COHERENCE OF INFERENCES

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ABSTRACT

It is usually accepted that deductions are non-informative and monotonic, inductions are informative and nonmonotonic, abductions create hypotheses but are epistemically irrelevant, and both deductions and inductions can't provide new insights. In this article, I attempt to provide a more cohesive view of the subject with the following hypotheses: (1) the paradigmatic examples of deductions, such as *modus ponens* and hypothetical syllogism, are not inferential forms, but coherence requirements for inferences; (2) since any reasoner aims to be coherent, any inference must be deductive; (3) a coherent inference is an intuitive process where the premises should be taken as sufficient evidence for the conclusion, which on its turn should be viewed as a necessary evidence for the premises in some modal range; (4) inductions, properly understood, are abductions, but there are no abductions beyond the fact that in any inference the conclusion should be regarded as a necessary evidence for the premises; (5) motonocity is not only compatible with the retraction of past inferences given new information, but it is a requirement for it; (6) this explanation of inferences holds true for discovery processes, predictions and trivial inferences.

1. INTRODUCTION

> Don't get involved in partial problems, but always take flight to where there is a free view over the whole single great problem, even if this view is still not a clear one. — Ludwig Wittgenstein
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> Like the gestalt switch, [the transition between competing paradigms] must occur all at once or not at all. — Thomas Kuhn

In the received view there are three main types of inferences: deductions, inductions and abductions. Deductions ensure the conclusion if the premises are true. They are monotonic, which is to say they are safe and can't be defeated by new information. Their downside is that they are non-informative because the conclusion doesn't contain nothing that it was not already stated in the premises. Inductions are different in that they are informative, but their ampliative character comes at a cost: they can be abandoned upon new discoveries. Finally, we have abductions, which are also informative and nonmonotonic, provide creative insights but can't justify hypotheses. This widely accepted view on inferences is at odds with the realities of any knowledge area. In the real world, where reasoners of flesh and blood rely on inferential practices, deductions can be informative and risky, inductions can be non-informative and safe, and abductions can justify hypotheses.

I attempt to propose a more cohesive view on the subject by arguing that any inference must be deductive. Here is the strategy. In section 2, I present the conventional view, which is criticised in detail in section 3. It's argued that in numerous cases the distinction is not illuminating. In section 4, I analyse the original source of the current distinction between deductions, inductions and abductions: Aristotle's *Prior Analytics*. Oddly enough, the examples of deduction presented by Aristotle can be plausibly interpreted as a property of logical consequence, namely, transitivity. The examples of induction and abduction in this work are deduction variations. Instead, the only genuine examples of inferences in the Aristotelian corpus are presented in other works, particularly *Topics* and *Posterior Analytics*. Aristotle also describes these examples of inferences as inductive, but they are intuitive and closer to the current understanding of abduction. Aristotle's view suggests two things: inferences are constrained by properties of logical consequence and are intuitive. This leads us to section 5, where I argue that the paradigmatic examples of deduction, both in the Aristotelian and the current sense, should not be interpreted as inferential forms, but as coherence requirements for inferences. It follows that any coherent inference must be deductive. The satisfaction of these coherence requirements also imply that the premises must be sufficient evidence for the conclusion, which on its turn should be necessary evidence for the premises in some modal range. There are no abductions beyond the fact that in any inference the conclusion is regarded as a necessary evidence for the premises. It is also argued that transcendental inferences are particular cases of abduction, namely, when the necessary evidence for x is also perceived as a necessary condition for x. So transcendental inferences can also be reduced in the aforementioned way. The importance of coherence for explanatory considerations, discoveries, empirical testing, monotonicity and consilience are also discussed. Section 6 revisit Hume's attacks to causality and reinterpret sceptical challenges to deduction, induction and abduction based on the deductivist view offered in this paper. It is argued that these different types of scepticism are incoherent. Finally, section 7 concludes with observations about the relevance of the present criticism to our understanding of science, knowledge and logic.

2. THE CONVENTIONAL WISDOM

According to the conventional wisdom, the main types of inference are deduction, induction and abduction. Deductive inferences differ from inductive and adductive ones because they aim validity. An inference is valid if, and only if, it is not possible that the premises are true and the conclusion is false. This state can also be described as follows: (a) the truth of the conclusion is necessitated by the truth of the premises; (b) the premisses entail the conclusion; (c) the premisses imply the conclusion; (d) the truth of the premises is preserved in the conclusion; and (e) the conclusion is a logical consequence of the premises. The successful deductive inferences that achieve validity status are instances of inferential forms that are identified, labelled and presented in taxonomies of logic textbooks. One example of inferential form associated with valid instances is *modus ponens*: $A \rightarrow B, A \vDash B$[1]. It is commonly believed that any instance of this inference form is truth preserving. Take the following inference: 'If John is late, he is going to miss the train. John is late. Therefore, he is going to miss the train'.

[1] I will adopt the notation where '→' stands for natural language conditionals. I will use capital letters such as A, B, C for both propositional and formula variables. For simplicity of exposition, I will not use quotes to highlight the use-mention distinction when there is no risk of confusion—the context will make it clear which one is being used.

In this example, there are no circumstances where the joint truth of the premises doesn't guarantee the truth of the conclusion. Naturally, not every deductive inference is successful otherwise all deductive inferences would be valid by definition. In fact, some invalid inferences are so common in daily discourse that they received their own names such as affirming the consequent: $A \rightarrow B, B \vDash A$. This inferential form is not truth-preserving because it has instances such as the following: 'If John is late, he is going to miss the train. John is going to miss the train. Therefore, John is late'. From the truth of both premises it doesn't follow that the conclusion is true for the simple reason that John could miss the train for an entirely different reason, let's say, if he suffered an accident in its way to work.

Deductive inferences are usually viewed as the bread and butter of knowledge areas are heavily dependent on conceptual analysis, *a priori* beliefs or formal methods of proof such as computer science, philosophy and mathematics. They are also commonly regarded as an important tool to test scientific theories, since the testable consequences of a hypothesis are inferred by deduction. Successful deductive inferences are also believed to have a certainty given that their validity is not affected by the addition of new premises. This feature of valid inferences being impervious to new information is what we call *monotonicity*. Finally, a valid deductive inference is commonly viewed as tautological, since its conclusion is already contained in its premises. In the instance of *modus ponens* presented above, the conclusion 'John is late' must be true given the acceptance of the premises 'if John is late, he is going to miss the train' and 'John is late', because it is already contained in them.

Inductive inferences couldn't be more different. One actual example involves what once was a widely held belief that all swans are white. The inductive inference in this case is 'All swans we have seen are white. Therefore, all swans are white.' This was a strong inductive inference supported by all the past observations of white swans until Willem de Vlamingh discovered black swans in Australia, in 1697. The Dutch explorer had to abandon his previous belief upon this new information. This example shows that a widely confirmed induction can be defeated by new information, i.e., they are non-monotonic. It also suggests that inductive inferences express our epistemic fallibility and cannot aim, or shouldn't be judged as aiming, validity, since these are goals that can only be achieved by deductive inferences. Inductive inferences are strong when they are supported by the relevant evidence in some manner to be specified or weak when they fail to do so. Inductive inferences can vary in strength and be more or less supported by the evidence. When an inductive inference is strong is merely unlikely, not impossible, that the premises are true and the conclusion is false.

In an induction, a general conclusion, 'All *Fs* are *Gs*', is inferred from its particular instances 'This *F* is *G*, that *F* is *G*, etc.'. From the properties of a sample of members drawn from a group, we distribute the properties to the whole group. If deductions are tautological, inductions are ampliative because the conclusion goes beyond what is logically contained in the premises. Inductions are associated with the use of empirical observations, controlled experiments and fine-grained considerations about statistical data and probability. They are viewed as the ultimate means by which scientific hypotheses can be justified. The advent of inductive methods brought profound changes to physics and astronomy during the Scientific Revolution and are regarded as having an essential role in the emergence of modern science. They preserved this royalty status to this day.

Last but not least, we have abductive inferences. Abductions are inferences involved in the creation of a hypothesis. The abductive inference is an explanatory act of insight during the context of discovery. One known example is Fleming's discovery of penicillin. Returning from holiday, Fleming notice that one petri dish containing bacteria colonies of Staphylococcus had one area with a growing clot of mould. The area around the mould was clear of bacteria. Fleming inferred by abduction that the mould had released something that killed the bacteria. If induction is an inference from certain observed facts to additional facts of the same kind (‘the swans observed in the past are white; therefore, all swans are white’), abduction might lead us to a different kind of fact that can unify diverse facts. Unlike deductions, abductions can introduce new ideas by a mental leap and create scientific hypotheses. Just like inductions, they are non-monotonic, because one might develop insights that might turn out to be mistakes. Perhaps because of this, abductions are still viewed by many as something that occurs in the context of discovery and not in the context of justification of a hypothesis. In order to determine whether a hypothesis is justified we need further inductive inferences and tests. This view of abductions and their relation with induction and deduction in science is clearly expressed by Peirce’s description:

> … **Induction**, is an Argument which sets out from a hypothesis, resulting from a previous Abduction, and from virtual predictions, drawn by deduction, of the results of possible experiments, and having performed the experiments, conclude that the hypothesis is true in the measure in which those predictions are verified.... (CP 2.96)[2].

To sum up, we obtain new hypotheses with abductive inferences which lead to deductive consequences that can be empirically tested by induction[3].

It is important to recognise that philosophers are mere mortals and can be influenced by cultural trends in the formation of their philosophical assumptions. In this particular case, it is arguable that the conventional view was heavily influenced by the prevalent cultural association between knowledge and science with the uncertainties of probability and statistics. But this inductivist mindset is the result of a long and complicated history of competing views about the nature of logic, knowledge and science. This millennial battle for the soul of science started in ancient Greece with the stoics endorsing a deductivist view of knowledge that was incompatible with the epicureans’ acceptance of induction. The deductivist view understood knowledge as certain, whereas the rival inductive viewed knowledge as a fallible and risky enterprise. Following the lead of Aristotle and the triumph of Euclidian geometry, the reign of deductivism would remain unrivalled for almost two thousand years. The predominant belief then was that scientific knowledge can only be obtained by deduction from self-evident and indubitable axioms.

Eventually, the inductivist camp would revolt and claim victory, but this victory took centuries and certainly wasn’t decided by the scientific revolution, contrary to what is commonly believed. This is evidenced by the writings of some of the paragons of modern science. Newton’s *Principia* was heavily inspired by an axiomatic mindset where natural laws

[2] Following standard practice in Peirce scholarship, I will abbreviate Peirce (1932–1958) as CP.

[3] It can be argued that this conventional view is outdated and doesn’t reflect the view of most philosophers of science nowadays. For example, the idea that scientific discovery should be relegated to psychology was popular when logical empiricism and Popper’s falsificationism were still influential, but diminished considerably after a resurgence in the interest of discovery processes championed by Hanson, new scholarship on the work of Peirce and numerous works about inference to the best explanation (IBE). It’s now evident that discovery requires more than arbitrary guessing. To this it can objected that the conventional picture is still very influential among most philosophers in general and commands cultural influence. It’s the view that scientists tend to accept when they express their philosophical opinions about the nature of scientific methodology. Besides, we should avoid the common identification of abduction with inference to the best explanation (IBE) adopted by many authors (Cf. Harman, 1965, pp. 88–89; Thagard, 1978, p. 82; van Fraassen 1980, p. 23; Hookway, 1995; Bartelborth, 1996, p. 138-48; Vogel, 1998; Lipton, 2000, p. 184; Psillos, 2002, p. 614; Douven, 2011). This identification should be avoided since IBE is usually understood as the acceptance or rejection of already available hypothesis, whereas abductions are inferences that create hypotheses. Of course, the problem of constructing a good hypothesis is analogous to the problem of choosing a good hypothesis (Fann, 1970, p. 42), but an abduction can occur even in the absence of alternative explanations. IBEs are also associated with attempts to reduce them to inductions, or vice-versa (Harman, 1965, pp. 88–89; Thagard 1996, p. 34; van Fraassen 1989, p. 132; Lipton, 1991, p. 58).

are presented as a deductive consequence (a theorem) of axioms (laws of motion and some propositions of Euclidian geometry). The fact that Newton wasn't able 'to deduce from phenomena the reason for these properties of gravity' was perceived as a personal defeat. Despite the experimental nature of his *Opticks*, Newton embraced in public a contempt toward experimentation and favoured a geometrical mindset, thus boasting 'that he first proved his inventions by geometry and only made use of experiments to make them intelligible and to convince the vulgar' (Manuel, 1968, p. 302). Empiricists and mathematicians were rivals then. The Royal Society had strongly empiricist values that remained undisputed until Newton's presidency started in 1703. For then on, the mathematical faction dominated the empiricists for 25 years. Only Newton's death allowed the empiricists to reclaim a leadership position in the Royal Society (Feingold, 2004).

But knowledge was still widely perceived as synonymous with certainty, which is also one of the cornerstones of deductivism. John Wilkins in the seventeenth century makes a harsh distinction between 'knowledge or certainty' and 'opinion or probability' (Wilkins, 1675, p. 5). This belief was so widespread that it was accepted by epistemological optimists and sceptics alike (Milton, 1987, pp. 62-3). Bacon, Descartes, Foucher, Bayle and Hume all endorsed this deductivist credo. Hume (1739, p. 181) expressed this view when he observed that 'knowledge and probability are of such contrary and disagreeing natures, that they cannot well run insensibly into one another'. That the deductive mindset was still predominant in the nineteenth century is suggested by Maxwell (1890, 2:419)'s remarks that methodological improvements were necessary so that 'the method of inductive philosophy will no longer be derided as mere guess-work.' This betrays a mindset that still held inductivism in low esteem.

This deductivist faith was also exemplified by the popularity of mechanicism since the 17th-century. The general assumption was a determinist vision of the universe as a clockwork. The expectation was that a few mathematical formulas and laws of nature could be used to explain reality and to predict future events with certainty. The epitome of this assumption was the triumph of Newtonian physics. In the late nineteenth century, this clockwork universe gave way to a statistical model of reality. The attempts to adopt an axiomatic Newtonian approach to different areas, such as biology and sociology, were unsuccessful. As the measurements became more and more precise, even known laws of physics and chemistry turn out to be only rough approximations. Now all science shifted to statistical models. In this new paradigm, the certainties of determinism were replaced by degrees of chance and probabilities. The nature is no longer viewed as a predictable mechanism, but as a mass of unpredictable phenomena that can be described by statistical models (Salsburg, 2001, p.15). The indirect consequent of this shift is also a more abstract and instrumentalist view of the 'things' of science. Instead of talking about observables, there is an increasing emphasis on mathematical functions that described the randomness of what we could observe (Salsburg, 2001, p.17).

This shift was particularly dramatic in physics. The optimist view of classical physics was ripped to shreds by the unsettling discoveries in the subatomic world. Heisenberg showed that in quantum mechanics we can determine either the position or the velocity of a particle, but not both. This and other strange phenomena, such as the results of the double-slit experiment, suggested that the nature of sub-atomic particles had nothing to do with the behaviour of everyday objects. By mid-1920s, Bohr decreed that we need a 'radical revision of our attitude toward the problem of physical reality' and that 'quantum physics tells us nothing whatsoever about the world… because quantum objects don't exist in the same way as the everyday world around us'. This interpretation of quantum mechanics became the norm, but was resisted by Einstein, who protested that 'God does not play dice'. To this Niels Bohr quipped: 'Don't tell God what to do with his dice'. Einstein never accepted this instrumentalist view of subatomic particles and defended the need of a realistic understanding of quantum mechanics. This is not surprising given that he was a deductivist, through and through. Einstein (1936, pp. 365-6) not only believed that there is no inductive method which could lead to the fundamental concepts of physics, as he also insisted that logical thinking is necessarily deductive. But Einstein's deductivist mindset was ridiculed as a sign of senility and dogmatism.

There were some sporadic focuses of resistance in philosophy such as Popper (1959; 1963)'s falsificationism, Hempel and Oppenheim (1948)'s nomologic-deductive theory, deductivism in informal logic (Gerritsen, 1994; Groarke, 1992; 2000; Lambert & Ulrich, 1980; Nosich, 1982; Thomas, 1981, Yezzi, 1992), Kitcher (1989)'s deductive chauvinism and Suppe's (1997) noninductive approach. But they were the exceptions that prove the rule. The cultural mindset of certain knowledge was replaced by a worldview that emphasises probability and fallibility. Deductions were reduced to a caricature of non-informative tautological exercises and even in formal logic there was a demand to create new deductive systems that seemed more in tune with the new inductivist values[4]. This was a victory without winners though, because while the public perception of science became inductivist, it did nothing to solve the underlying conceptual tensions that motivated the battle in the first place. This resulted in the present intellectual discomfort where ideas are repeated even when they are at odds with our best theoretical practices.

3. WHAT IS WRONG WITH THIS PICTURE

Taxonomies organise the objects of study in general categories. They can assist both in the understanding of the object of study and in the coordination of cooperative cognitive enterprises. But the latter task can only be carried out if the first task has been met, because the main function of taxonomies is to transfer information. For example, if a botanist classifies a plant as a Bryophyta or as a Equisetophyta is because they have distinct characteristics that are informative about their nature. It will be argued that the conventional taxonomy of inferences used in logic and philosophy of science has no epistemic justification. Depending on the circumstances, deductive inferences can display the characteristics associated with abductions, inductions can behave as tautological deductions, and abductions can fulfil the role of inductions. This is concerning because it shows that we are confused on such a basic level that our understanding of inferential standards, knowledge and science is compromised in its roots. The following sub-sections are attempts to raise to the surface the underlying conceptual tensions in the conventional view.

3.1 DEDUCTION

The starting point of the present analysis is the conventional notion of deduction. If deductive inferences are the ones that are aimed to be valid by a reasoner, then what is deductive or not is determined by the reasoner's point of view. This means that traditional examples of inductive inferences could be considered deductive if the reasoner was firm enough in his belief that the truth of the premises guarantee the truth of the conclusion. We cannot avoid this problem with the stipulative definition that deductive inferences are the ones that can be valid, because invalid deductive inferences can't be valid and because it would turn every deduction valid by default. So, there are no deductive inferences per se above and beyond the reasoner's aims with an inference. This is not illuminating.

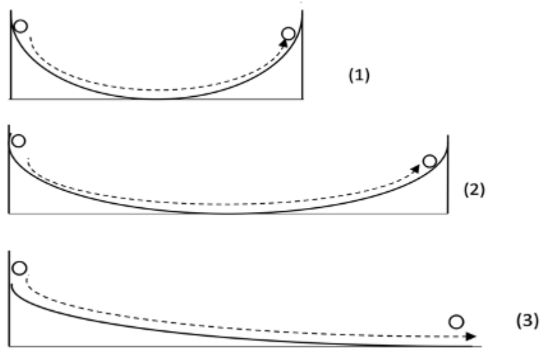
[4] Cf. Adams (1965; 1975).

The current view places validity at odds with informative inferences. If a conclusion is not contained in the premises, it can't be inferred from them; on the other hand, if it is contained in them, it is non-informative. But this means that informative inferences must be invalid, while valid inferences must be tautological. Thus, deduction is associated with a mechanical and uninformative derivation of consequences. It's perceived as a mere mechanical mean to test the empirical consequences of hypotheses. This view flies in the face of everything we know about deductions. Hintikka (1973, p. 222) describe this view as the 'disquieting scandal of deduction'. Indeed, deductive inferences not only can be informative, as they might require insights and imagination. This result is so puzzling that it prompted different hypotheses. For example, Peirce tried to come to terms with it by distinguishing between corollarial and theorematic deductions. Corollarial deductions occur when the conclusion follows directly from the premises, while theorematic deductions usually occur in proofs of mathematical theorems and require ingenuity and experimentation (CP 2.267; 7.204). The puzzle is that in theorematic deductions the conclusion necessarily follows from the premises, but also have the abductive character of insight (CP 3.363). Peirce's solution is unconvincing: in a theorematic deduction the reasoner is able to make a corollarial deduction of the conclusion after experimenting upon the premises (Peirce, 1976, vol. 4, p. 38).

The notion that the conclusion of a valid deduction is already contained in the premises is implausible. But its implausibility can be mitigated by arguing that the relevant conclusions implicit in the premises can be difficult to assess. This is the reason why conceptual analysis can lead to new discoveries that are implicit in the accepted knowledge, but in order to be drawn require insights and imagination. As Albert Szent-Georgi once observed, 'discovery consists of seeing what everybody has seen and thinking what nobody has thought.' This is also the reason why some important discoveries have an obviousness character to them in hindsight. Thus, it could be argued that even if the conclusions are already contained in the premises of deductive inferences, they are not contained in any trivial sense. In the words of Frege (1884, p. 101), the conclusion is contained in the premises 'as plants are contained in their seeds, not as beams are contained in a house'.

There are many reasons why many may ignore a conclusion that is implicit in the accepted premises. For starters, their understanding of the available premises can be superficial. They may not realise the consequences of the current premises for the simple reason they are not thinking deeply about them and don't have a proper understanding of their meaning. They may also know the premises individually, but fail to see how they can be combined to imply a conclusion. This happens when one might not see the forest for the trees occurs because knowledge areas tend to be vast and are composed by premises in the thousands. New and interesting conclusions can also be widely ignored when they contradict other widely accepted beliefs that are inconsistent. In these cases, a reasoner can see obvious truths that are widely ignored if he is able to overcome collective self-deception and abandon incoherent premises that are widely accepted.

The creative powers of deduction are exemplified by Galileo's thought experiment that led to the concept of inertia. His mental experiment involved a scenario with marbles rolling on inclined planes in the absence of friction and other resistant forces. Galileo conjectured that a marble rolling down a frictionless hill would run up to the same height on an opposite hill. Next, he reasoned that as the angle of the opposite hill decreases, the marble should travel increasingly greater distances. Galileo proposed then that if the opposite hill were made horizontal, the marble would continue to travel forever. This led to the law of inertia: a body moving with a certain speed along a straight-line path will continue to move with the same speed along the same straight-line path in the absence of external forces — see the image bellow.



Galileo's Thought Experiment on Inertia

Thus, it can be argued that the creativity in a deduction lies in the discernment of relevant patterns that are hidden in plain sight. But here we face another obstacle. If these deductive inferences are creative, they cannot be distinct from abductive ones, since they also involve the creation of new concepts and inferences from known premises. The only difference is that in abductive practices the known premises may also consist of propositions about empirical observations, whereas in the deductive practices the known premisses are usually about concepts or general hypotheses. But this is only a difference in the level of abstraction and generality used in the premises, which is a matter dictated by the needs of the research at hand. Empirical data is nothing more than phenomenological concepts interpreted according to a theory-laden observation. It is also a conceptual matter, through and through.

The conventional view of validity has other unpalatable consequences such as that any inference with contradictory premises or a necessary conclusion is valid. This implies, for example, that all mathematical (or logical) truths imply each other, since mathematical (or logical) truths are regarded as necessarily true. Briskman (1975, pp. 118-19) summarises the problem:

> If we start from the assumption, unquestioned by almost all writers on entailment, that mathematical truths are necessarily true, then an identification of entailment with strict implication leads to the seemingly absurd consequence that all mathematical truths entail each other, and so are logically equivalent, and thus, assuming our derivation rules are complete, inter-deducible. But this makes a nonsense of mathematical (and logical) practice; for in mathematics (and logic) we usually prove theorems from axioms and can only rarely reverse the procedure and prove the axioms from theorem.

This notion of validity is both epistemically useless and harmful. If accepted, it would trivialize mathematics and logic. One could resist this criticism with the argument that the conventional view of validity was never intended be an epistemic guide to truth. Validity does not increase knowledge. It preserves it. In a typical attempt of proof, a mathematician does not know yet the truth values of the conjecture they are trying to prove or which premises will be required to prove it. At least not until they have actually done it. If this attempt of proof is successful, it will be due to the conceptual connections between the premises and the conclusion (an epistemic requirement), not because of the truth values of either the premises and conclusion alone. The reasons to accept the validity of a proof are independent of the truth-values of the relevant propositions (we don't know whether the conclusion is true or not, since we want to establish its truth), and involve the conceptual connections between the premises and the conclusion. But this seems a cop out. If validity is the goal of deductive inferences, then the satisfaction of the stipulated goal should satisfy any reasoner.

The certainty of deductions is usually presented in contrast with the fallibility of induction, especially given the prominent role of induction in science. Since most inferences in science are thought to be inductive in nature, it is assumed that induction must be fallible in some special sense, which is encapsulated in their non-monotonic character. But this line of thinking is faulty, since any human endeavour is fallible, being inductive or not. There is no specific type of reasoning that encapsulates human fallibility. It is also committing a category mistake in that it conflates induction with fallibility and consequently deduction with infallibility. I can accept that a given inference is valid, but I could be wrong about it.

The cultural perception that deductive endeavours are certain in some sense is motivated especially by the rigours of formal logic and mathematics, but their actual practice suggests an entirely different picture. If anything, they are risk endeavours governed by intuition and instinct. In the history of mathematics there are numerous examples of conjectures that were accepted as theorems for significant periods of time, but whose proofs turned out to be incorrect. One famous example is Alfred Kempe's alleged proof in 1879 of the four-colour theorem, which was widely accepted for 11 years, until it was shown to be incorrect by Percy Heawood in 1890. Another example is Cauchy's proof in 1821 of the idea that a convergent infinite series of continuous functions is continuous. This proof was debunked by Abel five years later with certain Fourier series as counterexamples. Or consider Ampere's proof in 1806 that continuous functions are differentiable except at some isolated points. This inference that was refuted by Weierstrass in 1872, who presented a continuous but non-differentiable function. Naturally, not every single example can be mentioned here, since this is a pretty long list.

These examples reinforce the view of Imre Lakatos (1976, 1978), which argued that mathematics cannot be viewed as Euclidean science based on the historical record of the discipline. Not only mathematics is not infallible, as it is shown by the numerous mistakes and conceptual changes, as it is not purely demonstrative, because there is no mechanism to ensure that axioms are necessarily true. In fact, often axioms are picked and chosen in order to ensure the validity of certain theorems mathematicians want to prove. Certainly, it cannot be purely mechanical as it was demonstrated by Kurt Gödel's incompleteness theorems. There is no consistent system of axioms capable of proving all truths about the arithmetic of natural numbers and any of such systems will be unable to prove their own consistency.

The mathematical knowledge may be established by demonstrative reasoning, but the conjectures are general statements suggested by particular instances in a reasoning process that might be better described as inductive. Conjectures express mathematical patterns and each additional instance that reinforces this pattern increases the confidence in the conjecture (Polya, 1954, p. 7). Simply put, induction in mathematics may point to promising conjectures, given patterns that were verified numerous times in the past. It is also a known fact that probabilistic inferences in the theory of numbers motivate many discoveries, such as the observation that primes become rarer as they become larger led to the prime number theorem. In short, deduction relies on inductive inferences because they increase one's confidence in a conjecture and may encourage further search for a proof (Putnam, 1975, p. 76).

Mathematics is also inductive-based on a more fundamental level, since many of the fundamental beliefs required to do mathematics cannot be established by demonstrative reasoning. The proofs rely on numerous inductive assumptions such as that the linguistic conventions used to express their mathematical ideas will have the same meaning tomorrow, that mathematical patterns are stable, etc. Russell and Whitehead, important names involved in the logicist project, went as far as saying that:

> [the] reason for accepting an axiom, as for accepting any other proposition, is always largely inductive, namely that many propositions which are nearly indubitable can be

> deduced from it, and that no equally plausible way is known by which these propositions could be true and the axiom were false, and nothing which is probably false can be deduced from it (Russell & Whitehead, 1927, p. 59).

The reductive view of mathematics as a completely mechanical endeavour of proving theorems also ignores that mathematicians not only try to devise simpler, more precise and more illuminating proofs of known theorems, as their standards of precision change over time in an informal manner, if not vary from one mathematician to another. This process is not guided by rigid rules (Backhouse, 1998, p. 1850). The rules themselves are investigated, thus changing mathematics over time. The evolution of proofs led to an evolution of concepts and their understanding. Thus, what was accepted as a rigorous demonstration of a theorem for one generation can be viewed as incorrect for later generations. This change is driven by an intuitive idea of the concepts involved in the theorem (Ibid, pp. 151). Besides, you can have formal proofs that are logically rigorous, but that are difficult to comprehend and ultimately unconvincing. It is not enough to offer a verification that a conclusion follows logically from some premisses if there is no understanding of why it does (Dawson, 2006, p. 271). Mathematicians want to demonstrate why a theorem is true, not just that it is true (Mancosu, 2001, pp. 97–117), because a proper proof is convincing and provides understanding (Hersh, 1993, pp. 389–99).

