Experimentation versus Theory Choice: A Social-Epistemological Approach

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1. Introduction

The question of how scientists choose theories from a set of alternatives is an old one. Traditionally, it was assumed that there are *decision rules* or *methodological principles* of one sort or another that guide such choices. Furthermore, it was assumed that when a *community* of scientists chooses a theory, this must amount to the application of these rules or principles by each *individual* scientist in the community, or at least by a majority of them. I am at a loss for a good name that ends with "-ism" for the first view, but the second is described well by "methodological individualism". Both views prevail to this day, mutually supporting each other, so it is it time to question them and also to think about alternative views.

In my view, the strongest challenge to both of these views still comes from Thomas Kuhn. In the well-known postscript to *The Structure of Scientific Revolutions*, Kuhn wrote:

> There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision. In this sense it is the community of specialists rather than its individual members that make the effective decision (Kuhn 1970, S. 200).

The reason for this non-existence of a systematic decision procedure according to Kuhn is that there exist different criteria for theory choice and these criteria tend to pull into different directions. For example, one theory may be simpler than another, but at the same time less predictively accurate, e.g., like the Copernican and Ptolemaïc world systems initially. Kuhn has coined the term "incommensurability" for this kind of inter-theoretic relation (see Hoyningen-Huene 1993, § 6.3, for the development of Kuhn's view of incommensurability). The chief feature of incommensurable theories from a methodological point of view is the fact that rival theories may all have their merits and their shortcomings, but it is not possible to *weigh* these and aggregate them into a single measure. Kuhn (1977, p. 321f.) cites a standard set of criteria for theory appraisal, namely **accuracy**, **consistency**, **broad scope**, **simplicity** and **fruitfulness**. He then argues that in most interesting cases of rival scientific theories, these criteria do not determine a unique choice. The reason is that individuals in a community of scientists may weigh these criteria differently and thus arrive at different choices with respect to theories. Since Kuhn observed that scientists do sometimes *do* choose between different theories or frameworks, this leads him to a second claim, that such choices are made by the *community*:

> In the absence of criteria able to dictate the choice of each individual, I argued, we do well to trust the collective judgment of scientists trained in this way. "What better criterion could there be," I asked rhetorically, "than the decision of the scientific group?" (Kuhn 1977, p. 320f.)

This passage even suggests, in addition to the rejection of what I called methodological individualism, that Kuhn considers such collective choices to be *rational*. It is the combination of these claims of that I would like to focus on in this paper. My central question is how *communities* of scientists *rationally* decide between alternative theories or frameworks, which, according to Kuhn, seems to be something that is impossible at the *individual* level. Scientific rationality emerges at the community level; this view makes Kuhn a social epistemologist.[1] However, there has been little discussion of how this remarkable feat should be possible, either within Kuhn's framework or within the framework of other social epistemologies. This paper aims at precisely such a discussion. However, my discussion will not address the issue in its full generality; it will be restricted to experimental science, in particular experimental biology.

I shall proceed as follows. In the next Section, I would like to discuss an interesting reconstruction of Kuhn's argument in terms of social choice theory, which is due to Samir Okasha (forthcoming). This reconstruction renders the problem in sharper terms. In the Third Section, I shall discuss two more recent attempts to defend scientific communities as the proper agents of scientific decision-making. Such attempts are typically part of social epistemologies. In the Fourth Section, I shall suggest a *social mechanism* for the selection of a theory or framework by scientific communities that is inspired by what I take to be Kuhn's view. The Fifth Section will address the question of *what* exactly is selected. For it is not clear that theories alone are the appropriate units that are being selected. This will be illustrated on the example of experimental biology. Section 6 summarizes my conclusions.

2. Okasha's Reconstruction of Kuhn Using Social Choice Theory

Okasha (forthcoming) reconstructs theory choice problems as social choice problems. Traditionally, social choice problems involve a social group of people with different preference rankings. The problem is then to find a way of aggregating or integrating these preferences such that the group as a whole chooses from a selection of alternatives on the basis of the individuals' preferences. In general, such problems are only well-defined if certain

[1] Kuhn has no need for postulating collective epistemic subjects, as some authors do (e.g., Gilbert 1987). Neither do I, at least for the argument of this paper.

constraints are specified. These constraints are typically rules that any acceptable solution to a choice problem must satisfy. In Kenneth Arrow's (1963) classic treatment, the following rules are supposed to constrain the space of rational solutions:

(**U**) **unrestricted domain**: any individual preference ranking is admissible as an input into the social choice

