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ORIGINAL PAPER

The Science to Save Us from Philosophy of Science

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Abstract Are knowledge and belief pivotal in science, as contemporary epistemology and philosophy of science nearly universally take them to be? I defend the view that scientists are not primarily concerned with knowing and that the methods of arriving at scientific hypotheses, models and scenarios do not commit us having stable beliefs about them. Instead, what drives scientific discovery is ignorance that scientists can cleverly exploit. Not an absence or negation of knowledge, ignorance concerns fundamental uncertainty, and is brought out by retroductive (abductive) inferences, which are roughly characterised as reasoning from effects to causes. I argue that recent discoveries in sciences that coped with under-structured problem spaces testify the prevalence of retroductive logic in scientific discovery and its progress. This puts paid to the need of finding epistemic justification or confirmation to retroductive methodologies. A scientist, never frightened of unknown unknowns, strives to advance the forefront of uncertainty, not that of belief or knowledge. Far from rendering science irrational, I conclude that catering well for the right conditions in which to cultivate ignorance is a key to how fertile retroductive inferences (true guesses) arise.

Keywords Retroduction · Russell · Peirce · Scientific discovery · Guessing · Fundamental uncertainty · Ethics of science

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"Ignorance is light, knowledge is darkness"—Anonymous.
I have always, since early in the sixties, recognized three different types of reasoning, viz.: First, ***Deduction*** *which depends on our confidence in our ability to analyze the meanings of the signs in or by which we think; second,* ***Induction****, which depends upon our confidence that a run of one kind of experience will not be changed or cease without some indication before it ceases; and third,* ***Retroduction****, or Hypothetic Inference, which depends on our hope, sooner or later, to guess at the conditions under which a given kind of phenomenon will present itself (Peirce to F. A. Woods, MS L 477, 1913).*

1 Introduction

Bertrand Russell famously asked: "Will scientific advance continue to grow more and more rapid, or will it reach a maximum speed and then begin to slow down?" (Russell 1950).

What is the status of Russell's question today, sixty-odd years later? In our technological day and age, when sciences do seem to be advancing at an accelerating pace, are we in a better position to answer such a question? For one thing, look at the proliferation in what could be termed the sciences of the masses: Crowd-sourcing, Citizen Science, Science 2.0, and the political rhetoric allied with them. There are many intellectual activities that do not require specific talent, let alone genius. The science of the masses does not automatically mean scientific advance. In fact the contrary may be the case, if Ortega y Gasset's dark prophecies on the scientist a mediocre man, the learned ignoramus, a concoction of excessive specialization, become a reality. But how about the discovery and invention of something genuinely new? Russell is right that the real progress of science depends on its continuing success to attract the best minds. But does some global crowd-sourcing project make that happen? What would a downpour of inferior minds do to the standards of science? Will the attraction of science vanish in the cascades of sham reasoning and dubious data while unemployed scholars spend their days in montaging Wikipedia articles? Or will the ease of accessibility to data, however bogus, bring potential minds to bear on genuine problems increasingly more?

One approach to Russell's question may be to ask how the prospects of discovery may be affected by the current situation in science, especially by our seemingly adverse social, economic and political conditions to practice that precious method hardly three centuries old. But before we can address this socio-economic question, we need to understand better the nature of scientific discovery. For now, thus, let us not bother ourselves about Russell's question but rather look into what real scientific discovery consists of. I feel that the nature of discovery is of utmost need of clarification, given the number of philosophers of science who have taken questions of discovery to be just another hobbyhorse of non-mainstream philosophers of science, insignificant to the pursuits of real epistemic value. After we have come clean about the nature of discovery Russell's question may raise its topical head again.

There is a sense in which philosophy of science has become *desperate*. Questions of discovery and creative invention have been relegated, without much critical reflection, to the compartment thought to be separate from the activities of rational man—activities that see the rational reconstruction and definition of notions of knowledge, belief revision, and their reliability and justification, as the philosophical priority and constitutive of the rationality that drives the sciences.

This separation gets little evidential support from the actual sciences, however. It is only the dominance of epistemology—a hybrid of sceptical, Cartesian, Humean or Bayesian epistemologies—that led masses of philosophers of science to believe that the phenomenon of real discovery is fundamentally hazy, mystical, or beyond the reach of rational and logical analysis.

This anxiety represents the same personality type as one of the leading characteristics of contemporary analytic philosophy in general, its transmogrification into exegetic debates about what others have or have not said about someone else, rather than being a pursuit of new questions and approximate answers, right or wrong. The emphasis on the formal, be it in the sense of the proliferation of yet another formal calculi for some isolated epistemological concerns, or the obsolete belief that the goal of science is to search for universal laws or to explicate why things turn out to be in the way they do necessarily (and note how the standard textbook analytic philosophy of science[1] may resemble here the kind of physics-envy characteristic of the standard textbook economics) may have missed the opportunities to investigate what the changes in the meaning and conceptual comprehension of familiar terms have been. Instead, the emphasis has been on elucidating epistemic notions in systems of rules of inference that aim at, say, defining yet another stable belief closure or at securing the deductive validities of the inference in question. Yet discovery is never meant to be secure. But its bearings, if successful, may well supersede anything that the mere avoidance of mistakes can accomplish. The myth of the formal, the myth of science as a body of knowledge, or the myth that good research necessitates a close contact between provisional ideas, scenarios and hypotheses and scientists' degrees of beliefs concerning them, are still conspicuously present in the mainstream accounts and handbooks of philosophy of science.

But if you look at the situation in the late nineteenth century philosophy, the picture changes. There you will find the question of the origins of beliefs to be pressing, while their updates and revisions attracting only a secondary concern. The logic of science has to begin with the logic of discovery, it was notified, for how else could you account for the rapid developments and innovations characteristic of that revolutionary time in science and technology? Belief was interpreted as an achievement, not as a foundation or propositional attitude. Our beliefs come to be fixed, temporarily, only after the inquiry has reached sufficiently deep into its further stages. Charles Peirce went as far as to assert that "Belief has no place in science" (CP 1.634, 1898).

The pre-1914 optimism led to see the task of philosophy to be in paving the way for how something may be possible, or in the very least not impossible. Inductivism,

[1] By standard textbook (or non-post-autistic) philosophy of science (STePS) I mean any account that begins introducing the topic with an outline of the problem of induction (say, the relevant chapter in the *Blackwell Companion to Philosophy* or *What is this Thing Called Science,* just to give two prominent exemplaries).

already outmoded, did not bother about the sceptical problem, nor its heir of Bayesianism, neither of serious interest in the progressive field of philosophy of science. Rapid development of sciences made the 'why-necessary' questions secondary: those are to be determined after the fact, say by looking for some mechanisms, typically causal or explanatory mechanisms. Pragmatism was a reaction against necessitarianism; yet the latter still haunts us under various formalist, naturalist, physicalist, Quinist guises. A pragmatistic philosophy of science, in contrast, proposes a profound conceptual analysis into the changes in the meanings of central concepts in sciences. And that analysis is to be established along the course of the investigation.

Asking the questions of how something *could be* possible, or *not altogether impossible*, is as a matter of fact still the way how brilliant advances take place in the real, living world of passionate scientists. That earth rocks may have travelled to the moons of Jupiter is a recent discovery with magnificent repercussions. Whether some of them did we know not, but there is a positive appeal to the notion that a few of them could have. Even further, back in time when stars were closer to one another, life could have spread from one solar system to another inside such rocks, as astronomers have just calculated to be possible.

Another way of making a related point is to urge philosophers of science to change their epistemic priorities from what knowledge and beliefs one already possesses as those factors affecting decisions and actions, into taking action itself constitutive of believing and knowing. Maybe it is that we do not know first and later act on the basis of that knowledge; rather, action precedes knowing. Or, in somewhat milder terms, the two co-evolve: the search for information improves our decision making while knowledge is made possible by our actions. Knowledge and belief seem more like what gives the basis for formulating *conditional expectations* of what might plausibly be the case in the contexts of scientific assertions and decision making. The hypotheses that may be formed as a consequence are hardly describable as those things with which our propositional or cognitive attitudes must be tied in. Knowledge and belief have a similar function in scientific inquiry as in decision making: when on a firmer ground, we might start grounding our further decisions on them. The difference is that scientists may have to wait for twenty generations before acting on their hypotheses. Hypotheses are entities that evolve in deep time. Successful hypotheses induce beliefs only in the long future.

Supplanting knowledge and belief in inquiry with ignorance, doubt, action and hope was a natural move for classical pragmatists. Peirce took critical inquiry to be that which enables us to hold "*power* to ascertain future conditional truths as to future experience". In order to do this we must begin with *guessing* that which we *subsequently* may come to believe. Peirce noted that many of our guesses may be wrong, but "some one will, if inquiry is sufficiently persevered, be right" (MS 905, 1908). Peirce's own guess, if made somewhat tongue-in-cheek, was that 1 out of 256 guesses appears to be right in the sciences. But that is enough. It still far exceeds random luck. There is a thin role for belief, let alone knowledge, at this critical point of living inquiry.

But what is guessing, and what are these special powers that the scientific inquirers are claimed to possess?

2 What is Retroduction?

And this is where my story really begins. If epistemological questions in science largely can dispense with notions of knowledge and belief, something else takes their place. I argue that the logic of real discovery, or *abduction*, or what Peirce later termed *retroduction*, refers to a global phenomenon in the sciences the nature of which is to be solved, not evaded or segregated to some off-mainstream corners of contemporary philosophy of science *cum* epistemology. I would go as far as to claim that, bearing in mind Russell's questions, the future progress of science stands or falls upon the capacity of scientific minds to maintain an instinctive understanding of the nature of retroductive reasoning. And this understanding also extends to those in charge of the sciences' well-being; people responsible for securing that the right conditions are met in which scientific work and discoveries can best flourish.

To understand what retroduction means let us observe the upshot of the following, unpublished passage in which Peirce describes it from a fresh point of view:

> By Retroduction I mean that kind of reasoning by which, upon finding ourselves confronted by a state of things that, taken by itself, seems almost or quite incomprehensible, or extremely complicated if not very irregular, or at least surprising, we are led to suppose that perhaps there is, in fact, another definite state of things, because, though we do not perceive any unequivocal evidence of it, nor even of a part of it (or independently of such evidence if it does exist), we yet perceive that *this supposed state of things would shed a light of reason upon the state of facts with which we are confronted, rendering it comprehensible, likely (if not certain), or comparatively simple and natural.* (MSS 856, 1911, added emphasis)

This characterisation, which Peirce wrote very late in his life, is in fact much better than what we find in the published and typically quoted schematics from the 1903 Harvard Lectures. That standard or classical account gives abduction the following well-known logical form: "The surprising fact, C, is observed. But if A were true, C would be a matter of course. Hence, there is reason to suspect that A is true" (CP 5.189). The reasons why the 1903 schema does not carry out its duties in full may be summarised as follows:

1. The meaning of the term "surprising" is not an ordinary meaning of that term: A falling apple need not be unsurprising, but comparing it to the Earth's moon is.
2. The meaning of the conditional major premiss does not derive from the semantics of an indicative but from an understanding of the meaning of the subjunctive conditional.
3. The reason as to how the major premiss can emerge has to do with the nature of abductive reasoning itself.
4. A's and C's may be mistaken for singular propositions, while what they really mean are complex facts (the Cs) and some other, conceivable and alternative

states of things, the As, which are the states of things in which laws, correlations, functions and dependencies can hold.

5. The 1903 schema assumes that there is a truth-assignment ("reason to suspect that A is true") imparted on the confronted things, C, in the consequent of the major abductive premises suggested by the truth of its antecedent. But the assumption of an assignment of truth is in Peirce's later formulations such as above replaced by qualities such as comprehension, likelihood, simplicity and naturality of hypothesis.
6. The schematics might, misleadingly, suggest the interpretation that the essence of the abductive reasoning is that of the 'inference to the best explanation'.

I will not pause to examine these points here in any length with the exception of a comment on the last one. It is important to realise that retroductive processes do not coincide with inferences to the best explanation. The reason is that only the knowledge of the truth of the retroductive hypothesis could ground the status of the hypothesis as an explanation, and even then only as an explanation of some further knowledge concerning future conditional expectations, not as an explanation of the already established, objective facts. Retroduction is not intended to offer simultaneous explanations of why certain facts should hold (say, that in abducing the standard model we would explain the existence of the Higgs particle), because most of the relevant facts are totally unknown to the reasoner at the time (Higgs' theory predicted the existence of the particle in the 1960s, likely to have been found in 2012). At best retroduction can explain, Peirce observes, why we ought to arrive at the statements of conditional expectations as knowledge-engendering ones when those statements concern real possibilities, including future facts and future states of things (say, what *would* be the case if the standard model of particle physics were to be true).

How in Peirce's view retroduction is connected to knowledge and to what was mentioned above about the role of epistemology in inquiry is thus eminently important:

> In *all* cases a *knowledge* that the retroductive hypothesis was actually true would suffice to cause a knowledge that the original experience would be (or would probably be) such as they actually were. It thus, at any rate, "explains", if not the objective (or in my language, the "real") facts, at least how the *knowledge* of them would be produced by a knowledge of the truth of the hypothesis. (MS 905)

This noteworthy passage testifies Peirce's recognition of the fact that, in the case of real discovery of hypotheses that turn out to be true, knowledge may be the product of inquiry. Knowledge neither marks its beginning point nor its final destination. A good retroductively inferred hypothesis need not explain any actual fact. But if the hypothesis is in fact true, then the facts it talks about can be known by reasoning. In a sense perhaps not too far-fetched, knowledge could be said to be an unintended side-product of scientific activities.

Why are we led to trust in retroductive kind of reasoning at all? In the all-important MS 905 Peirce argues in favour of a number of points by virtue of which retroduction is to be trusted, despite the fact that the level of security of this reasoning and guessing faculty is low:

1. *Argument from Power* A scientist, as mentioned, "holds some *power to ascertain future conditional truths* as to future experience. But in order to do this man must begin with guessing that which he subsequently comes to believe. Many of his guesses may be wrong, but some one will, if inquiry is sufficiently persevered, be right."
2. *Argument from Affinity* The scientist "would practically never guess right if there were not some *decided affinity* between his thoughts and the three worlds of things, that of spirits, and that of ideas, of which he has experience, and to which his thoughts refer."
3. *Argument from Co-evolution* His "*mind has been developed* under the influence of the general tendencies at which he guesses."
4. *Argument from Naturality* Finally, in relation to discovery in physics, Peirce states that the "guess appeared to them to be the simplest, that is to say, *the most facile*, because the most natural [of] man's geometrical thought has been developed under the influence of visual and muscular experiences, the former of which rendered the path of light and the latter lines of undisturbed tension the easiest lines to conceive" (MS 905).

With respect to the last point, it is worth noting that Peirce, quite rightly, does not take the principle of least action to be a universal and immutable law but a derived concept of Newtonian physics. He even adds the strikingly modern qualification that, "as physics is pushed further and further from primitive experiences, a greater and greater number of hypotheses have to be tried before the essence of the different physical agencies is understood" (MS 905).