Formal logic is also tricky business since we don't have any decision procedure capable of showing whether an inference is invalid or not. Instead, we have attempts to interpret an inference with a logical form that is supposed to be invalid. But this diagnosis is not safe, because it does not preclude a different interpretation where the same inference is shown to be valid (Massey, 1981, p. 494). Consider the following inference: 'John took a walk by the river. Therefore, John took a walk'. This inference was widely assumed to be invalid until Davidson (1968) showed it otherwise by translating it with predicate logic and quantification over events. Or take into consideration this inference: 'Tom, Dick, and Harry are partners. Tom and Harry are partners'. This inference was deemed as invalid, unless enthymematic, until Leonard & Goodman (1940) presented a different interpretation that showed its validity with mereological predicate logic. Thus, the only criterion we have to establish the invalidity of an inference is to maintain that it has true premises and a false conclusion, but this trivial method is logic-indifferent method of proving invalidity (Massey, 1981, p. 494). It relies on nothing more than individual intuition and conceptual analysis.

There is also the fact that we are aware of the existence of numerous paradoxes of all kinds (e.g., sorites paradox, liar paradox, etc.) that remind us that foundational uncertainties are also an integral part of our understanding of logic. This was observed by Russell and Whitehead in the *Principia*: 'In formal logic, the element of doubt is less than in most sciences, but it is not absent, as appears from the fact that the paradoxes followed from premises which were not previously known to require limitations' (Russell & Whitehead, 1927, p. 59). Russell's work was the living proof that caution was advised since he discovered a paradox in set theory. Finally, the last nail in the coffin of the mechanical mindset is Alan Turing's and Alonzo Church's independent demonstrations that the logic of quantification is undecidable. To put simply, there is no algorithm that allows us to determine whether a sentence is a logical consequence of a premise using a quantificational language containing at least one non-monadic predicate.

It is also arguable that deduction can take place in language uses and contexts that have been traditionally associated with abductions and inductions. For instance, in Arthur Conan Doyle's novels Sherlock Holmes infers the identity of the culprit from a long chain of reasoning based on the available evidence. Interestingly enough, Holmes has a keen eye for detail, but his main strength lies in his powerful intuition. Most would be unable to draw the same conclusions even if they had access to the same evidence, because it is not the availability of data that matters, but how he sees and interprets the data. But the main point is that it is perfectly natural to describe these long chains of inferences as deductive inferences. It rings true to talk about how Holmes deduces who is the responsible for the crime. Another example from ordinary discourse is that we can also talk about how great poker players can deduce what other players think based on their behaviour in the game table. Again, this a deduction that is both intuitive and based on empirical observations. Isaac Newton himself, one of the paragons of modern science, observed that the 'main business of natural philosophy is to argue from phenomena without feigning hypotheses, and to *deduce causes from effects* [emphasis added], till we come to the First Cause, which is certainly not mechanical'. Newton talks as if the most basic inferences and insights based on observations are obtained by deduction and not induction. That this is hardly a slip of the tongue becomes evidenced by another passage from the *Principia*: 'Whatever is not *deduced from the phenomena* [emphasis added], … have no place in experimental philosophy. In this philosophy, *propositions are deduced from phenomena* [emphasis added], and rendered general by induction.' Once again, the first inferences based on the testimony of senses are described as deductive and not inductive. This view of deduction was pretty common in the 17th and 18th centuries. Robert Moray, while explaining the aims of the early Royal Society in 1660, stated that the institution will not 'dogmatically define, nor fixe Axiomes of Scientificall things, but will question and canvas all opinions[,] adopting nor adhering to none, till by mature debate and clear arguments, chiefly such as are *deduced from legittimate experiments* [emphasis added], the trueth of such positions be demonstrated invincibly' (Hunter, 1995, p. 172). The scientist is expected to deduce the conclusion from the experiments and do it in a demonstrably manner. David Hartley says something similar when he suggests that the technique of false position is handy: 'it is useful in all kinds of inquiries, to try all such suppositions as occur with any appearance of probability, to endeavor *to deduce the real phenomena* [emphasis added] from them' (Hartley, 1749, pp. 345–346). In an 1819 lecture to the City Philosophical Society, Faraday talks about the need of rigorous standards in science: 'Nothing is more difficult and requires more care than philosophical [scientific] *deduction* [emphasis added], nor is there anything more adverse to its accuracy than fixidity of opinion' (Jones, 1870, p. 300). This is not surprising because it is perfectly natural to talk as if we could infer the consequences from propositions about observations in a deductive fashion. So, we can reasonably talk as if any proposition assumed by means of sense-perception is obtained by deduction. Nevertheless, the point is not so much that we can endorse a deductivist view of induction, but that it is not clear what we mean by deduction and induction to begin with.

3.2 INDUCTION

No analysis of the conventional view will be complete without a deeper scrutiny of the received notion of induction. The first criticism is that the idea that induction is represented by a series of repeated steps ending with a general conclusion is obviously a fiction that has no bearing in real science[5]. Chemists didn't infer the water is H_2O after analysing a series of portions of H_2O. They are not 'beam counters' and enumerative induction can only produce 'more of the same' (Lipton, 1991, p. 16). As Schmidt (1966, p. 279) explains:

[5] Or real philosophy, which may require new distinctions at a more abstract level that are not provided by the data (Cf. Williamson, 2016, p. 265).

> From the fact that some vessels float on water we cannot judge that all vessels float on water; but if those which float on water are seen to agree in the property of displacing water of a greater weight than their own, then we can judge that every watertight vessel that displaces more water than would equal itself in weight floats on water.

The heart of science is not a mechanical procedure of enumerative induction that extends the properties of some members of a group to all of its members. Instead, it postulates something more profound such as deep structures, ontological frameworks, unobservable entities and counter-intuitive abstractions. In short, it provides explanations with theoretical postulations which are entirely different from the data which they intend to explain. Those sophisticated generalisations, such as the proton-electron constitution of the hydrogen atom, are the bread and butter of science, but cannot be obtained by simple enumeration of instances (Braithwaite, 1953, p. 11). As Ernst Mach observed:

> ...it is rather strange that most enquirers ... denote induction as the principal means of enquiry, as though the natural sciences had nothing to do but directly classify individual facts that lie openly about. … The name 'inductive sciences' for the natural sciences is therefore not justified (Mach, 1976, p. 231).

So, the inductive methodology that is supposed to encapsulate the essence of science reduces rational discernment to a mechanical process of enumerating cases. It's the replacement of creative insight by a mechanical summary of data. However, what matters is not the empirical inspection of every member of the specified class, but rational discernment. In his Chinese room thought experiment, John Searle (1980) correctly argued that syntax doesn't suffice for semantics, because one can produce answers in Chinese without understanding their meaning by blindly manipulating uninterpreted characters according to a series of rules. Real intelligence, contended Searle, requires semantics. Similarly, it could be added that real intelligence requires the kind of intuitive creativity that is absent in the enumerative view of induction. The inductive behaviour associated with enumeration of observations is the sort of associative learning that fails to differentiate between human and animal intelligence, as it is shown by Pavlov's experiments with animal conditioning and more recent experiments with animals as primitive as sea slugs (Foster, 2009, p. 107).

Enumerative induction cannot provide significant conclusions, because the particular instances, no matter how large in number, are irrelevant[6]. It's the mind that extracts their nature

[6] The problem of justifying induction has been traditionally associated with David Hume's sceptical challenges to causality, but Hume himself never criticised the belief in induction per se (Cf. McCaskey, 2006; Milton, 1987). The word 'induction' appears twice in the *Treatise*, but in both circumstances Hume is relying on inductive inferences. The first occurrence of the term is in Hume's inference against the ideas of the infinite divisibility of space and time: '*It requires scarce any, induction to conclude* [emphasis added] from hence, that the idea, which we form of any finite quality, is not infinitely divisible, but that by proper distinctions and separations we may run up this idea to inferior ones, which will be perfectly simple and indivisible. In rejecting the infinite capacity of the mind, we suppose it may arrive at an end in the division of its ideas; nor are there any possible means of evading the evidence of this conclusion' (Hume, 1739, p. 27). So, Hume is arguing that it is easy to demonstrate by induction that our ideas have limits and the human mind is finite. Hume again uses an inductive inference when he defends his hypothesis about the nature of belief: '*I conclude, by an induction* [emphasis added] which seems to me very evident, that an opinion or belief is nothing but an idea that is different from a fiction, nor in its nature, or in the order of its parts, but in the manner of its being conceived' (Hume, 1739, p. 628). Again, Hume relies on an inductive inference in order to establish a conceptual feature of his system.

Hume's causal scepticism can be easily confused with inductive scepticism because it presupposes that we cannot establish causality by induction. Thus, one might think that if his criticisms against causality are successful, induction is unjustified as a result. But this interpretation is problematic for two reasons. First, it assumes without additional reasons that the supposed inability to inductively justify claims about necessary connections in factual and understands their meaning, not the experience. An intellect with the knowledge of every particular observation in the universe, but devoid of imagination or creative abilities wouldn't be able to make any significant scientific contribution, since we cannot grasp scientific principles by enumeration, which is nothing more than an arithmetic operation of adding. The conclusion of an inference is substantial precisely because it goes above and beyond the particular instances, not because of it. Enumerative induction allows us to infer from certain observed facts only additional facts of the same kind.

Enumerative induction is usually associated with the discovery of observational laws (e.g., 'All gases expand when heated', 'All planets move in ellipses', etc.), which are regarded as the hallmark of genuine science. But in these examples the relevance lies not in the generalisation processes, but in the finding that certain natural kinds have these properties. They have little in common with the garden variety of enumerative conclusions presented in textbooks such as 'All ravens are black'. These generalisations are not illuminating because science does not depend upon mere regularities, but on the ability to see laws and principles. We are not interested in knowing that all havens are black, but in understanding why ravens are black.

According to the enumerative view, generalisations such 'All swans are white' are inferred from a variety of singular observations, such as 'swan$_1$ is white', 'swan$_2$ is white', etc. But in actual scientific practice, scientists usually rely on general observational claims, not singular ones. The observations are implicitly general because they require the acceptance of general concepts inserted in systems and their corresponding laws (Torretti, 1986, p. 6). For example, in J. J. Thomson's cathode ray experiments the observational claims are described not as 'effects $e_1, \ldots, e_n$ were recorded', but as 'effects of sort e occur' (Suppe, 1997, p. 407).

The enumerative notion also conveys the erroneous idea that more data will justify the initial inference that was made to explain this data in the first place. It is similar to the fallacy of affirming the consequent in the sense that you argue from the truth of a theoretical consequence to the truth of the theory. Worse, it leads to a soritical problem that dates as far back as Galen (129 – c. 216 CE) and his treatise on medical experience. Galen correctly objected that if any small number of observations, say, n, are insufficient to reliably establish the truth of a generalisation, then n + i observations will also be insufficient. Let's assume that 49 observations were not enough, and 50 were. This would imply that the 50th observation would be sufficient by itself, which is arbitrary and contradicts the initial assumption (Galen,

matters can be extended to all universal claims about factual matters. Secondly, even if we conceded that this position was a consequence of his criticism, this position was retrospectively attributed to Hume. It was not his position, as it is clearly evidenced by the aforementioned passages of his writings.

The real problem of induction is that it is impossible to infer a universal claim from particular ones, which can be dated back to ancient Greek philosophers. Sextus Empiricus presented the problem in clear terms in *Outlines of Pyrrhonism*: 'It is also easy, I consider, to set aside the method of induction. For when they propose to establish the universal from the particulars by means of induction, they will effect this by a review either of all or of some of the particular instances. But if they review some, the induction will be insecure, since some of the particulars omitted in the induction may contravene the universal; while if they are to review all, they will be toiling at the impossible, since the particulars are infinite and indefinite. Thus on both grounds, as I think, the consequence is that induction is invalidated' (*Outlines of Pyrrhonism*, book 2, chapter 15). This problem was also known and discussed by the first commentators of Aristotle, such as Clement of Alexandria—this topic will be discussed again in section 4, when the origins of the enumerative view of induction are discussed. The criticisms made by Hume against causality will be discussed in section 6.

It could be objected that this formulation of the problem is too restrictive because it only applies to inductive inferences that have a generalisation as a conclusion. But there are also previsions, which are inductions that have a particular statement in the conclusion, such as 'All emeralds previously found have been green. Therefore, the next emerald will be green'. The response to this criticism is that this inference is only reliable if another premise is added, namely, that all emeralds are green. But this additional premise can only be justified by generalisation, so we are back to the original problem.

1944, pp. 96–7). It is reasonable to argue then that the cut-off point in any enumerative induction will be based on intuition and not on the size of the sample.

Scientific reasoning is not a mechanical summary of previous data, but a creative insight pushing the bounds of understanding further than the available evidence. This process can't be reduced to a series of rules to be memorised considering that it requires creative insights, and they can't be reduced to an algorithm or computational method. The insistence on enumerative induction, and the ideal of a complete induction where every member of the class being investigated is observed, is motivated by an attempt to make science seem less conjectural and more objective. But it ignores the fact that genuine inductive insight depends not merely on the amount of evidence, but on the mental quickness of the knower (Groarke, 2009, p. 142).

The scientific understanding doesn't grow stronger as more cases of the same type are amassed because accumulation of data simply reinstate the facts themselves. Relevant scientific inferences are not focused on how many times a phenomenon *x* is accompanied by *y*, but on understanding what *x* is and why it produces *y* (ibid, pp. 148–9). It is not a coincidence that surprising and unexpected scientific insights in empirical matters have a speculative and risky flavour to them. William Harvey's discovery of the circulation of the blood was inferred from the existence of 'capillary channels connecting the arteries and veins ... from his [incomplete] knowledge of the rest of the system' (Robinson, 1927, pp. 211–3). Thus, it is not that we should expect the conclusion to be a safe summary of the observed data, because this is non-informative. On the contrary, inferences tend to be valuable precisely when they transcend the available evidence and move from observed to unobserved phenomena. In other words, inferences are valuable when they are speculative.

