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Comparatism—A constructive approach in the philosophy of science

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ABSTRACT

The fundamental idea in the comparatist approach presented here is that a scientific theory is not judged to be right or wrong in itself, but as more true or false in comparison with an alternative. A comparison between two alternatives needs a standard and ultimately, truth is the standard of science. Comparatism follows the objectivist tradition. The most significant deviation from major ideas in the objectivist school is proposing a less dominating role for falsification. The comparatism perspective is then applied to some controversies of methodology in economics. Comparatism is claimed to be a descriptive model for much of science and to be a normative model for constructive guidance of scientific inquiry.

1. Introduction

The comparative approach to be presented can be seen as a part of objectivist theory of science that is adopted by many scientists. Generally, however, it has not been considered as a specific approach. Sometimes it is seen more as a conclusion from experience, as common sense, but not as an idea with specific characteristics. Since I see interesting advantages, it seems justified to elaborate on this approach. It is practical with a label and I suggest "comparatism". This comparatist approach is later compared with the two leading groups of thought, "the old objectivist schools of science" and "the new relativist schools of science". Then comparatism is brought to the context of methodology in economics.

The fundamental thesis in this perspective is that a scientific theory is not judged to be right or wrong in itself, but is always judged as more true or false in comparison to an alternative; the advantages and disadvantages of theory A are compared with those of theory B. There is nothing special about scientific choices in this respect. The same approach is followed when we choose a car to buy, or a political party to vote for. Some people have a conviction that they have found the perfect alternative, and some others are so critical that they opt for not having a preference at all. The majority takes a stand although they clearly perceive some weaknesses with the chosen alternative.

In a long-term perspective the scientific process is not focused upon satisfying goals, or upon perfection. The better alternative of two competing theories will soon be challenged by a new alternative—perhaps an improved version of the alternative it just defeated. In each "duel" there is no perfection nor "satisficing" (Simon, 1947) goal, but relative performance. This view, I think, gives a better understanding of the scientific process and is a more constructive guideline for scientific work than other perspectives. Let me try to give some reason for that claim.

One aim of this paper is to paint a picture that is seen as descriptively relevant for a large part of science, the other aim is normative. It is often said about marketing that one half is productive and the other half is a waste, but that it is impossible to know which part a new suggestion belongs to. I will argue that science using the comparative approach more often qualifies to the productive part of science.

The discussion in the field of the philosophy of science has been extensive, but in my judgment dominated more by "classical" than recent contributions. Therefore this paper refers more to the first than to the second group of works. Now the major components of comparatism will be presented in the following section.

2. Components of comparatism

2.1. Truth

A first positioning might be clarified already at this stage—comparatism is part of a realist tradition. A comparison between two alternatives needs a standard for judgment if the

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ambition is normative and not just to notice differences without reaching any judgment. The standard of science is not an easily obtained measurement, but ultimately it can be nothing less than the truth. Truth is a word that many people shun because it might sound pretentious. One striking thing with true is the asymmetry with its antithesis—false, lies, and misunderstandings. These words come easily, but truth is a big word. It is, however, not to be used as traditional objectivism, as cast in stone or revealed in sacred scripts, but naturally as scientific objectivism, something elusive and always questionable. Comte (1844) even thought it was better to call truth relativism because of the elusiveness of the final goal. The essence of science is a steady climb up a ladder. The advances can be seen as relative, but they are relative on an absolute scale. The top end of the scale is not within reach for the scientist, but to improve a theory bringing it higher than the old version is a possibility and his mission.

One objection to the possibility of finding the truth is often called "the non-determination of theory by data". There is never sufficient data to disprove all theories so we cannot make a well-founded judgment between all slightly different theories. However this does not render the scientific project impossible. The scientific ambition is not to examine endless lists of "also-truths", but explanations with possible relative advantages.

Regarding religion one argument is that science cannot prove that God cannot exist. God is then proposed as an also-truth compatible with science and not proven non-existing. However, the atheist line is that "one should not believe in things if there are no good reason to believe them to be true" (Hedenius, 1949, the first postulate). Bertrand Russel used as an argument the hypothesis that there is a teapot circling the solar system in the Kuiper belt. Such a belief is certainly not proven positively wrong, but there are no good reasons to believe in such a teapot, so the idea should be rejected. Correspondingly, the conclusion of science is that there are not sufficient reasons for believing in the existence of a God or to take a deist or neutral position. Many ideas can be considered wrong on the bases that they lack a reasoning or observation that make them worthy candidates.

According to such a view not all suggestions deserves a serious analysis. The US patent board receives a steady stream of applications for designs of a perpetual-motion machine. Despite not having checked them all, this scientific institution has taken a stand: such patent applications are automatically rejected (Hardin, 1993). Science does not need to have a complete picture of all new propositions for making a well-founded judgment that a thesis – such as the second law of thermodynamics – is right. There is a crucial difference between sufficient knowledge for a decision and full knowledge by a comprehensive analysis of every detail of all applications.

Correspondingly, there is a never-ending generation of suggested influences of star configuration upon dating success, and new divine intervention from the transcendent world. Most children of the western world believe for some time in the existence of Santa Clause. They also observe that there are many fake Santa Clauses. Logically the fakes do not prove that there cannot be a real Santa, but nevertheless induction makes an impact. There is data for many interesting theories, while theories without data are often not serious contenders. It is justified to make some screening of the unlimited amount of possible hypotheses.

2.2. Inquiry

Inquiry is a central term indicating a position between fundamentalist belief and fundamentalist doubt. The present opinion is not assumed to be solid knowledge, but nor is knowledge considered out of human reach. Both certainty and doubt are essential components in scientific inquiry. The philosophical perspective often exaggerates doubt and verge on the limit to a fundamental skepticism with questions about absolute certainty and Cartesian doubt, but science has often succeeded having a constructive balance between beliefs and skepticism. An excellent metaphor for the focused work and the handling of some mistaken theories is Otto Neurath's classical illustration of ship repair at sea. Repairing too much at the same time is fatal for the ship, but each part of the boat is possible to replace if of poor quality. In the history of science even timber at the keel has been replaced, but the ship has not foundered. A good question in science is one leading to an informative answer increasing knowledge.

It is also important to defend the autonomy of scientific inquiry. Kaplan formulates this thesis: "It is the principle that the pursuit of truth is accountable to nothing and to no one not a part of that pursuit itself", and "Theology, politics, morals, and metaphysics have all exerted a dubious and often repressive authority" (Kaplan, 1964, pp. 3 and 5).

2.3. Proper objects of comparison

A way of making one's own contribution more compelling is to choose a weak alternative or to present a caricature. Compared to a straw man, a weakling can look impressive. Almost as popular is using some idealized theory, as a devastating alternative to something that one dislikes that actually exists. In a comment on such distorted comparisons, Holmes (1993, p. 135) notes: "The principal antonym of competition, however, was not love but monopoly". Such antonym substitution is a common way to get a biased comparison. When comparing alternatives it has to be ideal type versus ideal type and proven results versus proven results.

When making this comparison we use the advantages and disadvantages of different candidates. For alternatives with different scopes, this brings further complications; the theories may not be competitors at all. Far-reaching theories have, for natural reasons, more exceptions and complications than small and limited ones. This is a reason for the common preference for small puzzles (What Kuhn calls "normal science"). We are then not easily accused and proved guilty of saying too much. At the same time, there are forces favoring general explanations, such as the fame obtained by scientists introducing successful "grand theories". But there is hardly a problem that different scientists follow different inclinations in this matter since there is a need for architects as well as bricklayers. On both levels there are incumbents and challengers.

The explanatory power of the prevailing theory will have an effect on falsification of a challenger. A rival theory that has to be better than the dominant "90 percent explanation" cannot go wrong very often and still stay in the contest. For the challenger of a 50 percent incumbent, some shortcomings are not that fatal. This comparative situation has to be remembered as argued by James Burnham:

> . . . we must keep in mind an obvious principle of scientific method. To disprove the theory, it is not enough to show that it is not 100% certain, that difficulties confront it, and certain evidence seems to be against it. It must be further shown that it is *less* certain than alternative theories covering the same subject matter, that there are in its case *more* difficulties, *more* negative evidence than in the case of at least one alternative theory. No theory about what actually happens and will happen is ever 'certain' (Burnham, 1941, p. 274).

The mass of evidence will normally speak for the ruling theory, and some proponents will see this quantitative superiority as decisive. There is certainly an influence in the short run, since so many scientists are actively involved in projects in line with the ruling theory. However, if the little evidence there is about a challenging theory shows some promise of avoiding weaknesses and

finding strengths, some scientists will be attracted and the new case will successively be strengthened. Some weaknesses will be reduced and some advantages more probable; the initial bias can be corrected. Since this is a continuous evaluation process, a strong alternative has excellent chances of being recognized in due time.

2.4. The field of issues

Some scientists describe science as counterintuitive. This implies that science is elitist and never really understood by the masses. Even if this view is common, I think it is right to say that numerous and prominent scientists take an opposite stance. Kurtz (1992) argues convincingly for that science is not separate and distinct from common sense, but rather common sense supported by additional adequate knowledge.

A distinction is often made between understanding how the world works and what one should do about it—a distinction Aristotle called theoretical knowledge and practical knowledge. Many people see science as a theoretical world in an ebony tower that is best to keep apart from practical issues. This anti-practice line is seen as giving science more leeway, but is undercutting its usefulness. Ultimately also basic science is about practical application. This link to practice will often not show up until a later time. Even concrete inventions like electricity, the telephone and computers long generated very modest forecasts of their coming practical importance. Experience has now taught us that scientific progress leads to massive practical effects. There is no evident breakaway point in the following chain: explanation–causation–prediction–prescription–agitation–decision–implementation–effects. If science matters for the final effect, it does matter for the different steps in the process; but the whole process does not belong exclusively to scientists, so this causes conflicts of interest. However, splits in different areas of responsibility should be seen as more institutional than fundamental. Science is not restricted to a minor sphere, but it is important to inform the public and to motivate practical decision-makers to heed the advice of science.

The common opinion is that science has beneficial effects in the long run even if some basic research is hard to justify with a specific calculus. Obviously science has had innumerable effects on society, and not only on material conditions but on values and culture. To be able to give a wholehearted contribution to his field, the scientist has to make the evaluation that truth/knowledge is desirable. This issue has been raised metaphorically, as when Adam eats the apple of knowledge, or when Prometheus steals fire from the Gods to give it to man. The scientist needs to share the judgment of Prometheus. After this central judgment, the scientific process is more a generator of values than a product of it. John Mackie made a key comment on the relation between facts and prescriptions: "What is reasonable and rational to do may depend upon the facts, but the facts cannot depend on what is reasonable and rational to do" (Mackie, 1977, pp. 228–229).

One difference in opinion of special importance is that between philosophers of the enlightenment and later philosophers. The enlightenment view has been that the scientific project should expand from the natural sciences to politics and ethics. The logical positivists and Popper want to limit science to a world of facts and separate it from the world of values to avoid science being contaminated by new metaphysics and old prejudices. Max Weber drew a sharp line between social science and social politics (Weber, 1904). Here, there is an opposite protective ambition; the sphere of values from an intrusion of science and rationality. Also this split is at best a division of labor, but not of differences of fundamental kind. At worst it invites irrational politics by offering a dubious label of "value rationality" to irrational choices that are not instrumentally rational.

