, in any sense, to RP, these problems would vanish.

As Bangu notices, RP is problematic for two main reasons: (I) it is not always reliable; and (II) it has no justification other than its effectiveness in this case. These two reasons are, in a certain sense, strictly interwoven: we cannot justify RP *also because* it is not always reliable. An example of why RP is not always reliable is the following. Consider the case of the applicability of analytic funtions to thermodynamic: we know we can treat the critical temperature of a ferromagnet as an analytic function of the number of its dimensions. But, *applying RP*, we should admit magnets of dimension 3.5, or even $2 + 3i$. This is patently meaningless. Clearly, this is not the intended use of analytic functions. Analytic functions seem rather to be simply a calculational tool. Since we cannot calculate the problem for the 3-dimensional magnet, we calculate it for a 4-dimensional magnet, then we expand the function as a power series in a complex plane around the number 4, and finally we plug in the value 3. All these values are "formally similar", in a certain sense; notwithstanding, nobody will conclude from this (*via* RP) that we should look for magnet of such dimensions. We might still defend RP by saying that we cannot apply RP to this case because the elements of the formalism at issue are not "formally similar" in the required sense. But what is this "required sense"? This seems to be a case of *ad hoc* maneuver.

[8]In his 2012 book, Bangu seems to be no more interested in these methodological considerations. Rather, he accepts the shocking strangeness of the reification principle (or "identity principle", as he now prefers to call it) and relies on it to propose a new version of the indispensability argument. According to me, if we can offer an alternative reconstruction of the Ω^- case (not based on RP), and if this alternative reconstruction can be successfully exported to all the other cases to which this new version of the indispensability argument seems to apply, then I think we have no reason to introduce such a new version of the indispensability argument. However, I will not deal with this argument in the present paper.

These considerations show that we should add two other reasons why RP is problematic: (III) the condition for its application depends on the notion of "formally similar" which is too vague and it seems that we cannot specify it in a non-*ad hoc* way; and (IV) its extention is too wide; namely, it does not apply to *specific* mathematical structures (or formalisms), but it applies to the notion of mathematical structure (or formalism) *in general*. Wherever there is a mathematical structure having a physical referent for one of its elements Γ, we should expect that all the other elements "formally similar" (whatever this means) to Γ will have a physical referent too—but this is not always the case.

At this point we might be tempted to reformulate RP in order to avoid these difficulties. For example, we could say that RP is just an hypothesis. Namely:

> (RP*) – *Let us hypothetically assume that 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.*

This alternative may solve difficulties (I) and (II), but leaves difficulties (III) and (IV) untouched. For such an hypothesis is not always reasonable. For example, we have seen that it is not in the case of the application of analytic functions to thermodynamic. When is such an hypothesis reasonable and when is it not? Again, the notion of "formally similar" should give us the condition under which such an hypothesis can be formulated or not, but this notion is too vague and must be based on *ad hoc* considerations. Another attempt could be the following:

> (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' may (not necessarily) have *a physical referent as well.*

In this way we avoid difficulty (I), since this formulation does not force us to say that RP will always commit us to the existence of a physical referent for Γ'. However, we do not escape all the other difficulties. In particular, there seems to be no reason to assume such a principle (II); the condition for its application still rely on the vague notion of "formally similar" (III); and it scope is still too wide, since such a principle should apply to any mathematical formalism (IV). Further, RP** is highly uninformative, since it does not tell us when there is a referent for Γ' and when there is not.

More generally, any attempt to give RP a hypothetical turn (in order to weaken it), would result in a hypothesis which is *still too strong*. For we know from the beginning that this hypothesis would result in some failure (like in the thermodynamic case above mentioned), but it seems that this is not a *falsification* of such an hypothesis. Indeed we can apparently formulate it even if we know that in some cases this hypothesis will fail. This problem seems to be connected with difficulty (IV), namely with the fact that the extention of RP is too wide. In short, there seems to be no way to save RP (or some similar reformulation of it) and avoid at the same time difficulties (I), (II), (III), and (IV).

Moreover, we have previously seen that, according to Bangu, RP seems to play an indispensable role *only* in 'spelling out' the characteristics of the new particle from the mathematical formalism. The prediction of the new entity, on the other hand, is justified by the need of explaining away an anomaly—just like it happens in the standard cases exemplified by the prediction of the planet Neptune or of the neutrino.

