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Theoretical Medicine's Special Issue on the Nobel Prizes and Their Effect on Science

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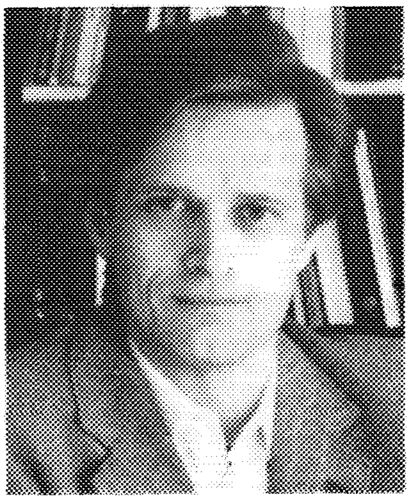
In the last two issues of *Current Contents*® (*CC*®), we reprinted an article by myself and my scientific assistant, Alfred Welljams-Dorof, on "Of Nobel Class: A Citation Perspective on High Impact Research Authors."[1,2] The essay that follows focuses on the articles in the special June issue of *Theoretical Medicine*[3] in which our article appeared. The special issue, published by Kluwer Academic Publishers in The Netherlands, is devoted entirely to papers concerning the factors influencing the selection of Nobel prize winners, or with the effect of the awards on science.

An abridged version of the introduction[4] to the issue follows. It has been specially prepared for *CC* by B. Ingemar B. Lindahl, the journal editor. He also edited the special issue. We've also included abstracts and author affiliations for the papers appearing in the issue, other than our own, in the table.

I was pleased to learn from Lindahl that earlier citation studies published in *CC*, especially those forecasting Nobel prize winners, were the prime source of inspiration for this special issue. He particularly referred to an early paper of mine in *Nature*,[5] which discussed the use of citation indexing for historical research. This paper was preceded by my 1963 paper in *American Documentation*[6] in which I proposed a "critical path" method of evaluating the impact of scientific discoveries. I stated back then that it might be possible, using computers and comprehensive citation indexes, to produce diagrams or "maps" that would not only show the chronological relationships between papers and discoveries, but also implicit impacts. Since then, ISI® has indeed developed a method to generate cluster maps based on co-citation analysis of papers that



B. Ingemar B. Lindahl

can be tracked over time, showing the critical point in the path of discovery.[7,8] But even these do not include necessarily the identification of important qualitative links.

As Lindahl points out below, the five articles in this special issue of *Theoretical Medicine* highlight the interaction between internal and external factors in the evolution of scientific knowledge. Interestingly, Stephen Toulmin's notion of the importance of studying this *interaction*[9] is the same perspective that sociologist Robert Merton brought to his 1938 seminal work "*Science, Technology and Society in Seventeenth-Century England.*"[10] Toulmin, it seems, arrived at the same conclusion as Merton did, albeit by a different path.

Harriet Zuckerman of the Andrew W. Mellon Foundation and Columbia University, who is one of the authors in this is-

Table. Author affiliations and abstracts of papers appearing in the special June issue of *Theoretical Medicine* 13(2), on Nobel prizes, except for Garfield and Welljams-Dorof, whose abstract was published with their paper in Part 1 of this series, *Current Contents* (33):3-13, 17 August 1992.

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This study forms part of a larger research project examining the election process for the Nobel prizes for Physiology or Medicine at the Karolinska Institute in Stockholm, and the role and function of the prizes in early 20th century Swedish and international medicine. The purpose of the study is to clarify the decision-making process which led to the Nobel prize for Paul Ehrlich in 1908, 'for work on immunity'. His award was preceded by the most dramatic conflict within the prize authority concerning any prizewinner prior to World War I, and thus is apt to illuminate both the implicit and explicit criteria and the strategies used in the prize deliberations.

Ehrlich's chemical ideas on the immune response were criticized by the physical chemist Svante Arrhenius who recommended the application of his disciplines' methods and principles on immunological problems. This criticism was brought into the Nobel prize debate by J.E. Johansson, a physiologist who asserted that Ehrlich's research was of little scientific value and therefore not worthy of a prize. Yet the majority of the Institute, led by its chairman, the chemist K.A.H. Mömer, succeeded in awarding Ehrlich.