Comparatism proposes a more expansionistic view, that an understanding of the facts is central to the normative sphere. Science has a central position when values and action are to be decided. The political scientist Allen Buchanan comments on the thorny issue of secession:

> The proper evaluation of theory is always, ultimately, *comparative*. In general, when we try to decide whether or not to accept a theory, the appropriate question is not 'Does it explain all the data or solve all the problems?' but, rather, 'Is it the best theory available?' In judging the moral views on secession presented here (whether one honors them with the title 'theory' or not), the fundamental question is not whether they solve all or even most of the moral dilemmas of secession, but whether they improve significantly upon the current prevalent way of thinking about the issue. In the end, the only way to refute a theory conclusively is to advance a better theory (Buchanan, 1991, p. ix).

In the comparative view there is no real benefit in restricting science and the comparative method to a world of facts separated from practical action, politics and ethics. The problem is not that there is too much reason and too little unfounded beliefs, but the other way round. Meddling into these less scientific fields might make science less respected and more controversial, but also more useful.

2.5. Standards for comparison

A major task in science is to refine good theories and discard less promising ones. Two theories might be in rivalry or complementary to each other. It is often the case that two different factors both have some effects on an outcome, but if both theories claim that their respective factor is the paramount factor for the outcome, there is an issue of rivalry for which comparatism will be useful. There is no simple general answer to the question "What to compare" but that is determined by the nature of the rivalry.

A helpful tool in science is experiment. By changing just one variable at a time there are often good possibilities to read the effects more accurately than by real life observation. This procedure has been most successful in the natural sciences, but there is also a potential in the social sciences. In medicine there is a strong trend, often described as a "movement", for evidence based practice (Greenhalgh, 2006). On top in the evidence hierarchy is since long randomized clinical trials (Sackett et al., 1991). Often, some patients are treated with the drug to be investigated, while some others receive a placebo. If there already is an effective treatment of a sickness, there are ethical problems with having a control group given no real treatment. The useful comparison is then between patients receiving the standard treatment and those obtaining the new treatment. Such a design illustrates the comparative approach. Conducting meta studies is another way to reach well-founded judgments. This method is well established in medicine and the social sciences since the 80-ties (Glass et al., 1981; Light and Pillemer, 1984).

One way of describing comparatism is as a calculus of different investment alternatives. For each, the benefits and advantages as well as costs and disadvantages are estimated, and a net value is calculated. It is not the lowest cost or the highest revenue that is decisive. Neither is a positive net result sufficient, but it has to be better than other solutions. The comparison is illustrated in Fig. 1.

For some economic issues there are good possibilities to reach a bottom line number, since costs and income are in the same currency. When evaluating e.g. two health care models the calculation of benefits and shortcomings becomes more problematic. However there are possibilities for simplification by taking out positive or negative components from both alternatives. Often there are

Comparing two rivaling theories or models

	Theory A	Theory B
Verification/Advantages	……	…….
	……..	……..
	……..	……..
Falsification/Disadvantages	……	……
	……..	……..
	……..	……..
Net value	……..	……..

Fig. 1. Comparing two rivaling theories or models.

similarities between the competing models and they share both theoretical and practical problems that therefore do not need to be quantified for the evaluation between the two alternatives. There might also be advantages and disadvantages that are not of the same kind but similar and more easily compared horizontally than calculated to the bottom line. Benjamin Franklin was a proponent of a cancellation model when comparing two alternatives in order to find the best one. By eliminating factors of similar magnitude or two small effects versus one of medium size, one of the two alternatives is reduced to zero advantages and disadvantages. The choice then depends upon whether the other alternative has a positive or a negative balance.

Of course, different evaluators will not always agree, but the method might still be helpful by specifying the difference of opinion. There are many factors to consider in this calculation. A theory successfully predicting rare events indicates that there is more behind the prediction than a guesswork; correct anticipation of eclipses showed the quality of astronomy as compared to astrology. A prediction grounded in consistent theory with high information content has a higher value than one linked to personal inclination rather than logical expectation from the theory. A convincing casual explanation has a value exceeding correlation and predictability. A constructive approach is to start by considering complexity, but then to proceed by simplifying the comparison through a series of judgments.

Comparatism should not only be applied to more specific theories, but also to aggregated theories as paradigms and research programs. Then the comparison meets some special problems. Lakatos (1978) described the problem of abandoning "a degenerated scientific research program". The facts contradict the theory, but the scientists involved close ranks and energetically defend the "hard core" of the program by suggesting new *ad hoc* theories and sacrificing ideas in what he called "the protecting belt". Little new knowledge is generated when prior successes are just duplicated and inconsistencies explained away. A shift to "a progressive scientific research program" is blocked or delayed. Since discredited ideas might make a comeback after a new significant finding or revision, it might be worthwhile that a few scientists try to resurrect the degenerated research program with a major change. However, it seems desirable that normal science, solving many small puzzles, should focus on progressive scientific research programs where they can be expected to generate more "added value". Therefore it is important to make a proper evaluation comparing the programs.

3. Comparatism and objectivist schools

The ambition here is to illustrate some questions of relevance for a comparatist approach. A comparison with other positions will demonstrate the differences and hopefully also indicate some comparative advantages of the comparatist approach.

3.1. Logic and deduction

The Greek way of science was essentially to start with evident truths and pass from them to non-trivial, robust conclusions. Euclidean geometry is a successful example. Since then, it has been justified to speak of a downgrading of logic and deduction. It is plain to many that logic in itself cannot advance reasoning very far, but can be of use for more limited purposes, such as control of consistency. Reasoning built only upon analytical statements has problems reaching beyond the trivial. It is unusual that one theory can defeat a major rival by presenting a logic conclusion while the other is logically incoherent.

3.2. Induction

Inductive methods are often dismissed in a categorical way. "As we have previously seen it is impossible to find an inductive method that guarantees that one from true statements about special facts constructs a correct hypothesis of a law" (Molander, 1988, p. 162). As rightly stated in this quotation above, an inductive method cannot issue any guarantee, but neither can any other method.

