

Is Falsification Falsifiable?

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Abstract This is a response to a claim by Sven Ove Hansson to the effect that Poppers dictum that falsification lies at the heart of all pursuit of science has once and for all been falsified by his study of articles published in *Nature* during the year 2000. We claim that this is based on a misunderstanding of Poppers philosophy of science interpreting it too literally, and that alternative readings of those papers are fully compliant with falsification. We scrutinize Hansson's arguments as well as giving an overview of Poppers falsification theory.

Keywords Falsification · Induction · Observation · Explanatory theories · Universal statements

1 Introduction

Popper is known as one of the most influential philosophers of science during the twentieth century, and if you want to characterize his philosophy of science in one word, the word to choose is **Falsification** (Persson 2014; Popper 2002a, p. 17). Poppers emphasis on falsification rather than verification, and with its implied rejection of induction, is often seen as startling, not to say counter-intuitive and perverse. Examples of such abound see e.g. (Dawkins 2003, p. 20)¹ or (Okasha 2002, p. 23).² More seriously Ernest Nagel claims that

¹ In which he confuses the falsification of a fact with that of a theory!

² Stating: *The weakness of Popper's argument is obvious. For scientists are not only interested in showing that certain theories are false...her arch-rival's theory perhaps...But much more likely she is trying to convince people that her own theory is true.* And goes on to claim that Popper purports to reject induction, which Okasha seems to confuse with verification.

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[Popper's] conception of the role of falsification in the use and development of theories is an oversimplification that is close to being a caricature of scientific procedure (Nagel 1979, p. 76).

Sven-Ove Hansson has undertaken a study to empirically confirm Nagel's view (Hansson 2006) in which he claims that the doctrine [sic] of falsification is indeed falsified. In short his argument rests on the fact that the only falsifiable statements are of the logical form $\forall x$ we have the statement $P(x)$ is true (universal statements, involving practically speaking an infinite number of x)³ while scientists in practice (as exemplified by 70 supposedly randomly chosen articles published in *Nature* in the year of 2000) do not avail themselves of such types of sentences, and indeed given their interests and the problems they address cannot even be expressed in this way. The tacit assumption is that science is a social enterprise and thus it is defined by its most successful and respected practitioners, and if their practice does not comply with that of Popper, so much the worse for Popper. However, it is not at all clear from the discussion, what sentences he has in mind. Does he refer to more complicated nestlings of quantifiers, which by their logical form neither admit verification nor falsification by finite beings? Or does he simply mean that no scientific laws are at issue, except those accepted and applied?

The fallacy of this approach is to take Popper too literally, something that is as we have seen commonly done, (even occasionally by Popper himself). Popper offers a criteria for scientific activity, historically to delineate it from theories with scientific pretensions, such as Marxism and Freudianism, fashionable during his youth.⁴ He does not offer a methodology nor a criteria for something being arrived at scientifically. In fact he indicates that this is impossible. Thus the statement that scientists in general do not adhere to Popper's scientific method is literally meaningless, for the simple reason that he has never advocated one.

However, the most interesting aspect of the paper (Hansson 2006), would be the claim that there are scientific statements with a more intricate quantification structure which hence are logically not falsifiable. Unfortunately the author does not give explicit examples of such.

In the next section I will present a brief summary of the philosophy of Popper, stressing that the latter makes no scientific claims, and in fact any such claims for a general philosophy of science would be circular. In the following section I will attempt to discuss Popper's view when it comes to everyday science and in the penultimate I will discuss some of the papers the Hansson refers to.

2 Popper's Philosophy of Science

Popper was once asked in an interview (Horgan 1996, p. 38) whether his philosophy was falsifiable. He considered the question idiotic, and did not hide his contempt for the interviewer. Popper was not a nice man, but as he is incidentally at pains to point out, the niceness of a person has no bearing on the hypotheses he formulates, be they philosophical or scientific.

One can discern a few metaphysical assumptions that Popper makes.

- A. He believes in an external world independent of us.
i.e. he is a realist not an idealist (nor a solipsist for that matter)

³ Typically x which will 'occur' in the future and thus cannot be tested (verified) in the present. This accords a way of evading actual infinities.

⁴ Being rejected as scientific should only be seen as negative, when such claims are being central. Popper emphasizes that not being scientific does not mean being nonsense. In particular metaphysics could be valuable as an inspiration for a nascent science.

B. He believes that there is a **Truth** of this world, which we cannot fully know, but asymptotically get indefinitely close to.

Thus there is a gap between epistemology and ontology, which can never be fully bridged. In particular statements that refer to an infinite of cases can never be verified, only possibly their negations.

Thus in spite of the pessimism that the impossibility of verification seems to indicate, there is on the other hand an indubitable optimism to which one may also take exception to. One should note that this shows that Popper is no post-modernist in spite of his insistence that any scientific knowledge is in principle provisional.⁵

Popper is taken to task for rejecting induction. A rejection which is considered absurd as without induction any scientific activity which is not purely deductive is seen as impossible. But Popper does not reject induction, he only reinterprets it. His aim is to correct Hume, who claimed that our belief in induction is irrational.⁶ It is not irrational because our beliefs are modified in a rational way. What Popper means is that we have expectations, and only when those expectations are frustrated are we forced to modify them. And this process lies at the heart of rationality (Popper 2002b, pp. 67–8). Expectations are always liable to falsification, but as long as they are not we are entitled to rely on them. In the words of Collingwood: *Deduction compels, induction permits* (Collingwood 1999, p. 10).⁷

Not surprisingly Popper uses Darwinism as an illustrative metaphor. Organisms have expectations, if frustrated they die, unlike the scientists whose hypothesis dies in their stead. And just as a species cannot be assured of an infinite life, a scientific theory can never once and for all be verified, but its reliability is provisional.

