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SIMPLICITY IN THEORY-CONSTRUCTION AND EVALUATION: THE CASE OF THE CHROMOSOME THEORY OF MENDALIAN INHERITANCE

This paper analyzes the role played by the criterion of simplicity in the construction and evaluation of the Chromosome Theory of Mendelian Inheritance. First, I briefly discuss some views on simplicity held by philosophers. We can see that despite their different views on scientific methodology and epistemology, most of them consider simplicity as a substantive value that is used in science to choose between theories. Then I analyze the arguments used by scientists to evaluate the hypothesis that genes are pieces of chromosomes to see whether simplicity played any role in their decisions. My conclusion is that the simplicity of the hypothesis that identified the genes with the chromosomes was not taken as a reason to accept it. Finally, I argue that the unifying character of a hypothesis and the ontological and descriptive simplicity achieved by it, should not be taken as reasons for its plausibility.

I. SIMPLICITY

Although philosophers have seldom presented detailed accounts of how simplicity works in science (outstanding exceptions being Forster 1988, and Sober 1984, 1988), there seems to be a tacit consensus that simplicity plays a substantive role in the way that scientists construct their hypotheses and choose between competing theories. Philosophers of different convictions have usually stressed the role of simplicity in science. Quine (1966), Popper (1959), Kuhn (1970), Salmon (1966), and Schaffner (1974) are among those who selected simplicity as a crucial principle for establishing plausibility judgments in science. The list is long but these few examples are enough to

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show that, despite their different positions, philosophers have taken simplicity as an important criterion to evaluate scientific theories. In fact, philosophers not only think that scientists use this criterion, but they employ it themselves to solve philosophical problems, e.g. Brant & Kim (1967) to argue for the identity theory in philosophy of mind. To justify the use of simplicity in arguing for their philosophical positions, some philosophers argue that scientists also use it. This form of reasoning, though, should be held suspect until philosophers show first that simplicity has in fact been a ground for accepting theories in science.

It is important to realize that philosophical discussion about simplicity has changed over the years. Traditionally, simplicity was a criterion considered necessary for inductive reasoning. More recently, however, philosophers have increasingly emphasized the role of simplicity in the construction of abductive arguments or explanations in general (e.g. Thagard 1978). Parallel to this shift from an interest in the role of simplicity in induction to its role in abduction, or perhaps as a result of it, there has also been a shift from an interest in simplicity as a syntactic or descriptive feature of our theories - as it was taken to be by the logical empiricists - to the consideration of simplicity as a semantic feature, i.e., one concerned with the relation between theories and the world. This shift has led realist and empiricist philosophers to different assessments about the value of simplicity. Whereas some years ago logical empiricists defended simplicity, it now seems that mainly realists defend the epistemological value of simplicity against the purely pragmatic character that modern empiricists assign to it [see for example Boyd (1985) and Maxwell (1974) for a defense of simplicity from a realist position, and van Fraassen (1980) for an empiricist account].

But simplicity is taken seriously not only by those who argue for the central role of explanations in science and those who define themselves as realists. Even philosophers who criticize the connection between simplicity and explanation do not question the validity of simplicity. For example, Lloyd (1983) rejects Thagard's idea that simplicity is a measure of the ad-hoc explanations introduced by a given theory, but emphasizes the important role of simplicity in the construction of Darwin's theory of evolution. Van Fraassen rejects simplicity as a special feature of a theory and only attributes a pragmatic value to simplicity. Nonetheless, he asserts that simplicity "... is obviously a criterion in theory choice, or at least a term in theory appraisal." (1980, p. 90). (V. Fraassen does not explain what he means by "theory appraisal", but I will later use this distinction to claim that simplicity is, *at most*, a value in theory appraisal.) Thus, despite the many disagreements among current philosophers on the issues of realism and explanation, there is wide agreement on the importance of simplicity.

The underlying assumption of philosophers is that scientists prefer simpler theories and, furthermore, that this preference is justified. But, we still must ask, *what does it mean for a hypothesis to be simple,* and *what could be the rationale for such a preference?* Among the features of a theory that a criterion of simplicity is supposed to measure are the following: the descriptive language of theories (Rudner 1965), the consequences of theories (Hesse 1974), their different of model types (Lloyd 1983), the set of auxiliary hypotheses needed by theories (Thagard 1978), the number of entities and processes introduced by different theories, etc. Although I cannot go into a detailed analysis of these different positions, I want to point to a couple of problems that I think affect all of them.