An analysis of the controversy shows it to be primarily based upon (1) a difference of scientific styles between the antagonists, resulting in incongruous definitions of immunology as a research field, and of the proper aims and methods of immunological studies. Other factors influencing the final decision were (2) the Institute's negative reaction to what was considered an intrusion in medical Nobel prize matters by a chemist, (3) Arrhenius' and Johansson's diverging views on what kind of work should be awarded a prize, and (4) Johansson's position as a non-conformist at the Karolinska.

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This two-part article examines the competition between the clonal selection theory and the instructive theory of the immune response from 1957-1967. In Part I the concept of a temporally 'extended theory' is introduced, which requires attention to the hitherto largely ignored issue of theory individuation. Factors which influence the acceptability of such an extended theory at different temporal points are also embedded in a Bayesian framework, which is shown to provide a rational account of belief change in science. In Part II these factors, as elaborated in the Bayesian framework, are applied to the case of the success of the clonal selection theory and the failure of the instructive theory.

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In the last two decades, prizes in the sciences have proliferated and, in particular, rich prizes with large honoraria. These developments raise several questions: Why have rich prizes proliferated? Have they greatly changed the reward system of science? What effects will such prizes have on scientists and on science? The proliferation of such prizes derives from marked limitations on the numbers and types of scientists eligible for Nobel prizes and consequent increases in the number of uncrowned laureate-equivalents. These would-be surrogates for Nobel prizes extend the reward system of science in its upper reaches but this change is not fundamental. The spread of rich prizes to new fields provides added incentives to potential winners, which has its own disutilities; it reinforces competitiveness, concern for priority and attendant secrecy, all this amplifying ambivalence toward the reward system in science. There may also be modest positive effects of such new awards in the form of heightened popular esteem for science and interest in it.

sue,[11] also has commented on the *interaction* theme, with an emphasis on the factors external to science that have affected its evolution. Her conceptions are contained in a lengthy review article in the *Handbook of Sociology*.[12]

Lindahl's Biography

The journal *Theoretical Medicine* was launched 13 years ago. Lindahl became its editor in 1989. He received his BA in theoretical philosophy from Stockholm University. His introduction to the theory of medicine came in 1978 when he joined a research project of the philosophy department on the concepts of health and disease and causal explanations in medicine. Through this project, he made his first contacts with the medical faculty.

In 1979, he became engaged in a long-term project on mortality in rheumatoid arthritis and systemic lupus erythematosus. This was in the Department of Social Medicine at the Karolinska Institute, where he received his doctor of medical science degree in 1985. That same year, he joined the editorial board of *Theoretical Medicine*.

In 1986, Lindahl was appointed to a three-year research position at the Swedish Medical Research Council. He helped found the European Society for Philosophy of Medicine and Health Care in 1987 and still serves as a member of the Executive Committee as well as on the advisory board of the *Journal of Medicine and Philosophy*, also published by Kluwer.

In 1988, he became docent (associate professor). He is presently in the Department of Geriatric Medicine, researching theoretical problems in diagnosing Alzheimer's disease

Lindahl has made most of his contribution to the theory of medicine in the field of causality. In his dissertation, he dealt with the problem of selecting the principal cause of death from a chain of events or a combination of concurrent conditions. In a series of papers following his dissertation, he has shown how this selection and weighting of causes create artificial trends in national statistics and influence the scientific value and practical relevance of cause-of-death data.

Lindahl also has served as temporary adviser to the World Health Organization on the cause-of-death registration rules for the 10th revision of the *International Classification of Diseases*.

In addition to his research, he also has developed curricula for education in the theory of medical science, both at the graduate and postgraduate levels, at the Karolinska Institute.

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DISCOVERY, THEORY CHANGE, AND THE NOBEL PRIZE: ON THE MECHANISMS OF SCIENTIFIC EVOLUTION. AN INTRODUCTION

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The theme of this issue of *Theoretical Medicine* focuses on a few of the many mechanisms governing the evolution of science. The five articles within this theme[1-5] deal with both internal research methodological factors, regulating directly the evolution of scientific knowledge, and external, social factors, regulating the constitution and performance of the research community. Among the former factors are the principles for hypothesis testing and theory change. Among the latter are the methods for measuring researchers' impact in science and criteria for judging the impact of scientific discoveries. (These latter methods and criteria function both as instruments for studying the evolution of science and as factors *influencing* the pace and direction of science.) The Nobel prize in physiology or medicine has been chosen as a paradigmatic example of both how the importance of scientific discoveries may be evaluated and what influence such an evaluation may have on the evolution of scientific knowledge. All five papers deal directly or indirectly with factors influencing the selection of Nobel prize winners or with the effects on science of the Nobel prize.