Aside from the above four arguments, there are also inductive justifications for retroductive reasoning. Those justifications derive from the history and development of science. To take an example here, one of Peirce's many cases concerns suggested explanatory conjectures for electricity as they were around at the turn of the twentieth century. He examined and listed them as candidates that demonstrate the actual workings of the logic of retroduction, including those that failed to deliver and so led into false conjectures about electricity. The following account was written in 1908 and Peirce added to it a prediction that "the prospect is that the nature of electricity will prove a much more difficult problem even than that of light did" (MS 905):

- "No. 1. Boltzmann gave a mechanical hypothesis to explain [electricity]; but it was so complicated and extremely unnatural that I think even its author only conceived of it as an analogy, and did not intend it to be understood as a hypothesis." [Peirce's comment refers to statistical mechanics and the statistical interpretation of thermodynamics, which Boltzmann presented during the 1870s and which consisted of over 2,000 pages of his publications over the years.]
- "No. 2. The last work of the brilliant Heinrich Hertz, who put the final touch to Maxwell's Theory of light by first exhibiting electrical waves in air, was a

volume expounding a hypothesis to explain, not only electricity but also instantaneous action at a distance by supposing occult connections between bodies, such that, when they appear to move under the action of forces whose centres are distant, they are really not moving under forces at all but only under constraints, somewhat as if they moved along slides." [The finding of Hertz's predated Einstein's discovery of the photoelectric effect in 1905, in the test that showed how electrodes illuminated with ultraviolet light create electric sparks more easily.]

- "No. 3. The mathematician Henri Poincaré despairing of explaining electricity, long ago proposed to say simply, that in substance, 'Electricity is a something of radically different nature from anything else, but governed by such and such laws'". [In 1900, Poincaré indeed re-discovered a relation between mass and electromagnetic energy, paving the way to Einstein's work on special relativity theory.]
- "No. 4. The theory of electrons, which was originated so long ago as 1873 by Dr. Johnstone Stoney may now be considered as proved. It is, as everybody knows, that a chemical atom consists of multitude of far smaller things called electrons moving in intricate orbits round a nucleus of ordinary matter, which however Fleming and others would, in some undefined way eliminate, supposing that there is no fundamental matter such as is ordinarily conceived." [Electron itself was discovered in 1897 by J. J. Thomson.]
- "No. 5. On the other hand that very original chemist Demetrius Mendeléef regarded electricity as a phenomenon connected with a hypothetical chemical element of vastly smaller atomic weight than hydrogen." [According to Kargon (1965), it was the "search for unity and simplicity that led Mendeleev to investigate the chemical properties of the ether, and to deny the existence of the electron and other sub-atomic particles." In a small book written in 1902 entitled "An Attempt Towards a Chemical Conception of the Ether", Mendeleev in fact casts some doubt upon the existence of "the, to me, vague hypothesis of electrons", unless such an hypothesis could be shown to derive from the principles of chemistry.]
- Peirce then also went on to note that "Maxwell showed that the so-called 'quantity of electricity' may be regarded as of the nature of the square root of the product of a mass M into a volume". [In 1873 James Clerk Maxwell had written equations that described the electromagnetic field, and predicted the existence of electromagnetic waves travelling with the speed of light.] Peirce continues that, "This certainly strongly supports Mendeléef's idea that electricity is of the same nature as matter, and that the electron is its atom." [Mendeleev didn't believe in the electron hypothesis if assumed to be independent from the notion of the ether and the chemical valency considerations. Finally, Rutherford measured the distribution of an electric charge in atoms in 1910 but that finding seems to have never arrived at Peirce's disposal.]

Endless examples from the history of science can be likewise adduced as an additional, inductive justification for the prevalence and the working of retroduction.

As these justifications are consistent with fallibilism, the conclusions that one can draw from such evidence in the history of science are therefore quite orthogonal to what often has been characterised as examples of ‘pessimistic meta-inductions’.

3 Examples of Retroduction in Contemporary Sciences of Discovery

I mention in this section a number of examples of retroductive reasoning across sciences, together with some of their characteristic features. My aim is neither to delve into the details of these examples nor to reconstruct any system of retroductive logic that one might be able to garner on the basis of actual cases of retroductive reasoning in the sciences in general (see Pietarinen 2013). A further point to be recognised is that the prevalence of retroduction strongly suggests that it is not only an unavoidable or indispensible mode of reasoning in science but also a pivotal factor in understanding the nature and prospects of scientific progress.

I will first marshal, in no particular order, a number of recent areas in the sciences, which span across the spheres of natural, human and social, in which retroduction is not only alive and well but that its widespread existence testifies the immense philosophical importance contained that mode of reasoning.

1. *Computational Sciences* Discovery Sciences, such as semantic data mining, unsupervised learning, and object and scenario discovery. Plan and intention recognition. Diagnostic and model-based reasoning with error-tolerant algorithms. Reverse engineering methods in software and AI systems. Penetration testing and vulnerability checking of IT systems.
2. *Engineering Design* The tasks involving conversions of functions into structures (especially in situations in which the context of the design is not invariant, such as those when designers recognise that the criteria of the success of design may change during the process or even be part of the process).
3. *Educational* Learning by Doing.
4. *Open Formal Systems* Systemic perspectives that take into account contextual change. Statistical model selection tasks. Identity-through-change.
5. *Mathematics* Inverse problems, especially ill-posed inverse problems. Applications of inverse problem solving in image sciences, oceanography, astronomy etc. (Ill-conditioned and regularized inverse problems may be treated in a Bayesian fashion and are therefore examples of inductive rather than retroductive reasoning. Well-posed inverse problems, in turn, can be subsumed under deductive inferences.) Reverse mathematics (What is the minimal axiom system needed to prove a given theorem?). Exploiting mathematical and semi-mathematical examples that do not generalise well. Stumbling upon right kinds of glitches and contradictions that are met in unsuccessful proofs and various searches for ways to neutralise them.
6. *Earth Sciences and Astronomy* Geographical abduction in geoinformatics (e.g., crime prevention and crime analysis). Geophysical image interpretation (e.g.,

determining shapes of asteroids and other small objects, shape-from-shade etc.). Remote sensing and modelling in climate research.