First, it is not clear how to measure and compare the descriptive language of alternative theories, the consequences of different theories, their model types, their auxiliary hypotheses, or any of the features that we are supposed to be comparing to determine which theory is the simplest among conflicting alternatives. Philosophers have not provided any general measure that could be used to assess how "simple" a theory is. This is an important point if simplicity is to function as a methodological device in science, as philosophers have argued.

Second, scholars have not identified any version of simplicity which has widespread use in science and have not developed detailed analyses of how different versions of the simplicity criterion have been applied in the history of science. Furthermore, philosophers who favor the use of simplicity have not explained how to account for cases in which simplicity was not used and cases in which it was used but led to the "wrong" answer. The role of simplicity in the history of science has been more complex than many of the philosophical discussion have suggested. I will here briefly mention some examples. In the history of science we can find episodes in which scientists rejected theories because they were taken to be too simple. For example, for a long time DNA was considered to be too simple to be the carrier of genetic information and, thus, biologists were inclined to think that genes were made of protein. We can also find cases in which there were two alternative ways of "simplifying". For example, after its rediscovery in 1900, scientists believed that the Mendelian law of segregation applied only to alternative inheritance or the inheritance of characters qualitatively distinct, like blue and brown eyes. E. M. East argued, however, that the law of segregation could also account for the inheritance of quantitative characteristics, like height, by postulating the existence of modifier genes. Thus, he suggested that there was only one type of process, Mendelian inheritance, but there were different types of genes. W. E. Castle, in contrast, thought that to introduce modifier genes was an ad-hoc maneuver and that a simpler picture was accomplished by postulating two

types of inheritance: Mendelian and blending. Thus, what was simpler in this case of conflicting ways of simplifying: to introduce more entities to avoid postulating a new type of process, or to have two processes and only one basic type of entity? (see Vicedo 1991 on the Castle-East discussion about the multiple factor hypothesis in genetic theory).

There are also numerous cases in the history of science in which it is not clear whether it is simpler to introduce more entities of the same type or fewer entities of different types. And, even more problematic for those who hold that simplicity should play a role in evaluating our theories, there have been situations where science actually advanced by introducing more and more auxiliary hypotheses and entities. In the history of genetics, for example, there has been a constant introduction of new types of entities, namely, different types of genes, to account for new experimental situations. Finally, we should also be aware of the many cases in which simplicity is used only as window-dressing by scientists, not as a substantive methodological criterion (see Vicedo 1991).

A third major problem for all the definitions of simplicity mentioned is that we have no means to judge when a hypothesis has gone over the threshold of the simple. We can certainly make comparisons, but the context-dependence and historical relativity of any comparative analysis would make the choosing of the simple hypothesis a question of pragmatic preference with no epistemological weight.

Let us now turn to an analysis of a case from the early history of genetics. I will then suggest a new way of looking at simplicity, namely understanding simplicity as a value in theory appraisal, but not as a methodological device for theory construction and evaluation.

II. THE CHROMOSOME THEORY OF MENDELIAN INHERITANCE

A text-book account of the birth of the Chromosome Theory of Mendelian Inheritance runs more or less as follows: around 1903 cytologists discovered the functional individuality and the morphological continuity of the chromosomes. Then, an analysis of their behavior in meiosis showed that homologous chromosomes separate in the formation of the germ cells. Thus, their behavior exactly paralleled that of the Mendelian genes. The connection between both phenomena was quite straightforward. Simplicity required the identification of chromosomes as the carriers of the genes. As the geneticist and historian A. Ravin put it some years later: "Sutton perceived that the chromosomes are *observed* to behave precisely the way Mendel's genes are *postulated* to behave... What could be simpler than to imagine that the gene is

carried on the chromosome, and the postulated behavior of the gene in inheritance is a reflection of the mechanics of chromosome transmission from one generation to the next?" (1965, p.17).

