A New Functional Approach to Scientific Progress

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Abstract

This paper develops and defends a new functional approach to scientific progress. I begin with a review of the problems of the traditional functional approach. Then I propose a new functional account of scientific progress, in which scientific progress is defined in terms of the usefulness of the problem-defining and problem-solving. I illustrate and defend my account by applying to the history of genetics. Finally, I highlight the advantages of my new functional approach over the epistemic and semantic approaches and dismiss some potential objections to my approach.

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1. Introduction

Alexander Bird (2007) distinguishes three approaches to characterising scientific progress: the epistemic approach, the semantic approach, and the functional-internalist approach. The epistemic approach defines progress in terms of knowledge. The semantic approach defines progress in terms of truth or verisimilitude. The functional-internalist approach construes progress in terms of the function of scientific practice. Correspondingly, the epistemic account of scientific progress is that an episode of history of science is progressive if it shows the accumulation of scientific knowledge. According to the semantic account, an episode of the history of science is progressive if either it shows the accumulation of true scientific beliefs or it shows increasing approximation of true scientific beliefs. According to the functional-internalist account, an episode of the history of science is progressive if it shows the success of the fulfilment of a certain function (for example, problem-solving), where the fulfilment of the function can be judged by scientists at that time. The recent debate (Rowbottom 2008; 2010; 2015; Cevolani and Tambolo 2013; Niiniluoto 2014) focuses on the epistemic and semantic approaches. The functional approach seems to be taken for granted indefensible. However, this view is unfair and unjustified. This paper aims to develop and defend a new functional approach to scientific progress. In section 2, I shall identify four problems of the traditional functional approach. In section 3, I shall propose a new functional account of scientific progress, in which scientific progress is defined in terms of the usefulness of the problem-defining and problem-solving and show how the old problems can be solved by this new approach. In section 4, I shall illustrate and defend my account by applying to the history of genetics. In section 5, I shall highlight the advantages of my new functional approach over the epistemic and semantic approaches and dismiss some potential objections to my approach.

2. The Problems of the Kuhn-Laudan Functional Approach

The most influential functional approach is first proposed by Thomas Kuhn (Kuhn 1962, 1970a), and mainly developed by Larry Laudan (1977, 1981)[1]. This approach emphasises the significance of problem-solving. Kuhn (1970b, 164) argues that the nature of scientific progress is the increase of "both the effectiveness and the efficiency with the group as a whole solves new problems." Laudan (1981, 145) is also explicit on the point that "science progresses just in case successive theories solve more problems than their predecessors." Kuhn and Laudan differ in the explication of problem-solving, though. For Kuhn (1970b, 189–91), a problem *P* is solved if its solution is sufficiently similar to a relevant paradigmatic problem-solution. For Laudan (1977, 22–23), a problem *P* is solved by a theory *T* if *T* entails an approximate statement of *P*. Nevertheless, both Kuhn and Laudan maintain that scientific progress is nothing to do with truth or knowledge if truth or knowledge is construed in a classical way. More specifically, whether a problem is solved is independent of whether the paradigmatic solution assumes any paradigm-dependent truth (for Kuhn), or whether the background theory is true (for Laudan). As the acceptance

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[1] Another representative of the functional approach is proposed and developed by Imre Lakatos. According to Lakatos (1978, 33–34), a research programme is progressive if it generates novel and well corroborated predictions. As the aim of this paper is to develop the Kuhn-Laudan functional approach, I shall not delve into the detailed discussion on Lakatos' account. However, it does not downplay its significance.

of a problem solution is determined independently of external factors like truth or knowledge, whether a progress is achieved can be judged by the scientific community itself. Thus, the central tenets of the Kuhn-Laudan functional account of scientific progress can be summarised as follows.

T1. Scientific progress is solely determined by the problem-solving power.

T2. The problem-solving power is assessed by the amount and significance of the problems solved.

T3. The problem-solving power is independent of whether the solution is true or knowledge.

T4. Scientific progress is judged and known by the scientific community.

There are two obvious problems of the Kuhn-Laudan functional approach. One is the problem of sufficiency. For Kuhn and Laudan, if a scientific community is working better and better on the effectiveness and efficiency of problem solving, it implies scientific progress. On the contrary, if the effectiveness and efficiency of problem-solving decreases, then it marks a regress in science. The problem of sufficiency can be illustrated with a thought experiment proposed by Bird (2007, 69–70). Suppose there is a widely accepted but false theory. The scientific community accumulates the solutions to the problems derived from the false theory suggests the progress, according to Laudan’s approach. However, it seems implausible for many to accept that there is an ongoing progress in science, as the false solution statements (derived from the false theory) accumulate. What is worse, the Kuhn-Laudan approach is even more problematic to account for the progress in a scientific revolution. Suppose at a later time t, the old false theory is replaced by a true theory. Bird argues that by Laudan's and Kuhn's standards[2], the theory change from the old false theory to a new true theory marks a regress, when the new theory solves fewer problems than the old one at the time t, as illustrated in the Figure 1. It is again implausible for many to accept that the shift from a false theory to a true theory is a regress rather than a progress. Therefore, Bird concludes that the upshot of the Kuhn-Laudan approach to scientific progress is that radical scientific changes in form of scientific revolutions are not progressive, and thus rejects T1.

[2] Though Kuhn's criterion of puzzle-solving is distinct from Laudan's, Bird's thought experiment is applicable to Kuhn's approach by assuming that the paradigmatic solution relies on a false universal generalization.

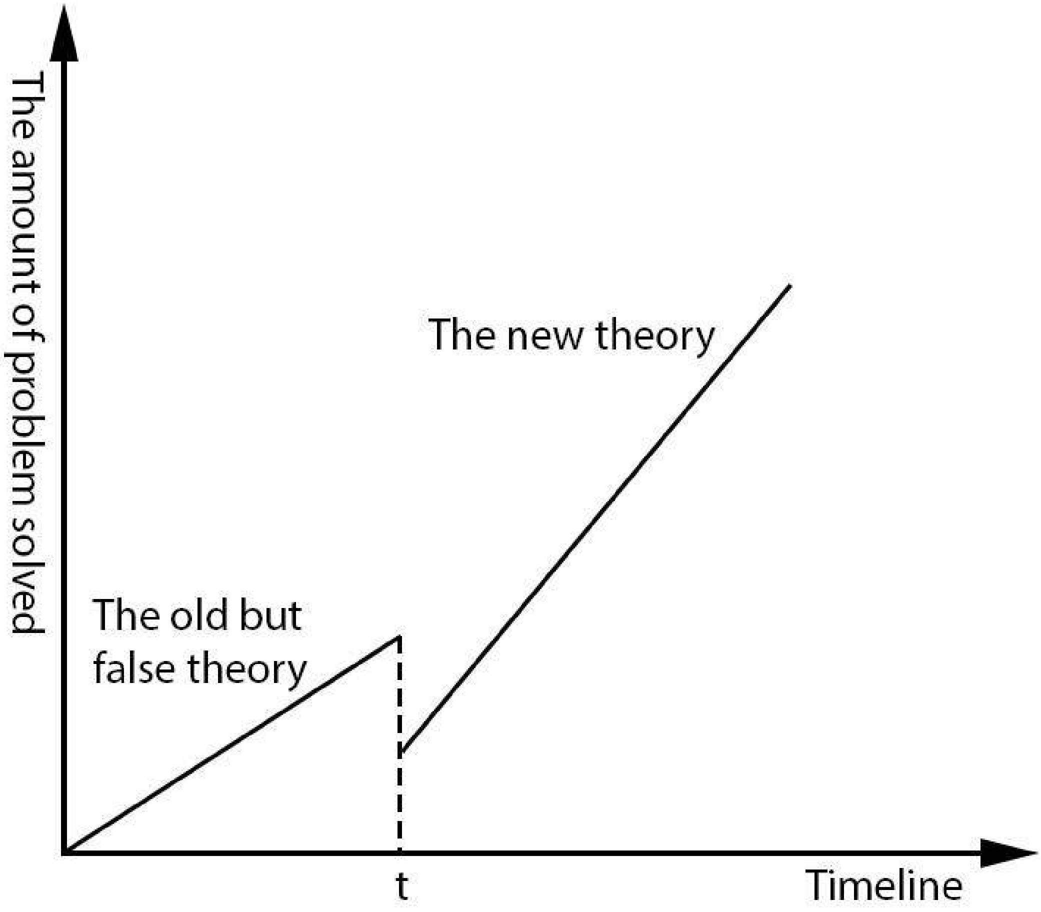

Figure 1

This conclusion is obviously not what Kuhn or Laudan would be happy to accept. Kuhn (1970b, 166) explicitly claims that "the outcome of revolution must be progress." For Kuhn, a new paradigm solves more problems than its predecessor. Thus, scientific revolutions are progressive. Nevertheless, Kuhn also well recognises the possibility that there is a loss of problem-solving power in some areas in a scientific change. Thus, it is an indispensable task for Kuhn and Laudan to work out a quantitative framework to justify that the increase of problem-solving power in some areas outweighs the loss in others. This is related to another obvious problem, namely, the problem of quantitative weighing, which is a persistent objection to the function approach (e.g. Collingwood 1965; Rescher 1984; Kleiner 1993): Is there a proper quantitative way to identify and calculate the problems of different significance? Laudan (1981, 149) proposes a straightforward formula to determine the problem-solving power: Scientific progress is achieved by maximising the number of important empirical problems solved and minimising the number of significant empirical anomalies and conceptual problems generated.[3] For example, a theory T_1 with 50 solved significant empirical problems and 10 significant empirical anomalies and conceptual problems has a better problem-solving power than T_2 with 10 solved significant empirical problems and 50 significant empirical anomalies and conceptual problems. Unfortunately, this strategy is still too oversimplified and vague to be helpful. Firstly, the significance of the empirical problems varies in degree. It is still unclear on how to evaluate the significance of the empirical problems in a quantitative way. Secondly, the significance of the anomalies is not obviously commensurate with that of the conceptual problems. Therefore, T2 is seriously challenged by its feasibility.

In addition, there is another neglected problem, namely, the problem of internalism. According to Kuhn and Laudan, a scientific community well recognises whether it is

[3] Laudan distinguishes two types of scientific problems that are designed to be solved: empirical problems and conceptual problems. For Laudan (1977, 14–17), anything about the natural world in need of explanation is in the realm of empirical problems. Why heavy objects fall towards the earth is an empirical problem. In contrast, conceptual problems are all theory-dependent. What is absolute space is a conceptual problem.

making a progress or not by examining its problem-solving power. However, this is not the case in the history of science. The long neglect of Mendel's work in the study of heredity in the second half of the 19th century is such an example. Today Gregor Mendel's paper (1865) is widely accepted as the founding document of genetics. Nevertheless, its significance was not sufficiently recognised or appreciated until 1900, even if, as William Bateson (1902) points out, Mendel's paper did provide a reliable and promising framework to solve a persistent problem in the study of heredity in the 19th century: the "inward cause" (or the mechanism) of heredity. Therefore, it seems that the community of heredity fails to recognise the progress made in Mendel's work, though it solves the problem. But why? If Mendel's work was so progressive and important for the study of heredity, how could it be ignored or neglected for such a long time? Why was its significance not recognised earlier? And more generally, if scientific progress is judged and known by the community in terms of problem-solving power, how could those who studied heredity in the 19th century neglect the progressive feature of Mendel's work? So, I argue that T4 is shown to be problematic.