The enumerative notion of induction assumes a conservative view of inferences where the premises and the conclusion should be isomorphic. Thus, the assumption is that the universal conclusion can be justified if it is sustained by premises that contain every single individual observation. The inference then is viewed as secure because it is reduced to an explicit counting process, but this fails to capture the ingenuity that is needed to obtain a relevant conclusion from the premises. We judge inferences not by how much the conclusions are similar to the premises in logical form, but by their ability to clarify and explain phenomena. The traditional problem of induction is motivated by this isomorphic mechanical view. The universal conclusion, argues the sceptic, can't be justified by the individual premises, because there isn't a formal match between the two, since the premises will always be finite. But the rationality and pertinence of an inference is not associated with matters of formal structure. Inferences are accepted by epistemological considerations that are indifferent to the straitjacket of logical isomorphism.

The enumerative view of induction as some sort of counting process has other basic conceptual issues that deserve a closer look. The notion that we should consider all the past observations of swans to infer their nature get things backwards. I can only count individual swans if I already have a concept of what is a swan and its defining characteristics. In other words, the conclusion that is supposed to be supported by particular instances needs to be assumed so that we can count them in the first place. This means that enumerative inductions cannot be used to create concepts (e.g., related to a species), because they need to assume well-defined concepts in order to take place. For example, we can only infer that all swans are white by means of criteria for knowing what is and is not a swan. But now enumerative induction will behave as a tautological deduction because the only samples taken from this whole will have the same features as the whole. Thus, universal statements will follow trivially from the samples (Davis, 1972, p. 38). This goes against the conventional view, because it suggests that the examples of enumerative inductions *are not ampliative*. The inference that all swans are white based on the premise that all observed swans in the past were white, more or less repeats what was said in the premise because every observation is made under the assumption that each instance of a natural kind will have the same properties. If you couldn't draw the universal conclusion from the premises you started with the wrong concept. It would be an incoherence.

That inductive based beliefs can have a monotonic aspect to them is evidenced by the following example: suppose that we found a substance resembling rubber, but that has no electrical resistance. Instead of abandoning our previous inductive commitments about rubber and concluding that not all pieces of rubber are electrical resistant, we would prefer to conclude that we found another material that resembles rubber in some superficial characteristics, but which is ultimately a different substance. After all, electrical resistance is a fundamental property in our concept of rubber. This monotonic aspect of inductions is usually ignored because the importance of concepts, especially stipulative definitions for science, is greatly underestimated. For example, Ohm's Law became true by definition because the resistance of a conductor was defined as the ratio of the electromotive force to the strength of the current (McCaskey, 2014, pp. 184-186). Besides, it is not obvious that typical inductive inferences are invalid when the hidden assumptions implicit in the inference are specified as additional premises. In the swans' example, it was assumed that all swans have the same colour. So, the inference is as follows: these birds are white; these birds are swans; all swans are the same colour; therefore, all swans must be white. This inference is valid even if the premise that all swans have the same colour is false (Groarke, 2009, p. 134). Inductive inferences can also be considered valid with the assumption of nomic necessities in the formulation of scientific laws. Scientific laws can express what is possible or impossible in different areas. In this sense, an inductive inference is valid when it is justified by a nomic connection between the relevant phenomena (Keynes, 1921; p. 251, 263; Johnson, 1924, p. 9; Braithwaite, 1953, p. 293; Kneale, 1949, p. 258; Buchdahl, 1971, p. 348). If inductive inferences are based on concepts, then most discoveries can also be regarded as deductive. Polanyi observes this fact when he states that:

> to an important degree all discovery is deductive. For no enquiry can succeed unless it starts from a true, or at least partly true, conception of the nature of things. Such foreknowledge is indispensable and all discovery is but a step towards the verification of such foreknowledge (Polanyi, 1961, p. 465).

One way to avoid this criticism is by discussing other examples of enumerative induction where the relation between premises and conclusion is no so straightforward. Suppose I have a bag, but I ignore its contexts. Let's say I draw 5,000 red beans in sequence. If we adopt the enumerative view, this suggests that it is highly probable that the next beans will be red. This reasoning assumes that if we take enough samples of particular instances we will uncover the nature of the class. But that is not true unless we already know something about the nature of the class to begin with, which renders the enumerative inference useless (Davis, 1972, p. 31). The fact is that I still can't make any previsions about the chances of getting new red beans if I don't know whether the bag has beans of different colours and how they are distributed. Only then the laws of inductive reasoning will apply (Ibid., 30). Thus, each inductive inference requires assumptions such as an interpretation of the sample and a hypothesis about the population in question that doesn't have a probability figure. They are speculative hunches and not mathematically sure probabilities (Russell, 1960, p. 323; Cheng, 1969, p. 159; Davis, 1972, p. 37). The only way to conclude that the same proportion of the colours of each beam probably prevails throughout the bag is by an abductive leap (Davis, 1972, p. 24). But since all enumerative inductions in obscure contexts will require assumptions of this kind, then inductive inferences will have to be considered abductions of a low order of creativity. That's why we wouldn't infer from the facts that the first and second students entering a room are fair-haired that all the remaining students will also be fair-haired. We know this inference is irrational due to abductive considerations (Minnameier 2004, p. 82). This also shows that a Bayesian analysis only works when the relevant prior probabilities are already established, but it is the nonprobabilistic plausibility assessments that will determine the prior probabilities in the first place. This means that probabilistic inductive logic can't provide the evidential basis for scientific knowledge because it is nonprobabilistic evidential conditions that carry the epistemic burden (Suppe, 1997, p. 394). So, inductions can also be considered abductive, or at least regarded as being based on abductions.

Another problem is that even if we could eventually take all the individual samples of a finite class, this would not be considered an inference about the properties of a class, because it would amount to the mere observation of every single one of its particular instances (Davis, 1972, p. 31). In fact, enumerative induction can be seen as a method of evidence gathering (Groarke, 2009, p. 117). The perfect induction can then be interpreted as an ideal of evidence gathering based on all the samples. But since in practice we only have access to limited samples, we are content with the second-best option, namely, to try to obtain a sizeable number of relevant samples in order to avoid hasty generalisations or incorrect previsions. This is the standard view that discards anecdotes as individual instances based on particular personal experiences without inductive significance. It is not possible to infer an accurate generalisation from a small number of samples (Moore & Parker, 2001, pp. 396–7).

But even in this methodological role of evidence gathering, enumerative induction still requires clarifications and faces counter-examples. First, it is obvious that considerations about sampling vary according to each area. For example, a sizeable sample is more relevant in areas where there is individual variation such as biology and psychology, but less important in areas such as physics and chemistry. Secondly, in areas where there is individual variation, the aim is to estimate the approximate number of a distribution of properties among different individuals. But in these cases, there is no semblance of generalisation because the only relevant quantifiers involved are 'few', 'many' or 'most' depending on the values involved, not 'all'. And even this observation wouldn't do justice to the use of modelling and statistics involved in these cases. In very broad terms, what we usually have is the data about individuals and their properties that is inserted in a formula that computes the proper value. If there is any thought and inference is about the source of the data, the proper methods, etc[7].

In fact, any methodological view that emphasises the need of a sizeable sample is prone to counterexamples. From the observation of a single triangle, I can infer that all triangles have interior angles that add up to 180 degrees. I can also infer from the observation of a single whale that all whales have lungs (Groarke, 2009, p. 140). There are also numerous examples of scientific discoveries and generalisations that were established on the basis of single experiments. The results of J. J. Thomson's 1897 cathode ray experiments are presented in both their singular and general forms and they were credited with establishing the existence of

[7] On this matter, Peirce (1992, p. 139) had a notion of induction that is different from the garden variety of enumerative examples. Peirce's example of induction is the following:

> These Ss are drawn at random from the Ms;
> Of these Ss, the proportion ζ possess the haphazard character π;
> Therefore, probably and approximately, the proportion ζ of the Ms possess π.

But notice that this doesn't fit easily with the conventional view that Peirce himself endorsed. This inductive inference is supposed to justify a hypothesis obtained by abduction, but there is no hypothesis in the example above. In Peirce's inference there is at most an attribution of property, the character π, to a group of Ss drawn from another group of Ms. Moreover, even if we ignore the lack of hypothesis, this description would only apply to subjects where there is individual variation.

electrons (Suppe, 1997, p. 394). Other examples in particle physics that have been accepted based on one or two events 'are the muon, the omega-minus, the cascade zero, and the first 'V' particles' (Galison 1987, p. 260). Or consider Marie Curie's inference that radium chloride would form the same type of crystal that barium chloride does based on a tiny sample of one decigram (1/10 of a gram) of the substance. What propelled her to this inference was an application of Hatiy's principle that all specimens of a chemical form the same sort of crystal (Norton, 2019, p. 19). Nothing in this particular case could tell why Curie was entitled to extrapolate the crystalline structure, instead of other properties (Ibid., p. 25).

The Curie example is particularly interesting because it also shows how murky and confusing is the distinction between deduction and induction. For instance, if Hatty's principle were universal, Curie's inference could have been considered a deduction; but because there are exceptions, Curie's inference is regarded by many as an induction (Ibid., p. 19). But notice that by this line of reasoning, 'had Curie made ten inferences based on ten chemical laws, without first checking which allowed exceptions and which did not, she would not have known when she was using deduction and when induction' (McCaskey, 2020, p. 3). It is not surprising then that some authors regard this inference as an example of deductive inference authorised by early crystallographic theory (Reiss, 2020, pp. 11–12).

The standard view concedes that a generalisation can be extracted from a small sample if the class being examined is homogenous (Moore & Parker, 2001, p. 397). But this requirement is also problematic. Let's consider the triangles example. For starters, it is not clear in which sense triangles are homogenous, because we have 'right triangles, obtuse or acute or equiangular triangles, scalene or isosceles or equilateral triangles, and so on' (Groarke, 2009, p. 140). What made the generalisation work is not the fact that specimens are homogenous, but that there is a necessary connection between the specified case and the generalisation it supports. Having angles equal to 180 degrees is a necessary property of triangularity. But then the conclusion of an inductive inference follows necessarily, because we are considering necessary attributes. The number of cases examined and the size of the sample don't matter (Ibid., p. 140). But isn't the fact that the conclusion follows necessarily from the premises one of the distinctive characteristics of deductive inferences?

Deductive inferences are heavily dependent on concepts, but one could argue that we obtain concepts through a process of abstraction that is inductive in character. The concept of, say, crystallisation, is obtained by analysing the formation of instances of crystals. This is a kind of inductive inference that passes from particular instances to a universal hypothesis (Clarke, 1889, p. 102). Thus, if inductive inferences are unreliable and concepts are obtained through abstraction, the concepts we use in deductive inferences are also unreliable (Groarke, 2009, pp. 161–63). These shared difficulties are reinforced by the charge of circularity that applies to both inductive and deductive practices. We need to rely on induction in order to justify induction, but we also need to trust on deduction in order to justify deduction (Carroll, 1895; Quine, 1936; Strawson, 1952; Carnap, 1968; Haack, 1976). Moreover, the acceptance of any deductive pattern involves the inductive assumption that every future proposition with the same logical form will behave in the same way and imply the same conclusions. In other words, it requires inductive projective beliefs about the nature of truth preservation.

That deductive and inductive inferences face similar epistemological problems is also indicated by the similarities between the problem of induction and Kripke's rule-following paradox[8]. The rule-following paradox is that the available evidence that involves past uses and

[8] The rule-following paradox is presented in Kripke (1982). The paradox was inspired by Wittgenstein's (1953, §201) observation that 'no course of action could be determined by a rule, because any course of action can be made out to accord with the rule' and the new riddle of induction presented by Goodman (1954, chapters 3 and 4).

disposition of future uses of addition are not enough to justify the use of the addition function, because this function holds for numbers of any size, but our uses and dispositions to use are finite. This difficult is similar to the problem of induction because it's not clear how we can justify all potential uses of induction from a limited sample. Wittgenstein himself, which lay the philosophical groundwork for discussion about rule-following, observed that we grasp the meaning of a word when we hear it: 'we grasp it in a flash, and what we grasp in this way is surely something different from the 'use' which is extended in time' (1953, §138). But this means that rule-following and meaning, just like induction, require an intuitive grasp of the essential traits that determine the nature of something. Kripke dismissed this approach to the problem, but it is clear that the problems are connected.

Empirical inductive knowledge is not perceived as deductive because it is not grounded in the certainty of a mathematical proof. The validity of deductive inferences is a rigid on-off switch that bypass more fine-grained distinctions, such as that some conclusions are more or less probable than others. Thus, there is a type of inference that should be measured in degrees of strength, since we can be more or less confident of the truth of a conclusion. Induction is characterised as a distinctive type of inference that is supposed to express our fallibility and contrasts with deductions, that are supposed to provide us with certitude. But as we saw, relevant deductions are anything but tautological and are heavily dependent on creativity and insight. On other hand, enumerative inductions can be just as rigid as tautological deductions.

The inductive character in science is usually associated with the reproducibility of experiments, but as rule of thumb any replication of experiment is defeasible. Not only there are cases where we accept a hypothesis even when experimental results provide evidence against it (Miller's test results contradicted relativity), as we can reject a hypothesis even when experimental results confirm it (such as intercessory prayer) (Norton, 2015, pp. 229–48). Examples of successful replication can be disregarded when there is disagreement about the satisfaction background facts that specify when the relevant effect is present in a veridical experimental outcome or that prevent spurious experimental outcomes. These background assumptions are not directly tested by experiment and are subject to interpretation (ibid.).

Norton's conclusion from these cases is that is the matter of the facts at hand that do the work. In his mindset, all inductions are local and there are no universal rules of inductive inference. However, it seems that the facts by themselves can't warrant inference of any kind because they don't speak for themselves. What matters the most is the intricate conceptual analysis of the researchers and their intuitive input. A hypothesis can be directly checked against 'the experience', but only in the sense that inferences from hypotheses can be directly checked by rational intuition. In a sense, post hoc empirical test is logically inconclusive. Since all observation is theory-laden, both the verification and the falsification of theories are theoretical procedures with a distinct coherentist character. The hypotheses are inferred from an observation influenced by theoretical assumptions and its consequences are tested against further theoretical assumptions.