An argument against the use of verification is that a person looking for support will always find some. But there are differences of quality. Some are just-so stories with little impact on a skeptical audience, and might even include many components harmonious with the common explanation. Considerably stronger are data that go against common understanding and strictly favor a new theory. A new theory does not only need to be right, it also needs to be interesting, which means saying something that is not already understood in a similar way. A comparatist way of thinking assigns little value to verification that supports not only the proposed theory but also its rivals.

3.3. Falsification

The superiority of a falsification compared with a verification (Popper, 1959) is often pointed out – an outcome "Q" says more about the above hypothesis than the outcome "Q" – but the importance of this imbalance is greatly exaggerated. A problematic result of falsification is that all theories can be falsified with limited effort. Thomas Kuhn dramatizes, but is right about the core message in the following:

> "As has repeatedly been emphasized before, no theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect. On the contrary, it is just the incompleteness and imperfection of the existing data-theory fit that, at any time, define many of the puzzles that characterize normal science. If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times" (Kuhn, 1962, p. 145).

To find something wrong is interesting, but does not change the fact that what is right is most interesting, since an endless purge of unlikely hypotheses gives little or no progress. The scientific method might be in accordance with the maxim that there is no construction without destruction, but construction is still the main point. That a theory is a simplification of reality is a truism. The same should be the case with the accusation that a theory does not give a total explanation. The popularity of the falsification view has neglected the need for a constructive side in criticism; the aim is not to find a theory rotten, but to support the best theory. The important thing is what a theory justly says. Since all theories have some shortcomings, the last survivor will be the alternative that is the last to be tested if one only uses falsification. This certainly

seems a method that is unlikely to sufficiently examine the strong alternatives, but will lose focus in a search for new alternatives that have not previously been tested seriously. It does not offer a productive research program, and is also committing an error: neither are some shortcomings fatal to the scientist's favorite hypothesis, nor is a verification necessarily killing the rival hypotheses. A falsification may lead to premature rejection when a stronger alternative is missing as pointed out by Davis and Holt:

> Theories are much more meaningfully evaluated in the light of alternatives. Rejection of reasonable alternatives strengthens a failure to reject the maintained hypothesis. Conversely, a theory that organizes some aspects of the data well should not be discarded until a better alternative is found (Davis and Holt, 1993, p. 29).

In many fields, such as politics, the efforts are focused on eliminating other alternatives. If they can be disqualified, a choice of the remaining party, one's own, will follow. Many political movements build their support upon an all-out attack on the status quo; if that alternative looks bad enough, the voters might be ready to try a challenger without many supportive arguments. Marxism and National Socialism are very short-spoken when describing their solutions to problems, but long on critique about the shortcomings and moral corruption of the capitalistic democracies. This is the case not only with agitators, but also with scientists. There have been many Marxist economists, but their contribution is basically only critique of capitalism. Democratic politicians also follow this falsification pattern, so it can be seen as standard political wisdom that attack and critique are superior to positive arguments for the own solution. At official occasions like scientific awards or disputations, the positive contributions of a scientist will be highlighted, but when normal science is at work, the falsification tendency is very strong. Every suggestion will be attacked for its deficits, while its contributions compared to rival theories are left in the background. Therefore it is tempting to drop the positive claims and start an attack by oneself—claiming not that one has a better answer, but merely that another answer is wrong, unsupported or inconsistent. The "critical rationalism" of Popper tilts to a non-constructive negativism.

When Comte coined the word "positivist" for the scientific view, he wanted to stress the aim of positive knowledge, and disrespect for negative knowledge expressed, for instance, as sophisticated humility about the complexity of problems (Comte, 1844). There is much to say for this ambitious attitude of enlightenment. On balance I would estimate that there is not too much verification and too little falsification, but the other way around. In philosophy there is a strong vein of fundamental skepticism that brings little progress and sometimes even personal despair as confessed by Hume (1740, p. 269).

3.4. Verification revisited

In his autobiography Popper (1992) has a chapter named "Who killed Logical Positivism?" He claims to be the executer and that is not unmotivated. Just asking that question illustrates the triumph of the hypothetical-deductive model. It motivates many scientists to reformulate their work, and much research seems to be in line with that model.

The real world of research probably looks a bit different. A lot of hypotheses collapse and leave some data unexplained. Are these of use to anybody starting from another angle? Sometimes they indicate a pattern that the prior hypothesis had not predicted, and which almost insists upon another explanation. In practical life as well as in science, one often begins with a problem—something that should not happen is happening. The scientist does not create the question or collect the data, because the problem is there and much material is already collected. The scientist's mission is to explain the strange pattern already recorded.

With the growth of information technology, much more data will be available and the scientist will not as often be forced to dig by himself for them. It may be argued that this change is insignificant: the scientist still has to cut his way through a jungle, even when the jungle is digital. I think this change has significant consequences. The improved access to data will increase the available data component relative to the personally acquired data component. This development will probably shift weight from pre-investigation hypotheses to post-investigation improvement of theory. The increased possibilities for meta studies should be considered.

In a more trivial sense, the critique of the scientist as driven only by objective facts is right. Some kind of aim, as a conscious or unconscious hypothesis, is needed to motivate the scientist in starting his mission. But many times it is rather the area of research that attracts the scientist, and the first hypothesis is made just to get into business. Frequently the data can be rearranged, and only through testing against a very different hypothesis is the spark of invention unleashed.

Since Bacon, most scientists have focused on facts and then brought their theories in line with empirical evidence. Evolutionary economists Nelson and Winter made the proper stand in this question:

> In evolutionary theory the ultimate discipline on the representation of firm behavior is considered to be empirical. Notions that firms pursue profits, that they satifice, that they follow simple rules, that they expand when profitable, and so on are all appropriate grist for the evolutionary theorist's mill, but only because (and to the extent that) they are plausible as empirical generalizations (Nelson and Winter, 1982, p. 410).