The *interaction* between internal and external factors in science, highlighted by the papers in this issue of *Theoretical Medicine*, is often overlooked in the study of scientific evolution. Philosophers tend to restrict the interest in the theory of science to the internal questions, while leaving the external to the history and sociology of science, and other empirical disciplines. Carried to extremes this way of working would be somewhat like studying biological evolution only from the point of view of molecular biology, without regard to findings and theories on more complex levels of organization—like those of cell biology, embryology, population genetics and sociobiology—and without a longer historical perspective—like that of paleontology. In order to further emphasize this point, I will make a few remarks about the analogy between the evolution of science and the evolution of nature, before presenting and commenting on the articles of this issue.

THE EVOLUTION OF SCIENCE

There are striking similarities between the evolution of science and the evolution of nature. These are clearly evident in the accounts of the evolution of scientific knowledge given by modern philosophy of science. For example, Karl Popper uses directly the terms of biology on the evolution of theories:

"We choose the theory which best holds its own in competition with other theories; the one which, by natural selection, proves itself the fittest to survive."[6]

Less literally, but in a similar way, one may see parallels between biological evolution and Thomas Kuhn's theory of the evolution of scientific knowledge.[7,8] Unlike Popper, Kuhn does not apply the biological concepts literally. Neither does he argue explicitly that the parallel includes his view on the selection process itself. But there are close similarities nonetheless. Just as biology views the evolution of nature to be regulated through an interplay between heredity, environmental influence and adaptation, Kuhn views the evolution of scientific knowledge as an interplay between paradigm, facts, and *ad hoc* modification.

The difference between Popper's and Kuhn's theories of scientific evolution lies in the fact that Popper essentially views the evolution of science as a piecemeal engineering process, whereas Kuhn sees it as both a gradual process of 'normal or paradigm-based research', consisting in 'puzzle-solving', and as a series of leaps and bounds, i.e. 'revolutions'.

In the light of the way Popper and Kuhn explain the evolution of scientific knowledge, it may be tempting to try to view the evolution of science as a whole as a system of several simultaneous ecological processes, in which the evolution of knowledge is, so to speak, the core (equivalent to the change in genetic material in nature), a subprocess contained in a larger social process of evolution. Seen in this way, one might say that science, like nature, evolves at all levels through a perpetual selection of the fittest individuals; in science, the fittest vehicles of knowledge. In his book *The Selfish Gene* Richard Dawkins develops a view on the evolution of human culture along these lines.[9] Dawkins introduces the concept 'meme' for an idea propagating in the human culture like genes in nature. The memes in Dawkins' analogy are the 'replicators' and the scientists the 'vehicles' of the memes. One could of course also think of other 'vehicles' in science, e.g. journals, articles, and other carriers of scientific knowledge.

Though there are always dangers in using analogies, the parallel between the evolution of nature and of science seems to have a great potential heuristic value. It is therefore not surprising, as Stephen Toulmin points out, that the attempts to use this analogy go as far back as to Darwin's own time.[10] The analogy between science and nature may help us see that similar questions can be raised concerning the evolution of science as philosophers have begun to raise more systematically concerning the evolution of nature. (For an overview of this latter field, see Michael Ruse *Philosophy of Biology Today*.[11]) For example: How shall the selection mechanisms be understood? What is meant by 'fitness'? What scientific value do theories about the individuals' (the vehicles') success in evolution have? Is it possible to predict a particular vehicle's success with the help of existing theories? What is meant by 'evolution'? Is evolution progressive or should these concepts, 'evolution' and 'progress', be kept apart? How shall the concept 'vehicle' be defined?

Just as the biological sciences cannot replace, but *do* enhance, the philosophical study of the evolution of nature, disciplines like information science, history of science and ideas, and sociology of science provide opportunities for a fuller philosophical understanding of the evolution of scientific knowledge.

The five articles of this issue of *Theoretical Medicine* deal with both levels. They partly analyse different aspects of the evolutionary process itself, and partly reflect upon this analysis.

THE PROPAGATION AND IMPACT OF SCIENTIFIC KNOWLEDGE

Eugene Garfield and Alfred Welljams-Dorof discuss citation frequency as a measure of researchers' impact in science.[1] They review previous studies of most-cited authors, discuss methodological problems in using data on citation frequency, and comment on the possibility of forecasting Nobel prize winners only on the basis of citation data.

Though the use of citation frequency data has great advantages, not least due to its reliability, it is also a complicated indicator whose appropriate use requires careful interpretation. Garfield and Welljams-Dorof discuss several important factors to consider.

At the beginning of their paper Garfield and Welljams-Dorof say, as an explanation of why high citation frequency is correlated with Nobel prize winners, both present and future, that their papers ought to be "seminal and more influential" than the average. It is probably not accidental that Garfield and Welljams-Dorof here distinguish between 'seminal' and 'influential'. There may in fact be a great point in this. (Note also that papers by Nobel prize winners are assumed to be *both* seminal and more influential than the average.) Dawkins points out that the fecundity, and consequently also the survival value, of a scientific idea may be measured "by counting the number of times it is referred to in successive years in scientific journals".[9] But if 'degree of scientific impact', which is what Garfield and Welljams-Dorof intend to measure, should not become *synonymous* with 'citation frequency', then, to use again the biological analogy, mere 'propagation' must be distinguished from actual 'mutation events'.

Despite the strong correlation between high ranking by citation frequency and Nobel prize authors, shown in Garfield's and Welljams-Dorof's review, citation frequency has (not surprisingly) proved to have a low predictive value for Nobel awards. The reason for this is of course the fact that there are so many other researchers with the same or even higher citation scores as the Nobel prize winners. Or, to use Garfield's and Welljams-Dorof's concept, the pool of authors 'of Nobel class' is so great compared to the few who actually win the prize.

What then distinguishes the works of Nobel prize winners from the works of others 'of Nobel class'? How have Nobel prize winners been selected? What criteria of scientific excellence can be discerned? These are questions Franz Luttenberger addresses in his paper.

THE SELECTION OF NOBEL PRIZE WINNERS

Franz Luttenberger examines the basis for the Nobel prize in physiology or medicine to Paul Ehrlich in 1908.[2] Ehrlich shared the prize with Elie Metchnikoff for their work on immunity. The principal issue of the paper is the Nobel Committee's evaluation of the importance of Ehrlich's work. Luttenberger shows how two of the main criteria in Nobel's will, the importance and recency of the discovery, were applied in practice. He also makes clear that additional considerations other than purely scientific ones, such as geographical distribution and the striving for consensus, influenced the deliberations and decisions to award the Nobel prize at this time.

Of principal philosophy of science interest is the significance that Luttenberger shows the difference in *scientific style* had for the evaluation of the scientific value of Ehrlich's work. As appears from Luttenberger's analysis of the arguments for and against a prize to Ehrlich, the chief opponent, Svante Arrhenius, as well as, through him, Johan Erik Johansson in the Nobel Committee, had a different theoretical approach than Ehrlich to the chemical interpretation of the immune reaction. As Luttenberger points out, Ehrlich's professional background was in organic and structural chemistry and he consequently focused on the molecular structure of the immune reactants; whereas Arrhenius had his training in physics, mathematics and chemistry and concentrated on the reactant's physical properties. Their different points of departure and ways of approaching the immunochemical problem coloured their views of what was a proper way to study the immune response, an appropriate way of interpreting the immunological facts, and ultimately what constituted good science.

This controversy about the scientific value of Ehrlich's work clearly illustrates Kuhn's point that the success and impact of a theory depend on the paradigm of 'normal science' in the particular scientific community in question and at the specific time in history.

Kuhn's theory about the influence of paradigms on the success and impact of theories in the history of science is of general interest when discussing the Nobel prize.

