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Mathematics and Physics within the Context of Justification: Induction vs. Universal Generalization

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Motivated by the analogy which holds within the context of discovery between mathematics and physics, we aim to show that there is a connection between two fields within the context of justification too. Based on the careful analysis of examples from science (especially within the domain of physics) we suggest that the logic of scientific research, which might appear as enumerative induction, is deduction, and we propose it to be universal generalization inference rule. Our main argument closely follows the analysis of the structure of physical theory proposed by theoretical physicist Eugene P. Wigner.

Keywords: Mathematics-physics analogy, context of justification, enumerative induction, universal generalization, Wigner's account of physics.

1. *Introduction—context of discovery vs. context of justification*

While it might seem unproblematic to defend the view that the analogy between mathematics and the natural sciences holds in the context of discovery, the idea to expand such an analogy to the context of justification seems to be far more problematic. We shall first introduce some preliminaries, that we shall take for granted in this paper and then present our main thesis.

We take the underlying ontology in the philosophy of mathematics to be a version of (standard) platonism but platonism in the philosophy of mathematics won't be discussed in this paper. The development of mathematical knowledge as well as the process of discovery in the natural sciences can be standardly analysed from different perspectives:

we might decide to opt for the cognitive science orientated research or a computationally orientated research, or a historically orientated research. In the context of the examples we further analyse we find the historically orientated research most appropriate.

Within the descriptive epistemic context there were offered three main epistemic routes: (1) perception—both visual and platonic; (2) experimentation and (3) positing (Trobok 2018). For each of these epistemic paths in mathematical research Trobok shows there is a counterpart in the domain of research in the natural sciences. As far as the underlying logic within the context of discovery goes, it is important to underline the difference between formal proofs in mathematics and the heuristic explanatory and exploratory procedures. Lakatos emphatically stresses the difference between formal proofs in mathematics and the heuristics of mathematical discovery (Lakatos 1976). Once such a distinction is brought to surface, the heuristics of mathematics and that in physics turn out to be analogous (Trobok 2018).

The question at this point is: How and to which extent, if at all, could the analogy (that holds between mathematics and the natural sciences in the context of discovery) be expanded to the context of justification? Namely, as Pòlya underlines:

> …many mathematical results were found by induction first and proved later. Mathematics presented with rigor is a systematic deductive science but mathematics in the making is an experimental inductive science. […] In mathematics as in the physical sciences we may use observation and induction to discover general laws. But there is a difference. In the physical sciences, there is no higher authority than observation and induction but in mathematics there is such an authority: rigorous proof. (Pòlya 1945: 117)

While in the context of discovery of both mathematics and physics we have reasons to accept Pòlya's view (Trobok 2018), the aim of this paper is to go one step further and show why Pòlya's view within the context of justification is not accurate. The aim is to show that, not just the two domains are analogous within the descriptive epistemic context, but that the analogy could be expanded to the context of justification as well. When focusing on the context of justification, we shall confine our research to the third epistemic path as above presented: *the experiment*.

2. *Context of justification specified*

In the domain of mathematics, Frege nicely explains what characterises the context of justification in his famous *Grundlagen* paragraph:

> …it is in the nature of mathematics always to prefer proof, where proof is possibile, to any confirmation by induction. […] The aim of proof is, in fact, not merely to place the truth of a proposition beyond all doubt, but also to afford us insight into the dependence of truths upon one another. After we have convinced ourselves that a boulder is immovable, by trying unsuccessfully to move it, there remains the further question, what is it that supports it so securely? (Frege 1884/1967: §2)

Let us analyse more closely the mainstream view regarding the difference between mathematics and the natural sciences within the context of justification according to which:

> The status of mathematical knowledge […] appears to differ from the status of knowledge in the natural sciences. The theories of the natural sciences appear to be less certain and more open to revision than mathematical theories. (Horsten 2017)

We shall try to show such a view being flawed by concentrating our line of argumentation on the notion of experiment. The standard view is that experiments belong to the empirical sciences, i.e. to the sphere of practical research. Experiments are practical procedures generally done by researchers in laboratories. Hence, what happens in experimental science might seem at first sight to be remote from the standard mathematical practice, mathematics being an armchair activity. And even if someone like Putnam (Putnam 1979: xi) admits that there are mathematical procedures that could be labelled as experiments (e.g. the adoption of the axiom of choice), experiments do not belong to the mathematical domain.

Standardly, experiments play several roles in science: we use them to test theories, to call for a new theory, to help us determine the structure or mathematical form of a theory, or to provide evidence for the entities involved in a theory (Franklin and Perović 2019). They hence belong to the intersection of the context of discovery and the context of justification. At first sight, someone might complain that experiments are practical procedures done in laboratories and that nothing in the mathematical domain can be analogous to such procedures, especially given the *a priori* nature of mathematical research. A closer analysis of the concept of experiment will show us, though, that the way we are accustomed to perceiving experiments does not correspond with neither the nature nor the role that experiments have played throughout the history of natural sciences (especially physics).

Galileo Galilei, the *father* of experimental physics, includes in his (Galilei 1638) the taxonomy of experiments. There are, according to Galileo, three types of experiments: real, imaginary and thought experiments. The real are those that have been performed in practice, the imaginary are those that could have been performed but haven't yet been, while the thought experiments are those that could not possibly have been performed due to the lack of technology or because impossible in principle. What is of interest to us is the fact that thought experiments are not marginal for the development of physical theories. Quite the contrary, such experiments have played a major role in the development of scientific theories in the work of Galileo, Newton, Einstein, Heisenberg *et al*. Let us mention some of the most famous thought experiments: Galileo's experiment with the result that all bodies fall at the same speed, Maxwell's demon, Einstein chasing a light beam, the twins paradox, Heisenberg's microscope, Schrödinger's cat.

Some of those experiments[1] are analogous to deductive mathematical proofs, so the analogy between the empirical physics and the *a priori* mathematics reveals itself to be of quite an importance. Let us have a closer look at the Galileo's experiment with the result that all bodies fall at the same speed (Galilei 1638).

Galileo proved, by using a thought experiment, Aristotle's theory of gravity to be flawed. According to Aristotle's theory, objects fall at the speed directly proportional to their mass. More than seventeen centuries later, Galileo writes:

> Aristotle says that "an iron ball of one hundred pounds falling from a height of one hundred cubits reaches the ground before a one-pound ball that has fallen a single cubit." I say that they arrive at the same time. (Galilei 1638/1914: [109])

The proof he offers is the following one: Galileo imagines two bodies H and L, one (H) heavier than the other (L), that are attached one to another. According to Aristotle, the compound body (H + L) falls faster than the body H, since the compound body is heavier. It means that the velocity of the united bodies is bigger than the velocity of the heavier one: $v(\mathrm{H} + \mathrm{L}) \geq v(\mathrm{H})$. On the other hand, as Galileo nicely explains:

> … when the small stone moves slowly it retards to some extent the speed of the larger, so that the combination of the two, which is a heavier body than the larger of the two stones, would move less rapidly … (Galilei 1638/1914: [109])

It follows that the velocity of the compound body should be smaller than the velocity of the H body: $v(\mathrm{H} + \mathrm{L}) \leq v(\mathrm{H})$. From the two equations it follows mathematically that the two velocities are equal: $v(\mathrm{H} + \mathrm{L}) = v(\mathrm{H})$. Galileo's result follows *deductively* from Aristotle's presumptions. Even though thought experiments clearly can serve as examples of deductive proofs in physics, such results are often treated as exceptions. The mainstream view being that:

> … the methods of investigation of mathematics differ markedly from the methods of investigation in the natural sciences. Whereas the latter acquire general knowledge using inductive methods, mathematical knowledge appears to be acquired in a different way: by deduction from basic principles. […] The status of mathematical knowledge also appears to differ from the status of knowledge in the natural sciences. The theories of the natural sciences appear to be less certain and more open to revision than mathematical theories. (Horsten 2019)

3. *Induction vs. universal generalization*

Are thought experiments marginal exceptions to the standard methods of discovering the laws of physics (science), and is the view that *in the*

[1] On the other hand, some of the thought experiments might be viewed as examples of inductive logic as advocated in (Norton 1991), but we would argue that in those examples as in the examples of real experiments, deductive rule of universal generalization is at work.

physical sciences, there is no higher authority than observation and induction (Polya 1945: 117) the right view?

Let us start with another experiment, this time from chemistry.[2] In 1828 Friedrich Wöhler was trying to synthesize ammonium cyanate from silver cyanate and ammonium chloride and obtained a white powder which he suspected was not the desired compound but could not test it as it was not obtainable in the pure enough form. He tried a different pair of chemicals, lead cyanate and ammonium hydroxide, and obtained what appeared to be the same white powder which he was now able to further analyse. What Wöhler incidentally discovered was an organic compound, urea,[3] and he prepared it outside a living organism which was later deemed as a breakthrough discovery (at the time it was believed an organic compound could be obtained within living organisms only[4]).

In order to be sure of the obtained result, Wöhler should have repeated the same experiment over and over again in order to be able to finally conclude, *inductively,* that it was possible to obtain an organic compound outside a living organic system. Wöhler, however, would have considered such number of repetitions of the same experiment to be unnecessary. Why? Because he was aware that the experiment he performed was an arbitrary experiment of this type. It means that whatever happened in that experiment would happen in any other experiment performed under same relevant conditions (say, having all the glassware very clean, certain temperature or pressure maintained etc.) and with same chemicals, and no matter where and when the experiment is performed.

His inferential step was, hence, of the form: in the experiment performed, the lead cyanate could be converted into urea. There was nothing specific about the lead cyanate used, nor was the experiment performed under some unusual conditions. Hence, whatever result would be obtained, was a general one, i.e. could be generalized as holding for *any* lead cyanate. This is the inference as far as the synthesis of urea goes. If one wants to further use it to disprove vitalism, that is to defend a general claim, that an organic substance could also be obtained outside

[2] It will be seen in the following sections how this example is easily transferred to modern experimental physics.

[3] The equation of the chemical reaction in question is: $Pb(OCN)_2 + 2\ NH_3 + H_2O \rightarrow PbO + NH_4OCN \rightarrow H_2NCONH_2$. The last chemical formula is the formula for urea.

[4] Actually, the full history of the refutation of vitalism (the then prevalent doctrine that organic compounds characteristic of the living organic systems could only be obtained within such systems) is a bit more complex. For, although Wöhler did perform the very first such chemical reaction of synthesis of organic molecule from inorganic ingredients, his ingredients originally came from living substances and so, some claimed, a part of *vis vitalis* (the living force which was actually responsible for producing organic stuff) could have somehow survived and affected the whole process. Wöhler's student Hermann Kolbe is credited as the one who was able to obtain the organic substance (acetic acid which is the main ingredient of vinegar) in a wholly inorganic process from carbon disulfide (Ramberg 2015).

a living organic system, then one only needs to establish that for similar chemical reactions (like the one Wöhler's student Kolbe performed) again there are no further relevant parameters or conditions which were not already present in Wöhler's original experiment (if it really had been perfectly designed which it was not as explained in footnote 4). Of course, one might want to test as many such reactions as possible to try to synthesize all the organic compounds from all the imaginable inorganic ingredients (which has been a larger portion of chemical research since the days of Wöhler!) but that amount of effort is wholly unnecessary in ordert to prove that at least one organic compound can be synthesized from inorganic substances and so to refute vitalism as well.

Now, we do not claim that all the results of experiments or all the discoveries in science were done by following enumerative induction, although for many one might believe that they were. What we do claim, is that all of those results that were thought of as examples of enumerative induction are actually examples of universal generalization. The art of experimentation is then chiefly consisted of finding the set of arbitrary parameters which will allow the reproduction of the phenomenon in question and not hinder its realisation, hence allowing for the relation between the right parameters to emerge for the observer. Here one can also think of further examples from physics, such as the discovery of Boyle's law (of the inverse proportionality of volume to pressure of the gas), or gas laws in general (where there is always a direct relation between two parameters). Neither Boyle, nor any other physicist involved did think there was any need for repeating the same experiment over and over again. It is true that one does repeat a certain experiment testing the dependence of certain number of parameters several times, but not because we should be more certain of the result after the n-th measurement, but because we want to minimize the errors that will, of course, always be present, nevertheless not compromising the result of the measurement.

Indeed, our analysis is not limited to physics only, although we deliberately decided to focus more on (fundamental) physics research. An example from chemistry—paradigmatic for that whole science—was already given. One can also think of many similar examples from biology. Take for instance the most fundamental discovery that every living organism has genes. Once genes were discovered in many examplars of living organisms and their function determined in any one of them, it was certain what their function will be in the specimen of the yet undiscovered species. Surely no one would doubt the degree of confidence of such a result. But can this degree ever be achieved by inductive reasoning alone?

Whenever we infer from an arbitrary situation (or object of the domain) to a general situation (or any object of the domain) we are applying the universal generalization, a deductive rule of inference. Formally we write:

$$\text{Fa} \vdash \forall \text{x Fx}, a \in D,$$

a is an arbitrary object of the domain (D), i.e. the name to be generalised upon must occur arbitrarily.

4. *How is research done in modern physics?*

To re-enforce our conclusion from previous section, that the logic of scientific research in physics (and more broadly natural science) has nothing to do with enumerative induction, we will here consider how is research done in modern physics. First, the analysis will be given due mainly to Eugene P. Wigner (1963; 1965) of the level of knowledge reached and the structure of modern physics, which should shed light on what significant changes happened already in the first half of the twentieth century fundamental physics (meaning quantum theory, relativity theories and quantum field theories). These changes were in how the theoreticians (among others Wigner himself[5]) changed the way of thinking about fundamental problems as well as the way the experimentalists changed the practice of setting up experiments. Second, we will offer what we believe should be the Wignerian reading of a class of experiments, namely the reactions between particles in particle physics.

As there are many accounts (Kaplan 1998) of scientific induction, we shall here focus on enumerative induction. However, it is our plan to undertake an expanded study of how essentially the same critique, based on Wigner's account of the structure of physics, can be used to argue against other types of inductive reasoning. One more caveat is required before proceeding further regarding the Norton's theory of material induction (e.g. 2003; 2005; 2010; 2014) as induction based on material facts that are relevant for the inductive case at hand and without relying on some universal inductive schema. We find Norton's approach very convincing in general, but feel that one can make a step further and deny that there is induction at all in science. Again, this will be elaborated in detail in a further work.

By looking at mostly physics before the twentieth century, one might be excused in thinking that (1) there is not much difference between physics and any other fairly established natural science, say chemistry; and (2) that if the logic of physical research is not always enumerative induction, it is by all means inductive logic of a kind. We, on the other hand, strongly believe, that neither (1) or (2) is acceptable. Why not? Let us look at the two cases individually. Firstly, why would (1) not be acceptable? Modern physics, since the advent of Einstein's

[5] Eugene P. Wigner was one of the first generation of quantum theorists and contributed significantly to research on quantum theory (applications of group theory to quantum mechanics) and its interpretation (especially the so called *measurement problem*) as well as to the theory (Wigner 1965) of symmetries of equations of physical laws which is what will mainly be of interest in this paper. For his contributions to fundamental research in theoretical physics he was awarded the Nobel prize for physics.

relativity and quantum theory (so since the beginning of the twentieth century) became much more general than any other science before or since. We will not go so far to state that it became akin to, say, applied mathematics, but the degree of generality of the most fundamental laws of physics as well as their great reductive power (to serve as foundation to the laws of almost all of chemistry, and therefore much of biology or geology etc.) is quite alike theories in the mathematical sciences. Furthermore, physics in general and theoretical physics in particular, employs great many mathematical techniques not only in what might be called its computational schema, but also in the way physicists think about the laws of nature. One example is the requirement that all the laws must be given as mathematical equations of a sort, most often as partial differential equations. Now, there is no such generally pronounced, and most definitely not generally accepted, view regarding, e.g. the laws of biology, or even genetics (which is much more mathematical than the average branch of biological science).

