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INDUCTIVE EXEMPLIFICATION IN REAL AND THOUGHT EXPERIMENTS

Abstract

The New Experimentalism, given its opposition between experimental practices and fundamental theories, had to address the problem of the relationship between the particularity of the former and the generality of the latter. Other authors (from Charles A. Baylis and Nelson Goodman to Catherine Elgin) used the concept of exemplification to clarify the relationship between particular and universal/general concepts or laws. But one may ask whether, while hinting at a point of view that can illuminate new aspects of the problem, they have not ultimately left the problem itself unresolved. This article, on the basis of considerations developed elsewhere and following a suggestion found in Aristotle, proposes to link the concept of exemplification to the problem of induction and abduction, both understood here as a multiplicity of methodical procedures aimed at establishing a cognitive relationship between the reproducibility of scientific concepts or laws and the concreteness, locality, and situated character of experimental practices. A necessary prerequisite for the solution of the problem raised by this relationship is the distinction between two senses – one reflexive-transcendental, the other genetic-methodological – of the discovery/justification pair of concepts. This will shed light, at the same time, on the long-standing problem of induction and on the problem of the relationship between the universality of scientific laws and the always local and situated character of experimental practices. The last part of the article shows how these conclusions also apply to thought experiments, briefly discussing John Norton's account.

Keywords: Exemplification; Induction; Inductive Exemplification; Norton's Argumentative View; Popper's criticism of induction; Thought Experiment

INDUKTIVE EXEMPLIFIKATION IN REALEN UND GEDANKLICHEN EXPERIMENTEN

Zusammenfassung

Der Neue Experimentalismus musste sich angesichts seines Gegensatzes zwischen experimentellen Praktiken und grundlegenden Theorien mit dem Problem der Beziehung zwischen der Einmaligkeit der ersteren und der Allgemeinheit der letzteren auseinandersetzen. Andere Autoren (von Charles A. Baylis und Nelson Goodman bis Catherine Elgin) haben das Konzept der Exemplifikation verwendet, um die Beziehung zwischen partikulären und universellen/allgemeinen Begriffen oder Gesetzen zu klären. Es stellt sich jedoch die Frage, ob sie zwar einen Gesichtspunkt andeuten, der neue Aspekte des Problems beleuchten kann, das Problem selbst aber nicht lösen. In diesem Artikel wird auf der Grundlage von Überlegungen, die an anderer Stelle entwickelt wurden, sowie in Anlehnung an eine Anregung von Aristoteles vorgeschlagen, den Begriff der Exemplifikation mit dem Problem der Induktion und der Abduktion zu verknüpfen, die hier als Bezeichnung für eine Vielzahl methodischer Verfahren verstanden werden, die darauf abzielen, eine kognitive Beziehung zwischen der Reproduzierbarkeit wissenschaftlicher Begriffe oder Gesetze und der Konkretheit, der Lokalität und dem situierten Charakter experimenteller Praktiken herzustellen. Eine notwendige Voraussetzung für die Lösung des durch diese Beziehung aufgeworfenen Problems ist die Unterscheidung zwischen zwei Bedeutungen – einer reflexiv-transzendentalen und einer genetisch-methodologischen – des Begriffspaares Entdeckung/Rechtfertigung. Dies wirft gleichzeitig Licht auf das Problem der Induktion sowie auf das Problem des Verhältnisses zwischen der Universalität wissenschaftlicher Gesetze und dem stets lokalen und situierten Charakter experimenteller Praktiken. Der letzte Teil des Artikels zeigt, wie diese Schlussfolgerungen auch für Gedankenexperimente gelten, indem auf John Nortons Auffassung eingegangen wird.

Schlüsselwörter: Exemplifikation; Induktion; induktive Exemplifikation; Nortons argumentativer Auffassung; Poppers Kritik an der Induktion; Gedankenexperiment

Introduction

The new experimentalism had to deal with the problem of the relationship between, on the one hand, the situated, local, and singularly unrepeatable character of experimental practices and, on the other hand, the claim of universality and reproducibility in principle of fundamental scientific theories (see especially Hacking 1993 and Cartwright 1983). The problem was raised by the opposition between experiment and theory, or, from another angle, between low-level phenomenological laws based on experimental practices and fundamental theories of physics.

Other authors – from Charles A. Baylis and Nelson Goodman to Catherine Elgin – have used the concept of exemplification to clarify the epistemological relationship between particular and general concepts or laws. Among those who first and most clearly introduced the term exemplification into contemporary philosophy was C.A. Baylis (see especially 1930, 1948, and 1951), who attempted to construct a theory of meaning and truth in terms of the relationship between "exemplification" and (as its converse) "characterization." In his view, using the term "meaning" for "universal" allows one to avoid old associations and sterile questions concerning the classical problem of the relationship between universals and particulars. For him, the relation of "exemplification" is "a distinguishing mark of meanings," because "any meaning whatsoever [...] bears either this relation or its converse to some other meaning. No particular bears this relation to any other particular" (Baylis 1930, p. 170). As he reasserted a few decades later:

> "Universals are the sort of entity which, except for conjuncts of mutually incompatible characters, [...] can characterize, that is, can be embodied or exemplified. If they are thus embodied we call them characteristics of that which exemplifies them. Universals also have characteristics. Particulars, on the other hand, though they have many characteristics, do not and can not characterize anything." (Baylis 1951, p. 642)

A Platonic accent in Baylis' conception is undeniable. In the 1930 essay, he agreed with the extreme realists insofar as he admitted that universals can be exemplified independently of being understood by anyone. But he himself later denounced the excessive use of the Platonic lexicon and pointed out that his conception is compatible with all traditional solutions to the problem of universals, with the sole exception of extreme nominalism. On the one hand, "the only meanings with which we have anything to do are those which are meant by us. A meaning which no one will ever mean, though it has that sort of being just described, is nothing to us." On the other hand, he says he agrees with moderate realists because "many meanings are exemplified in particulars." (Baylis 1930, p. 171)