7. *Linguistics* Discourse recognition. Gricean pragmatics (Speaker's meaning by guessing at the intentions of the speakers). Methodologies employed in historical linguistics and historical pragmatics, where intended meanings are reconstructed from sparse documents and testimonies, anecdotal data and recollections. Stemmatology. Forensic linguistics and forensic semiotics.
8. *Life Sciences* Medical diagnostics. Computerised tomography. Autopsies (but do not Google "autopsy and abduction", it won't turn up what you want here!). Forensic geosciences. The 'practice theory' of nursing and caring sciences. Brain decoding methods used in vision research in cognitive neurosciences.
9. *Economics and the Social Sciences*: Heterodox economics and game theory (where on finds recurrent appeals to notions such as focal points and salience, including forward induction arguments that attempt to 'sample the future' rather than predict from the past; clues and signals that are used in trying to solve social coordination problems). Futures Studies and scenario planning that have to deal with strongly under-structured problem spaces. The method of hypothetical retrospection. Public governance (e.g., the problems to do with how to draft general policy recommendations). Case studies in the social sciences, such as congruence analysis.

The list could continue for long; this is not the place to investigate in detail what the various retroductive elements may be that we encounter when looking into the practices and methodologies employed in these diverse fields. Just to take up on the last point, the case-study methodologies in the social sciences, let us elaborate the method of congruence analysis in use there a little. Congruence analysis is described in a recent *Encyclopedia of Case Study Research* as follows:

> Congruence analysis focuses on drawing inferences to the relevance of theories from the (non-) congruence of concrete observations with predictions deduced from these theories. In order to be able to draw inferences about the relevance of the theories, it is necessary that the researcher deduce predictions about what he or she can observe according to these theories. This is not, however, to state that congruence analysis starts with theory. The recommendation that the researcher should derive predictions about observations before the empirical work is conducted is justified only as a means to enhance reliability and objectivity. (Annamalai 2010, p. 210)

What else can congruence analysis be than an illustration of one type of application of abductive reasoning processes, where in the absence of sufficient empirical data the development of the theory and observational predictions recorded at the outset of the process have to co-evolve? After all the data has been analysed, some additional information, whatever the sources or their ultimate reliability may be where that information comes from as this stage of inquiry (such as interviews, background experience, collateral information, rumours), may necessitate some changes to be made to the models. In other words, conceptual development will take place in those models. But notice that these models merely concern what *might* be the case at the presence of a phenomenon. They are models about such relational phenomena which is taken and interpreted in the right and relevant contexts. They are models about conceivable cases stated in conditional forms. The case study models are not intended to reflect any individual units that could be treated as statistical random variables at the outset of the inquiry.[2]

One is likely to find such similar retroductive and co-evolving practices and reasoning methods across the sciences, human and natural. Together, their abundance testifies the prevalence of retroductive inferences in the practice of scientists even in those cases—which are likely to be the majority—in which such modes of inference are not consciously recognised to be such either by the actual investigators in these fields or by the philosophers and historians of science who have analysed the relevant cases.

There is one prevalent feature common in virtually all of these examples, which at the same time is something that to a large extent appears to characterise what the *quality of novelty* of a scientific finding consists of. That is, retroductive problem solving tends to operate in the context of *under-structured problem spaces*. Retroduction does not, at least not primarily, aim at finding out, or coming to know, or reaching certainty concerning the mechanisms or rules on the basis of which one could then trace the causal connections or make reliable predictions about the future outcomes of the experiments taking place in contexts non-identical with the antedating contexts. Retroductions cope particularly well with situations that are not clearly identifiable in terms of strict cause-effect relationships. They concern those conceivable but unanticipated and untouched areas the understanding of which involves more than collections of already established facts. If the retroductive inferences were to be based on facts only, the precious novelty of its conclusions would not be possible, since the relationship between what is conceivable and what are the known facts is something that is not established prior to drawing the retroductive conclusions.

Another way of putting a related point across is to observe that, taking retroduction only as a converse of deduction, or simply as reasoning from effects to causes, or from the major premiss and the conclusion to the minor premiss, is a limiting view of retroduction. The success of such limiting views depends on the success of the presuppositions of the systems under investigation: for example, whether they are well-behaved, such as deterministic or Markovian, or ergodic, time-reversible, well-posed, or well-conditioned. Only under such strong and separate assumptions it might be reasonable to suppose that the relevant inverse problems will be solved computationally or algorithmically. But in those cases

[2] One might also allude, as one of the reviewers points out, to treatises such as Strauss (2006), which aim at uncovering the very conditions that make experience possible in our various states of affairs with reality. One might also add that the approach promoted in that book makes the still-prevailing distinctions one encounters in the social sciences, namely those between individualism or atomism on the one hand, and holism or universalism on the other, to look not only outmoded but antithetical to the purpose of a comprehensive account of scientific reasoning that would work across the human and the natural.

retroduction would no longer be different from the demonstrative part of deductive reasoning.

But not nearly all systems share these stronger assumptions: maybe the future is not like the past, or maybe the solution will change in a complex manner with the changes in the data from which the model is to be retroductively inferred. A question may in fact be raised of the fundamental nature of reasoning concerning computationally or algorithmically solvable inverse problems here: those problems may in fact be examples of demonstrative and thus deductive rather than retroductive reasoning. Such is the case for example when the problems are well-posed in the sense that the relevant parameters or properties of the models are known. The question of whether some inverse problems such as the ill-posed ones are genuinely retroductive certainly calls for further investigation, especially since deduction is a complex stage of reasoning which contains its own retroductive moments (Pietarinen & Bellucci 2014); be this as it may for now, retroductions cannot be assumed to explain the facts that are unknown at the time theories come to be engendered. Retroductive reasoning seems to work particularly well under such fundamental uncertainty: when all we have are unknown unknowns concerning the problem space and situations highly sensitive to the context and initial conditions. Retroduction is that lowly stage of reasoning that loves such volatility and turbulence.

The presence of non-structured or severely under-structures problem spaces means that scientists are hard-pressed to find strategies that can cope with uncertainty in such fundamental sense. It is not that our beliefs may be prone to heavy revisions: we may not have the needed information to be able to formulate first beliefs concerning the status of possible facts yet at all. The best a scientist can do, faced with only a meagre amount of data but massive amount of doubt, is to do something like the following: to strive to get glimpses at some interesting phenomena; to collect samples concerning what a formulation of conditional expectations about future phenomena appears to suggest; to gain insights into what the further questions or questionnaires to be asked may be; to look for additional sources of information, including the statement or definition of the problem itself as a potential source of such new information; to revise the logical analysis of the key concepts involved in the statement or definition of the problem; to construct niches for the future facts to rest on which could be better scrutinised in the future and with better instruments. The goal of the fallible mind is not to arrive at the formulation of some maddening, big or ultimate questions; on the contrary, it is not uncommon that a scientist has to work with anecdotal data; one may have at hand only some mathematical examples that do not generalise well; there is only some case-based and context-dependent evidence that does not generalise well, as in the social sciences may often be; there is big but one-dimensional data; finally, one may even face the problems of model selection where the context is unstable and dynamically or even catastrophically shifting. All these factors call for new, humbling routes by which the cutting-edge research could shortcut into some smaller, ordinary questions; questions that can serve as workable premisses from which some sensible, or even testable, deductions may later come out.

The world of science, not nearly as neat as epistemologists have taken it to be, is richer in methods than the philosophers of science have believed it to be.

The prevalence of such severely under-structures problem spaces puts paid to wide uses of Bayesian methods to understand what goes on in the forefront of sciences. For those working at the frontiers have to encounter, first and foremost, fundamental uncertainty. Bayesianism concerns positive facts or propositions already known or believed, however preliminary. But novel, raw hypotheses that could predict, or even explain, some further phenomenon are seldom associated with probabilities. It is a hallmark of a well-thought-out scientific scenario that it may cease to be valid overnight. No axe to grind remains. But where would you get your priors when all that you have is some novel or anecdotal data? Even if you could compute inverse probabilities of causes from the transition probabilities between successive states given their effects, you need to bring in strong assumptions such as determinism and time-reversibility. Under fundamental uncertainty, however, the issues that scientists face concern the search for possible new sources of information. Here it is vital that scientists could succeed in formulating new but manageable questions which *could* be asked and which *could* serve as premisses in the series of inferences that aim at getting at some partial, tentative or, in some rare cases, conclusive answers to those questions. Oceanologists who have no background data on some massive basin strive to retrieve values from a highly limited number of observations leaving almost all of the underwater space unsurveyed: for the unsurveyed mass of water the connected parameters come to be assigned through repeated series of imaginative activity and informed guesses. But they have made a commitment. They trust—nay, they have faith in, uncertainty. But this is a scientific faith. It is a commitment to the continuous and relentless use of the method of exploiting uncertainty. For a scientist hopes that there will be some answers, though far from optimal, which are suggestive of further questions. At some point in this process, it is hoped, an experiment is suggested which, if its results conform to the hypothesis, counts as one measurement among many other possible and future measurements. And if its results do not—as is famously said—it counts as a *discovery*. In Peircean terms the latter is an invitation to a new and possibly improved abductive guess about the kinds of questions to be asked next.[3]

[3] In an unpublished draft letter to Victoria Welby (July 16, 1905, MS L 463), Peirce calls retroduction "reasoning from surprise to inquiry". He attributes a novel logical form, which is that of the Modus Tollens, to retroduction and in which the conclusion is put in the interrogative mood. The conclusion does not present a given hypothesis for contemplation; it merely suggests that it *would* be reasonable, given the overall goals and the context of investigation, to inquire whether a given hypothesis is true or not. The schema does not commit one to the truth of the hypothesis; it merely suggests the adoption of hypothesis to be reasonable in the sense of proposing specific strategic advantages to those who adopt it. That the logical form of retroductive reasoning has in Peirce's work as its conclusion a request for information vindicates Hintikka (2007), who proposed that abductive inferences aim at answering the inquirer's questions put to some definite source of information.