It is true that Sutton (1902, 1903), and Boveri (1903) pointed out the striking parallel between the behavior of genes and that of the chromosomes. Both the Mendelian factors and the chromosomes seemed to obey the same laws. Both phenomena had the same essential features, which pointed towards a correlation between them. In natural sciences, correlations invite further study. When two order of phenomena share the same features, this coincidence raises the question of whether they are "only" correlated, that is, whether they are phenomena co-occurring by chance, or whether there is a common cause explanation or a causal relationship between the two. In fact, geneticists understood that the parallel pointed towards a connection between genes and chromosomes. Wilson (1914, p. 335) remarked: "As soon as the Mendelian phenomena were made known it became evident that in broad outline they form a counterpart to those which cytology had already made known in respect to the chromosomes... A parallelism so striking inevitably suggested a direct connection between the two orders of phenomena."

Based on our current knowledge of cytogenetics, it seems almost obvious that the identification of genes with chromosomes was a step required by simplicity. It was an identification that led to an ontologically simpler picture by providing a unification of phenomena from two different fields: cytology and heredity. The law of segregation of factors, later called genes, and the segregation of chromosomes could be unified by postulating an identity or a part-whole relation between genes and chromosomes. The unification therefore provided an ontologically simpler picture of the world by reducing the number of entities or processes in the natural world. It also gave a simpler description of the world (I take the distinction between descriptive and ontological simplicity from Miller 1987). But the question at hand is: when the hypothesis was put forward, was it really considered simpler than its alternatives, and, if so, was this feature of the theory taken as a reason to adopt it, as a test of its truth, as Goodman once called simplicity? (Goodman 1958, p. 1064)

Certainly, the Chromosome Theory was seen as a simple or parsimonious hypothesis. The behavior of chromosomes in meiosis would be a sufficient mechanism to account for the segregation of the genes if genes were carried by the chromosomes. Thus, a mechanism perfectly able to account for the behavior of genes had been found in nature. Could it be possible that it was completely unrelated? Parsimony would seem to deny such a wasted effort in nature. In 1910, this is how Doncaster saw the situation:

> The suggestion that this segregation of chromosomes, which can be seen to take place, is the mechanism by which the members of an allomorphic pair of characters are segregated, is quite speculative; but it seems exceedingly unlikely that machinery so exactly adapted to bring it about should be found in every developing germ-cell, if it had no connexion with the segregation of characters that is observed in experimental breeding. (1910, p. 129)

Many philosophers have argued that the simplicity achieved by a unifying hypothesis is a feature which provides reasons for adopting the hypothesis; it is a confirmatory virtue. I believe, however, that when a hypothesis unifies two realms of phenomena by postulating a certain type of relation between them, its unifying and simplifying power cannot then be taken as a reason to argue for the theory, because the unification must still be tested by empirical evidence. To put it simply: unification is not a reason for belief, it is a conjecture for which we still have to present evidence. The unifying character of a hypothesis should not be a reason for believing in the hypothesis. On the contrary, the unification postulated is precisely what must be argued for.

Thus, the dichotomy sometimes established between simplicity and evidence is ill-stated. The simplifying and unifying character of a hypothesis is a feature of a theory for which we can and have to gather evidence. A unifying hypothesis is clearly a simple hypothesis because it usually allows us to account for what seemed to be different phenomena with only one scheme. The simplicity of the claim, however, should not be taken as a reason to believe in its correctness or adequacy, since we still must establish the validity of the unification in the specific way postulated. When we postulate that two realms of phenomena are connected in a certain way, we are making a claim that has to be evaluated in the light of the evidence, not on the basis of its simplifying or unifying power.

This point is illustrated clearly in the history of science by the case of the Chromosome Theory of Mendelian Inheritance. The historical step taken by scientists from a belief in correlation to a belief in identity was justified only after the accumulation of evidence, not by the weight assigned to the simplicity of the hypothesis.

T. H. Morgan, who first opposed the Chromosome Theory of Mendelian Inheritance, but years later obtained the Nobel Prize for his leadership of the group which proved that genes were part of chromosomes, clearly saw that the correlation between the behavior of genes and chromosomes did not necessarily imply a direct relation between the two. In 1910 he pointed out the key question. The chromosomes were a sufficient

mechanism to account for the segregation of genes. But, were they the real system bringing it about? The existence of correlation does not *necessarily* imply any other relation between two phenomena (cf. Morgan 1910). As Morgan later observed, the problem in the first decade of 1900 was this: "Even if it is admitted from such evidence that the chromosome mechanism suffices to explain Mendelian segregation and assortment, as Sutton pointed out, it still remained to be shown that we are not dealing with analogy or coincidence; but that the chromosomes are specifically related to genetic events." (1923, p. 172). Thus, it was first necessary to show that the parallels between chromosomes and genes were not due to mere coincidence, but to a correlation between the two phenomena. Then, geneticists needed to further show that the correlation had to be accounted for by the existence of an identity relationship between genes and chromosomes.