Baylis later extended the exemplification relation (and the converse relation of "characterization") to the truth relation, understood as a relation between propositions and facts, and the resulting view, with slight modifications, "could be transposed into a view consonant with Aristotelian realism or with conceptualism." With Frege, Baylis argues that every propositional expression has both denotation (*Bedeutung*) and signification (*Sinn*); but unlike Frege, he maintains that the denotation of a true proposition is not its truth value, but its exemplifying fact: "true propositions characterise facts, and [...] these facts embody or exemplify the abstract propositional meanings they make true." (Baylis 1948, p. 470) A general term such as "red" characterizes the denoted entities, the red things, and, conversely, red things "embody or exemplify" redness, or they "are instances or cases of redness"; but this relation also applies to abstract propositional meanings and concrete particular facts: "the relation between a true proposition and any fact in virtue of which it is true is in this sense one of characterisation and the converse relation one of exemplification." (Baylis 1948, p. 460)

Finally, Baylis extended these observations to scientific general propositions, arguing that the discovery of empirical laws is based on knowledge of the interrelationships of exemplification. Communicable knowledge, which requires shared meanings, in its simplest form, "is knowledge of the common characters exhibited by various objects and events. In the more advanced form of scientific knowledge it is knowledge of the interrelations of these characters in all their possible instances." (Baylis 1951, p. 636)

A few decades later, Nelson Goodman will take up Baylis' idea by speaking of "exemplification" as "an important and widely used mode of symbolization in and out of the arts." (Goodman 1968, p. 52) Goodman's "exemplification" is so well known that we can limit ourselves to a few brief hints. It is noteworthy that Goodman reproposed many ideas similar to those of Baylis. The similarity is such that, although I have not found passages in which the latter is mentioned, it is difficult to believe that Goodman's introduction of the term "exemplification" could have taken place quite independently. To clarify this point, I must defer to future, more historically oriented studies, although here it is worth pointing out at least some important similarities.

In Goodman we find Baylis' thesis about the inverse relation between the object denoted by a predicate and the corresponding property of the predicate being exemplified by it (Goodman 1968, p. 53). And we find Baylis's hints about the selective character of exemplification, although he restricted, with more clarity, its application to cases in which a symbol not only exemplifies certain properties but also refers to something that has those properties:

> "Consider a tailor's booklet of small swatches of cloth. These function as samples, as symbols exemplifying certain properties. But a swatch does not

> exemplify all its properties; it is a sample of color, weave, texture, and pattern, but not of size, shape, or absolute weight or value. Nor does it even exemplify all the properties—such as having been finished on a Tuesday—that it shares with the given bolt or run of material. Exemplification is possession plus reference". (Goodman 1968, p. 53)

It was from this thesis that Catherine Elgin later moved to extend Goodman's analysis to science, scientific experiment, and thought experiment (cf. e.g. Elgin 1983, 1996, 2004, and 2010). She reaffirmed Goodman's idea that exemplification is present in art as in science, adding that it "is the vehicle by which experiments make aspects of nature manifest." (cf. e.g. Elgin 2011, p. 399 and 410) Elgin pointed out that "what is common to fictions, thought experiments and standard experiments is that they exemplify, and thereby provide epistemic access to features of the real world." (Elgin 2014, p. 221) Moreover, according to Elgin, the same use of idealizations is common to real and thought experiments, and in no way calls into question their empirical controllability (Elgin 2017, pp. 1-2, 14–15, 27–28, and 61–62).

This paper will also arrive at similar positions, although drawing on considerations already developed in the past, as part of a more general discourse both on the nature of science (Buzzoni 1982 and 1995) and on the relationship between real experiment and thought experiment (cf. Buzzoni 2004, ch. 3, § 1; Buzzoni 2008, pp. 129-132). However, while partly agreeing with Elgin's positions regarding exemplification, real experiment, and thought experiment, one may wonder whether the underlying problem has actually been solved, or only reformulated. Certainly, formulating a problem in new terms can be an important acquisition, and the viewpoint of "exemplification," at least up to a certain point, illuminates new aspects of the relationship between, on the one hand, universal meanings and laws and, on the other hand, particular entities or states of affairs. But the doubt remains that instead of providing a specific answer to the problem, the solution remains largely only verbal. A properly philosophical explanation must be found for what Baylis wrote, concluding as follows in his aforementioned 1930 paper: "Meanings unexemplified are empty, i.e., exampleless; particulars exemplifying no meanings are blind, i.e., meaningless." (Baylis 1930, p. 174)

In this paper, I shall try not only to interpret but also to justify this paraphrase of the well-known Kantian statement on the relation between intuitions and concepts in a more radical sense than that assumed by the authors cited in this section (and, probably, in an even more radical way than Kant himself understood it).

First, building on considerations developed elsewhere (Buzzoni 1982 and 1995) and following a hint to be found in Aristotle's *On Rhetoric*, I shall propose to connect the concept of exemplification to the problem of induction and abduction, understood here as a multiplicity of methodical procedures aimed at establishing a cognitive relationship between the reproducibility of scientific concepts or laws and the concreteness, locality, and situated character of experimental practices. As I shall try to show, a necessary prerequisite for the solution to this problem is a new interpretation of the well-known relationship between the context of discovery and the context of justification. Unlike what, to my knowledge, has happened so far in elucidating the relationship between these two concepts, a clear distinction and at the same time an indissoluble link must be made between two points of view – the epistemological-reflexive one and the genetic-methodological one. This distinction, in fact, leads to an understanding of a precise sense in which universality, conceived in such a way that it must necessarily be translated in terms of methodological reproducibility, establishes the cognitive value of what might perhaps be called exemplifying induction, and does so in the case of both real and thought experiment.