In the next section I will take into closer consideration the role played by RP in Bangu's reconstruction. I will argue that in Bangu's account RP plays a role *not only* in spelling out the characteristics of the new particle, *but also* in determining the anomaly that, in his reconstruction, should motivate the 'explanatory' prediction. Bangu seems not to recognize this second role of RP in his account, and because of this he fails in recognizing an important distinction that we must trace between two different kinds of representing mathematical structures. By recovering and underlining this important distinction (sec. 3.3), it will be offered to us the key to solve and overcome the difficulties connected with RP (sec. 3.4).

3.2. Is there any anomaly here?

As we have seen in sec. 2.2, Bangu takes it that in the history of the Ω^- prediction as reported by Ne'eman and Kirsh, there is an 'anomaly' to be explained away. In this regard, he says, there is no difference between the Ω^- case and the other standard cases of existential prediction. However, when he tries to pindown this anomaly, he says that it consists in the fact that *if* no resonance (having the predicted characteristics) were observed, *then* there would be an empty place in the symmetry scheme: «by assuming the law-like generalization H, it *would* be an anomaly not to have a referent for the apex position» (Bangu, 2008, p. 247. Italics mine).

In other words, in Bangu's view there is not any anomaly yet: there *would* be only if no resonance were observed (or if a resonance without the predicted characteristics were observed). Bangu evokes a ghost anomaly as if it were real, but in effect we do not have to do here with an *actual* anomaly. In the standard cases, the anomaly is revealed by the measurements and its presence looms over the theory like the sword of Damocles. It is *actual* in this precise sense: that until the anomaly is removed, the ability of the theory to account for the phenomena at issue is seriously jeopardized. On the contrary, in the Ω^- case there is no empirical observation that jeopardizes our law-like generalization H. Thus, we could say, the prediction of the Ω^- particle seems to be made in order to *prevent* an anomaly, rather than to *explain it away*. Later on, in his 2012 book, Bangu seems to realize that the alleged anomaly is just a 'ghost' anomaly; he precises the point by saying that the Ω^- prediction has been made in order «to deal with (eliminate, prevent) an anomaly» (Bangu, 2012, p. 102).

But there is another aspect we should consider. The anomaly that we want to prevent may assume two different forms: either (A) we could find no particle at all; or (B) we could find a particle not having the predicted characteristics. These two cases must be carefully distinguished. In case (B) we would have a real anomaly, since the measurements cannot be accounted for by our theory. In case (A), instead, the anomaly seems to consist simply in the fact that the symmetry scheme could turn out to have an empty place. But if this were the case, would it be really an anomaly?

It would be an anomaly only if we already presuppose that any place in the symmetry scheme *must* have a physical referent—which is exactly what RP says. Thus, the presence of an empty place in the symmetry scheme (i.e., of a place in the formalism having no physical referent) can be regarded as an anomaly *only if* we presuppose the validity of RP. This is a very odd situation: the anomaly exists *only if the principle that should explain away the anomaly is valid*. Namely, the anomaly can be identified as such *only in the light of RP*.

Moreover, if (A) were the case, would we be compelled to drop our mathematical structure as no longer reliable? Suppose for example that experimentalist physicists had not found any new particle corresponding to the characteristics pointed out. Should we drop the SU(3) symmetry scheme? This seems unreasonable, for it can still be regarded as a valuable tool for *representing* the class of spin-$\frac{3}{2}$ baryons.

Hence, the fact that the formalism seems to commit us to the existence of an entity that does not exist cannot be regarded as wrong. There are many cases in which a formalism commits us to entities that we do not regard as actually existing, but still we continue to use those formalisms without worrying about these 'fictional' entities. Again, we can consider the case—previously discussed (see p. 7)—of the applicability of analytic funtions to thermodynamic. Cases like these—and the hypothetical failure (A) for the Ω^- prediction falls within this group – point out that there is an important distinction to be made here about the representative role of mathematics in physics. On a first approssimation, we can say that a mathematical structure can play a representative role *without being* fully *representative*; or, in a slightly different terminology, we can say that a mathematical structure playing a representative role can be either 'perfectly fitting' or 'redundant' (*i.e.*, 'not perfectly fitting'). In the first case, *every* element in the mathematical structure plays a representative role; in the second, this is not the case. Importantly, the fact that a mathematical structure is 'redundant' does not necessarily undermine its representative effectiveness.