Moreover, the overlook of the functional approach in the recent literature arises from an intuition that the aim of science is about knowledge or truth. As the aim of science is widely assumed to be the acquisition of knowledge or the pursuit of truth, it seems natural to define scientific progress in terms of knowledge or truth. However, the functional approach usually eliminates the role of knowledge or truth in scientific progress. Laudan (1977, 126–27), for example, explicitly indicates that neither truth nor knowledge should never be considered as the aim of science, and problem-solving is independent of truth or knowledge. Hence, the functional approach, especially T3, is somehow counter-intuitive to many.

To sum up, all the four theses of the Kuhn-Laudan functional approach are under attack. Accordingly, there are four main problems of the Kuhn-Laudan functional approach: the problem of sufficiency, the problem of quantitative weighing, the problem of internalism, and the problem of counter-intuition. Thus, it is not hasty to conclude that any attempt to develop and defend a functional approach has to solve these problems.

3. A New Functional Analysis of Scientific Progress

It seems to me that Kuhn and Laudan are correct to maintain the significance of problem-solving in scientific practice. However, problem-solving effectiveness is not the only virtue pursued by the scientists. Nor does it sufficiently account for the nature of scientific progress. In other words, along with Bird, I find T1 untenable. I suggest that problem-defining is also very important and should be another constituent of the nature of scientific progress. The well-defined research problems should be at least as important as the good solutions in scientific practice. In particular, how to define a good research problem is somehow crucial to guide the future explanatory and investigative study[4]. In the history of science, the introduction of the new research problem itself is often seen as a great scientific achievement. For instance, in *On the Origin of Species*, Charles Darwin

[4] The crucial difference between explanatory and investigative studies is that the explanatory study aims to account for some certain explanans, while investigative study aims towards an open-ended research. For a detailed explication of investigative study, see C. Kenneth Waters (2004, 793–800).

introduced many new research problems, which were never thought of or formulated before, like "How will the struggle for existence… act in regard to variation? Can the principle of selection, which we have seen is so potent in the hands of man, apply in nature?" (Darwin 1859, 80) These new research problems play a vital role to guide the further study of evolution. To this end, I propose a new functional account of scientific progress:

Science progresses if more useful research problems and their corresponding solutions are proposed.

My functional approach differs from the Kuhn-Laudan functional approach in two main aspects. First of all, I highlight the significance of problem-defining. I agree with Kuhn (1970a, 1970b) and Laudan (1977) on the point that the problems (or puzzles) are the focal point of scientific activities. Kuhn (1970b, 37) defines a puzzle as what is determined by the community to be solved, while Laudan (1977, 14) construes a problem as what is designed to be solved by theories. However, both Kuhn and Laudan implicitly assume that these problems are either simply pre-defined or defined in a straightforward way.[5] In other words, the significance of problem-defining seems not to be fully recognised. I argue that not all problems are simply defined in the ways that Kuhn and Laudan suggest. Problem-

[5] For Kuhn (1970b), puzzles are defined (or even pre-defined) relative to disciplinary matrices which assure the existence of their solutions. For Laudan (1977), empirical problems consist of unsolved problems, solved problems, and anomalous problems, He says little on how unsolved problems and solved problems are defined, and claims that anomalous problems often are generated by new observations.

defining is much more than proposing a problem. In fact, it usually consists of activities of problem-proposing (i.e. to propose an initial problem), problem-refining (i.e. to refine an initial problem), and problem-specification (i.e. to make an initial problem into more specific and practical problems). Most, if not all, of the research problems are defined, refined, and redefined in the process of problem-solving. In contrast to Kuhn's and Laudan's view that science is essentially a problem-solving activity, I argue that science consists of both problem-defining and problem-solving activities.

Secondly, my concept of problem-solving is different from Kuhn's puzzle-solving or Laudan's problem-solving. As I mentioned, Kuhn suggests that puzzle-solving is an activity of looking for a solution which is sufficiently similar to a relevant paradigmatic problem-solution, while Laudan argues that a problem *P* is solved by a theory *T* if *T* entails an approximate statement of *P*. In contrast, I do not think that the constituents of the solution to a research problem can be characterised in a monistic way. Scientists solve the problems in different ways, so it would be unwise for anyone to try to summarise some universally fundamental characteristics in their solutions. Therefore, what I would provide is rather a common recipe of a solution rather than a definition. By "a common recipe" I mean that a solution usually consists in such and such components, but definitely not exclusive. Here is my common recipe.

A solution to a research problem usually consists of the following components: a vocabulary, which is a set of the concepts employed in the problems and solutions; a set of practical guides, which specify all the procedures and methodology as means to solve the *problems; a set of hypotheses*[6]*, which are proposed to solve the problems; and a set of patterns of reasoning, which indicate how to use other components to solve the problems*[7].

Accordingly, problem-solving is a series of intertwined activities of experimentation, problem-refining, conceptualisation, hypothesisation, and reasoning. It is evident that these components are intertwined. For instance, the hypotheses are often formulated on the basis of the results of the experiments by employing the concepts in the vocabulary; the experiments are usually designed and undertaken with the purpose of solving the research problems (e.g. by testing the hypotheses); the concepts in the vocabulary are understood with the help of undertaking the experiments and applying the hypotheses, and so on.

Note that problem-defining and problem-solving are not two independent activities. Rather these are two intertwined activities. On the one hand, problem-solving is obviously dependent on research problems. On the other hand, research problems can be redefined with the process of problem-solving such as conceptualisation and hypothesisation. What is more, as Nicholas Rescher (1984) shows, solutions of problems often generate or propagate new problems.

[6] The hypotheses here should not be narrowly construed as statements or propositions. Rather I refer to "hypotheses" as all kinds of theoretical constructions made by scientists. In history, scientists use different terms to name this kind of work like "hypotheses", "assumptions", "principles", "laws", "theories", "models", "mechanisms", etc.

[7] An example of the patterns of reasoning is the hypothetico-deductive (H-D) model of confirmation, which applies an H-D model of logic to confirm a hypothesis by undertaking the experiments.

It should also be noted that more useful research problems and their corresponding solutions could be understood in two ways: (1) *more useful* problems and their corresponding solutions and (2) *more* useful problems and their corresponding solutions. What I take here is (1), the qualitative sense:

A problem and its solution is useful if and only if the way of defining and solving the research problems is repeatable, and provides a reliable framework for further investigation to solve the unsolved problems and to generate more testable research problems across more different areas (or disciplines).

The repeatability of the way of defining and solving the research problem P is a prerequisite for the recognition of its usefulness. For example, one main reason for W. F. R. Weldon (1902a, 1902b) to resist the Mendelian approach to the study of heredity was that he failed to repeat Mendel's way of distinguishing the dominating and recessive characters as a part of the solution to the problem of the transmission of morphological traits. In contrast, Carl Correns' acceptance of (the usefulness of) the Mendelian approach (1900) was based on his successful repetition of Mendel's practice, including problem-defining, conceptualisation, hypothesisation, and experimentation. Moreover, the mixed reception of the Mendelian approach in the first decade of the 20th century was also due to the different results of the application of the Mendelian approach to the problems of the transmission of characters in other species. Hugo de Vries' acceptance of the Mendelian approach (1900a, 1900b) was because that it was successfully applicable to study the transmission of the morphological traits in various plant species (e.g. *Lychnis*, *Papaver*, and *Solanum*), while the scepticism arose from the unfavourable results of the application (e.g. Whitman 1904;

McCracken 1905, 1906, 1907; Reid 1905; Prout 1907; Saunders 1907; Hart 1909; Holmes and Loomis 1909). As I shall show in greater detail in section 4, Mendel's problems and solutions introduced in his study on pea hybrid development are useful in the sense that they are repeatable in practice and provide the foundation for the 20th century study of heredity to solve the problems of transmission of the morphological traits of other species and to generate more potential testable research problems across the areas like cytology, evolution, and heredity.

In addition, it should be noted that usefulness is a relative concept. A particular set of problem-defining and problem-solving might be taken as useful by some scientific communities but not others. The Mendelian-Biometrician controversy in the first decade of the 20th century well illustrates this point. The Mendelians, led by Bateson, were optimistic on the future of the Mendelian approach to the study of heredity, while the Biometricians, led by Weldon, doubted the usefulness of Mendel's approach (especially the conceptualisation, hypothesisation, and experimentation) to study the phenomena of heredity. In other words, the Biometricians overlooked the progressive element of the Mendelian approach due to their failure of the recognition of its usefulness. Thus, the usefulness of a certain set of problem-defining and problem-solving is not obvious to a scientific community. The progress thus achieved is not judged or known by the community. It is in this sense that my approach is not internalist. This is another aspect that my functional approach differs from the Kuhn-Laudan one, according to which, scientific progress is assessed by the amount and significance of the problems solved. Thus, it is evident for the community to judge if any progress is achieved, according to Kuhn and Laudan. Therefore, T4 is not assumed for my functional approach.

I contend that this new functional approach well resolves the three main problems of the Kuhn-Laudan functional approach. Firstly, I have argued that whether science progresses depends on whether a new way of defining and solving problems is more useful than the old one. In order to determine whether there is a progress in science, one has to examine whether a new way of problem-defining and problem-solving solves any problem unsolved by the old one, and whether that new way proposes more testable problems across more areas. Given such a qualitative notion of usefulness, there is no need to look for a quantitative framework to calculate and weigh the significance and amount of the problems. Thus, the problem of quantitative weighing is inapplicable to my functional approach. Secondly, my functional approach is not internalist. Bird construes both Kuhn's and Laudan's approaches as internalist in the sense that scientific progress is only judged and known by a community, independent of any features unknown to them. However, this does not apply to my approach. The usefulness of problem-defining and problem-solving is not straightforwardly recognisable by the scientific community, as I just illustrated in the Mendelian-Biometrician conteroversy. Thirdly, I contend that by adding problem-defining as an essential constituent of scientific progress, the problem of sufficiency is resolved.[8] In the next section, I shall show how my new functional approach is applied to illustrate and explain the progress in the history of early genetics.

[8] The problem of counter-intuition will be discussed in section 5.

4. Case Study: How Early Genetics Progressed

Nobody denies that genetics has progressed greatly since Darwin's proposal of the theory of pangenesis. From a close reading of Darwin's, Mendel's, de Vries', Correns', and Bateson's writings, I argue that the progress in the origin of genetic could be well illustrated and explained as a process of the increase of the usefulness of new research problems and their solutions.