Central to Popper's thesis is that expectation precedes observation. We pose questions and Nature responds. This is the opposite of Francis Bacon's approach. To him we first observe, and from the observation we derive the truth. This old idea of science is still very much prevalent. To Bacon truth was manifest. If we just observe with an open mind and an objective spirit truth will become apparent. In particular this means that it is easy to find truth, there is in fact a method for it, a method that can be taught and learned, and when applied diligently with sufficient resources it can on an industrial scale produce facts which we can use to control nature and thus increase human welfare. As should be obvious, this is very much the view of politicians (Popper 1994 p. 82 ff).

Popper on the other hand holds that Truth is not manifest. It is in fact very hard to find (as opposed to test). Popper is less interested in the result of science, meaning a multitude of facts, than the building of explanatory theories out of which a wealth of consequences will follow, and to be 'understood'. The invention of new theories is a creative process. It does not matter how a theory or a hypothesis is arrived at. Its value is neither dependent on its author nor on the methods applied only whether its consequences comply with factual circumstances, which is something objective and beyond the control of its author. Popper somewhat fancifully refers to the Third World—the world of mental constructions, and how it interacts with the First World, the material world out there (Popper 1979 p. 74).

The imagination never works in a vacuum, it needs to be provoked by encountering obstacles. This is the true role to be played by falsification. Falsification are like diamonds

⁵ For Popper the content of a scientific theory is central, while Kuhn's vision of paradigms, emphasizes the sociological aspects.

⁶ Hume (1739) relied on induction as much as anyone else, he only claimed that it was not rationally founded but based on passion in the form of habit.

⁷ Taken to the extreme, and as such potentially misleading, we may refer to the observation of William James to the effect that we have a passion for belief. If possible we would believe everything (James 1890, (II) p. 299).

for ever, they cut off possible avenues, and thus allow us to probe much deeper into the configuration space of ideas. As Kuhn (1996, p. 160ff) puts it: *The reason we do not feel that philosophy has made any progress since Plato, is that no branch of philosophical inquiry has ever been rejected once and for all.* Thus the bewildering variety of the organic world and the astounding complexities of its members are due to selection, i.e. cruel rejection.

Scientific theories have consequences. They allow us technological applications. Theories are not just tested by scientists. In fact every time a GPS is used, the roundness of the Earth and a hoist of other scientific assumptions are tested, just as when we fly in an airplane, receive treatment for a disease, or any other humdrum activity we happen to engage in. Not even diamonds are for ever, and in principle also falsifications may in the very long run turn out to have been mistaken. In real life, unlike formal logic and mathematics, there is no clear demarcation as any falsification is in practice theory-based. Thus metaphors should never be taken too literally, when so done they merely become silly. Their purpose is to stimulate thought not kill it.

Nothing in the vision of Popper is really original, his role has been that of a poet, to formulate in striking ways the wisdom which is implicit in us all—the wisdom of trial and error. A scientist can of course pursue science without any awareness of Popper or of any philosophy of science whatsoever, just as a mathematician can successfully engage in mathematics without any formal training in logic.

3 Scientific Practice

The daily life of a scientist is humdrum. Very few scientists come up with new grand theories. They all learn various methodologies specific to their disciplines. Science is a social activity (and in fact Popper claims that science is impossible except in a large social setting) and you depend on others and hence need to trust them. Just as in real life, you have to take on trust what you are being told, because there is no way you can check it all by yourself.⁸ Thus the typical day of a scientist consists of applying known techniques, or in the words of Thomas Kuhn, solving small puzzles (Kuhn 1996, p. 35ff). There are no restrictions to the process of discovery, thus in particular a scientist may observe and generalize a pattern in the spirit of Bacon, and this is what is commonly thought of as scientific activity. Experiments are made, their results analyzed, scientific knowledge extracted by competent observers. And then on to the next run. An industrial production. There is nothing wrong with doing this and coming up with a hypothesis. In fact by Popper every dirty trick is allowed. The difference is that you are not allowed to appeal to this very experiment which made you formulate the hypothesis and consider that a verification. There is no limit to the number of patterns you can perceive. But of course the hypothesis can be used as an inspiration for further testing. There is nothing remarkable about this, it is commonly referred to as replicability.

Not all that is published in science involves the testing of a hypothesis, one may report on an experiment, an archeological excavation and suggest interpretations, without necessarily formulating a testable hypothesis. To this category also belongs applying scientific techniques, i.e. techniques based on scientific theories, to report on a measurement, the determination of the age of some archeological artifact, in short to apply science to finding out some fact that may be interesting beyond the scientific community as well as within it. The exploratory part

⁸ It is different in mathematics, where a strong mathematician can in principle check anything he uses by himself. Otherwise this leads to specialization and the concomitant risks of alienation, as there is nothing more satisfying than to have complete control of your knowledge, and when large parts of that has to be jettisoned, the activity may no longer seem as meaningful.

of science can be conducted in any way, but when all is said and done all scientific activity must eventually lead up to something testable, i.e. something that has consequences one way or another. As long as everything fits, the community is happy, but sooner or later there may be problems and you will have to go back and reconsider assumptions you have taken for granted. If the situation becomes drastic you have one of Kuhn's so called paradigm shifts, the theory of which is in perfect accordance with Popper.

The daily life of a scientist may thus have little to do with the vision of Popper, as opposed to that of the longterm trajectory of his discipline. Yet, like in a fractal setting, the trials and errors that pervade Popper also play their inevitable role in the small, even outside scientific work for that matter. Everyone who has ever programmed knows that the way to check a program is to run it. If it does not work you get a feedback and you modify and try again. The smaller the steps, the more reliable the progress. You cannot prove that a program will work and do what it is intended to do unless you try it. And if so, you can only trust the latter provisionally.

4 The Limits of Science and the Limits of Popper

Are there truths that we can never ascertain. Ontology never being fully covered by epistemology? One should be careful, the French sociologist A.Comte claimed in the nineteenth century that we would never know of what the stars are made of (cf. next section). Otherwise obvious candidates for those are inconsequential facts of the past. How many hairs did Caesar have on his skull at the time he was knifed down in the senate?⁹ Did he have more than fifty? Either he had or he had not. How can we find out which? Only if the world would look different depending on the answer. As observed by chaos-theorists, small changes can have tremendous long-term consequences, which of course does not necessarily make it easier to go backwards and reconstruct. Is it true that the past injects into the future and thus in principle can be reconstructed to arbitrary accuracy? If not, and there is true forgetfulness, does that mean that some amount of time-travel is possible, i.e. that we can at least in a limited way change the past without affecting the present and thereby evade obvious logical contradictions.¹⁰ Those are clearly metaphysical speculations and non-falsifiable. Can they nevertheless be thought of as scientific? The scientific community would probably not fund such an undertaking, at least not profusely. But could they inspire the formulation of real testable hypotheses? And if so would they qualify as scientific? But according to Popper anything can inspire the formulation of a scientific hypothesis. We have the spectacle of string theory which so far has not come up with a single testable physical prediction, and thus by many physicists is considered not even wrong. On the other hand it has developed through another selection process, namely that of mathematical beauty, and has had spectacular applications to pure mathematics. Even among its most vocal critics (Penrose 2004, pp. 887–9) there is an acknowledgment that something is going on.

That science, as we indicated, can never tell us everything, poses of course no problems to Popper. The vision of Popper seems vague enough to accommodate anything scientific, but are there parts of science, perhaps even most of science, which it does not cover. Are there

⁹ Caesar was bald but that does not mean that he was entirely devoid so that makes it easier to count. Of course we may have to specify the question further to make it logically unambiguous.

¹⁰ Incidentally if there is no free will, unlimited time travel does not entail logical contradictions. If there is no free will one may argue that time has a reduced meaning, as Popper pointed out as to the role of time in Einstein's special relativity. An example of the naivety of Popper (2002c, p. 149).

non-falsifiable scientific statements that are crucial to science and without which it would be drastically reduced? As noted no such explicit examples are furnished by Hansson.

5 Instrumentalism

Obviously the reason Comte never thought we would know the internal composition of stars was that we would never be able to travel to them and find out. In short we could make any claim about the actual composition and never be contradicted by direct hands-on experience. Any hypothesis would be for practical reasons non-falsifiable. The view that theories are but figments of the imagination and at best convenient schemes to organize computations¹¹ is known as the instrumental approach, and among its proponents one may note the physicist and philosopher E. Mach and the mathematician and mathematical physicist Poincaré (1905). Ultimately only direct sensual experience (getting your hands dirty with stellar material say) counts. The approach is based on a radical use of Occam's razor and as such very elegant. Popper points out that not even direct perceptions are in the form of irreducible data, but need interpretations using theories. Also that it does not bring anything new that the idealist Berkeley had not already introduced in the eighteenth century. An illustrative example is given by the measurements of astronomical distances. Those are based on an assumption of Euclidean Geometry and they are hard to falsify as measurements cannot be done in independent ways.¹² In the early nineteenth century Lobachevsky proposed that the geometry of the universe may be astral, i.e. hyperbolic and suggested empirical ways of deciding.¹³ Poincaré scoffed at it, claiming that it was basically a question of convention, depending on what we decided to be a physically straight line or not, and that ultimately convenience and elegance decided. The whole of cosmology depends on long chains of reasoning in which empirical findings are turned into definitions (e.g. by defining the distance to distant galaxies to be in accordance with Hubble's law the latter becomes a tautology).

It might not be taken amiss to briefly recall the outlines of the stepping ladder. First there is triangulation which can be carried to the extreme of measuring the parallax of close stars. It is indeed fortunate that there are such.¹⁴ Those measurements are on the limit of what can be done, furthermore they are complicated by the factor such as proper motions of stars, and the phenomenon of aberration.¹⁵ One may extend those by considering configurations of stars assumed to be rigid with respect to each others and extending their proper motions finding their vanishing points. Once again an elementary geometric argument. The real breakthrough came around 1910 with the

¹¹ Supposedly the Catholic church had no objection to the heliocentric worldview as long as it was merely an aid in computation.

¹² Of course in recent decades distances to close celestial bodies such as the Moon has been done in direct ways assuming the velocity of light, which, however, originally was based on triangulated astronomical distances.

¹³ At the time no stellar parallax had been established, which indicated that the geometry was Euclidean (if that was the alternative choice) to a very high degree.

¹⁴ Our visual world is a sphere. The lower half of it is sensed as flat, as we can explore it tangibly moving around it, the upper half is sensed as a vault, only accessible by the eye. This made a split into the terrestrial world and the celestial. The first gave rise to flat Euclidean geometry, the second to the spherical one of astronomy. Ontologically the stars could be literally infinitely far away, making up an unbridgeable split between the two realms, but this extreme position seems never to have taken root, instead there seems to have been assumed that the celestial bodies were part of space, and hence had finite distances.

¹⁵ The latter discovered by Bradley can be given an elegant and suggestive formulation (Penrose, p. 428) as the parallax of the hyperbolic geometry of world-lines in Einstein's theory of special relativity.

discovery of Henrietta Leavitt, of the regular relation between the periods of certain variables stars, called cepheids, and their intrinsic luminosity. This was based upon the fact that their distances were assumed to be roughly the same, all of them residing in one of the Magellan clouds. How do you know that they were inside it? Because their lines of sight coincided by those of the cloud. But how can you tell that this was not a coincidence, and they were actually in front of the clouds? Because there were so many of them, so statistically you expected a majority of them to be inside. Basically, the nice regularity found could best be explained by that assumption. Thus we see here an application of Occam's razor. The fact is too simple and beautiful not to be true. Now granted this we need to find a cepheid close enough to be within the traditional horizon of parallax measurement. Luckily there are, so we are able to calibrate. Then you find by the same method cepheids in other galaxies close to us. Then you discover the red-shift, you formulate a law—Hubble's, and for very distant objects, you use this law to infer their distance.

Taken together it presents a coherent picture in which possible falsifications take the form of merely complicating a theory. Clearly criterions of truths now adhere to general metaphysical assumptions of the simplicity of the universe (cf. string theory discussed above). The Big Bang theory was for a long time controversial¹⁶ but it did have precise and non-immediate consequences, such as background radiation, which to most cosmologists clinched the case. It is the occurrences of such unexpected and unintended predicted consequences which to Popper ultimately disqualifies the instrumental approach and makes a distinction between predictions and retrodictions as you can always fudge your theory to comply with data, it is much harder to do so with data not yet available (Popper 2002b, pp. 138, 144–53, 225–31).

Anyway the discussion should indicate that nevertheless one may talk about different degrees of scientificity, in particular that one discipline may have a higher metaphysical content than others, although one should beware of precise quantifications.

6 Discussion of Hansson's Survey

In (Hansson) the author makes a distinction between an open-ended explorative approach, where there is no (explicit) hypothesis to be tested, and one in which there are ones explicitly formulated. In the latter case he writes

Falsification is a viable strategy only if these four requirements are satisfied, namely a binary interpretative structure, status asymmetry, confirmatory asymmetry, and negative coupling between the two asymmetries. (Hansson, p. 278)

Those notions have been very carefully defined. Then the author proceeds to very precisely state his scheme of questions with which to address the articles selected for a careful study.¹⁷ To understand and hence to criticize Hansson's approach to a classification of the articles which leads to his critique of Popper, we need to do a recapitulation.

¹⁶ The most vocal critic—the astronomer Fred Hoyle, who incidentally coined the name with a disparaging intent, for a long time proposed an alternative theory involving the spontaneous creation of matter.

¹⁷ This is of course very commendable, there is a longstanding contention in philosophy that would only the concepts be clearly defined and the reasoning sufficiently strict the controversies that plague the subject would evaporate. In short, philosophy would be scientific and philosophical argument would turn into the form of calculations. We can compare with the remarks of Socrates in the dialogue Euthyphro (Plato), or with the vision of Leibniz pushing for a more precise language.

The essence of Popper's approach is, as Hansson rightly infers, to make a dichotomy between laws, which are in the nature of universal statements $\forall x P(x)$ which says something about in principle an infinite number of cases,¹⁸ and singular facts $\exists x P(x)$. Thus in particular there is an asymmetry between a universal statement and its negation, which is an existential statement. Universal statements can never be verified, but their negations can. (While by the same principle an existential statement cannot be falsified, only verified.) This is elementary logic and as such not at all controversial. Neither is the fact that universal laws are needed to make deductions and to predict things (of course predictions may turn out to be false, this is what makes them interesting). Without any universal law there is no science.¹⁹ It is once again the assumption of universal laws that makes it possible to reason about things. Theories are nothing but structured conglomerations of universal laws, which help us to 'explain' and 'understand' and can rightly be seen as the ultimate goal of science.