The idea that it would be possible to discern individual discoveries that have influenced science in a decisive and lasting way, and that are sharply marked off in time and each of them attributable to a single scientist, is, according to Kuhn, associated with the view that scientific knowledge evolves by accretion; a view abandoned by modern historians:

"In recent years...a few historians of science have been finding it more and more difficult to fulfil the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask. Perhaps science does not develop by the accumulation of individual discoveries and inventions."[7]

Considering the fact that the basic idea of the Nobel prize, according to Nobel's will, is to award important individual discoveries (and inventions, but not in Kuhn's sense of the term; see paragraph 1 below) and considering also the fact that the Nobel Committee, now for 90 years, has been successful in discerning individual discoveries and evaluating their importance, Kuhn's critical remarks give topical interest to several questions concerning the basis of the Nobel prize. Four questions are of particular interest: (1) What concept(s) of discovery has (have) the Nobel Committee applied? (2) Which criteria have the Nobel Committee used when selecting 'the most important discovery' within the domain of physiology or medicine? (3) If the Nobel Committee's evaluations of individual discoveries did not presuppose 'the concept of development-by-accumulation', on what view(s) of scientific change(s) were they based? (4) Which role(s), if any, did past and present paradigms, in Kuhn's sense, play in the Nobel Committee's evaluation of discoveries?

Without relating his analysis to Kuhn's theory of science, Luttenberger deals directly with the first two questions and, partly as a consequence of that, also indirectly sheds light upon the latter two issues.

1. A part of the explanation of why Kuhn finds it questionable and hardly feasible to try to discern individual discoveries, sharply marked off in time each attributable to a single scientist, lies in the fact that his concept 'discovery' is quite inclusive. It comprises the whole process from (i) "the awareness of anomaly", through (ii) "a more or less extended exploration of the area of anomaly", and ending in (iii) the adjustment or change of paradigm.[7] The last stage, (iii), appears to comprise also the construction ('invention') of theory. This seems to be exactly Kuhn's point, that discovery involves not only observation of fact but also the conceptualization of theory.

The Nobel Committee appears to have taken into account a process at least as comprehensive as the one Kuhn calls 'discovery'. If this is correct, a closer examination of the Nobel Committee's deliberations and decisions may provide a basis for challenging Kuhn's thesis about the difficulties in isolating and evaluating individual discoveries.

2. Luttenberger discerns six criteria for selection of 'the most important discovery' within the domain of physiology or medicine: (i) "that the results of a discovery had proven themselves to be of wide-ranging significance"; (ii) "that these results had been verified by the scientific community"; (iii) "that all issues of priority were established"; (iv) that "the work in question could clearly be distinguished as the candidate's own"; (v) "that [the research contribution] had been brought to a point where the given problem had been solved entirely, and the result constituted 'a finished whole' "; and (vi) that "[the discovery] carries both theoretical weight and practical significance".

3. All these criteria tell us a great deal about the Nobel Committee's view on science at the time of Ehrlich's prize. Judging from these criteria the committee viewed the evolution of science as a process of more or less distinct achievements, each attributable, in some cases at least, to a single scientist (although the prize may in exceptional cases be divided between two or three scientists). Whether or not the committee could be said to have embraced a concept of development-by-accumulation depends of course partly on how we should understand the committee's views on problems being 'solved entirely' and results constituting 'finished wholes'.

4. That paradigms played a role in the Nobel Committee's deliberations has already been noted, when commenting upon Luttenberger's analysis of the differences in scientific styles between Ehrlich and Arrhenius. The committee's evaluation of these two approaches/paradigms, as well as its comparison of earlier with more recent theories/paradigms, raises an interesting question about how dependent the committee itself was on prevailing paradigms when making these evaluations.

To sum up: Luttenberger's paper not only gives new insights into the process behind the selection of Nobel prize winners in general and the choice of Paul Ehrlich in particular. His paper also provokes the reader to reflect upon the roles of paradigms, and possibly also meta-paradigms in science as a whole.

INTERTHEORETIC COMPETITION AND THEORY CHANGE

Kenneth Schaffner's contribution consists of two parts, published as two separate papers.[3,4] In the first he develops a concept '[temporally] extended theory', partly based on Imre Lakatos' 'methodology of scientific research programmes',[12] and discusses three criteria for comparison and choice between theories—'theoretical context sufficiency', 'empirical adequacy', and 'simplicity'. In the second part he applies this concept and these criteria in an analysis of the rationale behind the success of one of the central theories in Macfarlane Burnet's immunological work, the so called 'clonal selection theory'; a theory originally proposed by Burnet in 1957.