The study of heredity in the 19th century was galvanised by Darwin's study of evolution, as he was looking for a mechanism of heredity to support his theory of evolution by natural selection. To start, Darwin (1868, 357) introduced a set of the problems of heredity to be solved:

> DP1. How is it possible for a character possessed by some remote ancestor suddenly to reappear in the offspring? (The Problem of Atavism)
>
> DP2. How can the effects of increased or decreased use of a limb be transmitted to the child? (The Problem of Inheritance of Acquired Characteristics)
>
> DP3. How can the male sexual element act not solely on the ovule, but occasionally on the mother-form? (The Problem of Sexual Influence)
>
> DP4. How a limb can be reproduced on the exact line of amputation, with neither too much nor too little added? (The Problem of Regeneration)
>
> DP5. How are the various modes of reproduction are connected? (The Problem of Reproduction)

Correspondingly, Darwin (1868, 374) also introduced the concept of gemmule and the hypotheses of pangenesis as the central constitute of the solutions to his problems of heredity:

> DH1. Cells propagate by self-division or proliferation to retain the same nature to become converted the tissues of the body.
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> DH2. Gemmules are the basic unit of hereditary material.
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> DH3. Gemmules are thrown off from cells, circulate freely throughout the organism, and become developed into cells again when supplied with proper nutriment during all the stages of development.
>
> DH4. Gemmules are transmitted from generation to generation and generally developed in the immediate successive generation.
>
> DH5. Some gemmules are in the dormant state in many generations and then developed.

Darwin (1868, 357) contended that these hypotheses were useful to solve his problems of heredity to explain both the sexual and asexual reproduction by "bringing together a multitude of facts which are at present left disconnected by any efficient cause." Although Darwin's hypotheses of pangenesis were heavily criticised by his contemporaries, his problems and solutions still marked a progress in the study of heredity by providing a useful, though imperfect or unsatisfactory, framework. In particular, Darwin's problem-defining and problem-solving is useful to classify the phenomena of inheritance and to complement his study of evolution. The explanation of atavism in terms of dormant germmules is a clear case.[9] Darwin's pangenesis approach was further developed by de Vries (1889). His main modifications, including the introduction of the distinction between active and latent pagens, the relocation of pagens, and the revision of the transportation of pagens, are conceptual. However, a persistent difficulty for both Darwin's and de Vries' theories was that there was no reliable way to test the hypothesis of pangenesis experimentally. (The problem of testability) Such a dilemma remained until 1900.

At the turn of the 20th century, Mendel's theory of hybrid development (1865) was introduced to be incorporated into a Mendelian theory to replace the theory of pangenesis (Darwin 1868; de Vries 1889) to account for the mechanism of heredity. The crucial progress made by Mendel's theory is, as Bateson (1902) points out, that it provides a reliable and testable way to study the "essential nature" of heredity. As I mentioned, the theorists of heredity at the time, especially de Vries, were struggling to test their theories experimentally. Bateson insightfully recognised that Mendel's approach would be useful to solve this problem. Mendel (1865, 4, 7) introduced a set of new research problems of transmission of morphological traits of peas by focusing on the paired traits in the successive generations.

[9] The phenomenon of atavism is explained by DH5 in the sense that some gemmules are in the dormant state and remain undeveloped in many generations.

mP1. How could one determine the number of different forms in which hybrid progeny appear, permit classification of these forms in each generation with certainty, and ascertain their numerical interrelationship?

mP2. What are the changes for each pair of differing traits in the offspring of *Pisum*? Or, what is the law deducible from the changes for each pair of differing traits selected in the successive generations?

Moreover, Mendel introduced various hybridisation experiments, the concepts of dominance and recessiveness, and the hypotheses (i.e. Mendel's law of pea development, law of combination of differing traits, and law of composition of hybrid fertilising cells) to solve the problems.

mH1. Of the seeds formed by the hybrids with one pair of differing traits, one half again develop the hybrid form while the other half yield plants that remain constant and receive the dominating and the recessive character in equal shares. (The law of development concerning a pair of differing traits)

mH2. The progeny of hybrids in which several essentially different traits are united represent the terms of a combination series in which the series for each pair of differing traits are combined. And the behaviour of each pair of differing traits in a hybrid association is independent of all other differences in the two parental plants. (The law of combination of differing traits) mH3. Pea hybrids form germinal and pollen cells that in their composition correspond in equal numbers to all the constant forms resulting from the combination of traits united through fertilisation. (The law of composition of hybrid fertilising cells)

Mendel's research problems and solutions provide an exemplary framework to study the mechanism of heredity experimentally. Thus, the progress marked by the shift from Darwin's approach to Mendel's approach is well characterised by the increase of the usefulness of the research problems and the solutions. Mendel's way of defining and solving the problem of transmission of morphological traits of peas by means of problem-defining, conceptualisation, hypothesisation, and experimentation was successfully repeated by Correns (1900) and Erich von Tschermak (1900a, 1900b). What is more, Mendel's problems of transmission of the differing traits of peas and laws were used to solve the problem of testability, unsolved by Darwin and de Vries. While many of Darwin's hypotheses are difficult to be tested experimentally, Mendel's mH3 as a hypothesis of the mechanism of heredity is well experimentally testable and confirmable. In addition, Mendel's laws and experiments were successfully applied to predict and explain the distribution of the differing morphological traits in the successive generations in various plant species[10] by de Vries (1900a, 87, 1900b, 846, 1903, 151) and Correns

[10] The plant species displaying the Mendelian ratio in the hybridization experiment include *Agrostemma Githago*, *Amarantus caudatus*, *Aster Tripolium*, *Calliopsis tinetoria*, *Chelidonium majus*, *Chrysanthemum eoronarium*, *Clarhia pulchella*, *Corepis tinctoria*, *Datura Tabula*, *Hyosoyamus niger*,

(1900, 160). Moreover, Mendel's approach generated more interdisciplinary and testable problems than the pangenesis approach. On the one hand, Mendel's problems and solutions provide a framework which is more experimentally testable than Darwin's. On the other hand, Mendel's problems and solutions are more interdisciplinary as they encompass the study of development, heredity, and hybridisation. Thus, I argue that the progress made by Mendel's work is his more useful way of defining and solving the research problems.