Now mathematics can be encoded into logical statements²⁰ but maybe statements about the empirical world are too complex to be so simplified? On the other hand if you reject logic altogether in scientific pursuits, any connection to rationality is abandoned, and you would expect chaos as a result. But it is of course true that you cannot put things into such clearcut cases as in mathematics. What is meant by the verification of a statement $P(x)$ for a particular x , or symmetrically a verification of the negation $\neg P(x)$? In practice this can only be made modulo some theory of some more basic and fundamental nature. This means that we have to do with a sociological issue. What is true, is what the scientific community believes is true. This very pragmatic view may seem very close to the post-modernist conception of truth as a mere social construct. But because $P(x)$ depends on some hidden universal assumptions, there could always be somebody, who questions them, and reduces to an even more basic question $Q(x)$ which the same scientific community may realize contradicts their first consensus. It would be pointless to try and formulate fundamental criteria which are, as we say theory-free. This is just the human condition, whose search for absolute truths is, as Popper calls his autobiography, an unending quest. This should clearly explain the purpose of falsifiability, it allows criticism. It makes the posing of guesses possible to anchor to an empirical reality. This can be illustrated by a more complexly quantified hypothesis. 'A person can jump from a great height without coming to grief, provided being sufficiently convinced that this will be the case'. How do you test such a hypothesis? Whenever anybody fails to survive, you can always blame a lack of conviction. You cannot falsify it! And furthermore if somebody would survive, it would nevertheless be very hard to find somebody else wanting to replicate it, even the first lucky individual would be reluctant for a second try, well-knowing whatever the conviction, it is bound to be fickle. In terms of quantifiers it can be formally written $\forall x \exists y P(x, y)$, where x denotes a person, y a conviction, depending on x such that

¹⁸ One may not take infinite literally of course, it can be argued that the infinite cannot be physically manifested, but is only an ideal construct of human thought. What is meant is that the statement holds for such large number of cases that it is not feasible to test them one by one, if for no other reasons that some of them are supposed to hold in the future.

¹⁹ One often says that history is no science, because it deals in particulars, not generalities. That might be true to some extent, but one can reason about the past, using more or less hidden universal statements, such as there were no airplanes in the seventeenth century. The same thing goes for natural history, we may with great confidence assert that no humans walked the earth during the era of dinosaurs. However, would human fossils be found together with those of dinosaurs, we would in principle be forced to reconsider the actual course of evolution, but using the general principle of Occam's razor, we would be rather convinced it was a hoax. Collingwood makes an important distinction between natural history, as an unfolding of a spectacle, and human history, in which the reconstruction of thought plays a crucial role, just as motive does in forensic investigations. This adds another element akin to universal statements into the picture.

²⁰ Although it is of course not as straightforward as non-mathematicians may suppose.

the statement P we just mentioned is true. Its negation would be $\exists x \forall y - P(x, y)$ whose verification, would involve the verification of a universal statement.²¹ This type of situation also ties in with the problem of immunization against falsification.²² Now someone may argue that science proves that this is impossible. There are no mental forces that are strong enough to violate the laws of physics. But this is of course a universal statement, and by assuming the truth of such, you may of course eliminate a quantifier in the statement.

Now in his paper Hansson makes a distinction between explorative research and those with predetermined interpretative categories. As an example of the first we can think of a measurement in which we have no preconceived notion of the result. We are making, as far as it is possible, a pure observation. In the second case we have some notion of what to expect. The measurement could be zero, negative or positive say. From a strictly logical point of view there is no distinction between them. Even a simple measurement is bound to land in some category, we expect a number if anything. The number of categories is of course huge. Thus one surmises that Hansson has in mind a very limited number of categories. The issue is not worth haggling over. More important though, Hansson interprets Popperism as if every piece of research (experiment) should hinge upon stating a hypothesis into two mutually exclusive categories. In other words, every experiment is reduced to a yes/no question. Furthermore if those hypothesis are, what he calls symmetric, meaning that no one is favored, there can be no question of a Popperian approach. Here he makes a logical mistake. Two universal laws which are mutually exclusive cannot both be right, but they can both be wrong. Logically there is no such thing that either one law is true and the other false and vice versa. If so the falsification of one, would be the verification of the other. This can only happen if we are dealing with a fact with no quantifiers, then of course we would have P or $-P$, falsifying one would be the verification of the other. Symmetry can only be maintained modulo some universal law, such as to ascertain that either something always happens or never happens. We will see concrete manifestations of this later. If one of the hypothesis is singled out, Hansson calls the situation asymmetric. This is a necessary condition for falsification. If universal laws are at stake, and we have a yes/no situation, I claim that symmetry is impossible. Now Hansson claims that given an asymmetry we can have either a positive or negative coupling. The first occurs if the hypothesis is more confirmable than its negation (i.e. more likely to be verified), the second occurs if its negation is. As noted given a yes/no situation, involving a universal law, this is bound to be the distinguished hypothesis in which case it is automatically a case of negatively coupled asymmetry, the case which is covered by Popper.²³

The argument may seem pedantic, but so is Hansson's scheme of analysis, and the purpose is to show that many of his putative cases cannot occur logically.²⁴

²¹ We may also think of the statement every human has a mother as of being in that form. Is it unscientific, because not being formally falsifiable? It hinges of course what we mean by the statement 'x' has a mother? Is it something that cannot be falsified, as it would in principle involve an infinite search? If so yes, it is not falsifiable, it means it is actually of the logical form $P(x, y)$ where you for each x , y have to ascertain whether y is a mother of x . But if you take a more pragmatic view? Also, note that the sharpening every human has a human mother is in contradiction with the Darwinian interpretation of the evolutionary record. Anyway the statement in its vaguer form is something we all believe in, and do not need science to inform us of.

²² When the movement of Uranus did not adhere to Newtonian theory, this was not seen as a falsification, instead it was explained by an ad hoc assumption of a previously unknown planet, later vindicated by the discovery of Neptune (Popper 2002c, p. 45).

²³ Of course we may make as our hypothesis that a universal law is not true, then of course it is possible to verify it, falsifying the law. But this is just a trivial play with words, signifying nothing.

²⁴ This may be thought of as a universal statement, and thus contradicting everything I am maintaining. But in logic as well as in mathematics, universal statements are allowed based on intuition. You may of course

What does Hansson find from his sample of 70 articles published in *Nature* in the year 2000?

49 of the 70 turn out to be explorative, according to Hansson. Meaning that the authors had not beforehand identified a few possible outcomes. As examples quoted are reports on molecular structures, gene sequences, the precise dating of the Egyptian pyramids (to which we will return later), or reports on new experimental techniques. This is hardly surprising. The humdrum activity of scientists is to use general scientific laws to deductively derive specific facts, or even to devise new techniques based on science, in order better to check individual facts. Not everyone is involved in creating new theories, most of the work is actually concerned with deriving consequences of theories. Those consequences can have intrinsic interest, such as the case with the pyramids, or be suggestive of new research, new theories, or used retrospectively to cut off alleys of speculation. Science is a collective enterprise, and fragments do not necessarily reflect the whole,²⁵ but are to be thought of as pieces in a giant jig-saw puzzles, deriving their meaning from how they fit into the larger picture.²⁶

As between the purely explorative papers and those that address specifically stated hypotheses, there is e.g. the question whether glacial exports of nutrients in the Arctic sector was higher, lower, or the same today. Thus three possible, outcomes, not two. From a logical point of view, how could we prove that something was the same? What is meant is clearly whether the exports were significantly higher or lower than today, or basically the same (note the inherent vagueness in qualifiers such as 'significantly', which of course will depend on the context). This is clearly the establishment of a historical fact, using general laws. The interest of the undertaking, as above, depends on the competence of the derivation, as well as the significance of the result. It does not differ from the cases above.

More interestingly Hansson reports three cases of status symmetry. In one paper (Lee 2000) supposedly, the question is *whether in embryonic development, the epidermal ectoderm and the roof plate have the same or different functions in patterning the dorsal neural plate* according to the interpretation of Hansson (2006, p. 280). The symmetry of the two assumptions is not obvious to me. Also reading the paper it is not clear to me either whether this is actually a fair characterization of the paper, I will thus choose it as one of the papers to put under the microscope. In another there is an *investigation whether or not similarities between Arctic and Antarctic species depend on gene transfer across the tropic*. Here we have a case of a historical investigation, using techniques of modern science. The result is meant to direct future speculations. In the third article, *the issue was whether or not activity in sensory pathways contributes to the development of cortical circuits. An experiment was reported in which cortical circuit development was influenced when nerve fibers from the retina were rerouted to the auditory pathway*. Hansson claims that in this case we have asymmetry, because if no effect had been recorded, the result would have been inconclusive, as the wrong type of sensory input may have been used. Thus we have a statement of type $\exists x : P(x)$. A certain x is tried and it works, thus the statement has been verified. Had it not worked, it would not have been falsified. A statement of such a form can hardly be said to form a law, but it can be used to falsify the universal law $\forall x : \neg P(x)$. One may, as Hansson

question intuition, but then logic as we know it disintegrates and we are in strange territory indeed. Less drastically I can simply challenge him to come up with such statements.

²⁵ But of course in any empirical undertaking, there is trial and error, and in the applications of the general laws, there are bound to be small guesses, to be tested and occasionally rejected, too technical to survive in the write-up. Thus there is indeed a kind of fractal structure, trial and error used at all levels.

²⁶ Although that this does not contradict the fact that individual scientists may not be concerned with the big picture, but caught up in their technicalities. Popper is concerned about science as a whole, not the individual practitioners.

suggests, in retrospect see the experiment as a falsification, although as in many other cases, this could be viewed as very contrived, especially if the experimenters did not conceive it in such a way.

Hansson finds 17 articles with status asymmetry, meaning articles where an explicit hypothesis is stated and tested. In eight of those he claims that there is no confirmatory asymmetry to be found. Recall that this means that the verification of one is the falsification of the other, and thus that the much touted difference between verification and falsification does not exist. Hansson gives two examples. One is the hypothesis whether superconductivity would arise in C_{60} at higher temperature if it was hole-doped, apart from the ambiguity of the there being superconductivity or not, there is a hidden assumption here. A more honest formulation would be if 'superconductivity would *always* arise'. Then the symmetry disappears. A positive result would not prove it, only falsifying the statement 'would *never* arise'. Those two qualified statements are obviously exclusive, but they can both be wrong. In the second article, there is a testing of the so-called eutrophication model, according to which lakes become more enriched in nutrients as they age. Call it a model, but it is basically a hypothesis that this will happen. Studies of 33 recently formed lakes in Alaska showed that the model did not apply to these lakes. In other words a clear case of falsification if any. Hansson then makes the strange comment that the crucial issue of interpreting this result hinges on whether those lakes were representative, and thus that the problem would have remained! There must be something in the formulation of the model which makes its falsification impossible. A classical case of what should be a meaningless proposition.