When turning attention to less practical and more philosophical judgments, many take a more defeatist stand: "There is no data free of theory" or "data is theory impregnated". I think the opposite view is more reasonable: "All theory is empirically influenced". One argument for this position is that most theories fall back on a metaphor. We all think more easily in terms of practical connotations, and even the mathematician focused on logical deduction is more of a biological survival machine than a calculating machine. The stress on theoretical development occasionally causes a neglect to realize that the refinement of data is at least as important for the scientific process. I think there is a message in the theory of evolution regarding the inductive method, well expressed by Flor:

> [O]ur way of thinking has developed as a response to practical problems of living and not as optimal solutions to finding the truth", and "Of course, all those conclusions [a perfect net of prejudgments] are extrapolations and therefore not conclusive. But nature does not bother about the objections against induction that have been voiced by many philosophers from Hume to Kant to Popper (Flor, 1987, pp. 195 and 192).

One way to acknowledge the importance of induction is to put it on an equal basis as deduction. The term "abduction" has rather commonly been used as a method of going back and forth between deduction and induction. This might be the most rewarding philosophy for the scientist himself, but even more so for a discipline. Specialization and personal preferences can be good reasons for the individual scientist to be single-minded in his choice, but for a paradigm, it is a serious fault to neglect the theoretical or the empirical side. Both the path of deductionism and that of inductionism have to be ambitiously explored. Friedrich Hayek is attributed the statement that "without a theory the facts are silent", but it could be added that without the facts, the theories are blind.

4. Comparatism and relativist schools

4.1. Paradigm shift and normal science

According to comparatism the change in paradigm is not arbitrary. The reason for a change to occur is normally a significant difference in quality. This difference has to be larger than in normal science, to overcome the stronger bias in favor of the default alternative. There are several mechanisms such as path dependency maintaining a paradigm and obstructing the advancement of a new paradigm (Sterman and Wittenberg, 1999).

In normal science, small advantages of one fraction over another can cause a shift of opinion. Regarding a specific issue, there are often some scientists for and some against, but there is a much larger group of competent and rather objective onlookers or judges. Certain ideas involve few people wholeheartedly, and a revision is not a major cost for most members of the scientific group. When a paradigm is threatened, the situation is different. Some core theories carry thick layers of other theories and, if they fall, a lot of people have to make revisions. Not only a pyramid of theories but also one of human rank and prestige become disturbed likewise, which generates a stronger defense for paradigm theories. A large group is actively defending the present paradigm; or rather, the two or more established fractions that constitute a paradigm. Scientists are not so often defenders of a monopoly as of an oligopoly of ideas. The attackers of paradigms therefore need strong reasons to make a breakthrough. This does not mean that the scientific reasons are unimportant, but that especially worthy arguments are needed to outweigh sociological resistance. Since the established paradigm has such advantages, the challenger needs intellectual superiority. As with revolutions, the change often occurs when the ruling group loses faith in its cause. One should expect a stubborn defense until a breaking point, when people realize that not only the intellectual fight is lost, but also the fight for the future. Then the system, too long defended, is abandoned rapidly. When change seems to be inevitable, it will be accepted.

Kuhn's paradigm theory is unclear in some important respects. Often, he positions himself as not being a relativist. However a central issue is the rationality – if any – in a paradigm shift and he argues repeatedly that the shifts are beyond scientific comparison: "it [paradigm shift] is a transition between incommensurables" (Kuhn, 1962, p. 149). Furthermore, Kuhn takes the situation in Orwell's book *1984* as a parallel to science. There, those controlling the present could remold the past as they pleased, and that approach creates the future. The regime's intellectual advocate in the book describes the philosophy as "collective solipsism". The collective construction is considered to be the reality, not just a more or less realistic perception of it. Another futuristic writer, Huxley (1932), suggested an operational definition of truth creation: 62,400 repetitions constitute a truth.

A paradigm gives normal science a focus and energy that, according to Kuhn, is more productive than in a pre-paradigmatic pluralistic state. As in an enlightened dictatorship, high-priority issues are attacked with force and determination. Another parallel is to see it as general social conformism. If the claim is that scientists are more conformist than the official guidelines, then Kuhn no doubt scores a point. However, much of Kuhn's description seems to be a general sociological observation, but little is done to highlight special features that distinguish science from other human activity. Feyerabend (1993) makes this point more drastically when saying that there is nothing in Kuhn's picture of science that makes it different from organized crime. The uniqueness of science is hardly the split in paradigms, but the unity over paradigms.

Apart from the descriptive part of a paradigm, there is also a normative question. In which way should science be pursued? The question of efficiency can be seen in relation to two other activities, business and military. According to Spencer (1884), the centralized and focused model has proven itself most adequate for military success, but a more pluralistic or even anarchistic model is the best design for business. Other people have argued for the centralized solution also in economics, but Spencer's view seems strongly supported by the experience of the ensuing century. That science should follow the same pattern as the military system seems counterintuitive, and I do not think there are sufficient arguments to swing that judgment around. There is scant foundation for considering unity as a basically positive factor, but instead justified to suspect that a high degree of unity in a paradigm is at the expense of pluralism—and the latter is more plausibly the progressive driving force also in normal science.

The remedy to narrow-minded conformism is to keep science a large republic rather than break it up into local states. Kaplan (1964) makes the important claim that there are no state rights in science. The existence of one world demands compatibility between theories of different kinds. This implies comparisons not only between different paradigms, but also between different fields of science. General human social behavior manifests itself in subgroup affiliation that in science can be called paradigm, but that is hardly a characteristic of science but rather fragmentation is less strict than for other human subgroup behavior.

4.2. Perspectives

The relativists often criticize the modernist view for harboring objectivist illusions. Science's ambition to break free of culture's moldings is futile. Instead, dependence of theory on empiricism is criticized and a more detached view on reality is suggested. Such a "post-empirical" (Giddens, 1984) approach relocates the interest from the observed to the observers. "New definitions of truth emerge as products of a socially negotiated consensus between truth makers" (Astely, 1985, p. 499).