Based on Mendel's approach, de Vries, Correns, and especially Bateson were developing the Mendelian approach to heredity by introducing new research problems, new concepts (e.g. anlage, unit-character), new hypotheses (e.g. de Vries' law of segregation, Correns' Mendelian rule, Bateson's Mendelian principles), and new experiments. The two main problems identified by the Mendelian approach were:

> MP1. What offspring are to expect if two germ-cells of dissimilar constitution unite in fertilisation? (de Vries 1900b, 845; Bateson 1902, 18–19)
>
> MP2. Are the Mendelian principles universally applicable? (de Vries 1900b, 846–47; Correns 1900, 168; Bateson 1902, 33)

Compared with Mendel's approach, the Mendelian approach is more progressive in several aspects. Firstly, the concept of anlage (Correns 1900) and the concept of unit-character (Bateson 1902), by incorporating contemporary knowledge of cytology, provide a better

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Linaria vulgaris, *Lychnis diurna*, *Lychnis vespertina*, *Oenothera Lamarckiana*, *Papaver somniferum Mephisto*, *Solanum nigrum*, *Trifolium pratense*, *Veronica longifolia*, *Viola cornuta*, *Zea Mays*.

understanding of the physical location of the bearer of hereditary material than the concept of cell-type (Mendel 1865). Cell-type is an inter-cellular element, while anlage and unit-character refer to some intra-cellular material within the nuclei, which are experimentally corroborated later. Secondly, de Vries' law of segregation (1900b), Correns' Mendelian rule (1900), Bateson's Mendelian principles (1902) were successfully applied to reinterpret and explain the hereditary pattern of more species, especially in human pedigrees. (Gossage 1908; Lundorg 1912, 1920; Nettleship 1909) Thirdly, the Mendelian approach provided a framework to generate better defined research problems of heredity across the areas of heredity, hybridisaiton, animal breeding, cytology, evolution, and even human medicine.

Therefore, now I can conclude that the development in the study of heredity from Darwin to Bateson can be well characterised as the progress in terms of the increase of the usefulness of problem-defining and problem-solving.

5. Beyond Knowledge, Truth, and Intervening

There is still one more problem, namely, the problem of counter-intuition, yet to be discussed. As I mentioned in section 2, the functional approach is somehow neglected in the recent debate for its conflict with the intuition that scientific progress is about knowledge and/or truth. However, I argue that my functional approach can be compatible with this intuition. Firstly, the usefulness of problem-defining and problem-solving could be well explained in terms of knowledge if knowledge is not merely construed as something propositional or theoretical. Knowledge is traditionally classified into know-that and know-how.[11] The way of defining and solving the problems is a clear case of know-how. Thus, that more useful problems and their solutions are proposed could be understood in the sense that more useful know-how is obtained.

Secondly, the usefulness of problem-defining and problem-solving is also explicable in terms of truth to some extent. In particular, it is well explained by the "contextualist" theory of truth (Chang 2012; Massimi 2018). Michela Massimi (2018), for example, proposes that truth in the context of scientific practice should be defined in a perspectival way.

> Knowledge claims in science are [perspective-dependent] when their *truth-conditions* (understood as rules for determining truth-values based on features of the context of use) depend on the scientific perspective in which such claims are made. Yet such knowledge claims must also be assessable from the point of view of other (subsequent or rival) scientific perspectives. (Massimi 2018, 354)

If truth is defined in this perspectival way, then the increase of the usefulness of problem-defining and problem-solving implies the increase of the unsolved problems with more

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[11] Jason Stanley and Timothy Williamson (2001) famously rejects this distinction by arguing that know-how is reducible to know-that. Whether there is a genuine distinction between how-that and know-how, my point still holds. Science does not only tell us something theoretical which can be formulated in the propositions, but also tell us something practical, whether which can be reformulated in the propositions or not.

useful hypotheses as the solutions. The Mendelian approach to the study of heredity, for example, generates more confirmable hypotheses (e.g. the law of segregation) and factual knowledge (e.g. the summary of the transmission of morphological traits of various plants). All these hypotheses and factual knowledge are true according to its perspective (i.e. the Mendelian approach) by means of experiments, while they are also assessable from the point of view of the subsequent scientific perspective (e.g. the Morgan approach) by new ways of experimentation. Therefore, more useful problems and their solutions are proposed could be interpreted as more perspective-dependent true knowledge claims are attained. [12]

Moreover, I argue that my functional approach is better than the epistemic and semantic approaches in one significant aspect. The epistemic and semantic approaches to scientific progress pay too much attention to theoretical achievements. However, such approaches overlook the significance of the non-theoretical aspect of science. As Heather Douglas (2014, 56) points out, science is not just about theory, and thus scientific progress should be examined in both theoretical and non-theoretical aspects. One advantage of my functional account is to highlight the significance of the non-theoretical aspect of scientific progress. Scientific practice is much more than theorising or modelling. It would be surprising if the introduction of new research problems and the improvement of the experimental methods and devices are excluded from the constituents of scientific progress. Reconsider the case of the origin of genetics. It seems plausible to argue that the progress

[12] It should be highlighted that the notion of usefulness can be explicated by the contextualist theory of truth does not imply that my functional approach assumes a contextualist theory of truth. It does not eliminate the possibility that it can also be explicated by other theories of truth.

made by Mendel was to propose the law of composition of hybrid fertilising cells to advance our knowledge of the mechanism of heredity. Similarly, de Vries' law of segregation, Correns' Mendelian rule, and Bateson's Mendelian principles provide a better knowledge of heredity than Mendel's law. Yes, we knew more and more about the mechanism of heredity with the theoretical development from Darwin to Bateson. However, it is definitely not the only aspect of the progress achieved in the study of heredity in that period. We learnt more and more on how to define a good research problem, how to design and undertake the experiments, and how to use the problems and experiments to study the mechanism of heredity in a better way. All of these cannot easily be accounted for in terms of theoretical knowledge by the epistemic approach or in terms of truth by the semantic approach. The non-theoretical aspect of scientific progress should be taken into account as the theoretical aspect. Thus, I contend that my functional approach provides a better account for the non-theoretical aspect of scientific progress.

Now there are two potential objections to my argument that my functional approach is better than the epistemic and semantic ones. Firstly, as my reply to the problem of counter-intuition involves the notions of truth and knowledge, one may wonder whether my approach can be still classified as "functional" rather than epistemic or semantic. It seems that my functional approach can be reinterpreted in the way that science progresses if more and more know-how is attained, or that science progresses if more perspective-dependent truths are obtained. Secondly, one may argue that the overlook of the non-theoretical aspect of scientific progress is not a serious problem for the epistemic or the semantic approach. There is lots of know-how in conceptualisation and experimenation, but this kind of progress is typically treated as secondary. The theoretical aspect of scientific progress is more fundamental as it explains the non-theoretical aspect.

In response to the first potential objection, I argue that my functional approach is not reducible to the epistemic or the semantic approach. As I have pointed out, the solution to a research problem is not something purely theoretical or propositional. There are some indispensable non-theoretical aspects. Mendel's solution to mP1 and mP2, for example, consists of a series of practical guide on how to design and undertake the experiments. Accordingly, I do not see that there is any true or correct solution to a research problem, given its practical nature. For example, it is implausible to claim that there is a true or correct way of experimentation or problem-refining. Moreover, I would like to highlight that my approach aims to capture the different aspects of scientific progress, but it does not imply that the functional, epistemic, and semantic aspects constitute the nature of scientific progress in equal shares. Rather more know-how (the epistemic aspect) and more well-corroborated hypotheses (the semantic aspect) may only partially constitute the usefulness of problem-defining and problem-solving (the functional aspect). It is in this sense that my approach is functional rather than epistemic, semantic, or integrated.

Regarding the second potential objection, I have to emphasise that the theoretical aspect of the progress is in no sense more fundamental than the practical one. Hypothesisation is mutually intertwined with and cannot be independent of other activities of problem-defining and problem-solving. The theoretical aspect of scientific progress is achieved with the non-theoretical aspect. Mendel's work cannot mark any progress without his novel problem-defining, conceptualisation, and experimentation, which cannot be completely accounted for in terms of knowledge or truth. Therefore, I argue that, as the non-theoretical aspect of scientific progress is indispensable and irreducible, the neglect of the non-theoretical aspect of scientific progress is a serious problem for the epistemic and the semantic approaches. Neither the epistemic nor the semantic approach provides a complete account of scientific progress. In addition, even if the usefulness of problem-defining and problem-solving can be understood in terms of knowledge or truth to some extent, the mere accumulation of scientific knowledge or the approximation of scientific truths, which do not contribute to the increase of the usefulness of problem-defining and problem-solving, do not count as scientific progress. I argue that the accumulation of scientific knowledge or the approximation of scientific truth is usually a result rather than an indicator of scientific progress. Therefore, it is in this sense that my approach is fundamentally functional rather than epistemic or semantic.

It should be noted that Douglas (2014, 62) proposes a functional account of scientific progress in terms of "the increased capacity to predict, control, manipulate, and intervene in various contexts." There are some similarities between Douglas and my approach. Both Douglas' and my approach highlight the practical aspect of scientific success. Both admit that scientific progress is not a concept which can be easily quantified. At first glance, the origin of genetics from Darwin to Bateson could also be explained and characterised in terms of increasing capacity to predict, control, manipulate, and intervene. Mendel's approach has a more increased capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of *Pisum* than Darwin's, and the Mendelian approach has an even more increase capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of various species than Mendel's. However, I have to highlight that there are some substantial differences between Douglas' and my approach (2014, 62). Compared with my approach, Douglas' approach overlooks the significance of the theoretical aspect of scientific progress to some extent. Not all the theoretical progress is adequately reflected in the increase of the capacity to predict, control, manipulate, and intervene. De Vries, for example, spent two decades developing Darwin's theory of pangenesis. Although it is still controversial if de Vries' approach has any increased capacity to predict, control, manipulate, and intervene the transportation of pagens than Darwin's, it is undeniable that de Vries made substantial conceptual revisions of the pangenesis theory to accommodate the development of cytology at the time. It is implausible to argue that de Vries' approach to the problem of heredity was not progressive, especially given that it did advance the understanding of the physical location of hereditary material by identifying that the pagens are located in the nuclei of cells. In addition, Douglas' approach pays insufficient attention to the significance of the introduction of new research problems, concepts, hypotheses, and experiments in practice. It seems plausible to characterise the progress made by Mendel's approach as the increased capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of *Pisum*, but such an increased capacity is achieved by Mendel's way of defining new research problems, introducing new concepts (e.g. dominance), proposing new hypotheses (e.g. mH1, mH2, and mH3), and designing and undertaking new experiments. Douglas might argue that Mendel's problem-defining and problem-solving consist in the capacity. Nevertheless, I argue that Mendel's problem-defining and problem-solving are more fundamental to account for the nature of scientific progress, as it is that Mendel's problem-defining and problem-solving underlie the capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of *Pisum*. It is Mendel’s problem-defining and problem-solving rather than Mendel’s capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of *Pisum* that guides de Vries’, Correns’, and Bateson’s work on heredity. Thus, the increase of the usefulness of Mendel's problem-defining and problem-sovling implies the increase of the capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of *Pisum* rather than that the increased capacity to predict, control, manipulate, and intervene the transmission of the morphological traits of *Pisum* implies the increase of the usefulness of Mendel's problem-defining and problem-solving.