Thus, Bangu did not distinguish between failure (A) and failure (B), and between 'redundant' and 'prefectly fitting' mathematical structures. For this reason, it escaped attention that only failure (B) would jeopardize the representative effectiveness of the mathematical structure here employed. Presumably, Bangu did not recognize these differences because, *in the light of RP*, these differences simply collapse into each other. In the next section I will present a more precise account for the distinction just sketched. On the basis of this account, it will be later possible (sec. 3.4) to reconstruct the reasoning that led to the prediction of the Ω^- particle in a way that avoids RP.

3.3. Mathematical representativeness

As a general framework, let us say that a mathematical structure M can be an effective 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.[9] In a slightly different terminology, we can also say that if there exists a monomorphism ϕ from S to M, then we can use M as a *model* for S.

[9]The expressions "physical structure" and "physical system" (or "physical phenomenon") are quite problematic. Actually, if we speak of "physical structure", it is much clearer in which sense a mathematical structure may preserve the physical 'structure' at issue, but how can we distinguish a mathematical structure from a physical one? On the other hand, if we speak of physical 'system' (or 'phenomenon'), then in which sense can a mathematical structure *preserve the structure* of this physical 'system' (or 'phenomenon')? I will not discuss this difficulty here, since it falls outside the scope of the present paper. A solution is offered in Ginammi (2014, 2015). For the present aims, we can say that there is an intuitive sense in which we can speak of a physical 'structure'; namely, we assume that a physical 'system' (or 'phenomenon') exhibits a certain inner order among its relevant elements, and the expression "physical structure" refers to this order.

Why exactly a monomorphism? What we want is that the mathematical representing structure grasps *univocally* and *distinctively* every *relevant* element of the physical domain we wish to represent. More precisely, 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*—and the only way to grant this is to impose that the homomorphism ϕ is injective, i.e. that it is a monomorphism.

It is important to notice that this monomorphism condition is just the *minimal* condition for representativeness; this means that if a mathematical structure does not *at least* embed the physical structure that it aims to represent, then it fails in representing it. However, stronger structure-preserving relations may hold. For example, a mathematical structure may even be *iso*morphic to its target, since in this case the monomorphism condition is trivially satisfied.

It is now clear in which sense this minimal condition may account for the distinction previously introduced between 'perfectly fitting' and 'redundant' mathematical structures playing a representative role. When the physical system at issue is monomorphic to a certain mathematical structure, then we can use this mathematical structure to represent (or as a model for) the physical system. When *only* the minimal condition is satisfied, the mathematical structure may be 'richer' than its physical target, since it contains elements that have no correspondence with elements in the physical structure. In this case, recovering our previous terminology, we say that the mathematical structure is 'redundant', or 'not perfectly fitting'. On the contrary, when the isomorphism holds, the mathematical structure is said to be 'perfectly fitting'. Thus, a mathematical structure can be 'redundant' and at the same time suitably represent a certain physical target. Moreover, we can now better express the distinction we made in sec. 3.2 between the two different failures (A) and (B) that might occur in testing the Ω^- prediction. Failure (A) would not undermine the monomorphism condition, whereas failure (B) would. Actually, failure (B) would show that there exists in nature a particle that is not represented by the mathematical structure employed; on the contrary, failure (A) would simply show that the mathematical structure emplyed is redundant. But, as we have just seen, a mathematical structure can be redundant and still be representatively effective.

Now, if a mathematical structure is prefectly fitting (*i.e.*, isomorphic to the target physical system), then, given the proper interpretation, we can just 'read out' from it relevant information about the target physical system. For, given the isomorphism, every element (function, relation, ...) in the mathematical structure will correspond to some element (property, process, relation, ...) in the target physical system, and with the proper interpretation we can 'convert' our knowledge about the model into knowledge about the target. The same can be done even if the mathematical structure is redundant (*i.e.*, non-isomorphic to the target physical system), but only if two important conditions are satisfied: (i) we have to know which elements of the mathematical structure *do* play a representative role and which *do not*; (ii) we have to know the proper physical interpretation for *all* these representing elements in the mathematical structure (namely, we have to know what these representig elements stand for in the target physical system). If these two conditions are satisfied, then we can just look at the *representing* part of the model and 'read out' from these *representing* elements (functions, relations, ...) relevant information about the target physical system. The non-representing part of the model will then be simply skipped.

Now, the question is: What if our model *does not* satisfy these two conditions?