Polkinghorne thinks the following metaphor illustrates an insight: "Truth is a construct which, under examination, reveals itself to be something of an onion; the layers of perspectival understanding can be peeled away until there is nothing left at the core. 'Reality' is views; it is not a thing that lies behind views and causes them" (Polkinghorne, 1983, p. 251).

This quote claims that subjectivism and conformism are the substance and implies that any such agreement is as good as another. There is a difference between those relativists focusing the subjectivity and those focusing the conformist side of the perspective. Both these versions are in conflict with the modernist claim that some theories are in accordance with a reality and that such correspondence is more interesting than the popularity of different theories with "significant others".

Shared illusions are still illusions, and so are the confessed illusions. There is a strange belief that consciousness of subjectivism eliminates any real problem—that there is a difference between "perspectival subjectivity" and "biased subjectivity" (Sandberg, 1995). Unfortunately there is no more in this than in the rhetorical excuse "I know I am subjective, but . . .". A confessed "perspectival subjectivity" is still subjectivity.

4.3. Different worlds?

A less radical idea than to make science a belief at par with any other ideas about the world is to suggest there are more grounded knowledge about some aspects of the world; science is not just belief, but sometimes it is overreaching. In a Kantian tradition many writers worry about scientism; the sphere of the true is invading the spheres of the good and the beautiful. Several thinkers as e.g. Habermas are critical to science, but try to limit their anti-scientism to what they see as imperialism outside the proper territory of sci-

ence instead of making a general attack on all scientific claims. Scientism is in a persuasive definition seen as "the exaggerated belief of science in itself" (Alford, 1985, p. 78). We can all agree that exaggerations are unwanted—but a comparatist will not discard a poorly supported scientific theory if it has less shortcomings than other available explanations. What damage is science doing when introducing an improvement in understanding?

A separate zone for science may, like many other compromises, sound good and attractive for those not too concerned with either position. Intellectually this is most problematic since there is a need for compatibility between different scientific areas and between science and other domains. Within science there can be different views about, say, human independence. But it is not possible to have one view in the field of psychiatry, another in sociology, a third in psychology and a fourth in economics and claim they are all true. A central question to ask is about the phylogenetic place of *Homo economicus*, *Homo sociologicus* and others all claiming to capture the spirit of *Homo sapiens*. Of course it is not necessary that one is true and the others all wrong, but the other theories cannot be seen as irrelevant, but as challenges to the view of the own field.

Science cannot sign peace treatises where contradicting theories are declared to be "also truths". Similarly, science cannot split the world into separate domains, NOMA, "Nonoverlapping Magisteria" as suggested by Gould (1997) for evolution theory and religion. These two explanations of the creation of the human species cannot ignore each other and pretend they deal with different issues.

I am certainly not equating science with religion, but there are some common roots. Religion has not only been focused on existential problems, but has also provided explanations for the mechanisms in the world. To a curious mind a myth is a step ahead to not having an idea. Most people then and now prefer the best available explanation to the humble "we don't know". At one time, religion had the best answers to most questions. Science has now outperformed religion as explanation through a cumulative build-up of knowledge. From a humble start grows a long list of desired capabilities for a scientific theory; causality is more than just correlation and predictability. However, correlation and predictability are impressive when rival theories cannot show up such capability. The sacred scripts talk about eternal truths, but science speaks of provisory truths. With such a view there is more of "the present position of science" and less of "scientifically proven". But even if using "true" instead of "True", the question is how to set the standards.

The most dubious standard is conformism. "Everybody thinks that . . . "is lightweight even if dressed up to "A consensus theory of truth", "The position of a discourse" and "given the perspective". Few ideas are so strange as dominant ideas in small subgroups. There are reasons to agree with Nietzsche: "Madness is something rare in individuals—but in groups, parties, peoples, ages, it is the rule" (Nietzsche, 1886, p. 85).

It is justified to be most critical of interpretations, ideologies and theories since they do not only bring possibilities to create an informative order, but also to create a distorted picture. I think Dagognet (2001) makes important points with his preference for focusing the outside. There is a strong correlation between the outside and the inside, and our reading of the outside with our eyes is more reliable than speculation about inner processes. Observations do not speak the whole story, but they say a lot. Collective speculation is still just speculation and the forces for "group think" are strong also in scientist groups. Many discourses are similar to religious sects where the faith of others form and confirm the own beliefs.

5. Comparatism and economics

To what extent can comparatism contribute to methodological issues in Economics? I will here revisit some issues and controversies with a comparatist perspective. There is a general external criticism against economics for having a limited scope in order to provide firm, but materialist, answers. But there is also an internal critique that the answers provided are too vague and imprecise. Simon (1995) is one prominent economist voicing such a critique; many economic theories do not say too much, but too little.

5.1. The missing quantitative question

McCloskey (2002) has persistently criticized the discipline for an overuse of math despite meager results, and she claims that mathematical elaborations of "existence theorems" are pointless. An idea stating a connection between a factor (A) and an outcome (O) is easily grasped and little is added on by formalizing this relationship. The central issue is to provide an answer to the question "how much?" and without an effort of quantification, no more is done than to give an introduction to the issue. In contrast, quantification opens up to a comparison whether A is more important than B, or if A should be modified to become AA. The mere connection supported by some assumptions provides little insight. Binmore considers the economist base model attacked by ignorant critics: "It is admittedly hard to admire the ingenuity of thinkers who deny the tautological" (1994, p. 173). Even if correct, this defense demonstrates a weakness. Mathematical tautologies are constructive because the resulting theorem is not trivial, but an economic reasoning can sometimes add so little that further messages are read into it.