Before finishing the section, I would like to ward off a common misunderstanding of the functional approach to scientific progress. Bird (2007), for example, regards accepting a functional approach as rejecting scientific realism or endorsing antirealism. However, I argue that a functionalist on scientific progress does not have to an anti-realist. My functional approach is neutral to the scientific realism/antirealism debate. Bird argues that the acceptance of the pessimistic metainduction argument and the rejection of transcendental truth are two sources of the functional approach. Nevertheless, the acceptance of the pessimistic metainduction argument and the rejection of transcendental truth do not necessarily imply an anti-realist position. John Worrall’s structural realism (1989) is such a good example. Worrall is explicit on the point that he accepts the historicist challenge raised by the pessimistic metainduction argument, but it does not undermine his realist position. Both realists and antirealists can take the functional approach to scientific progress, though they may differ in making the ontological commitment to the hypotheses. In addition, I do not think that my functional approach to scientific progress is dependent on the acceptance of the pessimistic metainduction argument and the rejection of transcendental truth. What the pessimistic metainduction rejects is a cumulative and convergent picture of the development of science. None of the epistemic, the semantic, or the functional approach implies a particular historiography of science. What my functional approach, as well other approaches, provides is an account of the nature of scientific progress. Similarly, how to define truth is not an indispensable task for my functional approach. In other words, my functional approach is neutral to the definition of truth. Nor does it endorse or reject any particular theory of truth.

6. Conclusion

In this paper, I have argued that the functional approach should not be taken for granted as indefensible. I propose that scientific progress is defined in terms of the increase of the usefulness of problem-defining and problem-solving with the illustration of the history of early genetics. I contend that this functional approach provides a fuller analysis of scientific progress, compared with the epistemic and semantic approaches.

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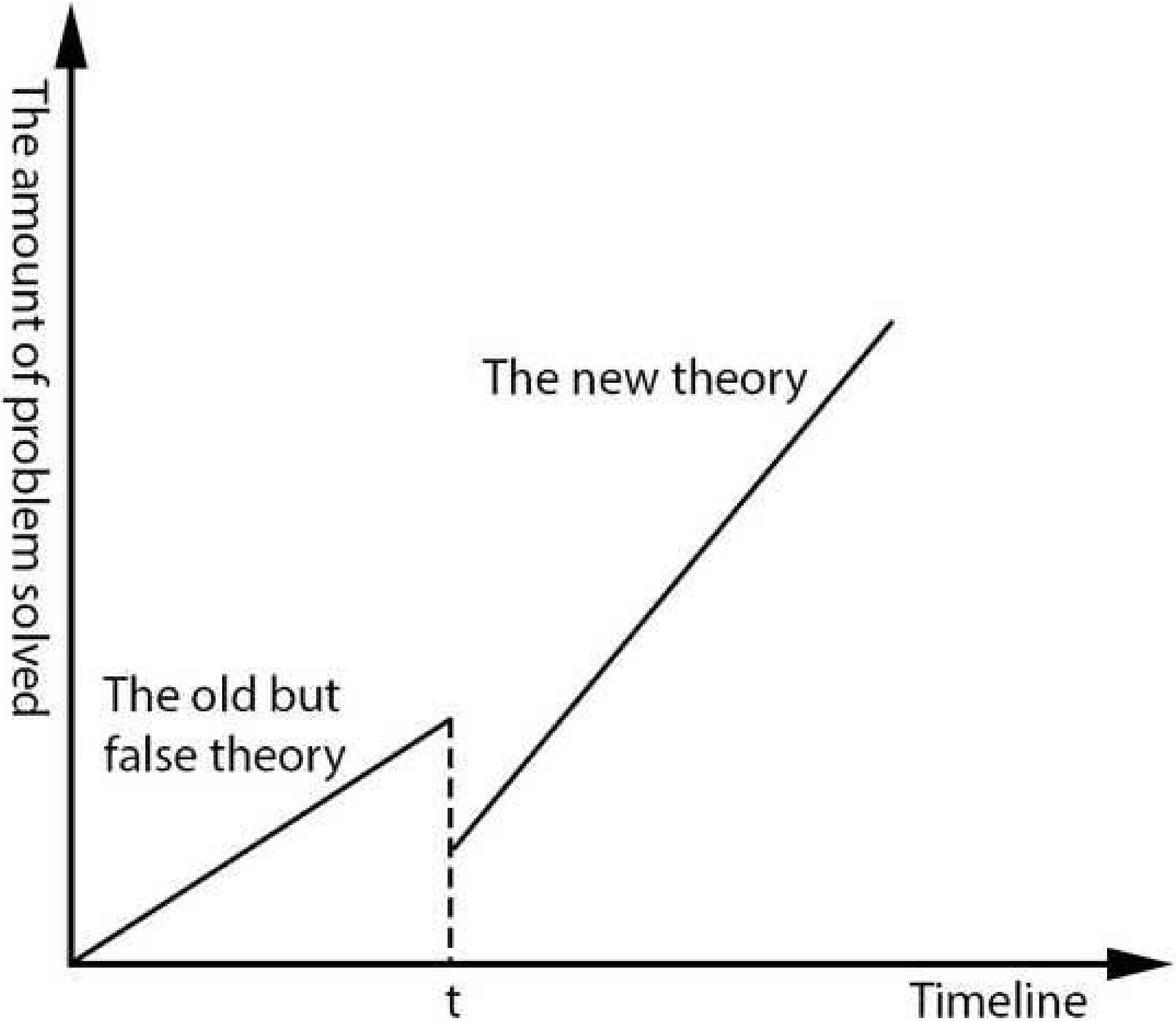
The amount of problem solved
The new theory
The old but
false theory
t
Timeline

Figure 1