Namely, what if we have reason to believe that a certain mathematical structure satisfies the monomorphic condition (*i.e.*, that this mathematical structure *is*, after all, a model for the target physical system), but we do not know (i) which elements of this mathematical structure *do* play a representative role and which *do not*, and (ii) we do not know the proper physical interpretation for *all* these representing elements in the mathematical structure? In these circumstances we cannot use the mathematical model in the way just mentioned; that is, we cannot simply 'read out' from it relevant information about the target physical system. However, all is not lost! Indeed, such is my proposal, we can still use this model (because this is, after all, a model, since it satisfies the monormophic condition) as a 'hypothesis generator'. In other words, we can still rely on the heuristic effectiveness of this model. This can be done if (iii) we know at least that *some* mathematical elements (functions, relations, ...) in the mathematical structure *do* play a representative role and if (iv) for these representing elements (functions, relations, ...) we have a proper interpretation that permits us to interpret them on the target physical system. Conditions (iii) and (iv) premits us ta have at least a minimal 'grip' on how the mathematical structure can represent the target physical system.[10] Now, if conditions (iii) and (iv) are satisfied, we can still use the model as a generator of *verifiable* hypotheses. Indeed, for any mathematical element (function, relation, ...) of which we do not know whether it does play a representative role or not, we can just make the hypothesis that it *does* play a representative role. Then we can find a proper interpretation in order to 'convert' this hypothesis *about the mathematical structure* into a hypothesis *about the target physical system*, so that we can empirically check whether our hypothesis is right or not.

3.4. Mathematical heuristic effectiveness

In my view, the approach sketched in the previous section is highly relevant for our analysis of the Ω^- prediction. As I see it, the situation in which physicists found themselves when they discovered that the proper symmetry scheme was the decuplet scheme, *exactly matches the situation just described*. Let me explain. The physicists were justified to believe that this symmetry scheme was the proper mathematical model for the class of the spin-$\frac{3}{2}$ baryons—namely, that the class of the spin-$\frac{3}{2}$ baryons was *at least* monomorphic to the symmetry scheme that they were employing (the decuplet scheme). They knew that *some* (but not all) of the elements of this decuplet scheme were playing a representative role, which satisfies condition (iii). Moreover, they knew how to interpret these mathematical elements on the physical system considered (they knew that nine state vectors in the decuplet scheme *do* play a representative role and that each one of these nine state vectors had to be interpreted as a particular spin-$\frac{3}{2}$ baryon), thus satisfying condition (iv). Now, if we can argue that the heuristic strategy just described is free from RP's difficulties, we found the solution to our problem. For this reason, we must analyse this strategy more in detail.

This strategy consists of three logical steps that must not be confused. Each step corresponds to the formulation of a hypothesis. The first step is the following one. Let us say that, of a certain element Γ of the mathematical structure M, we do not know

[10] After all, if we do not have such a minimal 'grip', it seems impossible to believe that the mathematical structure satisfies the monomorphic condition.

whether $\varGamma$ plays a representative role or not. We now formulate the hypothesis that $\varGamma$ *does actually play* a representative role. This is just a hypothesis on representative effectiveness of a single piece of a representing mathematical structure. This representative effectiveness is suggested by the fact that the mathematical structure at issue is assumed to satisfy the monomorphic condition. In this sense, the hypothesis is perfectly legitimate: if the mathematical structure—as a whole—can be used as a model, why should not we formulate the hypothesis that a certain element of such a structure does play a representative role?

However, this hypothesis *per se* does not say anything about the target physical system. In order to say something about the target, we need to fill the gap between the mathematical structure and the physical system represented. To this aim, we need to formulate the proper interpretation I for that element $\varGamma$ in the mathematical formalism. This means that we have to formulate a second hypothesis: that the proper interpretation for that specific element is so and so. It is only at this point that we can eventually move *from* the mathematical side *to* the physical system. Now we can formulate a third hypothesis: that there exists *in the physical system* a certain element so and so, which corresponds (*via* the interpretation I) to the element $\varGamma$ in the representing mathematical structure M. This hypothesis can eventually be verified. We can just check for the predicted element and see whether this element exists or not, thus confirming or rejecting the hypothesis.