Another criticism by Ziliak and McCloskey (1996, 2007) aims at the inductionists instead of the deductionists. The issue is the dominance of "statistical significance" over significance, meaning importance and again the missing question is "how much?". The star hierarchy of statistical significance is seen as objective and essential since it has technical qualities, but this focus on the level of noise in the data is crowding out the issue of importance. There are insignificant factors with little noise and there are significant factors with high levels of noise. In two studies, each covering a decade of articles in the American Economic Review, the authors find the "sizeless stare of the statistical significance" in a large majority. Again, the quantitative answer to the question about how important a factor is, opens up for comparatist possibilities. All paradigms, also modernistic, run the risk of developing into internal exercises producing "just so stories" in a secluded ivory tower.

5.2. From ideal to real situations

A controversial method, when launched, was Friedman's (1953) claim that the value of a theory can be tested in its ability to give correct predictions. The most controversial part was his dismissive view of actual context conditions. Instead of spending effort describing the actual conditions or looking for a field where the premises of the theory were met, he just forecasted according to theory "as if" these assumed preconditions were at hand. One can argue that it is not wrong to spend time on describing actual circumstances, even conducting a "thick description", but Friedman's focused approach has been fruitful. One reason is that a theory often describes ideal circumstances, making the predicted result more probable. However, that lacks in ambition, since such circumstances might be very rare in reality, which limits the relevance of the theory.

An important part in the development of economics has been a series of developments reducing the conditions set up in the pure ideal situation such as perfect information and no processing costs. Three concepts and their prominent promoters will illustrate this broadening ambition: "Bounded rationality" (Simon, 1947), "rational ignorance" (Downs, 1957) and "asymmetric information" (Akerlof, 1970).

5.3. Experiments and behavioral economics

The introduction of experiments raised methodological doubts. Was this not an expression of the standard sin of "the dismal science", reductionism? Smith (1982) argued successfully for the possibility of parallelism; a simplified situation could still involve the crucial choice and control of other variables that interfere in the real world. Smith contributed to an important condition reduction with his experiments with double auctions. In economic laboratory experiments efficient market exchange was obtained with few buyers and sellers. If efficient markets should have been impossible in oligopoly situations, it would imply a severe problem for the modern economy.

There is also a potential for field experiments. Easterly (2006, pp. 367–384) suggests using such testing when giving support to different villages in the developing world. The model with randomized trials can be used also in the social sciences. Effective help is scarce, so it is important to use possibilities for learning how to perform better.

A challenge to experimental economics was raised from the psychologist Daniel Kahneman who received the Nobel price the same year as Smith, 2002, but with almost an opposite position in an important question. The economic experiments documented rationality in the laboratory, while the psychologist experiments found irrationality (Altman, 2004). This, to a high degree, depends on there being "pay-off dominance" in the experiment; meaning that there was a salient economic gain likely to exceed the cost of effort in finding a rational solution. One conclusion from comparing different experiments is that test persons are not compulsively rational, but attracted to all kinds of simplifications when irrational choices are free or almost free of costs. The following quote illustrates the controversy: "Arguably, the analysis of financially unmotivated behavior can be left to those most equipped intellectually to handle it: psychologists" (Harrison, 1992, p. 1441). The degree of rationality in individual decisions remains a major controversial issue and is of central interest for behavioral economics.

5.4. The evolution of institutions

Another economic idea that is fruitful is institutional competition. On a complex level it is hard to combine deductive and inductive fragments to a functional system, so an orientation to experience might be more suitable to find out what works well and what does not. The European experience is that the institutional competition between states has been important for structural improvements of the market (Wohlgemuth, 2008). There is also interest for other institutions than the state and the market. Researchers are exploring the experience of "intermediate" institutions designed to solve conflict of interest (Ostrom, 1990). The present zeitgeist might favor harmonization, but that makes it even more important to learn from diversity by making comparisons, rather than the contemporary attitude to praise diversity and claim incommensurability.

One argument for crossing discipline borders is the success of prior innovations. Not least did economics import massively from the science of physics in the 19th century. Economics have influenced political science under the label "public choice". The sub-discipline, evolutionary economics, is influenced by Darwinism and might play an expanding role. These ideas have also been influential in psychology, while there has been a very limited interest in sociology (Sanderson and Cornwell, 2000). The most crucial force in evolution, according to Darwin, is selection. In nature and in culture we achieve a large number of mutations and a central issue is how this diversity is reduced and directed in creatures and cultures. In biology the selective criteria is fitness and in economics the suggestion is profit (Nelson and Winter, 1982). From a theoretical standpoint it is very important to have a hypothesis of selective forces when addressing the issue of dynamics.

Hirshleifer (1985) is an explicit proponent of "economic imperialism" and Becker (1976) might be the most expansive researcher in the sense of using an economic approach to topics not conventionally regarded as economic. Understanding serious social problem, a hypothesis of lack of, or even destructive incentives, seems to be promising (Waldkirch, 2001).

5.5. Economics and sociology

Not only is there economic imperialism, but also "sociological imperialism" (Baron and Hannan, 1994, p. 14) and economists and sociologists seem to be far apart in perception. A quote by Duesenberry provides an indication: "Economics is all about how people make choices; sociology is all about how they do not have any choices to make" (1960, p. 233). Another judgment brings this difference up to the macro level: The economists claim that "norms emerge and survive essentially because of their relative efficiency" while the sociology position is that "markets exist and flourish to the extent that norms for efficiency prevail" (Baron and Hannan, 1994, p. 19).

This controversy has many parts. One area is the advance of sociology into organization theory. Organization theory started with breaking out companies, and studying them as rational actors, but later the trend has reversed and organizations are perceived to be more and more dependent and integrated. Luhmann (2003) claims that there is a tendency for organizations to disappear into society. On the micro level the trend is that the agent concept is expanding (Meyer, 2009). The social iron cage does not look that firm in an age of proclaimed individualism. A change within sociology is to attribute less to the role model and more to a less stable identity (Udehn, 1996). This increased role-distance also implies a less harmonious view of society than in the classical sociology of Durkheim (1895) and Parsons (1937).

In organization theory the distinction between private companies and state organizations is often ignored. The distinction between government and governance is blurred and the line between the social and the professional is not distinct in a network society. It will be difficult to separate distinct spheres for sociology and economics. Rather the situation appears set for competing hypotheses for the same reality. Human interaction and order will include cooperation, competition, hierarchy and conformism. Economists and sociologists share the same areas of interest, and there are broken fences for trespassers. According to the argument in this paper, there is no good reason to enforce restrictions, but in finding out how well the trespassers succeed compared to the incumbent theories.

6. Conclusion—a root metaphor

Sometimes a metaphor can provide the core ideas in an instructive way. My illustration of comparatism is a detective investigating a crime. In the film format this is sometimes suggestively presented as different scenarios built on the stories of participants and witnesses. The stories are contradictory and the mission of the detective is to solve this puzzle and find out what really happened. This situation is similar to a comparatist scientist in several central respects.

Firstly, the ontology is clearly realist, there is just one truth, one chain of events has taken place. One person can of course feel threatened while the other did not intend to threaten, but a misunderstanding is one event with two perceptions, not two kinds of reality.

In addition to the court's decision as an approximate truth of guilty or innocent, there is the real truth. Being acquitted is one thing and innocence is another. As Oscar Wilde stated in his book *The Importance of Being Earnest* "Truth is rarely pure and never simple". But it is not out of human reach. The epistemology attitude of both detective and comparatist scientist is that knowledge is possible. There is not an idealistic reality, a Kantian nouminal world or the mystical creatures of Plato, only perceivable as shadows on a wall. Instead there is a real physical world that can be read and understood by human eyes and minds. The solution is seldom a contemplative reflection, but a combination of a clever mind and hard fieldwork. The detective also uses abduction. There is a need of induction from the crime scene and there is also a need for a theory about motives and a possible chain of events. Step by step a likely scenario can be built.

The ethos of the detective is not that of a lawyer. The mission is not to do his best "given the perspective", but looking detached upon the facts of the case. Empathy with the participants is helpful in understanding the situation, but sympathy will cloud the judgment. The mission is not to "help somebody out" or "nail" somebody else, but to find out what really happened. This is in contrast with the relativist definition of Richard Rorty that truth is something that convinces a special auditorium (Rorty, 1985, p. 162). No doubt Rorty will find support for this view among lawyers.

There is also a value commitment necessary for motivation and that is a conviction of the general positive effect of justice. If seeing theft and violence as justified means of poor men to obtain more resources, or seeing punishment as a way to push criminals deeper into crime, the search for truth becomes problematic. Correspondingly, a researcher must have a positive view that research is a social good. Occasional bad effects do not really threaten this commitment.

A problematic position is that of promoting only "emancipatory science". Science will not only produce good news but often bad news, so the scientist needs to believe in a duty and a long-term benefit in finding also bad news. One argument is that most problems do not disappear by being ignored or denied. It is extremely problematic when a scientist considers some positions good and some bad to the extent that he is seduced by a position that is false but judged good. The scientist's belief in truth is as important as the detective's belief in justice.

Humans are fallible both in cognition and in character so there is a need of an efficient support system. Generally the scientific system works rather successfully in successively sorting out inferior ideas. Popper (1973) expressed grave doubts about the objectivity of individual scientists, but still saw a qualified conclusion emerging.

> "No amount of political partiality can influence political theories more strongly than the partiality shown by some natural scientists in favor of their intellectual offspring. For by making their minds more 'objective' they could not possibly attain to what we call 'scientific objectivity'. No, what we mean by this term rests on different grounds. It is a matter of scientific method. And ironically enough, objectivity is closely bound up with the *social aspect of the scientific method*, with the fact that scientific objectivity does not (and cannot) result from the attempts of an individual scientist to be 'objective', but from the *friendly-hostile co-operation of many scientists*" (Popper, 1973, p. 217).

A crime investigation often follows a track with some hypotheses of likely motives and culprits. If this approach does not bring results, it might be constructive to shift to another major track, but such a shift is often problematic since there is always something more to follow up along the first track. Scientists have a similar problem exemplified by the degenerated research program. There are considerable sunk costs invested and a familiar path to follow. The hard question to ask is whether this track is still the most promising when considering the limited results and new clues indicating another direction for the search.

The least fitting in this metaphor is the strong demands in the court before altering the default decision to acquit the accused. This can be transferred to two interpretations that descriptively have much in their favor, but both differ normatively with the position of comparatism.

The first implies a high threshold for being considered "scientifically proven". However, comparatism is not concerned with a zero point on the truth scale since scant evidence is better than none and a qualified guessing is better than a random choice. Of course there should be an ambition to reach "proved without reasonable doubts", but this cannot be a demand. The best should not be the enemy of the good. As Churchill stated "perfectionism is spelled paralysis".

The other interpretation is to favor the ruling paradigm as a default alternative. Comparatism does not consider such a preference justified. The force of the *status quo* is a phenomenon in most human endeavors and in many practical issues it is motivated since change always carry costs. When considering to change an organization or to relocate a bridge, it is not sufficient with a minor advantage for the new alternative. For the theoretical and intellectual issues this is not as relevant. This skepticism of favoring a status quo alternative motivates to see the scientist as a detective rather than as a judge having a strong inclination for the status quo judgment, not guilty.

Conclusively, I claim that comparatism is an informative picture of how much of science is practiced; relative progress on an absolute scale. Normatively it has the advantage of focusing the constructive and avoiding normalizing conformism by seeing science, like any social activity, highly influenced by "significant others". Sometimes a picture created by little more than a consensus, an interpretation or a social construction, is temporarily accepted, but in the long run many popular human prejudices have been successfully challenged by science. The ideas, here presented under the term comparatism, seem to be essential parts of the scientific method and its success.

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