3.3 ABDUCTION

The last piece of the conventional view is the notion of abductive inferences. Supposedly it's by an abductive inference that a reasoner can have new insights and conceive a new idea, pattern or concept. But according to the conventional wisdom, abduction and the context of discovery are epistemologically irrelevant because they contain epistemologically irrelevant psychological elements (Reichenbach, 1938; Popper, 1959, pp. 30-32; Hempel, 1966, p. 15).

For example, Kepler's proposal of laws of planetary motion was accompanied by a mythical sense of celestial harmony that seems baffling today, and some discoveries, such as Kekulé's insight about the structure of benzene, occurred in dreams. In this view, the context of justification only takes place *after the original insight* by procedures such as enumerative induction and replication of experiments. This traditional view is empiricist in character because it favours experience as the main source of knowledge. It is also mechanically oriented because it associates scientific knowledge with methodological rigour and reliable procedures of justification. But since there are no reliable procedures to create new findings, the context of discovery is surgically removed from scientific activity. The improvisation and uncertainty of creation are portrayed in a negative light, as something that should be downgraded to art or religion, or not subject to conclusive proof (Davis, 1972, p. 26).

This view can be criticised on multiple fronts (Thagard, 1978; Blackwell, 1980). First, it describes inferences in the context of discovery as unjustified by default because some discoveries were motivated by false ideas. But this makes the task of science impossible. Scientists not only come to believe in hypotheses for evidential reasons they believe to support them, as they tend to be reasonable ones (Achinstein, 1970). If the rationale used in the context of discovery could be ignored in the context of justification, it would also be irrelevant to understand the finding itself, which is absurd. Kepler's reasons for accepting that Mars' orbit was elliptical are still good reasons for accepting that idea as an astronomical truth. Besides, if the context of justification works independently from the context of discovery, the process of discovery becomes mysterious and the quality of the hypothesis proposed in the latter would be a matter of random luck. If every discovery were entirely motivated by false ideas we wouldn't have knowledge as we know it.

The main reason why abductions are believed to be epistemically irrelevant is that insights and hypotheses may prove to be mistakes. In other words, abductions are regarded as epistemically irrelevant because they are non-monotonic. But then inductions should be epistemically irrelevant as well, because they are also believed to be non-monotonic. This incoherence may be influenced by the impression that discoveries and insights are more individualistic and subjective in nature when compared to the interpersonal and public character of inductive practices. But if an inference is epistemically relevant and can meet public epistemic standards, the fact that it was conceived or accepted by one individual won't make it epistemically irrelevant or amateurish. Inversely, if an inference fails to meet such epistemic standards, the fact that it is accepted by multiple individuals won't make it epistemically relevant or respectable. Otherwise, we would have to accept an ad populum fallacy.

According to the conventional view, both abductions and inductions are informative and non-monotonic, and the only difference between the two is that abductions are creative and inductions are justificatory. But how can abductions be informative if they are unjustified? Why should hypotheses be taken seriously enough to be tested if they are unjustified guesses? Or how could we discriminate between promising and uninteresting hypotheses if they are all equally unjustified? Suppose a promising hypothesis is one with explanatory power and simplicity. How can these theoretical characteristics be epistemically irrelevant? Inversely, if hypotheses can be justified before they are tested, what is the point of tests? Some different type of justification that reinforces the first one? Should we discriminate then between two stages of justification that take place before and after the test?

The emphasis of context of justification as something related to research reports placed outside initial explanatory considerations distorts our understanding of science and the creation of theories. The idea that science is determined by empirical tests that are largely independent of conceptual insights and reasoning processes ignores that the evaluation of a hypothesis is not qualitatively different from the design of a hypothesis. There is no meaningful distinction between the reasons employed in creating hypotheses for further test and those used in evaluating hypotheses in tests. If there is no logic of discovery, in the sense of a reliable algorithm to create new hypothesis, it is because there is no logic of testing either (Putnam, 1974, p. 238).

In the received picture, a scientist formulates an idea by abduction, which deductively implies a consequence to be empirically tested by inductive means. It's this empirical test that is supposed to determine whether the idea is justified or not. But this picture makes induction redundant, either because enumerative induction is uninformative or because enumerative induction can be explained as a type of elementary and almost mechanical form of abduction that is not applicable to more sophisticated science. The inference that all *Fs* are *Gs* based on observations of individuals that are *F* and *G* can be explained as an abduction of a low order of creativity (Buchler, 1939, p. 134; Davis, 1972, p. 24; Goudge, 1950, p. 197). The generalisation that 'All havens are black' does not fully explain why a particular raven is black, but it does explain in a superficial sense, namely, that it is a particular case of a general principle.

There is also a kind of mismatch between the supposed justificatory role of induction and the description of paradigmatic examples of induction. Inductions are supposed to empirically verify hypotheses that were obtained by abduction, but in an induction by generalisation the observations of individuals that are *F* and *G* lead to the hypothesis that all *Fs* are *Gs*. So, it is a formulation of a hypothesis, not a justificatory process. One can insist that additional observations confirm the hypothesis obtained by abduction, but in this case the inference would start with the hypothesis, proceed to gather some evidence and repeat the hypothesis. Let's use predicate calculus to present the premises and conclusion as follows:

$$\forall \mathrm{x}(\mathrm{F}x \rightarrow \mathrm{G}x)$$
$$\underline{\mathrm{F}a\&Ga,Fb\&Gb\ldots\ Fn\&Gn}$$
$$\forall x(\mathrm{F}x \rightarrow \mathrm{G}x)$$

In this revised interpretation, the induction is obviously circular, not only because the conclusion is one of the premises, as the second premise that is supposed to provide the justificatory function is a consequence of the hypothesis mentioned in the first premise. The way to circumvent this problem is to observe that the hypothesis is justified by the confirmation of testable consequences that are not logically isomorphic. For example, general relativity implies that light should be bent by gravity, which was verified in 1919. But this relation doesn't fit in the rigid enumerative view of induction. In fact, what we mean by induction in this case is not an inference, by a simple act of empirical verification by observation. So, the only way to save the epistemic role of induction is by destroying its inferential nature and reducing it to empirical verification.

It is also common to characterise abductive inferences as weaker than deductions, because a deduction is supposed to prove that something *must be* the case, whereas abductions merely suggest that something *may be* the case (Peirce, CP 5.171; Hanson, 1981, p. 86). The reasoning is that abductions, unlike deductions, suggest a hypothesis that is reasonable to pursue, but can go horribly wrong. In this view, the creation of hypotheses involves merely tentative conjectures we adopt 'upon probation' (CP 7.239, 1901), but even a mere hypothesis is only accepted because it is believed to explain the facts and we think it is true. Moreover, when one conceives a hypothesis by abduction it is assumed that it would explain the facts as particular cases of a general rule. So, the hypothesis is necessary to ensure the truth of propositions about the data, because it is a direct consequence of the propositions about the data. Thus, the hypothesis can be properly understood as deductive consequence of the phenomena it intends to explain. The other puzzling aspect is that real life deductions also require insights and intuition, so it is not clear how they can be distinct from abductions. There is no reason to posit the use of abductive inferences in deductions besides the erroneous notion that deductions should be tautological inferences. Besides, even if we conceded that there is a use of adductive inferences in mathematics, we will have to agree that they are apodictic in nature, which means that they are intuitions about something that *must be the case*, thus contradicting the conventional view of abductive inferences as weaker than deductive ones.

According to the conventional view, hypothesis creation has nothing to do with hypothesis selection, since abductive inferences posit hypotheses that require further inductive justification before they can be properly accepted. Even Hanson, an influential proponent of the epistemological significance of the context of discovery, stated that only inductive inferences can establish hypotheses (Hanson, 1958, p. 1079). But this view quickly leads to numerous difficulties. The attempt to distinguish abductive considerations from justificatory ones means that in an abduction we can only conclude that a hypothesis is plausible, not probable. However, we think that a hypothesis is plausibly true precisely because we create hypotheses that we conceive as being likely to be true. Since explanatory merit is the main criterion of hypothesis creation, the latter cannot be isolated from epistemic justification. It is also intrinsic to the concept of inference that it is advanced when the reasoner thinks that the passage from premise to conclusion is justified. Abductions are inferences. So, an inference from facts to a hypothesis can only take place if it is believed to be warranted. Therefore, the reasoner assumes that hypothesis should follow given the facts. That is, the hypothesis must be adopted for the sake of explaining the facts. Otherwise, there would be no need to posit the hypothesis in the first place. Besides, the verification of a hypothesis is not theoretically neutral. Indeed, in many cases the hypothesis that are conceived to explain the phenomena are made so that they will agree with the initial data, which severely limits the verification attempts. Rutherford's model of the atom was conceived to explain the results of the experiments on alpha particle scattering conducted by Rutherford, Geiger, and Marsden. But it was also verified by the same experiments. Finally, hypothesis creation for the sake of explanation and hypothesis selection on the basis of experimental evidence are not qualitatively different, because they are both attempts to ensure coherence.

Curiously enough, the contemporary notion of abduction is reminiscent of the original Aristotelian view of induction that rivalled with the enumerative understanding of induction since Late Antiquity. The notion of induction comes from the work of Aristotle, which credited Socrates as the pioneer of inductive inferences when he looked for universal definitions of moral concepts by considering their essential nature in different individual acts (*Metaphysics*, 1078b27-32). Aristotle viewed induction as an attempt to intellectually grasp the essential traits that determine the nature of something (McCaskey, 2006; 2007; 2014; Biondi, 2004; Biondi & Groarke, 2014; Rijk, 2002; Groarke, 2009). But Aristotle was wrongly interpreted as endorsing the enumerative view because the only example of inductive conclusion in Aristotle's *Prior Analytics* II.23 is a superficial generalisation about bileless animals that doesn't reflect most sophisticated hypotheses in science. Neoplatonists such as Clement of Alexandria argued in the second century that induction obtains force by completely enumerating components, because a definition (supposedly an inductive inference) is a sum (McCaskey, 2006, pp. 82-84). In this view, ideally, the enumeration should be complete, because that's the only way to ensure that the inductive conclusion has the certainty of a deduction. But this reading ignores Aristotle's view of induction in his remaining works, especially *Posterior Analytics* and *Topics*. According to Aristotle, the certainty of inductions is not ensured by the size of the sample, but by an intuitive skill that allows one to grasp the essential traits of the phenomena found in nature — we will discuss this topic in more detail in the next section.

Despite the influence of the enumerative view, it was the Socratic view that inspired the scientific revolution and most of the important developments of modern science. This is attested by the position of two of the main thinkers of the new science. Francis Bacon was dismissive of enumerative induction, which he described as 'childish' (*Novum Organum*, I.Io5). Something along the lines of Socratic view of induction was also embraced by William Whewell in *Philosophy of the Inductive Sciences* (1847). Whewell saw as the main goal of his books to 'bring the people to the right way of viewing induction', showing how there is a 'discoverer's induction' that is used to find out scientific laws, instead of a notion of 'induction by mere enumeration', which 'can hardly be called induction'. Whewell objects that most generalisations of the form 'All humans are mortal' as a mere juxtapositions of particular cases. Particular cases can form a general truth, but 'not by merely enumerated and added together, but by being seen in a new light' (Whewell, 1989, pp. 169–70). Whewell also offered more refined conceptual distinctions that bypassed some of the problems of the enumerative view. According to Whewell, the fact that inductive inferences start with a universal is expected and explained by a mental operation of colligation that brings together empirical facts by 'superinducing' upon them a conception which unites them under a general law (Whewell, 1847, II, 46). This inference can be creative, because 'there is a New Element added to the combination (of instances) by the very act of thought by which they were combined' (Whewell, 1847, II, 48). It requires intuition because the facts 'are not only brought together, but seen in a new point of view. A new mental Element is superinduced' (Whewell, 1858, p. 71). What is curious is that the most recent necessitarian and essentialist solutions to the problem of induction (Dretske, 1977; Tooley, 1977; Armstrong 1983; Ellis, 1998) are unnecessary if we adopt the Socratic view of induction[9].

If abduction is identified as the inference solely responsible for the process of discovery, then both deductions and inductions will have to be understood as uninsightful, but this seems implausible in face of the realities of each knowledge area. On the other hand, if deductions and inductions are interpreted as processes of discovery alongside abductions, not only we don't need to postulate abduction as a specific type of inference, as it becomes increasingly harder to distinguish deductions from inductions. Maybe we should look at the historical origins of these distinctions to see if we can find a clue that will allows us to surpass these difficulties.

4. WHERE IT ALL STARTED

The current distinction between deduction, induction and abduction has a long and complicated history that was heavily influenced by competing views about the nature of logic, knowledge and science. This distinction changed with time, but it started in ancient Greece with Aristotle's *Prior Analytics*, or at the very least it was heavily influenced by it. According to Aristotle, every inference is composed by two premises and a conclusion. Every premise affirms or denies a single predicate of a single subject. The subject and predicate terms of the different premises are connected by a middle term that appears once in each premise. This combination of terms implies the conclusion. The subject of the conclusion is the minor term and predicate of the conclusion is the major term. The major premise contains the major term and the minor

[9] The ease with which Socratic induction was associated with abduction and Whewell's colligation can be criticised, but the association is not without merit. In a Socratic induction the essential traits that determine the nature of something, or the cause of an event, are intuited, while in an abduction a surprising fact is explained by inferring the condition necessary for its occurrence, and in Whewell's colligation the reasoner unifies facts together in a new way. The three notions are about intuitive mental operations that occur when one is coming up with a scientific explanation. If the essential traits of something (or the cause of an event) are necessary conditions for its occurrence and unify its instances, then the three mental operations are one and the same.

premise contains the minor term. The deduction consists of the inference of a conclusion from a major premiss and a minor premiss (*An.Pr.* II, 23, 68b, 30-35).

In Aristotle's logic, every inference is a deduction (*sullogismos*), in which the conclusion results of necessity from the assumption of the premises (*An.Pr.* I.2, 24b18-20). The conclusion results of necessity from the premises when it is impossible for the conclusion to be false when the premises are true. It is what we currently understand by logical consequence. But Aristotle doesn't use syllogism as an inferential form that can be valid or invalid, since e stipulates that every deduction is valid. The paradigmatic example of deduction presented by Aristotle is the following inference:

> All bileless animals are long-lived;
> All men, horses, mules, and so forth, are bileless animals;
> Therefore, all men, horses, mules, and so forth, are long-lived.
> (*An.Pr.* II. 23. 68b31-35)

If we interpret 'bilelessness' by 'M', 'longevity' by 'P' and 'men, horses, mules, and so forth' by 'S', we have the following logical form:

> Major Premise: All M are P.
> Minor Premise: All S are M.
> Conclusion: Therefore, all S are P.