Because scientists in the early 20th century could not make direct observations that would justify the step from correlation to identity between genes and chromosomes, we must consider what kind of empirical evidence was possible. Although there was not direct evidence in favor of the general identity thesis, there could be evidence for or against its corollaries. In another paper I have argued that in the construction and evaluation of a hypothesis that unifies a functional theory - like the gene theory - with a theory postulating a physical structure to account for the behavior of the entities introduced by the functional theory - like the chromosome theory -, there are two ways of testing the corollaries of the integration of function with structure. One is the substantiation of interfield predictions between the two realms of phenomena, between the functional and the physical level; the other is the accommodation of each theory to the constraints imposed by the other. (See Vicedo 1990a for further details of these two processes.) These different ways of assessing the corollaries of a unifying hypothesis were actually used by geneticists in the episode analyzed here.

Soon after he rejected the hypothesis that chromosomes were the carriers of the hereditary entities, Morgan set out to work with *Drosophila.* After two years of unsuccessful work, a white-eyed male fly was spotted. Morgan bred the curious male to a normal red-eyed female. All the offspring of the first generation developed red eyes, but when they were crossed with each other, some males showed the white-eyed condition again. However, if a white-eyed male was crossed with females from the first generation, half the number of males and half the number of females would develop white eyes. This finding marked the beginning of the research that established that chromosomes were the carriers of genes.

Sex-linked inheritance was the first exception to the rule of the dual nature of the hereditary factors. As it happened, it was also an exception to

the dual nature of chromosomes. In many animals there was either only one sex chromosome or the effects of the Y chromosome were negligible. According to Morgan, "The unequal distribution of the X chromosomes in the two sexes has furnished an opportunity to examine the theory that the chromosomes are the bearers of the hereditary factors, because it can be shown that the inheritance of a certain class of characters follows the known distribution of the sex chromosomes." (1915, p. 422). In the study of a sex-linked character, e.g. color blindness, he found that "the inheritance of the chromosomes and of the defect is strictly parallel." (p. 422).

The identification of chromosomes with genes required a demonstration of correspondences between the two entities, between the behavior of chromosomes and the behavior of the characters in heredity (the effects of genes). In his 1903 paper, Sutton had pointed out the first clear constraint that this hypothesis imposed: since the number of chromosomes is smaller than the number of characteristics in an organism, there must be more than one factor in each chromosome. This means that all the factors carried in one chromosome must be inherited together. However, this seemed to contradict the facts of character distribution, since the characteristics of an organism were thought to be transmitted independently from each other. As seen by Morgan in 1910, this was the biggest stumbling-block for the Chromosome Theory of Mendelian Inheritance. However, in 1906 Bateson and Punnett had reported a case in sweet peas which did not give the expected ratios for two pair of factors. When a sweet pea with purple flowers and long pollen grains was crossed to a pea with red flowers and round pollen grains, the two characteristics coming from one parent would remain together. They called this phenomena "gametic coupling" in contrast to "gametic purity", the name given by Bateson to the Mendelian law of segregation. Soon others cases of gametic coupling or linkage were discovered. As Sutton had already suggested, this was to be expected if the chromosomes were the carriers for the genetic factors. All the factors carried in one chromosome would be linked and inherited together. The relationship between genes and chromosomes was looking more like a substantive correlation, rather than a mere analogy.

Linkage, then, was not exclusive to sex-inheritance. The group of genes carried in each chromosome would have to be inherited together if the Chromosome Theory of Mendelian Inheritance was correct. There would be, then, a concrete number of linkage groups in each organism. Research showed that there was a determined number of linkage groups in each organism and, moreover, that the number of linkage groups coincided with the number of chromosomes that the organism had. For example, in *Drosophila* the number of pairs of chromosomes is four. The characters studied by the

Morgan group also fell into four groups. This was a new "coincidence" and it thus further corroborated the hypothesis that genes were on the chromosomes. In Morgan's view: "The coincidence between the number of hereditary groups and of the chromosomes is sufficiently evident to render comment unnecessary." (1915, p. 424).