Furthermore Hansson reports that in six of these eight articles the hypothesis came out with more support. In other words attempts to falsify it had failed, which is hardly to be interpreted as a failure of Poppers dictum! In the case above, Hansson now refers to the hypothesis coming out with less support, and thus admits to some degree of 'falsification'. What is meant by that? Obviously by making some minor adjustment to the model or hypothesis, it can be brought into better agreement with reality. This is the classical trial and error approach, going back and forth, just as Popper preaches.

Seven of those hypothesis articles were classified by Hansson as having positively coupled confirmatory asymmetry. Meaning, as we recall, that they were more confirmable than their negations. Hansson gives two examples. In one there is a testing of the hypothesis that cosmic background X-ray radiation originates in previously unknown galactic nuclei. The authors found discrete sources accounting for a large proportion of this radiation. How should we interpret this hypothesis? We can think of it as an existence statement. Finding previously unknown sources verifies the statement. Not finding it, would of course be inconclusive and not falsify the hypothesis. The negation of the statement would then have been a universal law to the effect that all the radiation comes from already known galactic nuclei. This has been falsified, and the hypothesis can in principle not be verified. What is the problem? That the authors state the negation of a universal law as their main hypothesis? In another study the hypothesis was that neurotransmitter receptors of type D5 differ from those of type D1 in having special functional interactions with $GABA_A$ receptors. Once again the proper statement should have been that all neurotransmitter receptors of type D5 differ, so it cannot be verified. One may agree that what one D5 does so do all others (a tacit assumption of a scientific law) in which case once again we have a case of establishing an existence. If we do we have verified the hypothesis, if not the situation is inclusive. And if we want, we can see it as a falsification to the effect that D5 and D1 do not differ in their interactions with $GABA_A$.

Finally among the 70 articles Hansson narrows down the short list to two, which comply to the putative Popperian scheme of forming a hypothesis for which the negation is more

confirmable. One of them was a test of the standard Big Bang model, supposedly much harder to verify than any negation. In the spectroscopic observation of quasars, assumed to be relic radiation from the hot phase, those had properties compatible with the model. Falsification failed. However, to call this ‘verified’ is of course a bit perverse, as Hansson admits it merely lends more support to Big Bang. This is what falsification of successful theories typically involve, this is why those theories are called successful. Finally there is only one example left, according to the Popper model of Hansson, in which falsification is indeed successful.

To summarize. Science is not just about formulating laws, but also to deduct a lot of special facts from these laws. Facts which may have independent interest, or facts that stimulate and inspire future research, meaning in the wider sense asking questions and have they tested. Science is provisional in the sense that it relies on unproven (and unprovable) universal laws. They make up a big interconnected web. As long as no contradictions occur, we are permitted to rely on those laws, if there will be contradictions, we have to rethink and modify. Thus in every scientific activity, be it just exploration (or for that matter in daily living when we exploit scientific laws), there is an implicit testing. The scientific laws have consequences, at every level, some even appreciable by the layman.

Note that falsification is another word for testing, the point is that it does not have to be successful, only that it has a chance to be so. There is no shortage of supporting evidence for any hypothesis, one only needs to be reminded of all kinds of conspiracy theories. The point of falsification is to avoid this, to test rigorously is to look for something most likely to upset, if it fails to do so, you can start looking upon the hypothesis with a certain confidence. We do this in everyday life as well.

To interpret Poppers recommendation (Hansson, p. 283) to scientists to formulate unverifiable hypothesis and then prove them wrong, is indeed a caricature of Popper’s philosophy of science. The author would have been better advised to compare the research done in natural science with those in social sciences, involving didactics, gender studies, psycho-analysis and marxism,²⁷ to see in what ways they differ, and how the Popperian approach could bring illumination.

7 Two Case Studies

It would not be feasible to go through all seventy articles (most of which Hansson do not give references to) and challenge Hansson’s interpretations of them one by one. I will instead pick two representative ones. One which is a hard-core biological one (Lee 2000) and one which is a far softer historical one (Spence 2000).

The first paper is very hard for a layman to read.²⁸ In particular I would not be able to peer-review it. How do I know it is not complete nonsense? The fact that it appears in the respected journal *Nature* indicates it is not.²⁹ As noted it is a fragment in a much bigger web. It only makes sense as how it fits into it. It is basically an explorative paper in which the principles of established biological science make it possible to design techniques and interpret the investigations enabled by those. There are no explicitly formulated hypothesis to be found, at least not in the abstract, so Hansson, to his credit, has not shied away from probing deeper. This

²⁷ It is not my intention to dismiss those examples in bulk, psychoanalysis and marxism do contain very interesting ideas.

²⁸ But not as hard to make sense of as I presume a paper in mathematics would be to the layman.