Secondly, why would (2) not be acceptable? One cannot escape the question of whether (2) is somehow not quite the best suited account of the logic of physics research, once we appreciate: (a) the crucial differences between physics and other (natural) sciences, (b) the fact that its statements possess the degree of generality that statements of no other science even remotely approach, (c) how strongly mathematical its laws are in their character, (d) the level of abstractness of theoretical physics, (e) the philosophical nature of the deepest questions physics deals with, (f) the fact that we derive laws from other more general laws (often without even doing experiments to corroborate the derived laws!), and finally, (g) how we derive whole theories within physics from a more fundamental theory, or by linking a theory to another theor

5. *Wigner's account of the structure of physics*

Wigner in (1963) and to a lesser extent, but in more detail for some of the points, in (Wigner 1965), offers a very plausible account of the whole of physics which is based on our best fundamental theories as well as our landmark experiments. In fact, it can be said his account in the meantime became the keystone of the mainstream approach to discovering new laws of physics. His interpretation of physical theory is based on a symmetry approach to the laws of physics, a movement in physics research initiated by Einstein and founded on mathematics of Hermann Minkowski, Hermann Weyl and Emmy Noether (Rosen 1983). After having discovered that in spite of physics not after all being able to give the spatio-temporal description of phenomena in absolute terms of Newtonian system, there were still some quantities and, more generally, mathematical structures, which remain unaltered when the observer's reference frame is changed—the so called *invariants,* Einstein saw this as a guide for developing new theories. He saw what was later developed as theory of invariants under symmetry transformations as a new general framework for physics. *Symmetry* here means a transformation which preserves some structure (say a mathematical equation which expresses a law of physics) given certain change in variables (say changing the coordinates). Noether showed that to each so called *geometrical symmetry principle* there corresponds a law of conservation of a certain physical property (e.g. to a symmetry transformation with respect to spatial coordinate corresponds the law of conservation of linear momentum). It was later shown that one can (in quantum theory) make like connection for other symmetries. Given that the laws of conservation belong to the category of the most abstract and universally valid laws, one can find way in justifying Einstein's, at the time, bold claim that there is a symmetry approach to discovering the laws of nature.

Wigner stated this symmetry approach especially succinctly (and best in his Nobel prize winning lecture of 1963). In our research in physics we begin as ever with observations, more or less complex in nature or execution of experimental setup required to make these observations. At the next stage of the process of discovering laws there are certain generalizations from the observations: e.g. we abstract the specifics of the region of space and the interval of time pertaining to the observations made, or we abstract the material out of which the tested object is made etc. These *first-instance-generalizations* Wigner calls *correlations*. The correlations might be very crude and not of great degree of generality, which means that they will usually be expressed as mere approximations. Hence, valid only under certain conditions, say, Ohm's law of resistance in electric circuits is valid only for a very limited range of temperatures and materials. We can then perform further experiments to test the range of certain conditions, and here we might as well be using inductive inference techniques, but more on that will follow in the next section. So let us suppress judgement on the issue at this point. The process of further testing and refining the approximations can last for quite a time, sometimes centuries (as in the case of trying to find or refute the luminiferous ether), or for millennia (in case of discovering atoms!). The most important, however, is the next stage in development of a physical theory. And this Wigner calls the stage of forming the more general laws, which indeed can sometimes turn out to be the most general, the so called *correlations of correlations*. The laws of conservation (or, what turns out to be the same, the symmetry principles) are the most general example of correlations of correlations. We discovered each such law by the usual process of positing (hypothesizing) and experimenting on a small sample and for a limited range of values of a certain parameter. In the end, however, we have been rediscovering such regularities over and over again to the point that nowadays practically no physicist doubts the universal validity of the laws of conservation.

After we realized the general validity of the symmetry principles—and this is the crucial point in Wigner's analysis—we are better equipped for discovering further laws of physics which will be of lower level of generality and will, therefore, depend on the symmetry principles. This dependence is twofold:

1. The very existence of the lower level laws depends on the existence of higher level laws, and ultimately all the laws depend on the most general laws, some of which will be the symmetry principles.
2. The validity, or truth, of the lower level laws will depend on the validity of the higher level laws.

As Wigner himself explains regarding (1):

> It is natural, therefore, to ask for a superprinciple which is in a similar relation to the laws of nature as these are to the events. The laws of nature permit us to foresee events on the basis of the knowledge of other events; the principles of invariance should permit us to establish new correlations between events, on the basis of the knowledge of established correlations between events. This is exactly what they do. If it is established that the existence of the events A, B, C, . . . necessarily entails the occurrence of X, then the occurrence of the events A', B', C', . . . also necessarily entails X', if A', B', C', . . . and X' are obtained from A, B, C, . . . and X by one of the invariance transformations. (Wigner 1963: 10)

An example will be described in detail in the next section. It should also be noted that in the sense Wigner understood—and modern physics understands—invariance transformations (again, just another term for symmetry principles), they are to serve the purpose of a kind of selection principles, so allowing physicists to select among the several proposed possible new correlations. The one that will always be selected is the one which is in accord with symmetry principles (which usually means, one or more conservation laws). In this sense, a possible correlation cannot be declared a law of physics—so cannot really exist—if it would violate a law of conservation.