Thus, empirical research indicated that factors lying in the same chromosome pair would always be found together in the following generation. This is what would occur unless there was a process of exchange between chromosomes, whereby pieces of one chromosome were interchanged with pieces of another chromosome. This process was thought to exist between homologous chromosomes, a phenomenon known as crossing-over. According to Morgan, "such an explanation will account for all of the many phenomena that I have observed and will explain equally, I think, the other cases so far described. The results are a simple mechanical result of the location of the materials in the chromosomes, and of the method of union of homologous chromosomes, and the proportions that result are not so much the expression of a numerical system as of the relative location of the factors in the chromosomes." (1911, p. 384).

In summary, the behavior of chromosomes exactly paralleled the behavior of the genetic factors. And, in cases where the phenotypic characters showed an anomaly, cytological studies showed the same anomaly in the chromosomes, as it happened in Bridge's discovery of non-disjunction in sex inheritance. Bridges found unexpected results in the inheritance of sex-linked characters in a race of *Drosophila*. He found that 2 1/2 of the males were like their father, whereas they were expected to have been like the mother. There were also 2 1/2 females that were like their mother, whereas they were expected to be like the father. Bridges inferred that the mother had an additional sex chromosome, another Y chromosome. This inference was proved correct by cytological studies. This finding was taken by geneticists to be the most important evidence in favor of the hypothesis that genes were carried by chromosomes. In the conclusion to his 1914 paper in *Science* Bridges stated this result as incontrovertible: "there can be no doubt that the complete parallelism between the unique behavior of the chromosomes and the behavior of sex-linked genes and sex in this case means that the sex-linked genes are located in and borne by the X-chromosomes." (1914, p. 109). In 1916 a new journal, *Genetics*, published the results of Bridges' investigations in its first article entitled "Non-disjunction as Proof of The Chromosome Theory of Heredity". Bridges was explicit about his position:

> "The genetic and cytological evidence in the case of non-disjunction leaves no escape from the conclusion that the X chromosomes are the carriers of the genes for the sex-linked

> characters. The distribution of sex-linked genes (as tested by experimental breeding methods) has been demonstrated to be identical, through all the details of a unique process, with the distribution of the X chromosomes (as tested by direct cytological examination)." (1916, reprinted in Voeller 1968, p. 207).

Non-disjunction was a perfect example of the method of empirical analysis used to establish the identity hypothesis. Genetic results implied certain cytological facts and these were always found. Moreover, cytological results implied certain facts at the level of character distribution. Genetics always confirmed the predictions of cytology. As Sturtevant saw the situation years later, all this evidence "... made it inconceivable that the relation between genes and chromosomes was merely some kind of accidental parallelism." (Sturtevant, 1965, p. 48). In the analysis of the relation between structure and function there was a process of double-checking in which each science - genetics and cytology - provided evidence for the findings of the other and set constraints on the interpretations of the new phenomena encountered in the other field. The interaction between both fields by the establishment of constraints and the testing of predictions provided sufficient evidence to lead scientists to accept the Chromosome Theory of Mendelian Inheritance. The prediction of chromosome behavior from genetic evidence and the foretelling of genetic results from chromosome behavior led even the most skeptical researchers, like Morgan, to conclude that the parallel between chromosome and factor behavior "must be more than a coincidence." (Morgan 1915, p. 420). By 1915, the results of Morgan's own group, which showed sex-linked inheritance, linkage, crossing-over, non-disjunction, interference, etc., provided enough evidence for the conclusion that: "The chromosomes not only furnish a mechanistic explanation of Mendelian heredity, but in the case of Non-disjunction and in the case of the point by point correspondence between the Linkage Groups and the chromosomes, furnish a *verifiable* explanation of the results." (Morgan 1915, p. 429).

III. CONCLUDING REMARKS

For many philosophers the step from correlation to identity cannot be required by any observation, but it can be taken on the basis of simplicity. However, in the case of the Chromosome Theory of Mendelian Inheritance, geneticists did not accept the identification of genes as parts of chromosomes until evidence from different sources was available. In this case, the confluence of several independent lines of evidence reinforced the belief that the chromosomes were

the actual carriers of the genes (cf. Whewell's notion of consilience, as explained in Forster 1988). Does this mean that the unifying power of the hypothesis, and the ontological and descriptive simplicity achieved by it, did not play any role in accepting the theory?