²⁹ Here I am afraid I do not differ much from members at hiring committees, reduced to rely on formal authority.

does not mean that the research is unmotivated, that questions are not asked. They are asked all the time, enabling Hansson to identify at least one pair of hypotheses buried in the text. The paper addresses the issue, whether the roof-plate is redundant when it comes to signaling for the specification of dorsal neural cells. This is a clear hypothesis, which the authors try to explore, by removing the roof plate from participation. If redundant, there would be no effect. If there would be effects, it would falsify the hypothesis of redundancy. To me a clear case of falsification. The hard part is of course to inhibit the roof-plate, and to do so (the genetic ablation of the title) clearly constitutes the main achievement of the research in question. The paper abounds in expressions, such as ‘indicates’ and ‘it appears’ only occasionally being strengthened to ‘establishes’ when the main result is to be announced. Such guarded statements would be typical of attempts at falsifications that fail. There are lots of statements followed by possible objections to them, and to remove those objections, various procedures are suggested and conducted. Examples such as *We reasoned that the ability of roof-plate signals....would argue against the idea that their loss...is the consequence of autonomous ablation.. We therefore tested whether ..can be generated* (Lee, p. 738) can be produced. Clear cases of thinking of possible falsifications. Not all of them are pursued, however, but merely stated as caveats and suggestions for the future. None of this is surprising.

With the second paper (Spence 2000) the concern is to more accurately date a historical artifact (an Egyptian pyramid) than is possible by historical documentation (or such standard techniques as radiocarbon dating). Apart from its intrinsic interest, such a dating assumes added significance in a larger context and will contribute to the problem of calibrating different systems of chronology. Unlike the previous paper I would consider myself qualified to act as a peer-reviewer. It relies on a clever idea immediately graspable, and free of technical jargon. The crucial thing to observe is that while it depends on the insights of natural science—the precession of the equinoxes and the linear proper motions of stars accurately measured, it ultimately rests on some assumption about the thoughts of the historical actors—those who assigned and those who built the pyramids in question. Thus it is not part of natural science but of human history, the object of which according to Collingwood, and already explained above, is to take into account the thoughts and motivations of people of the past, something we only can do, as we are able to put ourselves into their shoes (see (Collingwood 1961, p. 115ff)). In this case that the pyramids are assumed as to be as accurately as possibly aligned with the north, and that the way to do so was to use a celestial marker of the north. Once this is assumed we can proceed to remark that the accurate alignments towards the north depends on the presence of prominent polar stars, or in their absence of prominent pairs of nearby stars whose chords go (nearly) through the celestial pole.³⁰ Which of course also is an assumption of not only their intent but also their method. It is now a straightforward exercise, using the known rate of precession and proper motions of relevant stars, to construct their positions at arbitrary points in the past (or future), this is as noted the scientific component.³¹ The results are falsifiable in the sense that independent determinations of age can be brought into play and cause conflict. However, which dating should take precedence is a subtle one, especially as long as there is a historical component, in the sense of a reconstruction of human thought. Thus a Popperian analysis is useful in pin-pointing the weaknesses of an argument.

More examples could be considered but for considerations of length.

³⁰ The present Polar Star is actually more accurate as such than it was during antiquity.

³¹ Standard caveats are to be supplied. We assume that the time-period is short enough to assume that the proper motions of stars are uniform and that the speed of the precession as well as its axis, do not change significantly, something that can be safely assumed for historical periods.

8 Conclusion

Popper's conception of science is a philosophical and hence metaphysical vision of what constitutes science, it does not present a method. In fact Popper argues that pace Francis Bacon there is no method. In particular induction can never be phrased in terms of sufficient conditions for being able out of a limited sample draw universal conclusions.³² It is in this sense Popper rejects induction. However, in much clinical and social science statistical confidence intervals tend to play that role.³³ One may argue that as far as science builds on explorations of statistical correlation it is not science, although the outcome of such test may serve, as anything else, as inspiration for setting up fruitful scientific hypotheses. What muddies the waters is that there are two main aspects of science. One is to formulate general explanatory theories, and this is the one that interests Popper the most, the other to establish facts using scientific methods, i.e. applying universal theories, and the one with whom the public is mostly concerned. Facts cannot be falsified, theories can. Applications of theories lead to facts modulo theories, when those type of facts start to contradict each other, there is something wrong with the theories.³⁴ However, in science, as opposed to mathematics, there is no clear distinction between hard, ultimate facts ('axioms') and contingent ones. (And even in mathematics it is not that clear either!). Anyway the crucial point is that science is constantly tested in real life, not only by scientists. A discipline that purports that it can only be tested by experts, cannot be a science, no matter how eloquently they may argue for the case. Thus in spite of not supplying a hands-on-methodology Poppers vision can be useful to weed out pseudo-science from real science.

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³² The hypothesis that every parent of a human being is a human being has been confirmed literally billions of times and not a single concrete counterexample has ever been found. Yet the conclusion would in one blow destroy the theory of Evolution, the key being that the notion of Human being is not specific enough for the larger question but of course sufficient in everyday life.

³³ The setting of those confidence intervals are more or less arbitrary. It is symptomatic that those are set to be much stricter in medicine than in the social sciences. Often those are set to acknowledge uncertainty, but that uncertainty is quantified. We claim that with such and such probability this is true. Popper, wisely refrains from making such quantitative statements as in a sense they tend to be circular.

³⁴ It is far from obvious which theories will be at fault, cf. the case of debugging a program!

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