In Section 2, I will briefly draw a distinction between the mentioned two senses of the discovery/justification pair of concepts. Section 3 will explore the consequences of this distinction for the long-standing problem of induction and for the problem of the relationship between the universality of scientific laws and the always local and situated character of experimental practices. Section 4 will apply these considerations to the case of thought experiments by briefly discussing John Norton's account.

1. Two Fundamental Senses of the Discovery/Justification Distinction

The distinction between psychology and logic of scientific research, which is one of the main pillars of Popper's philosophy of science, is a point that, in spite of other differences, he essentially shared with the logical empiricist philosophy of science:

> "I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. […] [T]here is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. […] From a new idea, put up tentatively, and not yet justified in any way […] conclusions are drawn by means of logical deduction. These conclusions

> are then compared with one another and with other relevant statements, so as to find what logical relations (such as equivalence, derivability, compatibility, or incompatibility) exist between them." (Popper 1935, pp. 4-6; Engl. transl., Popper 1959, pp. 8-9)

In this way, Popper essentially took up the distinction that the logical empiricist philosophy of science had drawn between the "context of discovery" and the "context of justification"n (for historical details on this distinction, see Schickore and Steinle (eds) 2009, above all Part I and Part II, and Buzzoni 2015). Logical empiricists and Popper used this distinction to grant empirical science cognitive autonomy from its cultural and historical context. But this was precisely one of the main reasons why exponents of what I would call the "relativist turn" in the philosophy of science of the 1960s (notably Kuhn and Feyerabend) and proponents of the sociological turn (notably Bloor and Latour) have rejected the distinction in question since the 1980s. According to Kuhn and Feyerabend, for example, *empirical-historical factors* such as scientists' prejudices and personal idiosyncrasies, aesthetic preferences, religious beliefs, etc., merely because they played an historical-causal role in the scientific process, are to be put on a par with more traditional *reasons* for maintaining or rejecting a theory, such as coherence, explanatory scope, unifying power, etc. (cf. Feyerabend 1970, § 14; Kuhn 1962, pp. 151-156; for typical exponents of the sociological turn, see e.g. Bloor 1991, pp. 36–37 and Knorr Cetina 1992, p. 116).

In this way, however, the baby had been thrown out with the bathwater. The baby was here the minimal sense, which I shall call reflexive-transcendental, in which reason is irreducible to empirical, particular causal factors, namely as an expression of its claim to represent, in principle, things as they really are (no matter how far this can succeed). Although a countless number of physical, biological, psychological, sociological, and, generally, contingent or accidental factors influence and limit human reason, the irreducibility of this latter, at least in an important sense, cannot be denied without denying all possibility of meaningful thinking or talking. Any claim to reduce reason to causal factors, necessarily presupposing its own truth, is irreducible to the causal factors to which, contradictorily, it grants a determining power over itself. In fact, to assert any empirical fact is to assert, implicitly, the distinction in principle between reason and facts, without which there would be neither one's own asserting nor one's own denying. At least in this sense, the distinction between the contexts of justification and discovery is constitutive of reason and cannot be denied without contradiction, since it is affirmed by the very act of negating it.

So far, I have defended the distinction in principle between the context of justification and the context of discovery in the reflexive-transcendental sense, which expresses the irreducible autonomy of reason. However, we should distinguish at least another sense, which I shall call *genetic-methodological*, which is the opposite complementary of the reflexive-transcendental just seen, a sense in which this distinction must be entirely rejected.

In fact, if the general claim of representing things as they are is not to remain devoid of any particular content and cognitive function, it must be realized by means of concrete methodological procedures that make it possible to reconstruct, to re-appropriate, and to evaluate in the first person the reasons why a particular truth-claim should be accepted. In other words, the truth-claim of our discourses tends by its very nature to translate (in principle without residue) into particular methods (or techniques). Not only did the logical empiricists, Popper and Lakatos, fail to clearly identify this sense, in which a genetic-methodological attitude is decisive for justification, but so did the exponents of the sociological turn. To test the truth value of a statement, in principle, we must always adopt this genetic and historical-reconstructive attitude and retrace the main methodological steps taken by those who first achieved a certain result through those steps. Pythagoras's Theorem can be used in a practical way without recalling the procedural steps of its demonstration. But if someone challenged its validity, we ought to test it by retracing in the first person the procedural steps that led to that theorem being asserted. By doing this, we *justify* a theory by historically reconstructing the context of its *discovery*. In this sense, context of discovery and context of justification are one and the same thing (for a more detailed justification of this thesis, see Buzzoni 1982 (ch. 3, § 1 and *passim*), 2008 (ch. 1, §§ 4-7), and 2015. It is this relationship of unity and distinction between the reflexive-transcendental and the genetic-methodological (operational) use of reason that ultimately justifies Baylis' apt rephrasing of Kant's well-known *dictum*, according to which unexemplified meanings are empty, i.e., devoid of examples, while particulars that do not exemplify meanings are blind, i.e., meaningless.

As we shall now see, this unity and distinction between a reflexive-transcendental and a genetic-methodological sense of reason casts some light, at the same time, on the long-standing problem of induction and on the problem of the relationship between the purported universality of scientific laws and the always local and situated character of experimental practices. I certainly cannot dwell on this issue as much as its theoretical depth would require, but I shall at least develop a few general considerations, valid for both real and thought experiments.

2. Popper, Gooding, and Inductive Exemplification in Scientific Experiments

It is not difficult to realize that the Popperian (or neopositivist) separation between the psychology of knowledge (or context of discovery) and logic of knowledge (or context of justification) is intimately connected with the rejection of the inductive method. If there is an insurmountable gap between the historical-psychological context of the discovery and the logical context of the justification, there is no rationally reconstructible way that can hope to bridge it, only inexplicable intuition.

It is, therefore, no accident that Popper is among those who have most radically rejected the inductive procedure. After having defined induction as an inference from singular to universal statements (cf. Popper 1959, pp. 3-4, and Popper 1972, p. 7), Popper can maintain that no finite set of particular assertions can ever justify such a logical transition. Moreover, according to Popper, induction does not even exist as a psychological procedure: even psychologically, what is actually used is the deductive method of testing (cf. e.g. Popper 1959, pp. 442; Popper 1963, pp. 45-46; Popper 1972, pp. 6-7; Popper 1974, p. 40).

In fact, as we shall argue, it is possible to move from the historical-psychological context of discovery to the logical context of justification only if the former is conceived from the outset as guided by the effort to conform to an independent reality, which in turn is assumed to be characterized by an immanent lawfulness, which can be brought out by the use of appropriate methodological techniques. Now, the rejection of one of the senses in which the neo-Positivist and Popperian distinction between psychological discovery and logical justification can be understood – that is, the sense that separates the truth of a theory from the methodological reasons we have for asserting it – allows us to set up the problem of the transition from the singularity and concreteness of experimental practices to the purported universality, objectivity or intersubjectivity more correctly.

As anticipated in the introduction, Aristotle had already sensed a close connection between the concept of exemplification and the problem of "induction" (or "abduction," as one might perhaps also translate "epagôgê") when referring to knowledge that is based on paradigmatic examples that we have experienced in the past. In *On Rhetoric* 1393 a-b, he distinguishes two species of "paradeigma" – a word which may be translated by "examples" (cf. Aristotle 1984, transl. by W. Rhys Roberts) or "paradigms" (cf. Kennedy 2007). According to Aristotle, there are "historical" and "fictional" examples or paradigms. While fictional examples or paradigms only have a value of rhetorical persuasion, as they exemplify in a fantastic form a general concept that is already possessed and is intended to be communicated in a persuasive form, historical examples or paradigms, on the contrary, are not only one of the basic tools of persuasion (*písteis apodeiktikaí*), but they are also "similar to an induction [*epagôgê*]". The bias in favor of theoretical knowledge prevented Aristotle (despite some very interesting insights concerning biological and technical knowledge) from fully recognizing the importance of the inductive-experimental aspect of inquiry. According to him, reasoning by means of examples has only a heuristic-probabilistic value, since numerous examples from the past never confer the demonstrative value that can be obtained by the use of syllogism (see Kennedy 2007, 1356b). But the problem of the reliability of knowledge through exemplification was clearly posed and closely related to that of induction.

In light of the considerations made in the previous section, however, two aspects should be distinguished in the problem of induction (or abduction), united and distinct at the same time. On the one hand, induction and abduction should be regarded as inductive and abductive methods and procedures that we devise to exemplify concretely, in a context that is always at least implicitly experimental, certain general concepts or laws. The rejection of the separation between the logic of discovery and the logic of justification, in fact, implies first of all that the universality of scientific theories should not be understood in the merely logical sense in which logical empiricism and Popper have understood it, but rather in the sense of the principled reproducibility of certain experimental actions and their results. The universality of scientific assertions, in fact, consists in the reproducibility in principle of certain operations, invariant both with respect to the person of the experimenter and with respect to the measuring instruments used by him/her. This principled reproducibility is how the universality of scientific theories actually lives. The universality of scientific propositions is achieved by ensuring that the instructions for obtaining the experiences described by these propositions can be carried out in principle by anyone.

The generality or universality of theories depends, in short, on their technical reproducibility in principle, that is, on the possibility of retracing the procedural steps that led others to a certain result, a possibility that depends on the ability to understand what background knowledge, what specific assumptions, what resources, and what skills are needed to reproduce a certain experiment in one's own laboratory.

On the other hand, induction presupposes from the outset the assumption of an *in re* regularity, which must be recognized as it is, even if, again, this assumption must necessarily be exemplified through the use of particular methodological techniques. The rejection of the inductive procedure by Peirce, Popper, and Hanson depends on a fundamental error of the classical empiricist approach, which seeks to conquer the generality of scientific concepts and laws by moving from the initial situations of their genesis, assumed to be particular and unrepeatable. It is only possible to arrive at the context of justification by moving from the context of discovery, if the latter is from the outset conceived as having in itself an immanent regularity, as exemplifying a nature that conforms to laws, which, moreover – as is implicit in the resolution of the universality of scientific theories in their controllability and reproducibility in principle – can be brought to light by means of appropriate methodological procedures.

It is therefore no coincidence that the problem of induction (or abduction) cannot be solved even in the diametrically opposite case to that seen so far (exemplified by Popper's rejection of induction), that is, if, after abandoning the separation of discovery and justification in its genetic-methodological sense, we wanted to abandon it in its reflexive-transcendental sense as well. To illustrate this point, let us briefly examine David Gooding's rejection of the distinction between discovery and justification.

As mentioned at the beginning, the new experimentalism (of which Gooding can be considered an important exponent), starting from an essential opposition between experimental practices and fundamental theories, necessarily had to address the problem of the relationship between the particularity of the former and the generality of the latter (see especially Hacking 1983 and Cartwright 1983). Nancy Cartwright tried to solve the problem by denying the general applicability of scientific laws, since they, strictly speaking, lie about the real world and would only apply to it after many local modifications. Gooding was not satisfied with this answer and tried to explain the way scientific facts or laws are freed from particular contexts by resorting to Latour's concept of "demodalization" of factual assertions, achieved by removing references to people and places from scientists' reports. From this point of view, the universality of experimental results would be something that is acquired through a process that progressively abstracts from the singular and historically unrepeatable reality of experiments to the point of making a certain result, obtained in a particular laboratory, reproducible in any other laboratory:

> "Scientists can and do *later* identify and eliminate [...] local or contingent factors. What can be manipulated in any laboratory when all the relevant variables are controlled, is taken to be natural. Contingency may be eliminated by practical means (as Michelson and Morley eliminated vibrations affecting the interferometer in their Cleveland laboratory) and by ascent to a theory which identifies them as instances of still other universal laws (e.g., Helmholtz argued that temperature effects on the interferometer arms would mask actual fringe-displacements). [...] This process dissociates results from particular individuals or laboratories and it complements the mastery and dissemination of skills" (Gooding 1990, p. 190).