It's clear that a deduction in this sense is tautological, since the conclusion is already contained in the premises, and certain, since deduction is an inference from premisses of which we are certain. The other interesting point is that both the premises and the conclusion are about biological matters, a subject it is not currently associated with deduction.

Now, an induction (*epagōgē*) according to Aristotle would be the following syllogism:

> Major Premise: All men, horses, mules, and so forth, are long-lived.
> Minor Premise: All men, horses, mules, and so forth, are bileless animals.
> Conclusion: All bileless animals are long-lived.
> (*An.Pr.* II. 23. 68b31-35)

If we adopt the same interpretation from the previous example, we have the following logical form:

> Major Premise: All S are P.
> Minor Premise: All S are M.
> Conclusion: All M are P.

Aristotle's example of inductive inference relies on an old and incorrect theory that animals had four bodily fluids or 'humours' (black bile, yellow bile, phlegm, and blood) that affect their personality, behaviour, and health. Black bile was supposed to cause diseases. The inductive inference then is that because some bileless animals are long lived, all bileless animals are long lived, i.e., that bilelessness itself is a cause of longevity. This inference is apparently invalid, but Aristotle claims it is indeed valid. His reasoning is that the terms in the second premise are convertible because they refer to the same nature. After the conversion of the second premise, the original inference becomes:

> Major Premise: All men, horses, mules, and so forth, are long-lived;

Minor Premise: All bileless animals are men, horses, mules, and so forth;
Conclusion: All bileless animals are long-lived.

And the logical form with the minor converted premise is:

Major Premise: All S are P.
Minor Premise: All M are S.
Conclusion: All M are P.

It was this example that motivated the enumerative view of induction because the natural reading of this passage is that the conversion can only be justified by a complete enumeration (actual or presumed) of all observed particulars. Aristotle thought different. His reasoning is that the conversion is justified because there is a property that is essentially attached to individual observed members of a kind. But induction here is nothing more than the property of transitivity after the conversion of the second premise. It is worth mentioning that the choice of a superficial biological example is also prone to an enumerative reading. The attribution of a property (being long lived) to species that are bileless is a scientific hypothesis that it is almost mundane and taxonomic in nature. Aristotle was a biologist after all. But this type of scientific hypothesis is certainly not a good representative of science as whole, because it doesn't postulate deep structures and it doesn't require much creativity.

Finally, we have abduction (*apagōgē*) (*An.Pr.* II. 25. 69a20-85). Aristotle presents two examples of abductions. In the first example the major premise is certain, but the minor premise is uncertain. In the second example, the minor premise is also uncertain, but can be asserted with additional middle terms. While abductions are not demonstrative inferences, they take us closer to knowledge because the conclusion is at least as credible as the minor premise. The conclusion of an abductive inference is not certain, but it is plausible. The first example is:

Major Premise: Knowledge is teachable.
Minor Premise: Justice is knowledge.
Conclusion: Justice is teachable.

According to Aristotle, the first premise is evident, but not the second, so we cannot deductively demonstrate the conclusion. The second example concerns the problem of squaring the circle, i.e., constructing a square by means of straightedge and compass, having the same area as a given circle:

Major Premise: Any rectilinear figure is capable of being squared.
Minor Premise: The circle is a rectilinear figure.
Conclusion: The circle is capable of being squared.

Aristotle wrongly assumed that the minor premise could be proved by means of an additional middle term, 'lunes'. A lune is a crescent-shaped figure formed on a sphere or plane by two arcs intersecting at two points. This is a reference to Hippocrates attempt of squaring the circle according to which the sum of lunes can be equal to a rectilinear figure and also equal to a circle. This is impossible. There is no square with the same area as the circle.

But notice that just like deduction and induction, abduction in an Aristotelian sense has no resemblance whatsoever to the way we currently understand the term. The prevalent notion of abduction came from Peirce, who stated that abduction could be found in Aristotle's *Prior Analytics* II.25 (CP 1.65; CP 2.730; CP 2.776; CP 5.144; CP 7.249). But Peirce's exegetics is dubious, at best (Flórez, 2014, pp. 268-270). In order to make Aristotle's passages fit his interpretation, he replaced the minor premise with the conclusion in each example. The first example is reformulated as follows:

> Major Premise: Knowledge is teachable.
> Minor Premise: Justice is teachable
> Conclusion: Justice is knowledge.

His rationale for such extravagant interpretation is that it was an undeniable fact that virtue can be taught (CP 7.251) and that the conclusion was an abductive explanation of this fact. But knowing whether virtue can be taught is far from trivial. Plato presented several examples of virtuous men that failed to educated their sons in virtue (*Meno,* 94e2; *Protagoras*, 320b4–5). Indeed, whether virtue can be taught is the main topic of Meno.

Peirce modifies the second example by replacing 'capable of being squared' with 'equal to a sum of lunes'. This reinterpretation includes the term that was implicit in the syllogism, 'lunes':

> Major Premise: Any rectilinear figure is equal to a sum of lunes.
> Minor Premise: The circle is equal to a sum of lunes.
> Conclusion: The circle is a rectilinear figure.

Again, according to Peirce, the minor premise is a fact and the conclusion is the abductive explanation of this fact. But just as in the first example, the minor premise is controversial. The proposition 'the circle is equal to a sum of lunes' comes from Hippocrates' geometrical experiments that were controversial. Peirce himself observed 'that it was only two or three special lunes that Hippocrates had squared' (CP 7.251). So, it is clear that Peirce had no textual evidence to suggest that abduction in the way we understand the term can be found in Aristotle's text.

Interestingly enough, it can be defended that the main example of deduction discussed by Aristotle is not a genuine inference. Instead, it is the expression of a property of logical consequence, namely, the transitivity of entailment. The premises and the conclusion can be interpreted as inferences in their own right. Take the first premise. It can be plausibly interpreted as individual inference as follows: the statement 'All bileless animals are long-lived' is the claim that from any individual bileless animal it can be inferred that it is long-lived. If this and the second inference are valid, so is the third. Thus, Aristotle's paradigmatic example of deduction is actually a metalogical principle. Since both induction and abduction are variants of deduction, neither one of them can be properly described as a type of inference. Aristotle was unintentionally identifying a metalogical property of an inferential system.

The closest thing to an inference in Aristotle's corpus was the aforementioned Socratic view of induction discussed in works such as *Topics* and *Posterior Analytics*. Aristotle's examples of inferences in these works are about first principles obtained by induction. They are not empirical generalisations (Milton, 1987, p. 52-53). These include the principle that whatever is posterior in the order of development is prior in the order of nature (*On the Parts of Animals*, 646a3o); the principle that contrariety is the greatest difference (*Metaphysics*, 10055a6); the principle that excellence is the best position, state or capacity of anything that has some employment or function (*Eudemian Ethics*, 1219ai). Another example of induction is presented in *Topics* (105a15-7): 'if the skilled pilot is the best pilot and the skilled charioteer is the best charioteer, then in general the skilled man is the best in any particular sphere'. Notice that none of these examples fit in an enumerative reading of induction.

Induction in this sense is an intuitive process that starts in the sense perception of particulars, though the familiarity with particular cases, etc., and end up in a general understanding of an entire species or genus, which is presented in a universal proposition or concept. In this view, induction is as an intuitive expression of immediate understanding and plays a significant role in Aristotle's theory of science, because 'it will be intuition that apprehends the primary premisses' and 'will be the originative source of scientific knowledge.' (*An. Post.* II 18, 100b8ff; *Metaphysics*. A1). It's also by intuition that we obtain concepts, laws of logic, empirical necessities and even the concepts of different virtues (*Nicomachean Ethics*, VI.6.1141a7). According to Aristotle, there is a cognitive ability involved in inductions and attempts to identify the cause of an event, *anchinoia*, which can be translated as 'acumen', 'quick wit', 'sagacity' or 'discernment' (Groarke, 2009, p. 142). It is an intuitive skill that allows one to grasp the essence or nature of something. Aristotle presents the following example of this skill in causal reasoning:

> [*Anchinoia*] would be exemplified by a man who saw that the moon has her bright side always turned towards the sun, and quickly grasped the cause of this, namely that she borrows her light from him, or observed somebody in conversation with a man of wealth and divined that he was borrowing money (*An.Post*. I, 34, 89b, 10-15).

The idea is that some people have 'a talent for hitting upon the middle term in an imperceptible time'. By 'middle term' Aristotle means the essence or nature of something. So, there is a talent associated with the creativity associated with inductions. It is also a risky inferential process, since the essential account of what it is to be a certain kind of thing must be assumed (*An.Post*. I 10, 76a31–6; I 2, 72a14–24). Notice that this type of inference is precisely what we understand by abduction. So, the original Socratic understanding of induction is equivalent, or at least very similar to the current notion of abduction. Given that the two other examples of putative inferences are simply instances of transitivity, we only have one type of inference in the Aristotelian corpus: intuitive inductions, or, to put it simply, abductions. But what can fulfil the role of deductions? Or better yet: what is a deduction?

5. COHERENCE REQUIREMENTS

Aristotle's examples of deduction express the transitivity of entailment. The premises and the conclusion can be interpreted as inferences in their own right. Thus, if the premises are valid, so is the conclusion. This explains why one would be led to accept a mechanical view of deduction as a mere tautological exercise. If deductive inferences are better exemplified by figures of syllogism in which the conclusion is explicitly contained in the premises, then its inference will be a mechanical and uninformative exercise. Richard Whately (1826, p. 223) expressed this notion early on when he observed that 'since in a syllogism the premises do virtually assert the conclusions, it follows at once, that no new truth can be elicited by any process of reasoning'. But since the processes of reasoning cannot be reduced to a metalogical principle of transitivity, this view is unjustified.

The same explanation holds for the current paradigmatic examples of deduction. Consider a *modus ponens.* The conditional is usually interpreted as a premise because of a criticism advanced by Quine (1961, p. 323). He argued that the attempt to interpret conditionals as inferences is a use-mention fallacy, in which the antecedent and consequent are mentioned as the premise and conclusion of an implication relation. Instead, insisted Quine, genuine conditionals do not mention statements, but use them to express a relation between facts and objects in the world. Quine's view is baseless though. When a conditional is asserted, it's the whole proposition that is asserted and not its propositional constituents. The assertion of a conditional is made as a statement about a relation between the propositions expressed by the antecedent and consequent. In other words, the antecedent and consequent are mentioned, not used. The speaker is stating that the consequent follows from the antecedent. So, the interpretation of conditionals as inferences is still the correct one.

But this means that patterns such as *modus ponens* are not really inferential forms, but principles that every inference must satisfy. The same holds for hypothetical syllogism, *modus tollens* and other known patterns. These principles are coherence requirements. Consider again a *modus ponens*. If you think that B is inferable from A, and A happens to be true, you need to accept that B is true as well, otherwise we would have a contradiction. This interpretation enables us to explain the scandal of deduction: these principles are not inferential forms, but coherence requirements about inferential commitments, which in the case of *modus ponens* is a conditional. Thus, the conclusion seems to be contained in the premises, but only in the sense that it simply reinstates a consequence of an initial inferential commitment. But since they are coherence requirements, to interpret them as inferences is a category mistake.

If valid inferential forms are coherence requirements, so is the conventional view of validity. Consequently, the instances of vacuous validity should be interpreted as coherence requirements as well. Consider the vacuous validity due to contradictory premises. The hypothetical acceptance of two contradictory propositions would mean the acceptance of an incoherence. It would entail that any belief whatsoever can be inferred, since coherence requirements were abandoned. The notion that a necessary proposition is implied by any proposition can be explained as follows: suppose a given proposition, let's say, A, could be accepted as true, yet a necessary truth with different propositions, say, $B \vee \neg B$, turn out to be false. This would be an incoherence, because A can only have an established truth value if any other proposition, say, B, has an established truth value as well. But then $B \vee \neg B$ must be true as well, since B will be either true or false.

Coherence doesn't come in different degrees. An inference is either coherent or not. It can't be more or less coherent. The use of probability by itself doesn't change this basic fact. If we accept that the premises make the conclusion highly probable, we should accept that the conclusion is probable to avoid an incoherence. The addition of probability considerations to the mix only means that the premises necessitate a conclusion that involves degrees of belief in a proposition. What matters is not whether some conclusions are more or less probable than others, but whether the credence in a conclusion follows from the credence in the premises in a coherent manner. Moreover, as Groarke (2009, p. 104) correctly points out, it is a category mistake to suggest that the inference that led us to a conclusion is uncertain because the conclusion is uncertain. By relying on probability calculus I can accept that a conclusion has a 50 percent chance of being true, but this doesn't mean that this inference itself has 50 percent chance of being true. Since certainty is associated with beliefs with probability 1, and most beliefs will have an intermediary value between 0 and 1, all knowledge will be portrayed as uncertain. But the assumption itself that a belief has an intermediary value given the circumstances specified in the application of the calculus is 1, and not an intermediary value.

Since validity patterns are coherence requirements, there are no inductive inferences as an inference type that doesn't aim coherence. On the contrary, it is arguable that the traditional putative examples of inductive inferences aim coherence, because the calculus of probabilities is a system of rules about coherent probability attributions. For example, Bayes' theorem presents constraints on permissible combinations of degrees of belief. However, this shouldn't be viewed as attempt to force every inference into a formal straitjacket, especially considering that these coherence requirements do not work as a validity criteria as it is usually understood — validity is simply coherence, and this comes cheap. It certainly doesn't involve an approach which tries to reinterpret strong inductive inferences as valid deductive inferences in disguise, because that would still assume that coherence requirements are inferences.

The supposed patterns of validity are actually coherence requirements about inferential commitments. Thus, the notion that an inference is valid when the truth of the premises necessitates the conclusion should be reinterpreted as a claim about inferential consistency, namely, that the acceptance of an inferential commitment necessitates certain conclusions. But the fact that these are simply coherence requirements raises the question: can genuine inferences be valid in the conventional sense? It is arguable that inferences that achieve their epistemic goals are ones whose premises necessitate the conclusion. Otherwise, any reasoner would be justified to ignore *modus ponens* and similar principles and reason incoherently. But it is also clear that the aimed range of necessitation will vary according to the subject. For example, inferences in mathematics will range over all logically possible worlds, whereas inferences in physics can aim only the physically possible worlds. But the validity of a given inference can't be assessed by pure formal means, since it is determined by its explanatory merit.

It is also arguable that coherence requirements are a central element of explanations, justification and science as a whole. Harman (1986, p. 72) correctly observed that 'if all explanations were deductive, the coherence-giving quality of explanation might be derived from the coherence-giving quality of implication'. That is indeed what happens, but Harman shunned this conclusion because of probabilistic considerations we already observed to be deductive-friendly. If explanations weren't deductive the conclusion wouldn't follow from the premises and the inference wouldn't be coherent. Others even denied the notion that logical consistency can be considered a form of coherence, since it can be easily satisfied by sets of trivial and unrelated statements (Bartelborth, 1999, pp. 210-11). This thinking is motivated by considering logical consistency in abstract and dissociated from any meaningful inferential goal, but logical coherence (consistency) can't be isolated from epistemic coherence (explanation) for the simple reason that epistemic coherence is established by our inferential commitments, which are ultimately ensured to be coherent by rules of logical consistency.

Since any inference is an act of epistemic justification, our patterns of epistemic justification mirror the properties of logical consequence. They are the only properties that are truth-preserving, after all. It follows that coherence cannot be a matter of degree, since inferential relations will be either truth-preserving or not. Ideally, a theory, knowledge area and, ultimately, a belief system, should aspire to be a unified coherent structure[10]. For example, the occurrence of an anomaly or strange phenomena that are not properly understood in a knowledge area should be interpreted as a theoretical gap that need to be explained as a consequence of a new theory, or at least an improved version of an already accepted theory.

There is also something to be said about the importance of intuition. As it was previously mentioned, according to the Socratic view of induction there is an intuitive skill of acumen involved in inductions that resemble what is currently described as abductions. The relevance of intuition was also observed in the need to establish the cut-off point in any enumerative induction or while making assumptions about the properties of a population. This is not a coincidence. It seems undeniable that the intellectual powers of epistemic agents are integral to scientific activity. Even Hans Reichenbach, who made the distinction between context of

[10] The notion that coherence is intrinsic to our understanding of scientific explanation is not new (Cf. Whewell 1847; Feigl, 1970; BonJour, 1985; Harman, 1986, ch. 7; Thagard, 1989; Bartelborth, 1999; Schurz, 1999; Minnameier, 2004) and has also been associated with a search for systematic unification in which a wide variety of phenomena is explained by the same theory (cf. Friedman 1974; Kitcher 1981; 1989; Schurz 1988; Schurz & Lambert 1994; Bartelborth, 1996), or at least seems to be implicitly suggested by considerations of unification (Cf. Hempel 1966, p. 83; Lipton 1991, pp. 56–68).

discovery and context of justification, conceded that cognitive ability relies on an intuition of sorts:

> There seems to exist something like an instinct for the hidden intentions of nature, and whoever possesses this instinct, takes the spade to the right place where gold is hidden, and thus arrives at deep scientific insights. It must be said that Einstein possesses this instinct to the highest degree (Reichenbach, 1942, p. 94).

Unsurprisingly, Einstein himself espoused the importance of intuition for scientific achievements by saying that 'All great achievements of science must start from intuitive knowledge'. Michael Polanyi (1958) also made an interesting case for the importance of intuition in science. He was particularly critic of a view of science that emphasises bureaucratic factors in an attempt to convey an image of impersonal reliability to the general public, thus reducing science to formulae and procedures. These trivial features, insisted Polanyi, could never be a proper substitute for the role of individual intellectual powers. Polanyi (1966, p. 88) also insisted that the mark of scientific talent goes beyond the ability to sense patterns that most people ignore, because it also involves the ability to sense what problems are worth pursuing in the first place. Perhaps more importantly, he associated this psychological sensitivity to yet undiscovered things with coherence: 'It is a skill for guessing with a reasonable chance of guessing right; a skill guided by an innate sensibility to coherence' (Polanyi, 1966, p. 89). He also observed that 'We can pursue scientific discovery without knowing what we are looking for, because the gradient of deepening coherence tells us where to start and which way to turn, and eventually brings us to the point where we may stop and claim a discovery' (Polanyi, 1966, p. 88). If we accept Polanyi's description, the deductive character of inferences determined by coherence requirements should not be antagonistic to intuition.

Predictions and experiments are also coherence tests. A hypothesis includes all attributes of a fact, including its relations, so it must be consistent with whatever facts that follow from it (McCaskey, 2014, p. 174). The theories have to cohere with the data (Bartelborth, 1999, p. 209), which is to say that if a hypothesis has an empirical consequence that can't be detected by experiment, it can be abandoned for the sake of coherence. Naturally, hypotheses can still be defended from a failure of empirical verification with additional auxiliary hypotheses, provided that they are illuminating enough and there are no interesting alternatives. In fact, some hypotheses that are now perceived as ad hoc were interpreted in the past as promising strategies to find new truths. Leverrier explained how Uranus' orbit was not a counter-example to Newtonian physics by postulating the existence on a new planet, Neptune. Naturally, he expected to adopt the same strategy six years later in order to explain the perihelion of Mercury with the postulation of a new unseen planet, Vulcan. But this was a mistake, because Vulcan doesn't exist (Hanson, 1960, p. 186). The attempts to save a paradigm and explain away anomalies with ad hoc hypotheses are episodes of creativity. Consequently, there is no meaningful difference between ad hoc hypotheses and rational inferences since they are both attempts to ensure coherence. The attempts to save theories that are now abandoned are labelled as ad hoc in hindsight, but only because assumptions implicit in the previous inference are now discarded. Ad hoc manoeuvres are failed attempts to ensure coherence.

New attempts to ensure coherence can also be inconsistent with a previous coherence schema. In such circumstances, the attempt to ensure consistence by creating inconsistence elsewhere in a knowledge area is motivated by a bold bet that this new theory will be better than the alternative when fully developed. This bet is undaunted because it usually implies that an entire area needs to be reformed to fit into this new hypothesis. There are numerous examples of coherence bets in the history of physics, where crucial theories were developed towards a more satisfactory, but as yet unknown and incomplete view. Thomas Young revived the wave theory of light in 1802 with his paper ‘On the Theory of Light and Colours’. This theory was inconsistent with the then dominant Newtonian particle theory of light, which was widely supported by experiments. Young’s wave theory provides a way to explain some observations about the behaviour of light, including his double slit experiments, which show that light interacts like a wave when passed through two spaced slits. It was a coherence bet because Young assumed that his explanation would have the last word against a theory that had extensive empirical support. In 1913, Bohr published three papers where he develops the first quantum theory of the atom. In order to explain the atomic phenomena, Bohr assumed that radiation is not emitted or absorbed in a continuous way, but only when the atomic system passes from one stationary state to another. This was inconsistent with the widely accepted classical electrodynamics, but it was a much-needed bet to ensure a coherent understanding of the atomic system. In those cases, a local incoherence, such as an unexpected fact or anomaly, is explained by a surprising hypothesis that produces new decoherence elsewhere in our belief systems. This trade-off is acceptable if there is an expectancy that this new hypothesis will pay off and that this new inconsistency will be eventually reintegrated in a higher conceptual change that at the present moment remains out of reach. These new pockets of incoherence are accepted as long as the new hypothesis is more promising or illuminating than the previous decohering threats. Inconsistencies with an accepted theory suggest the alternative of a new theory that intends to uncover an even deeper coherence that lies elsewhere.

The centrality of coherence can also explain the appeal of an Euclidian view of science, in the sense of an attempt to ensure the global coherence of a scientific system. As Braithwaite writes:

> A scientific system consists of a set of hypotheses which form a deductive system … arranged in such a way that from some of the hypotheses as premises all the other hypotheses logically follow… The establishment of a system as a set of true propositions depends upon the establishment of its lowest-level hypotheses … (Braithwaite, 1953, pp. 12-13).

These axiomatic systems are indeed important, but only as the coherence requirements of a completed research report. Kepler’s laws of planetary motion can be explained as being deducible from Newton’s laws of motion and universal gravitation. These axiomatic systems, however, don’t represent the research itself. These orthodox approaches, as Feigl (1970, §19) correctly pointed out, might help us clarify the logical structure of scientific theories, but they are reconstructions made in hindsight and have little to do with the work of a creative scientist.

The present explanation is deductivist, but couldn’t be more different from Hempel’s and other traditional deductivist approaches. According to Hempel’s model, the proposition that describes the phenomenon is explained when it is deductively derived from premises involving the law in question and some specific conditions. But in the present deductivist hypothesis, the law is inferred as a necessary condition from the phenomenon precisely because the phenomenon was perceived as a sufficient evidence to postulate the law in the first place. The following relevant derivations will result after the initial inferential explanatory commitment was made. Alternatively, the current view that a theory of explanation should encompass only relevant explanations should be also taken with a grain of salt. The reason is that a theory of explanation associated with inferences should leave some room for trivial inferences and, thus, trivial explanations. Let’s consider a simple inference such as simplification. We can say that a conjunct, say, *A*, is a necessary evidence for *A&B*, which happens to be a sufficient evidence for *A*. This is a coherence requirement and superficial explanation in the sense that it shows the truth conditions of a simple conjunction, but it is an explanation nevertheless. To be more specific, a successful theory of explanation should recognize and explain the difference between trivial and relevant explanations.

Now, since every inference aims to be coherent, every inference should be monotonic. How is that possible? The monotonicity of classical logic is a coherence requirement in the sense that any accepted inference should be robust given new information. This goes against the prevalent notion that most inferences are non-monotonic, since many of the past inferences were abandoned by their proponents after new discoveries. The best answer to this is that no one advances an inference assuming that it is going to be abandoned. Instead, one argues for an inference given the expectation that will be resilient upon new discoveries. It would be an incoherence otherwise. If I think I know p, I can't sincerely question my belief that p is resilient to new information because this would result in infelicitous statements such as 'I know p, but I might be wrong about p' or 'I know p is false, but p still might be true'. If the inference doesn't meet this requirement, it is going to be abandoned. This means that motonocity, properly understood, is not only compatible with the retraction of past inferences given new information, but it is a requirement for it. If the inference is not information resistant, it should be abandoned because it should be robust. Otherwise, an inference would be turn out to be inadequate upon new findings, but we should stick with it despite being defective.

That monotonicity is implicit in our inferences can be illustrated as follows: let's assume that my belief in B can be justified based on the evidence of A, so $A \rightarrow B$. But I was told that inferences are non-monotonic, so I accept that my initial belief in $A \rightarrow B$ is compatible with $(A\&C) \rightarrow B$ being false. But in order for that to happen I would have to accept that my initial inference, $A \rightarrow B$, can be maintained simultaneously with its defeat by the new finding, C. The only way to avoid this incoherence is to defend that $A \rightarrow B$ implies $(A\&C) \rightarrow B$. If the inference turn out to be unsustainable when it is reinforced by a proposition about new findings, it is because the initial inference that implies it was incorrect all along.

It is conceptually intrinsic to the notion of inference that it should be perceived as resilient until it isn't. That a bit of evidence must be certain for us doesn't imply that it can't be challenged or abandoned later on (Minnameier 2004, p. 89). Notwithstanding the fallibility of epistemic agents, this non-monotonic phenomenological aspect of inferences suggests that the deductivist view of knowledge as certain has some merit. The reliance on probability considerations shouldn't be a hindrance. Let's say I accept that a conclusion has a 60 percent chance of being true. I'm 100 percent certain that it has a 60 percent chance of being true. Now imagine you insist that I can only be 60 percent confident that the conclusion has a 60 percent chance of being true. I could question how you obtained this figure. It seems you would have to rely on another probability distribution that is 100 percent reliable. How else would you be able to ascertain such precise number? So, in order for probability distributions to work you need to be certain in your inferences. In point of fact, despite probability calculus being traditionally regarded as an inductive logic, it is deductive, since the probability of the conclusion is deduced from the probabilities of the premises (Peirce, CP 2.267-268; CP 7.207).

The present emphasis on coherence also explains the importance of William Whewell's criterion of consilience. According to Whewell, a theory proves to be plausible not by explaining what it was designed to explain, but by explaining what it was not meant to explain in its inception. This will include evidence that was not present in the original formulation of the theory. Prima facie, consilience considerations are distinct from coherence aims, because the latter appears to favour the explanatory power of a hypothesis given the data available during its conception, whereas the first points towards a post-discovery empirical support (Whewell, 1860, p. 191). But consilience not only is not independent, as it can be seen as consequence of a coherence requirement. What we expect from a successful hypothesis is that is going to be susceptible to causal unification, either as a smaller component of a vast theory or as a fundamental principle. The assumptions should fit together, especially as new hypothesis is added when new phenomena are discovered. The point it is not simply that a hypothesis should be able to explain something that couldn't be considered during its conception, but that it can point towards the coherence of unification, because knowledge 'is like a building in which the addition of higher floors helps strengthen the lower levels' (Foster, 2009, p. 94). For example, Kepler's planetary laws are further supported when they turn out to be consequences of Newtonian physics, especially universal gravitation law. This is why discoveries are similar to recoveries of past anomalies because they are atemporal coherence requirements. Einstein's 1915 recovery of the anomalous motion of Mercury is a postdiction of a known fact. The difference between prediction and postdiction should be negligible from a logical point of view. The systematic integration of hypotheses within a wider theoretical frame characterised by consilience is presented in Whewell's tables of induction, which have a distinctive Euclidian aspect. The tables provide a summary that indicate how the lower-level hypotheses can be deducted from higher-level hypotheses. But the deductive subsumption of lower-level hypotheses under more fundamental laws presented in the Inductive Table is not a mere 'schedule of accounts' that are 'enumerated and added together', because they are 'seen in a new light' (Whewell, 1989, pp. 169-170). The broader perspective aimed by taxonomies of different kinds should also work for science as a whole.

