ABSTRACT. Scientists often disagree about whether a new theory is better than the current theory. From this some (e.g., Thomas Kuhn) have inferred that the values of science are changing and subjective, and hence that science is an 'irrational' enterprise. As an alternative, this paper develops a 'rational' model of the scientific enterprise according to which the scope and elegance of theories are important elements in the scientist's utility function. The varied speed of acceptance of new theories by scientists can be explained in terms of the optimal allocation of time among different scientific activities. The model thus accounts for the 'rationality' of science in a way that is broadly consistent with the empirical evidence on the history and practice of science.

INTRODUCTION

Scientific progress did not pose a problem for the logical positivists. Science progressed because scientists sought simple, general and rigorous explanations of phenomena. Science was defined by the universal, eternally valid values that scientists pursued. Scientific progress only became a problem when Thomas Kuhn (1962) highlighted historical cases of theory acceptance that seemed to show that scientists did not share common values.

Those who value science might safely ignore the problem of scientific progress if models of science had no impact on the actual practice of science. It is plausible, however, to think that the accepted model of science has a significant impact. One channel for such impact would be the attitudes of the educated lay public. Such attitudes are important because they effect the level of support, charitable, political and moral, that science receives. A second channel for the impact of the accepted model of science is through the scientist himself. The model of science that a person accepts will effect his occupational choice and, if he chooses science, his allocation of time and effort between science work and non-science-related leisure. The psychic returns from advancing science have long been identified as one of the primary components in the
compensation of scientists. Adam Smith even seems to suggest that such psychic returns are the main component of a scientist’s compensation:

Mathematicians, ..., who may have the most perfect assurance, both of the truth and of the importance of their discoveries, are frequently very indifferent about the reception which they may meet with from the public. ... The great work of Sir Isaac Newton, his *Mathematical Principles of Natural Philosophy*, I have been told, was for several years neglected by the public. The tranquility of that great man, it is probable, never suffered, upon that account, the interruption of a single quarter of an hour (p. 124).

Even though we now know that Smith was wrong about Newton’s tranquility (see: Hall, 1980, *passim* and Westfall, 1980, pp. 698–780), Smith may still be correct in his more general claim that scientists receive psychic returns from the belief that their research is true and important.

The problem that Kuhn raised of how to reconcile scientific progress with the actual history and practice of science is thus an important one for the reasons that I have just sketched. The attempt to solve the problem has touched off a debate over the proper criteria for evaluating models of science. It is generally agreed that logical positivism is flawed by its failure to deal with the history of science, while Kuhn’s position is flawed by its failure to account for scientific ‘rationality’ (note that when scientists and philosophers refer to scientific ‘rationality’, they are using a stronger sense of ‘rationality’ than the mere consistency usually meant by the economist when he uses the word).

The first criterion for evaluating a model of science is thus that it accounts for our maintained prior belief that science is ‘rational’ in a stronger sense than that its practitioners are consistent. The precise definition of ‘rationality’ in science is still a topic of live dispute among philosophers and scientists. For a model of the behavior of scientists to account for the ‘rationality’ of science does not, however, require that this dispute be resolved, but only that the model imply behavior that is consistent with commonly accepted historical examples of rationality in science (Laudan, 1977, p. 160). Any acceptable model of scientific rationality would have to imply, for instance, that a scientist’s acceptance of astrology in 1985 would have been irrational.

The second criterion for judging a model of science is that it explains the stylized facts from the history of science about the actual behavior of scientists. There may be several such stylized facts that are intrinsically worthy of note. But so far only one has been given significant attention: