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REALISM AND SIMPLICITY IN THE CASTLE–EAST DEBATE ON THE STABILITY OF THE HEREDITARY UNITS: RHETORICAL DEVICES VERSUS SUBSTANTIVE METHODOLOGY

1. The Problem: Modified Genes or Modifier Genes?

MENDEL'S REPORT of his experiments with *Pisum* put forward the law of segregation. According to this law, the paternal and maternal elements responsible for the phenotypic characteristics always separate from each other in the formation of gametes.[1] Mendel experimented with varieties of peas that differed with respect to clear-cut characteristics, like smooth *v.* wrinkled, yellow *v.* green seeds, etc. The offspring of parents with opposed characteristics display one or the other of these characteristics. Thus, an offspring of a cross between a yellow and a green parent would be either yellow or green, but not an intermediate color. The inheritance of characteristics of this type in a population forms a pattern known as discontinuous variation. However, in the inheritance of certain characteristics, like height, the offspring presents an intermediate state between the states of the two parents, giving rise to a continuous pattern of variation in a population. The characteristics like yellow and green are known as qualitative; the ones whose inheritance shows a continuous range of variation, like height, are known as quantitative.

In the first years after the rediscovery of Mendel's work in 1900 it was not clear that the inheritance of quantitative characteristics could be accounted for

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[1]It is unclear whether Mendel himself conceived of two pairs of alleles for each phenotypic trait. Literally, he only talks of *elemente*, elements, in the germ cells being transmitted in the hereditary process. However, his scheme, in my opinion, does not make sense unless we grant that he did have this idea in mind. For discussion on this point see: A. Brannigan, 'The Reification of Mendel', *Social Studies of Science* **9** (1919), 423–454; J. Heimans, 'Mendel's Idea on the Nature of Hereditary Characters', *Folia Mendeliana* **6** (1971), 91–98; E. Mayr, *The Growth of Biological Thought* (Cambridge, Massachusetts: Harvard University Press, 1982) pp. 710–726; R. Olby, 'Mendel no Mendelian?', *History of Science* **17** (1979), 53–72; V. Orel, *Mendel* (Oxford: Oxford University Press, 1984); M. A. Simon, *The Matter of Life: Philosophical Problems of Biology* (New Haven/London: Yale University Press, 1971), pp. 87–146. Important works on the period of genetics analyzed in this paper are: L. Darden, 'Reasoning in Scientific Change: The Field of Genetics at its Beginnings' (Ph. D. Dissertation, University of Chicago, Illinois, 1974); E. Mayr, *The Growth of Biological Thought* (Cambridge, Massachusetts: Harvard University Press, 1982); W. B. Provine, *Sewall Wright and Evolutionary Biology* (Chicago: University of Chicago Press, 1986).

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by the law of segregation proposed for the inheritance of qualitative characteristics. W. Johannsen's work with *Phaseolus vulgaris* (Dwarf Bean) in 1909 had shown the need to distinguish between variation due to environmental factors and variation due to genetic factors. The distinction between genotype and phenotype was essential to clarify the issue of the inheritance of quantitative characters. The experimental work of H. Nilsson-Ehle in 1909 and E. M. East in 1910 supported the hypothesis that the existence of multiple Mendelian factors, each influencing the same characteristic, would give rise to a continuous pattern of variation. This idea was later named multifactorial Mendelian inheritance or the multiple factor hypothesis. When the effects of many genes are combined to produce a phenotypic characteristic, the phenotypic variation is continuous. But the variation of the genotype is discontinuous, and the transmission of the genes follows the law of segregation. Thus, there is no essential difference between the heredity of quantitative and qualitative traits.

W. E. Castle, a mammalian geneticist at Harvard, disagreed with this extension of the Mendelian scheme to the inheritance of quantitative characteristics. The most famous and important experiment that provided the foundation for his ideas was carried out with hooded rats. These rats are predominantly white with a black hood and a thin black stripe on their backs. Castle selected rats in two opposing directions. A plus series was selected for an increase of the hood and back-stripe pigmentation, and a minus series was selected for a decrease of these characteristics. Castle's effort was successful. Soon he had a set of rats almost completely pigmented and another set almost all white, except for a very small hood. He concluded that 'the inheritance in such cases in non-Mendelian, since neither dominance not segregation occurs. I called it blending'.[2]