In my opinion, there are two main reasons why the capacity of a hypothesis to unify two realms cannot be in itself a reason to believe in that hypothesis. The first is that a hypothesis which unifies two realms makes an empirical claim, and, as such, its evaluation requires empirical and experimental evidence. When we say that a = b we are stating a hypothesis about objects in the world. Empirical evidence decides in favor of or against the hypothesis. The issue is not simplicity versus evidence, it is evidence for or against a hypothesis which postulates a specific relation between objects or phenomena in the world.

The second reason is that there are many ways of unifying two realms of phenomena. Thus, it is an empirical matter not only whether two realms are unified, but also in what particular way they are. Therefore, the unifying character of one hypothesis should not be taken as a reason for believing the theory since it is the specific way of establishing the unification that has to be shown to be correct. For example, in the case of the Chromosome Theory, the geneticist R. Goldschmidt thought that the unification of genes with chromosomes should be made by eliminating genes altogether and adopting the chromosome as the unit of inheritance. This hypothesis also unified the realms of heredity and cytology, and it was simpler than the one defended by Morgan's school, as Goldschmidt was quick to point out in every single one of his articles by appealing directly to the rule of parsimony (Goldschmidt 1954). His hypothesis postulated an eliminative reduction by neglecting the existence of discrete genes. The number of entities required by the theory of heredity was thus reduced and there was a gain in ontological simplicity. However, evidence did not favor this specific way of relating chromosomes and genes.

In his analysis of Whewell, Forster (1988) has also argued for a separation between the empirical evidence for a theory and the evaluation of its simplicity. Although a consilience of inductions can lead us to a simplification in ontology, Forster argues that the two steps should be kept separate. According to Forster, "Consilience is not the same as simplicity; the former merely gives rise to a *convergence towards* the latter. The goal - of simplicity and ontological unification - is only *achieved* once the observed consilience is deemed to be non-accidental and is subsequently *explained* in terms of an identity. Consilience is the evidence for accepting the identity. After that, the unified theory can be further tested by its deductive consequences, but it is the *discovery* of the identities and unity in nature that is the admirable part of the process." (1988, p. 79).

Although I have not presented my analysis in a Whewellian framework, I have argued for this separation. In the case of the Chromosome Theory of Mendelian Inheritance it was also the confluence of independent lines of empirical evidence that led to the acceptance of the theory. Thus, both Forster and I agree that evidential considerations are *primary*. However, Forster thinks that it may still turn out that simplicity is a "derived" goal, in the sense that maximization of confirmation *leads* to greater simplicity (personal communication). I recognize that this is a possibility, but I do not think it has much support from the history of science.

The upshot, in my opinion, is that simplicity often does not and should not play a substantive role in the construction and evaluation of scientific theories. However, simplicity is a feature of some of our theories that we highly value. We could thus say that simplicity is a value in theory appraisal, a value which we should not take into account when assessing the plausibility of a theory, but that we value when we get it. I would say that when we accept a theory which unifies different realms and, thus, it gives us a simplified picture of the world, we value this feature of the theory as important. However, the simplicity achieved by the unifying power of the theory is not an appropriate guide to our acceptance of it in the first place.

The value of simplicity in theory appraisal explains why many scientists explicitly emphasize the simplicity of their theories. After one hypothesis is accepted as the correct one, our picture of the world is seen as more simple and systematic, and so it is pointed out by many scientists. This behavior need not be seen as devious, as scientists dressing up their theories with valued features only after they have accepted them on other grounds. I think this can be understood in terms of what Nickles (1986) has called the internal reconstruction of science. Although he borrows the term from Lakatos, Nickles does not refer to the "philosophical" reconstruction of historical scientific episodes, but to the very making of science by the scientists. According to Nickles, "scientists themselves engage in a good deal of conceptual reconstructions of previously proposed problems and solutions as part of their work." (p. 262). In my opinion, it is in the "conceptual reconstruction" of the specific problem they are dealing with that scientists value simplicity.

I think that understanding simplicity as a value in theory appraisal, but not in theory construction and evaluation, allows us to account for the fact that scientists do not take simplicity as a criterion to adopt theories, but do praise any theory which gives us a simple account of the world. But philosophers should be aware that valuing a feature does not automatically convert it into a good methodological device, a good evaluative criterion, or even a good

regulatory aim. Scientists have in fact recognized this in their practice. This is, I believe, as it should be.

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