According to Gooding, every experiment is, in principle, unrepeatable and entirely dependent on the context in which it takes place, and it is the very effort to eliminate this dependence on the context that generates the "recalcitrances" that scientists seek to eliminate:

> "Unexpected events show where 'theory' (by which I mean a complex of theories and enabling assumptions) does not match the complexity of nature as implicated by the practices associated with a particular method of observation or experimentation. Theoreticians cannot work out every implication of a theory. Recalcitrance in experiment helps identify just those assumptions (or associated theories) that are actually implicated by the experimental methods adopted by a particular laboratory. [...] Recalcitrances indicate a discrepancy between theory, instrumentation, practice and results. Because they shape and refine practices, they are as important to the invention of a simple device like Faraday's rotation motor as they are to building, operating, and learning how to read a complex system such as Morpurgo's quark detector." (Gooding 1992, p. 69)

That experimental knowledge can be interpreted as the knowledge of ways of eliminating a series of disturbing factors is not an entirely new thesis. Hugo Dingler had argued this in the context of his "exhaustion" procedure (cf. e.g. Dingler 1938, p. 142-146) and, inspired by Dingler, Janich more recently argued that a theory is strictly speaking "knowledge concerning the ways of avoiding certain disturbing effects" (*Störungsvermeidungswissen*) (cf. Janich 1998, p. 110).

In fact, from the perspective adopted here, it is certainly correct – because it is perfectly in line with our rejection of the Popperian and neopositivist sense of the discovery/justification distinction – not to disentangle the results a scientist arrives at from the methodological paths followed to move from the local character of each experiment to its general reproducibility in different contexts. Explaining the transition from the concreteness of experimental practices to the principled objectivity or intersubjectivity of its results corresponds, to some extent, to correctly rejecting one of the senses in which the Neopositivist and Popperian separation between the context of discovery and the context of justification can be understood, namely, that which separates a theory that we consider intersubjectively true (or reliable, probable, consistent, or whatever other adjective one prefers, depending on the different epistemological position adopted) from the methodological procedures we have for considering it so.

But Gooding, from the outset, seems to also deny the sense in which the discovery/justification distinction is ineradicable without contradiction, that is, in the sense that it presupposes the principled ability of our reason to tell how things are or, what is ultimately the same, to grasp the lawfulness immanent to our experience of the world. Indeed, how can we discover and, even before that, conceive of "recalcitrances," if not by assuming underlying law-like regularities, which guarantee *a parte rei* that we shall still, by adopting the same methodological means or procedures, arrive at the same results? The very effort to identify disturbing factors ("recalcitrances") cannot do without a theoretical interpretation of the apparatus, which specifies the regularities against which they make sense as disturbing factors and without which they could not be grasped as such at all. Experimentation, in other words, presupposes from the outset regularities against the backdrop of which nature's recalcitrances and our attempts to eliminate them only acquire their meaning. If one does not assume this point, it all boils down to describing how de facto scientists methodologically attempt to rule out perturbing effects, and nothing has been said about the conditions of possibility of this methodological procedure, the success of which – given the singularity and locality of experimental practices – remains unexplained.

The crucial problem remains for Gooding to offer an explanation of the possibility of decontextualizing the results obtained in a particular laboratory. To simply assert, with Latour, that we drop the specific "modalities" in which an experiment takes place is a truism that leaves the decisive question unresolved: what authorizes us to think that by dropping this or that particular "modality" we will find a regularity having general (in principle reproducible) validity, rather than, as would seem obvious, another singular and unrepeatable reality, only more or less different than the starting one? Having posited a singular and unrepeatable reality (no matter how complex), removing some particular element from it can only have the effect of arriving at another reality that is equally singular and unrepeatable, only deprived of some elements that the former had in addition. If I remove a brick from the house that I was building, I have indeed strictly speaking removed from it a singular and unrepeatable element, but what remains of it, however different it may be from what it was before from different points of view (it may even be reduced to a pile of rubble), does not change at all from the point of view of its singularity or universality. The opposition between the singular and the universal does not tolerate intermediate degrees: if certain statements represented phenomena in a singular way, they could not, in a merely gradual process, ever again acquire the value of universality.

Peirce, Popper, and Hanson's rejection of induction is of course correct, if by this word the mere logical inference, already formalized, from singular propositions to universal propositions is meant. But if, for example, by induction (or abduction) one meant the mental procedure that enables us to grasp regularities in the (always implicitly experimental) interactions between our bodies and our environment, the conclusions should be quite different. Such a capacity of the mind, which cannot be further explained without incurring a logical circle or regress to infinity, is an original fact of reason, which is given along with our interaction with the world and which ultimately coincides with the very empirical use of reason as such: we cannot think anything as real without conceiving it as such to express some regularity and vice versa. Inductive inference, taken in its most general epistemological sense, consists in the ability to infer not from many cases to all cases, but from a concrete case to cases of the same species or from a concrete case to the immanent law of its being so and not otherwise.

From this point of view, the inductive procedure is typically embodied in the scientific experiment. The specific content that determines the meaning of the experimental question is discovered by applying many different methods of deliberate and systematic variation to some aspects of a certain phenomenon. More precisely, the theoretical point of view adopted in formulating the experimental question must be operationalized, i.e., it must be translated into active methodical interventions on natural processes. This typically happens through the construction of what I would call an “experimental machine”, which extends the original operativity of our organic body. Herein lies the technical and operational significance of the “method of variation” (Mach) as a trait intrinsically linked to the scientific experiment. Scientists intervene technically on reality when, carrying out experiments to test the value of a hypothesis, they perform in principle repeatable actions and when they intentionally modify independent variables in a way that is in principle reproducible in order to determine the consequences of these modifications on one or more dependent variables (and conversely, every successful modification of reality involves an implicit connection between variables that is in principle intersubjectively reproducible). The functioning of an “experimental machine” exemplifies a nomic connection that exists in nature: given a certain input, there follows a chain of events that is independent both of our will and (as to its contents) of the conventions we use.

That the “secret of induction” does not consist in going from a multitude of observations to the totality of observed cases, but is already contained in the investigation of a single case, had already been recognized by several authors, of whom Cassirer is one of the most important. According to Cassirer, the problem of induction can only be solved by expanding its meaning. Already in the single case, he writes, “[t]here must be a factor concealed,” which “raises it out of its limitation and isolation.” (Cassirer 1910, p. 327; Engl. Transl., p. 247) The function by which we follow an empirical content beyond the chronological limits in which it is given to us is the very core of the inductive procedure. Cassirer saw the solution to this problem in the fact that the transition from the individual to the whole “would not be possible, if there were not a reference to the whole already in the element,” that is, a reference to a “law-like form” (*gesetzliche Form*), which only needs to be isolated and brought to light conceptually (Cassirer 1910, p. 327; Engl. Transl., slightly modified, p. 247).

In fact, the empirical object, that which is empirically real (in the sense of anything with which I can interact practically, first and foremost through my body), must necessarily be reproducible in principle. We believe that an effect is real because we have made it reproducible: the assumption or conjecture of its reproducibility implicitly contains the assumption of its real existence. For this reason, on the one hand, it is usually assumed that a hallucination does not express anything real, because its content is usually not reproducible, but, on the other hand, even a hallucination can be considered real, insofar as it is in principle reproducible: we can, for example, reproduce it by recreating certain conditions that, in a predisposed subject, set it off.

This reproducibility, as we have already said, is the way in which the universality of scientific theories concretely lives and is the fruit of a process that could be called either "inductive" or "abductive". This reproducibility could not be lacking in either case: not in traditionally understood induction, because the individual instances from which it starts must already potentially contain some lawfulness that will be made explicit in the transition to universal utterances, nor in abduction, because the generation of the abductive hypothesis (as is well known, Peirce also designated abduction as the method of hypothesis) would be blind arbitrariness if it did not explicate a lawfulness already contained in some way in the evidence from which it moves. This methodological process can certainly also be described as a gradual elimination – as Gooding puts it – of all "recalcitrances" that limit its reproducibility, but we have seen that, first, these recalcitrances can be discovered on the basis of specific experimental procedures and, second, this necessarily presupposes the assumption of law-like regularities immanent to reality.

As a result, inductive inference has two aspects, distinct from each other but also absolutely inseparable: on the one hand, in its most general epistemological sense, it is irreducible to any particular set of methods (such as John Stuart Mill's rules) and consists in the capacity of reason of hypothetically mediating facts or events. On the other hand, this capacity of reason is expressed in the construction of counterfactual scenarios that will make these facts or events appear as the results of the procedural steps in which properly consists the inductive exemplification that is the focus of this paper. From the latter point of view, induction moves from the context of discovery only in the sense that it arrives at generalizations that have their foundation in the very process that leads to them, and thus, on closer inspection, overcomes at root the opposition between the context of discovery and the context of justification. Herein lies the reason for the fact that even a single experiment can suggest and at the same time justify (in a way that is, of course, always contextual and therefore provisional) a theory. A few experiments are sufficient to understand that a piece of copper, whenever it is heated, expands, and it would make no sense to multiply the number of experiments to try to logically justify the universality of the statement that "All pieces of copper, when heated, expand."

The general conclusions we have thus reached will now be restated for thought experiments by a brief analysis of John Norton's argumentative view. As we shall see, the relationship of unity and distinction between the reflexive-transcendental and the genetic-methodological (operational) use of reason leads us to recognize the key role that inductive exemplification plays not only in real-world experiments but also in thought experiments.

3. John Norton's Argumentative View of Thought Experiments[1]

John Norton set forth an important variant of the empiricist conception of thought experiments first developed by Mach. For Norton, thought experiments "are arguments which (i) posit hypothetical or counterfactual states of affairs, and (ii) invoke particulars irrelevant to the generality of the conclusion" (Norton 1991, p. 129; see also Norton 1996, p. 336): for example, the fact that in Einstein's elevator experiment the observer is a physicist is an irrelevant particular. The explanatory power of a thought experiment derives from the fact that it can be reconstructed by an argument with explicit premises that do not refer in any way to imagined particulars (see Norton 1991, pp. 130-131). Despite their shared empiricism, Norton and Mach differ in a fundamental respect. Mach connects real and thought experiments very closely; in contrast, Norton believes that thought experiments can always be reconstructed as deductive or inductive arguments ("reconstruction thesis") and, more importantly, that they must always be evaluated as such:

> "The outcome is reliable only insofar as our assumptions are true and the inference valid. That is not to say that all thought experiments are instances of perfect deductive or inductive inference. Thought experiments can be bungled, just as arguments can. Rather, when we evaluate thought experiments as epistemological devices, the point is that we should evaluate them as arguments. A good thought experiment is a good argument; a bad thought experiment is a bad argument." (Norton 1996, p. 335)