(**P) weak Pareto**: If all individuals in the group prefer x over y, then the group as a whole should also prefer x over y

(**N**) **non-dictatorship**: there cannot be an individual whose preference ranking with respect to any two alternatives determines the group's choice with respect to these alternatives. In other words, no-one's preferences can trump or over-ride what the group as a whole chooses

(**I**) **independence of irrelevant alternatives**: this conditions says that the group's choice between alternatives x and y can only depend on the individuals' preferences with respect to x and y, not on their preferences with respect to other alternatives. If I want my community to build a new concert hall rather than a theatre, then the group's preference of a concert hall over a casino or vice versa can only depend on my preference between these two options and not, say, my preference of a theatre over a casino.

The problem now is to find a preference ranking for the whole group that satisfies these four constraints. Arrow has examined the conditions under which this kind of problem is solvable. A famous result is *Arrow's impossibility theorem*. It states that there is no way of integrating so-called *weak* preference orderings under the four above-mentioned conditions for more than two alternatives. These are preference orderings such that ties are permitted, i.e., individuals are allowed to be indifferent with respect to alternatives.

Could Arrow's theorem possibly be relevant to theory choice problems in science? Okasha suggests a scenario how they might. He first construes Kuhn's different criteria that are supposed to inform theory choices as *individuals*. Under this construal, each of these criteria will return a preference ranking of a set of alternative theories. Thus, simplicity might favor theory 2 over theory 1 and 3, while accuracy prefers theory 3 over 1 over 2. Consistency could rank all at the same level, while fruitfulness prefers theory 1 over 3 over 2, and broad scope as well. Arrow's theorem now says that there is no algorithm or decision procedure that obeys the conditions unrestricted domain, weak Pareto, non-dictatorship and independence of irrelevant alternatives and that returns an unique overall ranking for the three theories, taking into account how Kuhn's values rank them. It should be noted that the five rankings are associated with individual *criteria*, not individual *scientists* in Okasha's reconstruction of the problem. Thus, it is not strictly a social choice problem in Okasha's account. It is merely an application of social choice *theory* to a structurally similar kind of decision problem. Nonetheless, perhaps it could transformed into a *bona fide* social choice problem by assuming that individual scientists rank theories by applying only one of Kuhn's values. Thus, I could be Mr. Simplicity (obviously), while you are Mr. or Mrs. Accuracy or Mr. or Mrs. Consistency (whichever you prefer!) This would mean that I only look how simple theories are when ranking them, while you only look at how accurate or how consistent they are. But such a social choice scenario is not necessarily a part of Okasha's argument.

Could this be a formal proof for Kuhn's incommensurability thesis by application of Arrow's impossibility theorem? This is not quite Okasha's goal, even if he sees important parallels. First of all, it should be noted, as Okasha does, that Kuhn's claim is not that there is *no* algorithm that can yield a unique choice, there are *many*. Thus Kuhn's claim is different, which, I suppose, is why Okasha's paper is subtitled "Kuhn *versus* Arrow". These differences notwithstanding, Kuhn's thesis (many algorithms) and the Okasha-Arrow thesis (no algorithm) seem just as devastating for the possibility of rational theory choice, at least if there is no way of overcoming these difficulties. Fortunately, there is, at least for experimental sciences, as I will try to demonstrate.

Second, Arrow's theorem does not apply if there are only two alternatives. Perhaps there is a reason why the grand historical debates in science are typically about two alternative theories or frameworks (e.g., Copernicus vs. Ptolemy, phlogiston vs. oxygen chemistry, relativistic vs. classical mechanics or quantum theory vs. classical mechanics). Third, Arrow's theorem only applies to an *ordinal* ranking of theories, in other words, a ranking that does not contain any information about the *strength* of the preferences or the differences in preference. This is good news for Bayesian confirmation theorists and other kinds of formal epistemologists. For according to Bayesianism, theories are not merely ranked ordinally by scientists, they receive a probability value. Arrow's theorem does not apply to such a quantitative ranking. Okasha (forthcoming) suggests various ways of how the Arrovian impossibility result can be avoided, the Bayesian approach being one such way.

I would like to suggest an altogether different way out of the Kuhn-Arrow-Okasha predicament concerning theory choice.