We have a chain of hypotheses, each one suggesting the following one. The assumption that the mathematical structure M plays a representative role (namely, that it satisfies the monomorphic condition for representativeness) suggests that other elements as well might play a representative role. If one of these elements plays a representative role, then there must be an interpretation that connects it with something in the physical target. This spurs us to look for such an interpretation. When (*if*) this interpretation is eventually found, it immediately casts light on the physical target by suggesting a verifiable hypothesis on it. These three hypotheses are so tightly connected that the three steps may pass unnoticed and be misunderstood as a single step. However, they must be logically distinguished. The first hypothesis (that $\varGamma$ may play a representative role) implies that $\varGamma$ must have an interpretation; now we formulate an hypothesis about the proper interpretation I for *varGamma*; the existence of this interpretation I for $\varGamma$ implies that the element represented by $\varGamma$ *via* the interpretation I must be found in the physical target; eventually, we formulate the hypothesis that this element *is* in the physical target, and we proceed to check this final hypothesis.

Any single hypothesis of this chain of three may turn out to be wrong at a certain point. We may realize that this mathematical element $\varGamma$ *does not play* any representative role (*i.e.*, that it belongs to the 'redundant' part of our representing mathematical structure); or we may realize that the interpretation we offered is wrong; or, again, our final hypothesis on the physical target may be disconfirmed. For example, in the thermodynamic case discussed above, it is the second hypothesis to fail: we assume that a certain element in the mathematical structure plays a representative role, and we find the most reasonable interpretation for it; but our 'most reasonable' interpretation is that this element should be interpreted as a possible value for the dimension of the magnet, and this is patently absurd—so absurd that we not even have to check it empirically! At this point, we might be tempted to save the first hypothesis by finding a *new* interpretation for this element in the mathematical structure. However, since apparently we cannot find any other reasonable interpretation, we must admit that this mathematical element does *not* play any representative role (and therefore we can consider it as belonging to the 'redundant' part of our representing mathematical structure).

In this reconstruction RP appears *at no point*. Most importantly, the mathematical formalism is *not reificated* in any sense. What premits 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. If we can still talk of "reification" at all, then it would apply to the interpretation and *not* to the mathematical formalism. Importantly, this interpretation is just another hypothesis, and as we have seen it may turn out to be wrong. In some cases we are not even able to suggest a reasonable interpretation. There is no worry in this. We are simply generating hypotheses that we may then verify, and confirm or reject on the basis of their fruitfulness.[11] In different words, we are just employing a mathematical structure as a model for a target physical system and we are exploiting the heuristic potentialities of this model. In this sense, the employment of mathematical models for heuristic aims should not be more problematic than the employment of *physical* models for the same aims.

Moreover, notice that by means of this strategy we eliminate not only reference to RP, but also all the difficulties previously raised in connection with RP (see sec. 3.1). Firstly, the heuristic strategy here described may be successful or not; sometimes it will be, sometimes it will not. But in general it does not raise any methodological worries, and seems to be a reasonable strategy also in cases in which it does not turn out to be successful. Indeed, even when this strategy does not turn out to be successful, it still increases our comprehension of the mathematical model at issue. For, as we have seen, if one of our hypothesis fails (either because we do not find any corresponding element in the physical system, or because we are not able to offer a reasonable interpretation for the mathematical element), then we are reasoned to conclude that this mathematical element *does not play* any representative role, and thusly we gain more knowledge about the representative extent of our mathematical model. In this sense, this strategy is fully justified, and difficulties (I) and (II) do not come up. Secondly, in presenting this strategy we also gave two indispensable conditions under which this strategy can be applied. Namely, this strategy applies only if we know at least that *some* mathematical elements (functions, relations, ...) in the mathematical structure *do* play a representative role

[11] According to (Bangu, 2008, p. 255), we must distinguish between two notions of "interpretation": the standard and innocuous one occurring in statements such as «one needs to interpret the symbol F in "$F = ma$" in order to get empirical predictions»; and the non-standard and problematic one occurring in the Ω^- prediction: Gell-mann's «interpretation proceeded (consciously) along analogical-pythagorean lines and was brought to a specific conclusion by using the (RP)» (p. 255). The second notion is problematic just because it relies on RP. However, in the Ω^- prediction the tenth state vector has been interpreted as a tenth spin-$\frac{3}{2}$ baryon just because the other nine state vectors had already been interpreted as spin-$\frac{3}{2}$ baryons. It was the most obvious interpretation, since it was coherent with the other interpretations already given for the other nine state vectors. So, what is the problem here? According to Bangu the problem is that the interpretation of the tenth state vector has been offered by means of RP. But what about the other nine state vectors? They have not been interpreted by means of RP. So, why should we think that this tenth interpretation of a state vector as a spin-$\frac{3}{2}$ baryon should imply RP? And why should it be more problematic than the previous interpretations of the other nine state vectors as spin-$\frac{3}{2}$ baryons? I do not see any reason to say that RP played a role in the interpretation of the tenth state vector, and to say that this interpretation is more problematic than the other previous nine interpretations we have already given for the other nine state vectors. We are just staying 'loyal' to our previous interpretation.