Norton's view has been widely criticized for a number of mostly unconvincing reasons. Some criticisms simply miss the target, since they do not

[1] This section largely takes up considerations already made in Buzzoni 2008, reformulated, however, from the point of view of the notion here developed of inductive exemplification.

take into account Norton's explicit insistence that the argument may be inductive: "A very broad range of argument forms should be allowed here; in particular they should include inductive argument forms." (Norton 1991, p. 129; see also 1996, p. 335; 2004a, pp. 52-53; 2004b, pp. 1144)

Other objections even end up strengthening the position they intend to criticize. For instance, it has been objected that whenever there is a dispute about the actual result of a thought experiment, at least *two* different arguments are produced to discover the true meaning of *one* thought experiment so that the latter cannot be identified with an argument. Such was, for example, the case in the dispute between Einstein and Bohr about the actual result of the thought experiment of the clock in the box: Bohr and Einstein, it was noted, were analyzing only one thought experiment, but they proposed two different arguments that they assumed to be identical to the thought experiment. Now, in the dispute between Bohr and Einstein, there are two kinds of arguments, but only one thought experiment, so the thought experiment cannot be identified with one argument (cf. Bishop 1998 and Bishop 1999, p. 538-540).

Now, this argument reinforces the thesis it intends to criticize, because, on close examination, it assumes that such disputes are attempts *to identify the argument that establishes the validity of the relevant thought experiment*, which is one of the main points defended by Norton. Moreover, from Norton's point of view, a thought experiment contains irrelevant details that are removed in its reconstruction as an argument, so there is nothing to preclude a dispute over the correct way to reconstruct the same thought experiment argumentatively.

A third type of objection is more interesting, namely that the translation of a thought experiment into formal terms (i.e., the elimination of pictures and diagrams by translating them into propositional contents) causes a partial loss of the original meaning or content, to the point that the experiment becomes unintelligible (cf. especially Brown 1997 and 2022).

Taken literally, this objection is not conclusive either. Nonetheless, it contains an important element of truth, which is intimately connected with the main object of this paper. Taken literally, the objection is inconclusive since it suggests that the elimination of certain intuitive elements may *de facto* make a thought experiment reconstructed as an argument unintelligible, but sets forth no reason *in principle* why this must happen. The objection leaves it open for Norton to retort by *de facto* reconstructing many paradigmatic thought experiments as arguments. To the extent that this has happened successfully in the past, it is plausible to assume that the same may be done even for experiments that have not yet undergone such a reconstruction: "As far as I know, all thought experiments can in fact be reconstructed as arguments, and I have little hope of finding one that cannot." (Norton 2004a, p. 50)

For the objection to become conclusive, we need a reason in principle why a thought experiment, reduced to a pure argument, would lose all content and all empirical sense. Certainly, the presence of irrelevant particulars neither strengthens nor weakens the demonstrative power of a thought experiment: in the thought experiment in which Einstein considers a magnet and a conductor in relative motion (cf. Norton 1991, pp. 135-136), Einstein could speak, say, of a multicolored magnet to keep the reader interested. *But it is not irrelevant that thought experiments are generally formulated and performed by constructing particular cases, which need concrete elements that are in principle reproducible in specific spatio-temporally individuated examples.*

The concrete particularity, the individuality of the examples of empirical thought experiments are not irrelevant since these examples are the touchstone for testing the coherence, explanatory power, and technical-practical translatability (i.e. empirical truth) of the hypotheses that underlie the experiments. Norton claims that thought experiments have explanatory power only if they can be reconstructed as arguments with explicit premises and no reference to imagined particulars – that is, only if they can be reconstructed as no longer thought experiments, given the autonomy of these arguments from the concrete particular situations described by thought experiments. But this claim implicitly reduces *empirical* thought experiments to logical-formal thought experiments. Thought experiments, stripped of any reference to concrete experimental situations, are confined to a domain of purely theoretical statements and demonstrative connections.[2]

It is perhaps to avoid objections of this kind that Norton, as we have seen, takes the term "argument" to include inductive ones (Norton 1991, p. 130-131). However, this strategy fails, since Norton by "inductive argument"

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[2] Cf. Buzzoni 2004, pp. 157-160, and Buzzoni 2008, pp. 67-68. Stuart 2016 has developed this point in a detailed and very convincing way, rightly stressing that a wholesale reduction of thought experiments to arguments would empty their use in empirical knowledge. As he aptly notes: "whether thought experiments are reconstructed as arguments or not, applying the norms of laboratory practice seems prima facie to be a good way to evaluate their level of justification for an empiricist." (p. 462)

means an argument that is valid within a formal calculus (cf. above all Norton 2004a, pp. 54-55). This restriction of the meaning of the term "inductive," far from solving the difficulty and enabling us to distinguish between empirical and logical or mathematical thought experiments, just confirms that difficulty. In this sense, an inductive argument, like a deductive one, can be tested with regard to its logical validity (properly understood: for example, with regard to its ability to preserve probability), but not (at least directly) with regard to its possible confirmation or refutation by experience. In another sense – that is, if we do not take the expression "inductive argument" in a narrowly logical-formal sense –, we clearly must overstep the logical-discursive horizon of argument (as well as the limits of Norton's notion of the thought experiment) by referring to the real world and to our ability to identify nomic regularities in it. It is impossible to speak about inductive (or abductive) procedures without acknowledging that at least their starting points are particular and concrete. This holds whether we understand induction in the empiricist sense (espoused by Logical Empiricists and criticized by Popper) as the transition from a set of particular propositions concerning a certain number of cases to a universal proposition concerning all similar cases, or in the sense suggested here, as the methodical-discursive ability of human intelligence to grasp general and reproducible structures immanent in real events and processes.