Burnet shared the Nobel prize in physiology or medicine with Medawar in 1960 for the discovery of acquired immunological tolerance. Schaffner points out that Burnet's clonal selection theory was then not generally accepted. It was even considered refuted only two years after Burnet had been awarded the prize. The experiments 'disproving' the theory later turned out to be partly artefacts. The theory gradually gained acceptance and was, Schaffner points out, generally accepted by 1967. The main competitor of the clonal selection theory had been the so called 'instructive theory', originally developed in the early 1930's.

By seeing the clonal selection theory and the instructive theory as *extended theories* Schaffner is able to show how these two competing theories may be understood to have been constructed and, when they were compared with each other, how the evaluation and support of only *a part* of one of the theories influenced the development as well as the acceptance or rejection of this theory as a whole. The concept 'extended theory' also explains in what sense a theory can be said to survive and remain 'the same', in spite of the fact that essential parts of the theory have been re-evaluated and modified.

Schaffner's analysis leads up to an interesting interpretation of the conditions for a theory being accepted in favour of another and how this weighing may be understood in quantitative terms, using Bayes' theorem. Schaffner assumes that the acceptance of one theory (or hypothesis) T_1, in favour of another T_2, is based on a judgment, that the probability of T_1 being true, given the produced evidence (e.g. experimental results), is greater than the probability that T_2 is true, given the same evidence. Schaffner shows not only how the probabilities of two competing theories at the same point in time may be calculated, but also how changes in the probability of one and the same theory at different points of time—i.e. the probability of a temporally extended theory—may be calculated.

The result of Schaffner's analysis is a model for explaining theory change, in several respects far more elaborate than Lakatos' methodology of scientific research programmes.

THE ORIGIN AND EFFECTS OF PRIZES IN SCIENCE

Harriet Zuckerman broadens the perspective of the discussion, from physiology and medicine to science at large and from the Nobel prize to major scientific awards in general.[5] She calls attention to the fact that during the past 20 years the number of prizes in the sciences has increased dramatically, five times in North America alone. Zuckerman discusses some of the principal explanations for this increase and whether the new prizes have amounted to any major changes in the reward system of science. She also enters upon the perhaps most fundamental issue concerning the significance of the Nobel prize for science, namely what effects awards of this magnitude may have on the work of individual researchers and on the evolution of scientific knowledge as a whole.

Zuckerman points out as an explanation of the creation of new awards in science, that the scarcity of Nobel prizes, the limit of prizes to three fields and to three recipients for each prize, have ruled out the recognition of co-workers of larger projects and a great number of outstanding contributions to science.

Referring back to Kuhn's critique of the attempts to explain major turning points in the history of science by pointing out individual discoveries, each attributable to a single scientist, it is not difficult to see how much more complicated this task must be when it comes to science in our times. In addition to the problem Kuhn centers on, that the process of discovery may be so long that several scientists will succeed one another before it is completed, and besides the well recognized problem of collaborative research, there is also the problem of simultaneous independent discoveries. In *Scientific Elite* Zuckerman discusses how this transition has affected the role of the Nobel prize in science.[13]

Zuckerman's paper shows, through its wide approach, partly based on her comprehensive empirical study, *Scientific Elite*, how the Nobel prize serves as a model for other major awards, and which roles the reward system as a whole plays in the evolution of scientific knowledge.

CONCLUDING REMARKS

I began this introduction by emphasizing how the five articles of this issue of *Theoretical Medicine* highlight the interaction between internal and external factors in the evolution of scientific knowledge. I would like to conclude by referring to Toulmin's penetrating analysis in *Human Understanding* of the importance of seeing the change of internal and external factors as two aspects of one and the same historical process.[10] Toulmin sees Darwin's theory of natural evolution as an example of a more general form of historical explanation. He analyses the process of conceptual change, e.g. in science, as only another example of the same form of explanation.

In addition to illustrating the interaction between internal and external factors in the evolution of science, the articles in this issue also clearly show how essential the studies of the theory and history of discovery (and not only of the logic of justification) are for the understanding of the evolution of scientific knowledge. As Thomas Nickles points out in *Scientific Discovery, Logic, and Rationality,* the distinction between 'discovery' and 'justification' may not always be so easy to uphold.[14] We have seen an example of this in the discussion of Luttenberger's paper. When the concept 'discovery' is so inclusive, as Kuhn's seems to be, and perhaps also the Nobel Committee's, that it comprises the adjustment or change of paradigm, much of the 'justification' must already have been made before the 'discovery' is completed.

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