As for (2), Wigner makes the following remarks:

> The preceding two sections emphasized the inherent nature of the invariance principles as being rigorous correlations between those correlations between events which are postulated by the laws of nature. This at once points to the use of the set of invariance principles which is surely most important at present: to be a touchstone for the validity of possible laws of nature. A law of nature can be accepted as valid only if the correlations which it postulates are consistent with the accepted invariance principles. (Wigner 1963: 12)

In other words, if we need to assume the validity of the invariance principle(s) in order to accept the newly proposed law as valid (or, more cautiously, potentially valid), so to assume more general principle in order to prove that the specific, and more particulate, law holds, it means we do not have inductive reasoning at play, but at least in part also a form of deduction. Which form, remains to be examined. What we propose is that at least in some instances of reasoning in physics, or science, it is universal generalization.

Before we proceed to examine a typical case of such reasoning, a further remark is required in order to complete the exposition of Wigner's account of physical theory and, indeed, of physics research as such. Although symmetry principles are very important in physics, it would not be all that good if everything was symmetrical at all times. Pre-requisite for even contemplating an experiment is to know (and appropriately materially realize) the so called *initial and boundary conditions*, so values of parameters which are not included within the symmetry account of the possible situation, and so present an asymmetry of a sort. Only with full specification of all the relevant symmetries, other more general laws and initial and boundary conditions might we approach discovering a new law!

6. *Experiments in particle physics and conservation laws*

The knowledge of conservation laws (symmetry principles) is of paramount importance for not only performing experiments but for even contemplating a new experiment in particle physics or nuclear physics research. What is the reason for this? It is the fact that a nuclear or, generally, a reaction between particles cannot take place unless all the relevant conservation laws are satisfied by the reaction. Physicists have, starting from around the beginning of 20th century up to today, discovered that a reaction between any number of any type of particles can in principle happen given that there is enough energy and that the specific conservation laws are satisfied. For each reaction there is the accompanying list of conservation laws[6]. For example, the list for the reaction[7] of nitrogen (^{14}N) with alpha particle (^{4}He) which has oxygen (^{17}O) and a proton (^{1}p) for products—the famous first ever nuclear transformation of elements, performed in Rutherford's team—would be:

law of conservation of energy,
law of conservation of momentum,
law of conservation of angular momentum,
law of conservation of number of baryons (this is actually easy to show from the equation of reaction, as $14 + 4 = 17 + 1$),
law of conservation of charge (the calculation is same as for the number of baryons if we assume all the particles are bare positive charges).

If any of the listed laws would be violated by what was the proposed reaction, physicists would immediately know that the reaction would not

[6] A good and standard survey of the role of conservation laws in particle physics reasearch and their connection to symmetry principles is (Henley and García 2007: 195–220).

[7] The reaction equation in standard notation is: $^{14}N + {}^{4}He \rightarrow {}^{17}O + {}^{1}p$.

take place and would not even start preparing the experimental setup. The emphasis is on the fact that there is such a complete list for each imaginable reaction and that physicists can check whether a reaction satisfies all the laws from the reaction-specific list.

Let us pause here and ask, *But how can physicists know there is such a list?* Obviously, each conservation law was discovered first as a singular fact of observation, say, it was noticed that the law of conservation of charge is valid for some chemical reactions, and later it was noted that it holds for nuclear reactions too, and so forth. Each time, however, it was valid for a particular instance of a specific reaction. The problems of inductive method of inference are already all there. Let us mention but a few:

The problem of repetition: How do we move from an observation valid for an instance of a type of experiment (a type of reaction[8]) to a conclusion valid generally for all instances of a type of experiment? Next, how do we move to establishing the same conclusion (that a particular quantity is conserved) for a different type of experiment but within the same domain of experiments (reactions between particles of certain type)?