The aforementioned coherence requirements have an implicit concept of inference that can be summarised as follows: if an inference is valid, the premise is sufficient evidence for the conclusion and the conclusion is necessary evidence for the premise, in the sense that it must be inferred necessarily from its truth. This definition might seem too strong, but any attempt to deny it will lead to contradictions. Let's suppose, per impossible, that A is sufficient for B, but B is not necessary evidence for A. This implies that we could have a circumstance where B should be inferred from A, while $\neg B$ could also be inferred from A, since B is not necessary given A. Thus, both B and $\neg B$ could be inferred from A, which is absurd. Now suppose that B is necessary evidence for A, but A is not sufficient evidence for B. If that were possible, B would be required for A to be true, but at the same time it wouldn't be required for A to be true, since A wouldn't be enough to infer that B is true, which is an incoherence. Thus, the notion of evidence is intrinsically tied to the notion of inference, which is ultimately a logical matter.

This relation between converse evidentiary claims and inferential coherence can also be illustrated as follows: let's assume that A is sufficient evidence for B, but B is not necessary evidence for A. This implies that from the joint truth of $A \rightarrow B$ and A, we can't infer B by *modus ponens*, and implies that from the joint truth of $A \rightarrow B$ and $\neg B$, we can't infer $\neg A$ by *modus tollens*. Ergo, in order to preserve both *modus ponens* and *modus tollens* we have to admit that sufficiency and necessity evidentiary claims are converse. Another way to understand this dependency is as follows: suppose that B is necessary evidence for A. So, if B is not true, A is not true, i.e., $\neg B \rightarrow \neg A$. But if A is true, it follows by *modus tollens* that B is true as well. So, if B is necessary evidence for A, it follows that A will be sufficient for B as well. Finally, consider the use of reductios. They rely on the fact that when x (an impossibility) is a sufficient evidence for y (a contradiction), then x must be false, since y is a necessary condition for x.

I won't go in details about which modal system needs to be assumed. It suffices to say that the modal range in which these sufficiency and necessity evidentiary relations are considered will be determined by the proponent of the implication and her beliefs on the subject. The implication is valid when the truth of the premises necessitates the truth of the conclusion relatively to a certain range that is aimed by the inference (whether we are talking about a logical, epistemic, physical or metaphysical necessity, for example).

We can sum up what has being discussed until now as follows: putative valid inferential forms are actually coherence requirements that suggest some evidentiary relations between the premise and the conclusion; the enumerative view of induction results from an erroneous reading of Aristotle's text, whose notion of induction are intuitive attempts to extract the essence of something; the current notion of abduction resembles Aristotle's notion of induction in that they are intuitive inferences associated with explanatory considerations; inductive and abductive inferences can't have non-deductive properties because that would make them incoherent. Perhaps we could take a step back and look again at the traditional notions to see if we can find more connections using the conventional wisdom on the subject before moving forward. Let's start with abductions, which can be interpreted as follows:

Abduction
A B is a necessary evidence for A ——————————— B

The interpretation is that we have a fact A that needs to be explained, and B is a hypothesis that is necessary for the truth of A. Now, let's consider a deduction:

Deduction
A A is a sufficient evidence for B ——————————— B

It starts with a simple proposition about a fact, say, A; followed by an inferential claim in the second premise, A is sufficient evidence to B, which leads inexorably to the conclusion, B Similarly to abductions, deductions have an inferential claim and a premise about a fact. According to the conventional wisdom, the only difference between deductions and abductions is that only in a deduction the joint truth of both premises ensures the truth of the conclusion. However, given the evidentiary relations suggested before, this simply can't be true. The truth of both premises in an abduction also guarantee the truth of the conclusion. In an abductive inference, B is a necessary evidence for A, which means that if B weren't true, A wouldn't be true, so $\neg B \rightarrow \neg A$. From $\neg B \rightarrow \neg A$ we can infer that $A \rightarrow B$ by contraposition and double negation. Now, suppose that A is known to be true. It follows that B is true, by *modus ponens*[11]. So from the assumption that B is necessary evidence for A, and the truth of A, it follows that B is true, deductively. But this is precisely what we should expect if evidentiary claims were converse. If A is sufficient evidence for B, B is necessary evidence for A, and vice-versa. So, if a deduction is valid, so is the corresponding abduction, and vice-versa; because deduction and abduction are two sides of the same inference.

This also allows us to make another connection of abductive inferences with transcendental inferences, which are supposed to proceed from a fact to the necessary conditions of its possibility. They also have the same logical form of abductive inferences because they are particular cases of it, namely, when the necessary evidence for x is also perceived as a necessary condition for x. See the image bellow:

[11] Another way to interpret this is that given the relevant coherence considerations used by the reasoner, if the hypothesis B weren't true, then the fact that makes A true would not occur, so $\neg B \rightarrow \neg A$. But given that A is indeed true, we can infer that B is true by *modus tollens*.

Transcendental Inferences
A B is a necessary condition for A ——— B

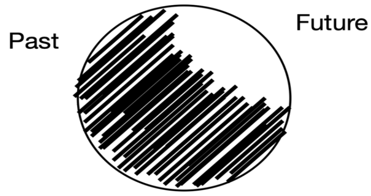
The relation between deduction and transcendental inferences is also evidenced by the fact that some putative examples of deductions are clearly transcendental. Consider the following: 'Philip Glass is a composer. Therefore, there are composers.' The conclusion is inferred from the premise by a transcendental inference. Or think about Euclidian geometry, which presents geometrical truths in an axiomatic fashion. Euclid's *Elements* are an obvious attempt to extract the necessary preconditions of known geometrical truths by transcendental inferences. The Euclidian axioms were handpicked in order to ensure the validity of known theorems at the time. So, the relation is that if B is inferred as a necessary precondition of A, it is a necessary evidence for A. But by the principle that evidentiary claims are converse, this means that A should be sufficient evidence for B, which implies that B will be a consequence of A. Inversely, if B is not a consequence of A, it cannot be a necessary precondition of A. In other words, the satisfaction of the standards for good transcendental inferences conceptually implies the satisfaction of the standards for deductive validity. The hypothesis is an inferential consequence of the facts, precisely when its truth is a necessary condition for the facts. The necessary condition inferred from the facts is a missing part that makes the phenomena whole and ensures coherence.

In a Socratic induction the essential traits that determine the nature of something, or the causes of an event, are intuited, while in an abduction a surprising fact is explained by inferring the condition necessary for its occurrence. Consequently, Socratic induction can be subsumed as the abductive part of any inference. The evidentiary concerns present in the context of discovery can be described as follows: the conclusion is intuitively extracted from known facts by an insight because it is regarded as necessary evidence for the premise, which is the phenomenon that is supposed to explain. The creativity then lies in imagining what evidence is necessary in order to explain the facts. The reasoning then is that it is not likely that the evidence would obtain were the world such that it was governed by another hypothesis or theory. The necessary evidence can be a necessary condition (discovery), but also a necessary effect of a hypothesis (prediction). This explains why the significant consequences of a known theory can be far from obvious. There is a need to extract all the necessary evidence of a hypothesis with imagination. It's not a mere mechanical exercise. That's why the notion of induction was also described as an attempt to ensure coherence:

> An inductive problem is an intimation of coherence among hitherto uncomprehended particulars and the problem is genuine to the extent to which this intimation is true. Such a surmise vaguely anticipates the evidence which will support it and guides the mind engrossed by it to the discovery of this evidence (Polanyi, 1961, pp. 465–466).

Ludwig Wittgenstein defended something similar when he stated that '[t]he process of induction is the process of assuming the simplest law that can be made to harmonize with our experience' (1921, 6.363). In the same vein, the problem of justifying induction can also be reinterpreted as matter of ensuring coherence. One usual way to view the problem is about how you can justify an inference about the future made on a limited number of past observations:

But from a logical point of view, it makes no difference whether the phenomena occur in the past, the present or the future. Instead, the problem should be viewed as an attempt to ensure coherence—see the image bellow:



If the properties of the members of the group are homogeneous, the empty space will be filled with the same properties.

It's also important to observe that the validity or invalidity of each inference is dependent on the matters at hand. It depends on knowing whether the premise is sufficient evidence for the conclusion, or whether the conclusion is necessary evidence for the premise. For example, the only circumstance where an abduction fails to preserve truth is one where the conditional premise is vacuously true due to the truth of the consequent alone, i.e., a circumstance where *B* is not a necessary evidence of *A*. In other words, whether the premises of genuine inferences are truth preserving is a question that should not be determined by formal logic.

6. THE INCOHERENCE OF INFERENTIAL SCEPTICISM

There is still the question of how to deal with inferential scepticism, which has traditionally been one of the hurdles of the conventional wisdom. First, I will revisit Hume's attacks to causality because they can undermine induction indirectly, even if they are not criticisms of induction per se. Then I will reinterpret and address sceptical challenges to deduction, induction and abduction based on the criticism of these notions offered in this paper. Finally, I consider potential sceptical challenges to the inferential theory defended in this article.

Let's start with Hume's scepticism. According to Hume, propositions are either about matters of fact or relations of ideas. Propositions about matters of fact are contingent and are justified exclusively by experience. Relations of ideas are necessary and justifiable only by thought. The proposition that snow is white is an example of a proposition about a matter of fact, while Fermat's last Theorem is a proposition about a relation of ideas. The challenge presented by Hume is that our beliefs about causality are propositions about matters of fact, but can't be justified by experience. We believe that there is a necessary connection between causes and effects, but experience only provide us constant conjunction. It's the habit of seeing constant conjunction of events that impel us to postulate a necessary connection between them, but this postulation is irrational. One natural reaction is to try to justify our beliefs about causality with statistical analysis and probability theory, but this is circular reasoning since both presuppose a uniformity in nature that can only be discovered through causality. So, we can't justify our causal beliefs with statistical analysis and probability theory.

When we think of Hume's scepticism, all the focus is placed on the proponents of causality and their burden of proof, while Hume's hypothesis is presented as a mere epistemological challenge devoid of theoretical commitments. But it is arguable that this sceptical hypothesis relies on the same causal beliefs it intends to displace. Hume's empiricism is based on the notion that all beliefs about matters of fact are originated from our sense impressions, but our ideas can only be originated from sense impressions *if they are caused by them*. So, we can't make sense of his empiricist doctrine without assuming causality. Hume also relies on causality when he explains the mental habits that lead to a belief in causality because the belief in causal connection *is caused* by the constant conjunction of events. So, without the assumption of causality Hume can't begin to explain why we accepted the notion of causality in the first place.

This dependence on causality will also present itself in any sceptical scenario where our causal beliefs are challenged. Suppose that overnight we notice some important differences, say, that when two billiard balls with different velocities undergo a head-on collision they don't ricochet, but stop. In this scenario, should we conclude that our trust in causality was misguided all along? Of course not. This would only mean that we had a superficial understanding of mechanics and that a new theory is needed. Or imagine a worse scenario where there is no discernible order in the external world and nature seems completely chaotic. Even in such doomsday scenario we would have to assume that this chaos has a cause and will have future effects. The notion of causality is more fundamental than any attempt to displace it[12].

Let's move on to inductive scepticism. It was argued that (1) enumerative induction is better explained as a form of evidence gathering; (2) Socratic induction is abduction by another name; (3) transcendental inferences are a particular case of abductive inferences; (4) and these 'types' of reasoning are present in every inference in the sense that the conclusion is perceived by rational intuition and insight as necessary evidence for the premises. So inductive scepticism can be reinterpreted as the inference that we are never entitled to assume that the conclusion is a necessary evidence for the premises. But this inference is self-defeating because the conclusion of the sceptical inference is not necessary to accept its premises, thus violating a basic coherence requirement. Another way to interpret inductive scepticism is as the thesis that we cannot capture the nature of something by intuitive reasoning because this would require a mysterious faculty of rational intuition. But this assumes that we can rationally 'see' what is the problem involved in rational insights. In sum, it relies on the same faculty that it should avoid. So, it is incoherent as well.

Deductive scepticism will face a similar fate. In its traditional formulation, it is already prone to accusations of inconsistence, for in order to draw sceptical conclusions from its premises, the sceptic needs to rely on a deductive inference. In the present interpretation, the contradictory nature of this scepticism is even more evident since deductive principles are not nothing more than coherence requirements. So, it stands to reason that any attempt to deny

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[12] It is also worth mentioning that Hume assumes that the belief in causality is motivated by the repeated observation of a conjunction of subsequent and contiguous events, but there is empirical evidence to suggest that relations of causality are perceived directly as an experiential gestalt (Lakoff & Johnsson, 1980). A gestalt consists in the perception of a complex of properties as a whole in a way that is more basic to our experience than their individual occurrences. According to this hypothesis, the perception of causation is an experiential gestalt which starts to be constructed at a very early age. For example, a child discovers by gestalt that different objects resist to different degrees (Andersson, 1986, p. 157). This suggest that our perceptual system is disposed to organise individual facts into a causal chain, all at once. What is curious is that we can also interpret gestalts as coherence procedures. For example, in closure gestalts that involve incomplete objects, people perceive the whole by filling the missing information. Even more surprising, we can interpret gestalts as abductions, both creative or uncreative. In a creative abduction we can put facts together into something new, whereas in an uncreative abduction the facts are organised according to a known pattern. But since abductions are nothing more than taking a proposition as a necessary evidence for another, gestalts could be explained by this same rationale. Pursuing this route in detail, however, is a task for another occasion.

coherence requirements will be incoherent itself. Finally, we need to consider the sceptical stance about the notion of inference defended in this article. The sceptic will deny that if the premises are sufficient for the conclusion, the conclusion should be necessary evidence for the premises. But this is also incoherent because if it is to be accepted, even if the sceptical premises were sufficient evidence for the conclusion, we wouldn't have to accept the conclusion because it wouldn't be necessary evidence for the premises.

7. CONCLUDING REMARKS

A pragmatist might argue: 'Why should we care?' Mathematicians will still be able to make proofs, biologists will go on doing experiments and making models, and philosophers will continue analysing concepts and presenting distinctions. The conventional view of inferences doesn't seem to matter in the grand scheme of things where predictions are made and academics carry on doing their theoretical work. The reason why we shouldn't be indifferent to such concerns is that the conventional view of inferences presents a view of how the world works. This view will influence what problems will be studied, how data will be interpreted and how knowledge and science will be perceived in the wider culture. It has deeper significance for our self-understanding, but it is failing miserably in this task and it is making us stranger to ourselves as a result. It generates perplexity, puzzles and incompatibility where there should be order, clarity and empirical fit. It results from a superficial understanding of logic concepts, which leads to a superficial view of logic, knowledge and science. This matters a lot. The only way out of this prejudicial view is a crucial revision of the basic concepts. This is the only way to ensure coherence.

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