Norton's attempt to avoid the reduction of empirical thought experiments to logical-mathematical ones is insufficient if *new* knowledge has a properly empirical (and not merely logical) meaning. In this sense, a thought experiment cannot be exhausted in an argument (whether understood in the formal sense of deductive or inductive logic), because it subjects the *empirical* truth or falsity of a hypothesis to the critical judgment of the scientific community. In its root, which is epistemological before being formal, although it may contain deductive checks aimed at testing the internal consistency of a hypothesis, an empirical thought experiment is properly an example of the inductive-experimental use of reason.

Norton's reconstruction as an argument is only correct on the condition of accepting the experiment as a reproducible inductive exemplification of a law. The general value (or, which is the same, the epistemic value) of a thought experiment can be argued neither deductively nor inductively or abductively, if by these words we mean a formalized logical procedure. One can only argue in favor of its generality by arguing in favor of its concrete exemplification in and through the functioning of a certain experimental apparatus.

As in the case of real experiments, and also in the case of thought experiments (of the empirical kind), the inductive/abductive procedure presupposes the fact of having such an experimental apparatus (or a natural reality conceivable as such), which even before resorting to the real experiment, one has reason to assume functions according to known laws. By anticipating a specific experimental situation in thought, thought experiments open up new conditions for formulating and testing a hypothesis, by bringing to light either a family of cases in which it will almost certainly be confirmed or a family of cases in which, given our knowledge of familiar facts and well-confirmed theories, it will probably or almost certainly be refuted by experience.

It should not be necessary at this point to point out that any hypostatization of the concepts of "general" or "universal" is excluded here from the outset. What is and what is not universal (or, in other words, what is or is not a "natural genus") can only be determined by the success of its concrete exemplification to specific cases. In the inductive-experimental use of reason, the universal, in the light of the things said in the first part of our paper, must be resolved (on pain of remaining entirely abstract, in the sense of lacking any intelligible particular content), in the multiplicity of experimental methods or procedures that are capable of exemplifying it concretely.

Stevin's thought experiment is not demonstratively powerful because it can be reconstructed as an argument; on the contrary, it can be reconstructed as an argument because, as soon as we see the apparatus built by Stevin and follow through the few steps of his experiment, we are persuaded of its generalizability. On reflection, if we reconstructed Stevin's thought experiment as an incorrect argument, we would question our reconstruction rather than his thought experiment. As we have seen, what is discovered as empirically typical or universal by the inductive/abductive process is simply that which, relative to our interests or values, is, to an extent sufficient for the purposes of our lives, empirically-technically or operationally reproducible in the first person. Also, in the case of empirical thought experiments, induction (or abduction) is *the passage from a single case (the experimental machine) to the discovery of the nomic regularity or regularities that it exemplifies by its general functioning*.

This also explains the importance of visualization in thought experiments, emphasized by thinkers from different, sometimes even opposite perspectives[3]. Visualization is important because empirical thought experiments bring general hypotheses to life in concrete situations: since thought experiments, like real ones, exemplify the essential aspects of a hypothesis (those relevant for testing its truth or falsity), their construction, like that of any example, requires perspicuity, intuitive appeal, clarity, and didactic value.[4]

In summary, our discussion of Norton shows that exemplification in particular concrete situations is an important feature of empirical thought experiments, to the extent that they describe operational procedures that exemplify the generality of scientific laws in experimental machines and that are always again appropriable and methodologically reconstructible in the first person. In this respect too, real and thought experiments are similar. The decisive reference to a particular concrete situation that exemplifies a nomic or law-like nexus is a key feature of empirical thought experiments as well as real-world experiments.

Conclusion

Each experiment takes place in a particular laboratory, in a particular place and time, and this immediately leads to serious methodological problems for the scientist and difficult epistemological problems for the philosopher of science. How can an experimental practice that is always "local," that is, that happens in a specific space and time, arrive at general and reproducible results in contexts different from the original one? If there were no other dimension to experiments than the concrete or "local" dimension of the discovery, or, which in the end comes to the same if the historical context of the discovery of a theory were separated from its genetic-methodological justification, then the experimental results could not claim any cognitive truth-value beyond the *hic et nunc* of the act of the individual experimenter; they would appear as a singular reality, which no longer allows any transition to a universal or general law.

But, as we have seen, there is a sense in which the context of discovery and the context of justification are two aspects of empirical knowledge that are

[3] Cf., e.g., Brown 1991, p. 1; Yourgrau 1967, p. 872; Miščević 1992, pp. 220-221; Miščević 2022, pp. 58-59.

[4] As I have argued elsewhere, this applies in a similar manner to classical as well as to quantum physics (cf. Buzzoni 2004, ch. 3, § 2 and ch. 4, § 4; for an essential hint, see Buzzoni 2008, p. 70).

at once distinct and indissolubly united. The logical value of justification expresses the reflexive-transcendental need of reason to say how things are in fact, but this need cannot be satisfied unless it is translated into concrete genetic-methodological procedures. The latter exemplify this need in particular methodological paths, the universality of which is not to be understood abstractly, but as the principled possibility of a first-person reconstruction of them (which, in the sphere of empirical knowledge, are always, implicitly or explicitly, experimental).

It is precisely in this translation of the need to justify our discourses into concrete and personally re-appropriable methodical paths that a plausible answer to the problem of induction or abduction lies, here both understood as the passage from the concrete and in a certain sense unrepeatable reality of experience to its reproducible exemplification – reproducible, of course, in relation to the aims we have set ourselves. This applies both to real-world and thought experiments. All empirical thought experiments are based on an experimental apparatus (or a natural reality that can be conceived of as such), the functioning of which exemplifies nomic, reproducible connections. In both cases, scientific laws are universal meanings that are exemplified and inductively/abductively inferable from experimental contexts, in the sense that we have found operational procedures that exemplify them in an experimental machine and that are always again appropriable and methodologically reconstructible in the first person.

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