4 Guessing Right

Cleverly put questions are the ones that lead to or at least hint at new questions. Serious inquiry is in this sense fundamentally strategic. Peirce talks about retroduction as that mode of reasoning that is capable of suggesting hypothetical conclusions which are like actions that snooker players characterise by those that “may give a good ‘leave’” (EP 2:110, 1901). Though initially perhaps less consequential, they nevertheless point at a certain direction, or provide glimpses at something that may draw further attention. Since given fallibilism, most if not all of our ideas, models, hypotheses and ultimately theories, are liable of turning out ill (albeit only gradually, piecemeal and in the long run), the ones that are suggestive of new questions that replace the old ones are guessed first. Our guessing faculty does not so much engender hypotheses that are correct than those that are, in Peirce’s equally colourful terms, “gravid with young truth”[4]: those that may, when examined, analysed, reasoned about and suitably modified in the course of inquiry lead to something noteworthy in the future. Peirce’s term for this phenomenon was “the possible esperable *uberty*”, “value in productiveness”[5] of the scientist’s guesses at some novel viewpoints, scenarios or hypotheses. He did not simply mean the productiveness or fruitfulness of tentative hypotheses per se, since many of our ideas may be indefinitely productive or fruitful without being right. Uberty is the further property that, besides, has a degree of juvenile truth in such productiveness.

I am tempted to interpret this secretive term of uberty as the *connectedness* of the results of a guess with some other, more or less equally uncertain answers that a scientist is in the process of obtaining from her questionings. Now the adoption of a presumptive hypothesis is always “on probation” (Peirce to F. A. Woods, CP 8.385-388, 1913). Only substantially large chunks in the network of uncertainties in one’s field of investigation can be assigned significance or a definite outcome. A single guess, one definite-sounding answer, a one-off test or an isolated outcome of a measurement cannot be said to be more or less likely, as it cannot be assigned any positive probabilities. But if you cannot improve your probabilities, you can still try to improve your payoffs. Here the convexity effect kicks in with its bare and naked force, when inquiry is still in its early stages. At such early stages, guesses have to be made that are suggested by some collateral information gathered from the results of a number of related guesses at one’s disposal. As the results of the former guesses are not yet available at this immature stage, connectedness of the results of a guess with some other answers cannot yet be established with degrees of belief let alone be said to be known.

Moreover, without such seeds of truth planted in our guesses the existence of self-correcting mechanisms in the sciences might be in jeopardy. It would

[4] “Observations may be as *fruitful* as you will, but they cannot be said to be *gravid* with young truth in the sense in which reasoning may be, not because of the nature of the subject it considers, but because of the manner in which it is supported by the ratiocinative instinct” (EP 2: 472, *An Essay toward Improving Our Reasoning in Security and in Uberty*, 1913).

[5] “I think logicians should have two principal aims: 1st, to bring out the amount and kind of *security* (approach to certainty) of each kind of reasoning, and 2nd, to bring out the possible and esperable *uberty*, or value in productiveness, of each kind” (Peirce to F. A. Woods, 1913; CP 8.384, 1913).

nevertheless be quite absurd to claim, undetected frauds in science notwithstanding, that there were no such self-correcting mechanisms in place at various stages of the total complex of that living process we call scientific investigation.[6] By the total complex I mean the intricate manner in which the three stages of inquiry, abduction (retroduction), deduction and induction are interconnected, continuous and implanted into one another, as shown in (Pietarinen & Bellucci 2014). Even the necessary, deductive reasoning reposes on the phases of logical analysis, definition and hypostatic abstraction, all of which involve their own retroductive moments. And one justification among many others concerning retroduction is deduction. It is only under the strong but often unspoken and, as follows from Pietarinen & Bellucci (2014), in fact unjust assumption of the three stages of reasoning being three entirely *separate types* of reasoning, together with the excessive focus that has been laid on inductive inferences, that the criticism of the self-correction thesis such as that of Laudan (1981) could hold any merit. But the three stages are not independent, although they are autonomous.[7]

One further aspect of the "esperable uberty" is in its being a species of a special *economic* quality: uberous hypotheses are those that have expedited investigation by allowing scientists to rely on early guesses slightly more than later ones. By so doing an investigator might be able to open up lines of inquiry that otherwise would never have been seen or realised and thus would have been closed off at the outset. Here again, Peirce duly noted this feature of science that resists blocking the road of inquiry, when he argued from the history of science that it is, remarkably, the first or the second guess that turns out to be the right one (e.g., as follows from his analysis of Galileo's and Newton's discoveries). His remark might have been wittily exaggerated, and maybe the complexities of contemporary sciences speak somewhat against the optimism of that day and age, but maybe it is still on the average the early guesses that we could do well to hold on to. And the reason is not that scientists, somehow half-mysteriously, are able to fathom their early guesses to be more prone to be right in proportion to the later ones. Rather, the reason is that the useful degrees of freedom are preserved at those stages of investigation in which the connectedness of the results of a planned guess to the consequences of a number of related guesses has not yet reached the conscious or public levels of investigation.

5 Conclusions

In the light of the above observations, some issues and opportunities present themselves that I have not found philosophers of science taken a particularly considerate look at, even though such issues seem congenial to the understanding of the workings and practices of actual sciences. One might only surmise why; Peirce's

6 See Bakalis (2011) for a recent defence of the self-correcting thesis in relation to what came to be known as the chemical revolution.

7 This connectedness of the three stages of reasoning does not jeopardize the Autonomy Thesis, as it would be a misinterpretation of the Autonomy Thesis to state that the three stages of reasoning may not involve some elements of the other. What matters are the fine details in which deduction and induction enjoy their retroductive moments (Pietarinen & Bellucci 2014).

mature conceptions of retroduction and scientific reasoning are, after all, resolute attempts to articulate what he observed to be going on in the sciences and in the actual practices of scientists, including inspection of his own practices that lasted his entire life. Now it is true that we are now witnessing, a century later, the emergence of so-called practice-based approaches to philosophy of science. But I fear those to have been formed for the sake of yet another professional society whose motivation arises from distressed reactions to the various dead ends, on the one hand, of the epistemologically overloaded philosophy of science, skepticism and scientism, and of the constructionist and dogmatic fancies of the anti-realist interest groups on the other. What we may also need are some balanced pragmatistic and logical analyses of scientific practices and imaginative discovery.

I end with a note concerning scientific discovery from the point of view of research ethics. Right now, a collective hysteria has overtaken decision makers to direct the R&D funds to projects and areas evaluated primarily on the basis of them being useful, or perhaps reasonable, or at least agreeable. The criterion has been spelled out in terms of the *impact* of research proposals. Those scoring highest on the impact are the first ones to be passed on to further rounds of evaluation, which then may concern scientific quality, feasibility and implementation. And it is this mysterious impact that needs to be written down in your project proposal—preferably on one page if you address it to the EU bureaucrats *cum* venture capitalists lurking for the next big thing well past its 4th cycle of development. But all this represents a sham approach to science. Coercive impact is an outright offence to the scientific mind. Dirac could not have foreseen the invention the PET machine developed 40 years after his dual-electron theory, notwithstanding the fact that he rightly predicted the existence of the new positron particle.[8]

It may be useful to summon up three points. First, sharp distinctions between pure and applied sciences are moonshine: the nature of the reasoning and even the epistemological issues appear largely the same in both realms. Recall T. H. Huxley's famous though often misinterpreted remarks warning against the loose uses of the term "applied science" in his 1880 essay "Science and Culture", as well as the well-established fact that "the applied" has often been chronologically the first in the order of discovery,[9] making it extremely doubtful whether there really is such knowledge that could be taken from the 'pure' sciences and applied in the 'applied' sciences. Second, it is not in the nature of science to close the doors in advance from how something might be used later on. But this might happen if the 'high-risk-high-gain' mantras gain prominence. Brussels asks for the justification of the impact first, not caring a whit of the justification of the scientific. Good science is a derivative quality. Yet fundamental uncertainty and true innovations are not probabilistic in

[8] Dirac's delta-function is, incidentally, often used in tackling inverse problems.

[9] As well as Peirce, who echoes: "The investigator who does not stand aloof from all intent to make practical applications, will not only obstruct the advance of the pure science, but what is infinitely worse, he will endanger his own moral integrity and that of his readers" (EP 2: 29, 1898, *Philosophy and the Conduct of Life*). This remark communicates us how badly the true nature of pragmatism is still being understood; say, how the innocent reader of Menand's 2002 book *The Metaphysical Club* may have come to understand it.

their nature. Risk analysis ceases to apply in the realm of real discovery. There are no earnest contingency or feasibility plans at the frontiers of science, no matter how important they nowadays seem to be to science funders and evaluators. Should Dirac have been denied funding for failing to provide one? Third, what is thought to be useful at one time may well be judged bogus by the future generations, making the entire rhetoric of usefulness no better than what Oscar Wilde remarked about fashion: a form of ugliness forced to be changed every 6 months. The use-talk only generates an oscillating feeling of movement, not a real progress. True invention does not fix its degrees of freedom in advance, and one cannot fully and truthfully communicate those degrees at the proposal stage.

None of what was said above should surprise a working scientist. She knows that what solves a hard problem or a grand challenge is the right method, not the knowing agents (see here Pietarinen 2003, p. 37). At the same time, philosophers of science *cum* epistemology may find the overall thrust incredulous. But it is the philosophers who have taken the curiosity out of the sciences, thinking that discovery is the irrational, non-measurable, sundry, impressionistic, serendipitous component better to be discarded, or at all events peripheral to the understanding of how sciences theoretically, formally and epistemologically speaking work. What I have argued is that it is the contrary that is more likely the case. The curious and playful, unpredictable and uncertain, are the hallmarks of the living, rational, and investigative reason.

As a reply to Russell's question, then, we could borrow Yogi Berra's words: "the future ain't what it used to be." That is, the future progress in science is scarcely based on the predictions of its past.

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