If we take recourse to enumerative induction to make the first generalization, then the question arises, what if there appears a case of an instance of a reaction of a certain type (like the one above mentioned) for which a certain law does not appear to hold? What is the procedure then? We test again, but for what: to disclaim the negative result hitherto found, or to reconfirm this negative result, thereby in effect negating that the particular law is valid for a particular type of reaction? It is not clear, and *prima facie* cannot be clear, as we, by embracing only inductive methods of reasoning in science, cannot accept any *a priori* given fact, or any deductively posited fact. We believe the method here—and in practice of physics (or for similar situations in other sciences) might rather be universal generalization. It makes much more sense, for the reason it avoiding the aforementioned dilemma and also for it immediately being clear how to generalize not only to other instances of the same type of experiment, but also to similar types of experiments (other reactions of different particles or particle type). Taking the other instance of one type of reaction or changing for a reaction between different particles but of the same type of reaction, or switching to another type of reaction is just another arbitrary name to generalize upon.

The problem of generalization: Moreover, if the method of inferring is allowed to be from the range of deductive methods, then it is by no means unusual that we should be guided by other deductively inferred

[8] By a type of reaction it is roughly meant any reaction between a certain type of particles (e.g. a nuclear reaction is between nuclei, decay processes are transformations between nucleons, or constituents of a nucleus, etc.).

facts. Such as the fact that symmetry principles are used across the disciplines of physics, that they can guide research in physics in general (as Einstein and a battalion of first class physicists have been showing for over a hundred years now) and that there is a universally (and mathematically precisely) established connection between symmetry principles and conservation laws (Noether's famous theorems). Finally, as Wigner reasoned, we actually assume the universally valid conservation laws—and a reaction-specific list—each time we embark on testing another possible reaction between particles, or probing matter at a higher energy level, or trying to find a new particle (which is always a product in some particle reaction), most recently (in 2012) Higgs boson particle. If any of the laws on a reaction-specific list of conservation laws is violated by such a reaction, we know in advance of actually performing the reaction that it will not go.

The aprioricity of knowledge: If induction is the whole story behind reasoning in science, there really cannot be any talk of *a priori* knowledge of facts or laws, or theorems. There is always the problem of validating our inferences based on such assumptions and without deductive techniques admitted on the same footing with inductive ones. Take the last claim we made in the previous paragraph, that we can know in advance whether a reaction will go. It might seem innocent enough, indeed a practicing nuclear or particle physicist does not give it a second thought in a day-to-day laboratory work. But what a claim it is! We can know whether something will happen in advance of it happening—and we can know it with certainty, if it will or will not happen! But, making inferences by induction only, we could never reach such certainty!

Moreover, think of how we actually got to this claim: at the very first we observed a singular fact for an instance of a particular nuclear reaction; then we assumed it for all such nuclear reactions; then we generalized that a discovered correlation (a law of conservation) is valid for all reactions in nuclear physics; then we found same law holds for an instance of a reaction between some particles beyond the domain of nuclear transformations, so for a reaction in particle physics; then we generalized for all reactions in particle physics. Finally, we do not anymore question the validity of the discovered law of conservation at hand, or, for that matter, of any of the conservation laws: no one actually anymore investigates the validity of conservation laws in particle physics, they are ASSUMED, indeed so much so, that no planned experiment will ever go operational if only one of the laws from the reaction-specific list is found to be *just theoretically* violated by a reaction in question. As Wigner said, symmetry principles (or conservation laws) are to be regarded as *a touchstone for the validity of possible laws of nature.*

7. *Conclusion*

Starting with analysis of an example of a thought experiment which uses a deductive rule of inference and moving through examples from basic physics and chemistry to, finally, paradigmatic example of experiments in modern particle physics, we are drawn to conclusion that a large and significant portion of physics (science) is deductive in nature. We tried to demonstrate that what were previously thought as prime examples of application of (enumerative) induction in physics or chemistry can best be interpreted as examples of application of universal generalization inference rule. Furthermore, and by relying on an elaborate analysis of Eugene P. Wigner (one of the pioneers of quantum and nuclear physics as well as one of the foremost theoretical physicists of his generation), we showed that a deductive schema of guiding the research in physics is really the most appropriate to at least fundamental parts of that science. It is our aim to review other main purported inductive schemas and to compare with our own approach in the near future.

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