and if for these representing elements (functions, relations, . . .) we have a proper interpretation that permits us to interpret them on the target physical system (conditions (iii) and (iv), see p. 12). These conditions are sufficiently precise and, by avoiding vagueness, they also avoid difficulty (III). Eventually, this strategy—and the hypotheses that we formulate within this strategy—is always relative to a specific mathematical structure that already proved to be representatively effective, and not to mathematical structures in general. In this way, we avoid also difficulty (IV).

Finally, we may offer our logical reconstruction of the reasoning that led Gell-Mann and Ne'eman to predict the existence of the Ω^- particle. As it should be clear at this point, our claim is that Gell-Mann and Ne'eman just exploited the heuristic effectiveness of the mathematical structure they were employing (the decuplet scheme) to generate a verifiable hypothesis. They first assumed that the tenth (and last) state vector in the mathematical structure *does actually play* a representative role.[12] This led them to look for a possible and reasonable interpretation for this tenth state vector. Since physicists already interpreted the other nine state vectors in the mathematical structure as corresponding to as many spin-$\frac{3}{2}$ baryons (and the properties of these state vectors as specific properties of the corresponding particles), they naturally interpreted this tenth state vector, in the same fashion, as a tenth spin-$\frac{3}{2}$ baryon (and the properties of this state vector as specific properties of the corresponding particle). Eventually, this interpretation led them to explicity formulate the prediction that a tenth spin-$\frac{3}{2}$ baryon, with such and such characteristics, exists—which was then confirmed.

4. Conclusion

In the present paper, I offered a reconstruction of the reasoning that led Gell-Mann and Ne'eman to predict the existence of a new spin-$\frac{3}{2}$ baryon, the so-called Ω^- particle. This reconstruction is alternative to a previous attempt by Bangu (2008), which was based on the employment of a so called "reification principle". Bangu himself considered this principle to be highly problematic, but he saw no way to avoid it. The alternative reconstruction here offered *does* avoid this reification principle, and shows how we can account for a prediction like the one of the Ω^- particle without postulating any sort of 'reification' of the mathematical formalism. This alternative reconstruction is instead based on the representative role of mathematics, and on the heuristic effectiveness of mathematical structure that satisfies some precise conditions. I proposed that when these conditions are satisfied, we can employ these mathematical structures as 'hypothesis generator', in a way that resembles the way in which we may use physical models to formulate verifiable hypotheses on the target physical system.

This heuristic effectiveness of mathematical structure is a general resource on which we can rely also in many other cases. Indeed, this account may well be adopted to account for other cases of existential prediction in physics that do not seem to be accountable by means of the so-called DN model. For example, cases like Dirac's prediction of the positron (in 1930) or Mendeleev's prediction of new elements on the basis of the periodic

[12] The fact that this tenth state vector is also the last 'free' state vector in the mathematical formalism implies that this hypothesis accidentally coincides with the hypothesis that the mathematical structure is *perfectly fitting* (or non-redundant), namely that the mathematical structure is isomorphic to the target physical system.

table may well be exposed to the same theoretical treatment that has been employed here for the prediction of the Ω^- particle.[13]

Finally, in so far as Bangu's reification principle supports Steiner's 1998 conclusions about the peculiar and controversial role played by mathematics in contemporary physics' advancements, I hope that the considerations here presented may convince the reader that this role may be peculiar in *some* sense (more precisely, in the sense that it cannot be accounted by means of the DN model as other standard existential predictions), but it should *not* be taken to be controversial or unaccountable for by means of standard methodological criteria. More work is certainly needed in order to reject Steiner's conclusions, and the single case-study here presented is not enough. But I hope that the considerations here presented will be of some interest for future works in this direction.

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[13]The first example, Dirac's prediction of the positron, is discussed by Bangu (2012), who reconstructs the reasoning that led to this prediction in the very same way he reconstructed the reasoning that led to the prediction of the Ω^- particle (by employing RP). The same considerations we made here for the Ω^- particle, may be easily exported to this other case of existential prediction (with reference to another mathematical structure, of course). I trust that the same thing can be done also with Mendeleev's prediction.