I begin by pointing out that in scientific decision-making, it is not at all obvious that Arrow's condition of **non-dictatorship** applies or ought to apply. While in democratic decision-making, which is what Arrow was concerned with, there are good reasons for giving all individuals equal weight when aggregating their preferences, there is no reason why science should be committed to weigh all theory choice criteria equally. As for Kuhn himself, there are indications that he granted **fruitfulness** a special role. Here are two relevant passages that support this view:

> At the start, a new candidate for paradigm may have few supporters […]. Nevertheless, if they are competent, they will improve it, explore its possibilities, and show what it would be like to belong to the community guided by it. And as that goes on, if the paradigm is one destined to win its fight, the number and strength of persuasive in its favor will increase. More scientists will then be converted, and the exploration of the new paradigm will go on. Gradually, the number of experiments, instruments, articles and books based upon the paradigm will multiply. Still more men, convinced of the new view's **fruitfulness**, will adopt the new mode of practicing normal science, until at least only a few elderly hold-outs remain (Kuhn 1970, p. 159; emphasis mine).

This revealing passage suggests that the reasons why an *individual* scientist joins a new paradigm (and thereby, a new theory, see below for more on this) may vary. That is, which of the five criteria for theory choice (if any) influence an individual choice varies, as does the weight that any of the criteria may have. Kuhn captures this aspect of theory choice by suggesting that it's *values* rather than *rules* or *algorithms* that form the basis for such choices (Kuhn 1977, p. 330f.). The difference is that, while the latter determine a unique choice, the former are subject to interpretation and judgment. I take it that such interpretation and judgment also include a weighing of criteria, should these be in conflict with respect to a specific choice. At any rate, **simplicity**, **accuracy**, **consistency**, **broad scope** and **fruitfulness** appear as values that *influence*, but do not *determine* theory choice at the *individual level*.

But things look entirely different when it comes to the *community level*. The passage from *The Structure of Scientific Revolutions* cited above suggests that what the community chooses is the paradigm that is most prolific in turning out "experiments, instruments, articles and books based upon the paradigm". These are precisely the marks of a fruitful paradigm, and I claim that Kuhn's position is that fruitfulness ultimately dictates the community choice.

An obvious objection to this view (whether or not it's actually Kuhn's) must be addressed right away. Doesn't this view put the cart before the horse in suggesting that fruitfulness alone informs community choices? Paradigms don't reproduce like rabbits do (i.e., all by themselves), it's the *scientists* who proliferate it *by means of* "experiments, instruments, articles and books based upon the paradigm". And don't they reproduce a paradigm-associated theory only if it satisfies the criteria of theory choice? Well, not according to Kuhn they don't. On his account, the grounds for judging a new puzzle solution as successful or unsuccessful are provided *directly* by the *similarity relations* to the exemplary problem solutions (Kuhn 1970, p. 45). Rules or criteria of theory choice such as our five candidates are simply irrelevant for judging something a successful puzzle solution in line with a specific paradigm.[2]

Thus, if Kuhn is right, a choice among incommensurable frameworks or theories is possible on the basis of the *problem-solving capacity* of these frameworks. In other words, problem-solving capacity, which I take to be the same as **fruitfulness**, can be viewed as dictator in

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[2] Of course, there may be a standard of **accuracy** associated with a paradigm, but such standards are highly variable. It is worth noting that judgments about the success or failure of problem solutions are only possible from *within* the paradigm, as it were. Outsiders who have not acquired the salient similarity relations cannot judge whether or not some problem solution conforms to the paradigm. This is a result of Kuhnian incommensurability, which precludes translation into the lexicon of another theory. However, it is always possible to introduce outsiders to the practice such that they acquire this competence.

theory choice according to Kuhn. This, in my view, is the main reason why Arrow's theorem is not an obstacle to rational theory choice. At the same time, this is the main reason why Kuhnian incommensurability does not imply incomparability.

I find Okasha's analysis helpful because it forces us to reflect on the conditions that constrain theory choice situations. However, I would like to press a different construal of theory choice as social choice. In doing so, I shall remain true to Kuhn's idea that the choice of theories or frameworks does not boil down to choices made by individual scientists, but by the whole community. In other words, even if problem-solving capacity or fruitfulness is the criterion that dictates the choice of a research framework in science, it is not the case that each and every scientist, or a majority of them, make this choice deliberately. Instead, I will suggest there is a *social* mechanism that effects this choice. This mechanism will be the subject of Section 4. But first, I will draw out a connection to other views in social epistemology.

3. Social Groups as Agents of Scientific Decision-making

Some social epistemologists argue that groups are proper subjects, or even the *only* proper subjects, of knowledge. There are different kinds of social epistemology, some are veritistic or truth-directed, others not. Furthermore, we can distinguish between approaches that accord a central role to *deliberation* and approaches that don't. An example of the first kind is the social epistemology of Helen Longino (Longino 2002). On Longino's view, a group can be said to know something if it has engaged in a suitably organized process of deliberation. "Suitably organized" means that the group must be committed to certain procedural norms such as providing public forums of criticism and temperate equality of intellectual authority.

Her approach is perhaps best encapsulated in her definition of *epistemic acceptability*, which plays the role that justification has in standard epistemologies:[3]

> Some content *A* is epistemically acceptable in community *C* at time *t* if *A* is supported by data *d* evident to *C* at *t* in light of reasoning and background assumptions which have survived critical scrutiny from as many perspectives as are available to *C* at *t*, and *C* is characterized by venues for criticism, uptake of criticism, public standards, and tempered equality of intellectual authority (Longino 2002, p. 135).

It should be evident in this account that Longino sees a major role for deliberation in scientific decision-making; for what it means for some content to "survive critical scrutiny" is to be accepted by the group after an exchange of arguments. Longino does not specify how we should think of group acceptance, whether this involves acceptance by a majority, or if group acceptance is something over and above the acceptance by individual members (as, for example, Gilbert 1987 argued). At any rate, it has remained controversial as to whether deliberation really plays a role in the choice of theories by scientists.[4]

An example of a social epistemology that does not see deliberation as constitutive for knowledge is Miriam Solomon's social empiricism (Solomon 1994). According to Solomon, individual scientists cannot possess knowledge in isolation because their beliefs are always biased. However, scientific collectives nonetheless may be said to possess knowledge,

[3] The role of truth is played by a concept called "conformity", the satisfaction conditions of which are context-dependent (unlike classical correspondence truth).

[4] It should be noted that there is no reason to think that Longino's deliberators will not run into the difficulties associated with Kuhnian incommensurability.

because they sometimes choose theories that are empirically more successful than others. In other words, scientific collectives are *responsive* to empirically successful theories (p. 339). This responsiveness provides that a certain kind of counterfactual claim is true, namely claims of the form: physicists would not have accepted quantum theory if it had not been empirically successful. Solomon argues that this responsiveness is due to the fact that different individuals in the community are *differently* biased. Thus, even though every individual scientist has some personal reasons for preferring one alternative, and these reasons may not always be cognitive in nature, these personal reasons are variable in the community and will cancel each other.

Solomon thus opposes a widespread consensus in the philosophy of science according to which personal biases by individual scientists, while they exist, are more often overruled by a general preference for empirically successful theories than not. Using examples such as the case of plate tectonics in earth science to show that scientists do not assess a theory's empirical success in an unbiased way. Only the community as a whole can make an unbiased choice, i.e., a choice that depends solely on the theories' empirical merits.

In that she does not rely on deliberation doing the magic, I find Solomon's view more realistic than Longino's. However, I also think that there is something missing in Solomon's account. What's missing is some plausible story how a community that consists of biased individuals can be "responsive" to a theory's empirical success. This responsiveness is the cornerstone of Solomon's theory. She needs some account of how this responsiveness arises in communities. And this account had better not see groups as responsive because of some property that each individual in the community has. If it were suggested that, even though individual scientists exhibit personal biases, they have nonetheless an individual propensity to select empirically successful theories, this would make the social epistemology collapse into an ordinary individual epistemology. I would also find it unsatisfactory if it were suggested that a preference for empirically successful theories is the only bias that does not cancel out in the community, while all other biases do so cancel out. For this would make scientific knowledge a lucky coincidence. Thus, what we need is a truly *social* mechanism for theory choice, a mechanism that explains how the community as a whole selects empirically successful theories. This is what I turn to now.

4. A Social Mechanism for Theory Choice

The solution I wish to propose is strongly inspired by Kuhn's account. The starting point is provided by passages such as the one from *Structure* (p. 159, cited above) where Kuhn describes how a paradigm is established in the community. ("Gradually, the number of experiments, instruments, articles and books based upon the paradigm will multiply".) We should also remember that Kuhn was *very* serious about there being a strong analogy between scientific change and *biotic evolution*.[5] Just as certain types of organism enjoy more reproductive success than others as a consequence of some heritable characteristics, some theories are propagated more rapidly than others because they give rise to more successful problem solutions:

> The resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such

[5] This analogy has recently been criticized by Renzi (2009) and defended against Renzi by Reydon and Hoyningen-Huene (2010). In a nutshell, Renzi criticizes Kuhn for using evolutionary and ecological concepts that are inadequate on biological grounds. Reydon and Hoyningen-Huene argue that this is irrelevant because Kuhn's use of evolutionary theory is not an application of evolutionary theory. It's just what Kuhn says it is: an analogy.

> revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge (Kuhn 1970, p. 172).

Theories reproduce, as it were, by being successfully applied to new cases. Furthermore, scientific specialties that are guided by a paradigm may give rise to new specialties by a process that is like *speciation* in biotic evolution:

> [B]reakdowns in communication do, of course, occur: they're a significant characteristic of the episodes *Structure* referred to as 'crises'. I take them to be the crucial symptoms of the speciation-like process though which new disciplines emerge, each with its own lexicon, and each with its own area of knowledge (Kuhn 1994, p. 100f.).

I contend that Kuhn's account, while it's not intended as an application of evolutionary theory, contains a truly *social* mechanism for theory choice. Scientific communities are socially organized to propagate theories that have a high problem-solving capacity. An important part of this social organization is the *professional reward system*, the system that allocates credit to members of the community. Anyone who has ever worked in a laboratory (as I have) knows that a scientist can only get credits for "positive results", that is, successful problem solutions. "Negative results", i.e., failures to solve some problem in the expected way, are hard to sell, be it to learned journals, funding agencies, tenure committees, even Ph.D. thesis committees (though a Ph.D. student may get away with producing only negative results if it is evident that he or she worked really hard and demonstrated skill and that it is therefore not his or her fault that there were no positive results, just bad luck or poor supervision).

This social characteristic of the scientific profession ensures that theories with high problem-solving capacity (i.e., fruitfulness) outreproduce theories with lower problem-solving capacity.[6]

The social mechanism that I have just described has the following features:

(1) It can only operate at the community level. In other words, it is not what individual scientists *believe* about a theory, for instance, whether or not it is empirically successful that determines the community's response. Nobody cares what Professor Bloggs or Dr. Muller *believe* about a theory. It's what they *do* that's of interest to the community. Specifically, it is when they present a solved problem that the community responds. It responds by giving Bloggs and Muller credit for what they did, which allows Bloggs to land a research grant to continue her research, or Muller to get tenure, or someone else to receive a Ph.D. The results are presented at conferences, published in an A-journal, and perhaps end up in the textbooks. Thus, the theory reproduces. If a theory does not or only rarely give rise to solved problems, it goes extinct. And it would go extinct without anyone ever *deciding* or even *believing* that it is false. Theories are not refuted, they just cease to be pursued because they fail to provide rewards to those who pursue it.

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[6] A similar evolutionary account can be found in Hull (1988), which focuses on different systems of biological systematics. In contrast to the present account, Hull seemed to view his approach as a bona fide application of evolutionary theory to scientific development. I prefer to stick closer to Kuhn's account, according to which biological evolution is at best analogous to science as a process.

(2) The social mechanism of theory selection explains why scientific communities are *responsive* to a theory's empirical success in the sense of Solomon (1994). I define "empirical success" broadly, as any kind of solved problem. Unlike in Solomon's account, this responsiveness is not a coincidental feature of the community. It is the social mechanism that warrants such counterfactual claims as they are made by Solomon: The community of physicists *would* not have accepted quantum theory, *had it not* been empirically successful.

(3) This account does not depend on deliberation, yet it is social in a thoroughgoing way. In particular, it cites non-cognitive factors as instrumental in the scientific community's ability to reach its *cognitive* goals. It is not the otherwise disinterested, noble quest for truth that determines the community's behavior, but the selfish career interests of individual scientists. The postulated social mechanism ensures that it is nonetheless a theory's *empirical* credentials (at least fruitfulness) that determine its fate. Thus, my account is *social* but not *externalist*, in other words, it doesn't have to invoke extra-scientific factors to explain scientific change. In this, I follow Kuhn's rejection of externalist sociological accounts of science such as the "strong programme" (Kuhn 1992).

(4) The social mechanism also explains why Okasha's condition of non-dictatorship doesn't hold, and why it's fruitfulness that dictates a theory's fate. The scientific community is socially organized to select empirically successful theories, no matter what methodological preferences scientists have. They all want to advance their own careers, and they belong to communities that ruthlessly select for theories that are fruitful. There is also no conflict between the claim that scientists are driven by their career interests and the idea that science exhibits epistemic rationality, so long as selecting the most empirically fruitful theories counts as an epistemically rational practice.

So far, I have been speaking as if scientific communities chose theories. In the following section, I will argue that, in reality, they select something more inclusive.

5. What Exactly is Selected? Lessons From the New Experimentalism

"New Experimentalism" is a label for a very loose family of views in philosophy of science that sail under Ian Hacking's famous slogan "experiments lead a live of their own". What exactly this slogan means, of course, depends on the specific brand of New Experimentalism we are talking about. I am here particularly interested in a version that has been popular especially in the historiography of biology. The best known and also most developed account is by Hans-Jörg Rheinberger (Rheinberger 1997). The core of this account is the idea that what Rheinberger calls "experimental systems" plays a major role in biological research. Often, new experimental systems have been at the beginning of major developments in molecular biology. An example is a so-called in vitro system for protein synthesis that was used in the 1960s to crack the genetic code and subsequently to figure out the mechanism of protein synthesis. This system consists basically of a cell extract from either bacterial cells, yeast cells, or mammalian cells (normally immature red blood cells, because they have a very high rate of protein synthesis that they need to make hemoglobin). If such a cell extract is fed with the right chemicals, it will synthesize protein in the test tube. In the early days, this was detected by adding radioactive amino acids and then measuring the incorporation of the radioactivity into protein.

In his book, Rheinberger details how much experimental work was organized around this system rather than around some theory. One of the core postulates of Rheinberger's theory is that experimental systems are "the smallest integral working units of research" (1997, p. 28). Biological research, on Rheinberger's account, "begins with the choice of a system rather than with the choice of a theoretical framework" (p. 25). Furthermore, he characterizes experimental systems as "systems of manipulation designed to give unknown answers to questions that the experimenters themselves are not yet clearly to ask" (ibid.). Thus, according to Rheinberger, experimental research in biology does not usually start with a theory, or with well-formulated research questions.[7]

Experimental systems may precede the problems that they eventually help to solve. What is more, experimental systems do not just help to *answer* questions, they also help to *generate* them. Where this process leads is impossible to predict; experimental systems give rise to unexpected events. Thus, Rheinberger draws our attention to the unpredictable, open-ended nature of the research process – an aspect that seems so fundamental about basic science.

I found much to agree with in Rheinberger's account. A major problem, however, is the obscurity of the term "experimental system". Not unlike Kuhn's term "paradigm", it is sometimes this, sometimes that, and the term does different kinds of work. I will not attempt to remedy this situation by providing a conceptual analysis or a refined definition, I will only make two points of clarification.

First, experimental systems are not merely collections of material things (as Rheinberger makes it sound sometimes). Experimental systems also include *instructions for action* (sometimes documented in lab manuals). These include recipes how to prepare the biological materials, how to measure certain parameters, how to analyze the data, and so on. More than anything else, experimental systems are *ways of acting in a laboratory*. They may even

[7] Note the stark contrast to Popper (1959: 107): "The theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions, and to no others. All other questions he tries hard to exclude."

include ways of acting collectively, where experiments are performed by more than one person. Importantly, there are ways of acting associated with such a system that are not documented anywhere, that can only be learnt by doing. To sum this point up, experimental systems consist of certain kinds of materials *and* certain ways of acting. Note also that this already implies that they must be associated with *concepts*, for action is not possible without concepts. Roughly speaking, action is behavior guided by concepts.

Second, experimental systems are closely intertwined with *theory*. New Experimentalism arose as a movement to counter the extreme theory-centrism that has dominated classical philosophy of science, where experiments entered at best in the form of an "observation sentence *o*" or "evidence *e*". But as usual, welcome attempts to redress the balance sent the pendulum to the other extreme, leading to a discourse about science that completely ignores theory. It is time to emphasize that theory and experiment are equally important and deserving of philosophical and historical scrutiny. What New Experimentalists have tended to overlook is that experimental systems always come with *theoretical interpretations* of what happens in an experiment. The aforementioned in vitro system for protein synthesis would not be operational without various theoretical assumptions, the most important one being that what the system does is to make protein from amino acid building blocks and nucleic acid instructions in the same way as a living cells makes proteins. Furthermore, the *outputs* of the system or its *inscriptions*, as some people would say, need to be interpreted. In Rheinberger's case study, for example, it is a *theoretical* claim that the amount of radioactivity in some extractable protein fraction represents the system's protein synthesis activity. Such assumptions used to be called "auxiliaries" in classical philosophy of science. No experimental system could function without them.

To sum this point up, experimental systems are complex structures that include research materials and instruments as well as ways of acting, concepts and theories.

With this concept of experimental system in mind, I wish to defend the claim that, at least in experimental biology, scientists select a more inclusive structure than just a theory. What gets selected is usually a theory or theoretical framework *together* with an experimental system, or perhaps a set of such systems (nobody knows yet how to count them).

A similar view has already been put forward by Kuhn, albeit in somewhat different terms. It is clear from his account that what is selected in science is usually not a theory, but a *paradigm*. Kuhn uses this term in more than one sense, but in one sense he means "the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community" (Kuhn 1970, p. 175). This is quite similar to my characterization of experimental systems.

To sum this section up, scientists do not select theories on the basis of their fitting a given body of evidence, at least in experimental biology. They select a conglomerate of theory, concepts, research materials, techniques, instruments, and ways of doing experiments. As it were, theories reproduce by giving rise to successful experimental work (an experiment is a theory's way of making another theory, and vice versa). In other words, theories reproduce by making scientists *act* in certain ways. As I have argued in the previous section, scientific communities are built to reward certain successful actions. These actions, because they are rewarded, then give rise to similar actions and are thus selected by a social mechanism. Theories merely piggyback on the actions that are selected by the community by this social mechanism; this is how "theory choice" works.

I suggest that a beautiful example for this claim is provided by *classical genetics*. This will be elaborated a little in the final section.

6. The Case of Classical Genetics

Genetics as we know it was established early in the 20th century, after the rediscovery of Mendel's experiments in peas. In these experiments, Mendel had discovered two regularities, known as the "law of segregation" and the "law of independent assortment". These laws concern organismic traits that occur in two different states, known as "dominant" and "recessive". The first law states that in crosses of two pure lines, the recessive trait will disappear in the first generation. In the second generation, it reappears with a ratio of 1:3 to the dominant trait. The second law states that two traits will segregate and assort independently. The different combinations of the dominant and recessive traits occur in the ratio 9:3:3:1, which is simply the binomial expansion.

Mendelian genetics is intimately tied to doing this kind of crosses on different varieties of some species. It turned out that this kind of experiment can be done on plants as well as on animals. One of the most productive systems for doing such experiments turned out to be the fruitfly *Drosophila melanogaster*. Thomas Hunt Morgan and his associates crossed thousands of naturally occurring mutants of this fly. From the beginning, they found scores of exceptions to Mendel's rules. Many of these "exceptions" gave rise to new defined inheritance patterns, such as sex-linked inheritance, for example (Morgan 1910).

If there was such a thing as a genetic *theory*, it was closely tied to these crossing experiments (Waters 2004). It was noticed that the original Mendelian pattern can be explained by assuming that the stable heritable factors causally responsible for the visible trait differences are located on sister chromosomes. Other inheritance patterns found in Morgan's lab could be explained by assuming factors that are located on the very same chromosome. Detailed genetic maps were constructed that showed the locations of hundreds of genes on the

Drosophila chromosomes, it was a whole industry or a "system of production" or "breeder reactor", as Robert Kohler described it (Kohler 1994). Needless to say, it also qualifies as an experimental system in the sense that I outlined in the previous section. In this system, theoretical principles such as Mendel's laws are tightly interwoven with certain ways of doing experiments. You can't understand the principles of classical genetics without understanding how to do such experiments, and you can't do these experiments without understanding the principles of genetics.

By the end of the 1920s, the ways of the Morgan school were widely accepted as a sophisticated new science of genetics.

How did classical genetics become established? To see this, it is important to appreciate that there were *rival approaches* at that time. Around the turn of the century, for example, there was the biometric school founded by the English statisticians Karl Pearson and Raphael Weldon, which was heavily influenced by the ideas of Darwin's cousin Francis Galton, who is also known as the father of eugenics. The biometric school attempted to provide a quantitative theory of genetics that was mathematically much more sophisticated than Mendelian genetics with its simple rules. They argued that genetic theory must be based on precise quantitative measurements of an organism's traits, not merely qualitative trait differences as they feature in Mendelian crossings. Of course, the biometricians were aware of Mendel's laws, but they did not think they were very relevant. The biometricians followed Darwin in thinking that what matters in evolution is *gradual* variation, for it allows the finely tuned adaptation of organisms to their environment.

Other biologists that time, too, thought of Mendelian genetics as some kind of laboratory artifact that may be interesting, but not relevant for understanding evolution or development.

How was this initial opposition eventually overcome by the proponents of the new genetics? I suggest that they achieved this not so much by presenting their theory and some supporting evidence, but by simply doing what they were best at: *by breeding fruit flies*. Their enormous output of experimental results that were successful by classical genetics' *own* standards started to attract funding and gifted young people to the enterprise. They filled the spaces of the leading journals with their genetic maps and new puzzling cases. The alternative schools in genetics did not stand any chance of matching this productivity. That's how the *community* selected the experimental systems complete with the theoretical principles of classical genetics as the dominant paradigm of genetics. There was nothing like a discourse weighing different theories by methodological criteria, and if there was such a discourse, it was at best epiphenomenal.[8]

It is time now to summarize my conclusions.

7. Conclusions

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[8] Radick (2005) has made a strong case for the view that the displacement of the biometric by the Mendelian school was a contingent event; geneticists were not inevitably drawn to the Mendelian Truth, as it were. I agree with Radick insofar as there is not one Truth that science inevitably homes in on. However, I maintain that *given* the social organization of 20th century science there was some necessity in the Mendelian's winning, because that organization selects for a certain kind of productivity that only post-Morganian genetics exhibited. In particular, the approach of the Morgan school was fruitful not just for the goals of transmission genetics itself, but also for neighboring disciplines such as evolutionary biology (Weber 1998) or developmental biology (Weber 2005). This might also be Kuhn's spawning of new specialties.

At least in experimental science, there is bluntly put no such thing as philosophers call "theory choice", at least when this is taken to mean that scientists choose theories on the basis of their fit with a *fixed* body of evidence and possibly other criteria such as **simplicity**, **accuracy,** and so on. Experimental scientists pick a more inclusive entity that includes theories as well as investigative methods, experimental procedures, sometimes also certain research materials such as model organisms. I have argued that the entity in question is pretty much what Rheinberger calls an "experimental system". I have further shown that experimental systems are *conceptual* entities as much as they are *material*, i.e., they mainly consist in certain ways of acting in a laboratory, which is not possible without concepts and theories. I have tried to use the case of classical genetics as evidence for these claims.

I have not only argued that scientists select more inclusive structures than just theories, I have also tried to take seriously Kuhn's claim that the selection of what he calls "paradigms" is not made by individual scientists but by the entire community. This claim makes Kuhn a social epistemologist. I have contrasted this idea to two more recent social epistemologies, namely Helen Longino's and Miriam Solomon's. Longino's approach is committed to deliberation as the salient choice procedure. I think that's an over-idealization, to say the least. Scientists may sometimes deliberate, but major decisions about research directions are not effected in this way. So I was leaning more towards Solomon's approach, according to which scientific communities as wholes are responsive to empirically successful or – as I prefer – fruitful theories, which overrides any personal biases that individual members of the community may have, which may also include apparently rational biases such as preferring simple theories, or theories with broad scope.

I have amended Solomon's account by a social mechanism that explains *why* scientific communities as a whole select empirically successful or fruitful research programs. The social mechanism has to do with the reward system of professional academic science, which mainly rewards so-called "positive results", which I take to be successful problem solutions. I have not said much as to what counts as a successful problem solution, but I do not think there is a general answer to this question. This strongly depends on the theory and set of investigative procedures in question. This reward system leads to a proliferation of the approach in question by a social mechanism that is akin to natural selection. Like Kuhn, I mean this merely as an *analogy*, as I am not a big fan of "meme theory", which holds that there is a generalized selection theory that applies to genes as well as to human ideas or "memes". I have argued that it is such a mechanism that supports counterfactuals of the kind that Miriam Solomon holds to be true, claims such as: the physics community would not have adopted quantum theory had it not been empirically successful.

Finally, I think that my approach solves the puzzle that raised by Samir Okasha's application of social choice theory to theory choice problems. Okasha's problem only arises under Arrow's assumptions concerning social choice situations. These assumptions include a condition called "non-dictatorship". In social choice theory, it means that no-ones preferences can override those of other members of the community. In our present context, this condition means that no single criterion for theory choice can override the others in cases where different criteria of theory choice pull into different directions. *That* the criteria of **accuracy**, **consistency**, **fruitfulness**, **simplicity** and **broad scope regularly** cannot be aggregated such as to effect a unique choice is also the heart of Kuhn's incommensurability thesis. My account, I hope, shows why this does not matter in scientific practice, because fruitfulness or problem-solving capacity always dominates, given the way science as a profession operates.

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