[2]W. E. Castle, 'The Inconstancy of Unit-Characters', *American Naturalist* **46** (1912), 360. Castle's work most directly relevant to the issue discussed in this paper is: 'The Laws of Heredity of Galton and Mendel, and Some Laws Governing Race Improvement by Selection', *Proceedings of the Amercian Academy of Arts and Science* **39** (1903), 223–242; 'The Mutation Theory of Organic Evolution, from the Standpoint of Animal Breeding', *Science* **21** (1905), 521–525; 'Heredity of Coat Characters in Guinea-Pigs and Rabbits', *Carnegie Institute of Washington Publications* **23** (1905); 'The Origin of a Polydactylous Race of Guinea-Pigs', *Carnegie Institute of Washington Publications* **49** (1906), 17–29; 'Yellow Mice and Gametic Purity', *Science* **24** (1906), 275–281; 'On a Case of Reversion Induced by Cross-Breeding and its Fixation', *Science* **25** (1907), 151–153; 'A New Color Variety of the Guinea-Pig', *Science* **28** (1908), 250–252; 'The Behavior of Unit Characters in Heredity', in *Fifty Years of Darwinism* (New York: Henry Holt and Co., 1909), pp. 143–159; 'The Nature of Unit-Characters', in *The Harvey Lectures 1910–1911* (Philadelphia and London; J. B. Lippincott Company, 1911), pp. 90–101; *Heredity in Relation to Evolution and Animal Breeding* (New York and London: D. Appleton and Company, 1911); 'On the Origin of a Pink-Eyed Guinea-Pig with Colored Coat', *Science* **35** (1912), 508–510; 'The Inconstancy of Unit-Characters', *American Naturalist* **46** (1912), 352–362; 'On the Inheritance of Tricolor Coat in Guinea-Pigs, and its Relation to Galton's Law of Ancestral Heredity', *American Naturalist* **46** (1912), 437–440; 'Some Biological Principles of Animal Breeding', *American Breeders' Magazine* **3** (1912), 270–282; 'Simplification of Mendelian Formulae', *American Naturalist* **47** (1913), 170–182; 'Multiple Factors in Heredity', *Science* **39** (1914), 686–689; 'Pure Lines and Selection', *Journal of Heredity* **5** (1914), 93–97; 'Review of *Problems of Genetics*, by W. Bateson', *Science* **40** (1914), 241–245; 'Variation and Selection: A Reply', *Zeitschrift für induktive Abstammungs- und Vererbungslehre* **12** (1914), 257–264; 'Mr. Muller on the Constancy of Mendelian Factors', *American*

Blending inheritance, though, was a somewhat confusing name to describe the phenotypic patterns, because it also could be applied to the blending of the genetic material, not only to blending-like effects. For Castle, in his initial uses of the term, blending inheritance referred only to cases 'in which the offspring are intermediate between the parents, and this intermediate condition persists in the next generation'.[3] However, this description of the phenotypic level did not specify what was occurring at the genetic level. Are these cases of 'blending' effects caused by the cumulative effects of several units that are subject to segregation? In other other words, is this continuous pattern caused by the combined effects of several genes or does the gene responsible for the phenotypic character in question change?

Castle was aware that there were two possible explanations of his results. One, that the gene was fluctuating. The other, that the modification was brought about by a stable gene whose expression was modified by the summative effect of other genes. The first alternative implied that the genetic factor was unstable, the second did not. Castle opted for the first alternative. According to him, the contamination of the genes in the zygote plus the selection of the extremes brought about a change in the genes themselves. Castle thought that in cases like the pattern of the hooded rats, size inheritance in rabbits, etc., the phenotypic pattern did not indicate the existence of more than one gene. Therefore, the one gene involved in these cases had to change. The changes in the phenotype, he argued, were due to changes in the genotype:

> I have shown in numerous specific cases that when unlike gametes are brought together in a zygote, they mutually influence each other; they partially blend so that after their separation they are less different from each other than they were before.[4]

One of the proponents of the multiple factors hypothesis, East, initiated a debate with Castle about the interpretation of the results obtained with the hooded rats. Although multifactorial Mendelian inheritance could account for the phenotypic patterns shown in the inheritance of quantitative characters, Castle rejected this idea in the name of simplicity. Castle also accused East of

Naturalist **49** (1915), 37–42; 'Some Experiments in Mass Selection', *American Naturalist* **49** (1915), 713–726; 'Can Selection Cause Genetic Change?', *American Naturalist* **50** (1916), 248–256; *Genetics and Eugenics* (Cambridge, Mass. 1916, 4th rev. edn., 1932); 'Is the Arrangement of the Genes in the Chromosomes Linear?', *Proceedings of the National Academy of Sciences of the United States of America* **5** (1919), 25–32; 'Piebald Rats and Selection, A Correction', *American Naturalist* **53** (1919), 370–375; 'Piebald Rats and the Theory of Genes', **53** (1919), 126–130; 'Heredity: the General Problem and Historical Setting', in *Our Present Knowledge of Heredity*, Mayo Foundation Lectures, 1923–1924. (Philadelphia and London: W. B. Saunders Co. 1925); 'The Beginnings of Mendelism in America', in L. C. Dunn (ed.), *Genetics in the 20th Century* (New York: Macmillan, 1951). For biographical details on Castle, see L. C. Dunn, 'William Ernest Castle', *Biographical Memoirs: National Academy of Sciences of the U.S.A.* **38** (1965), 31–80; and G. E. Allen, 'W. E. Castle', *Dictionary of Scientific Biography*, (New York: Scribner's, 1970–8), vol. 3, pp. 120–124.

[3]W. E. Castle, 'The Nature of Unit-Characters', p. 96.

[4]W. E. Castle, 'Can Selection Cause Genetic Change?', *American Naturalist* **50** (1916), p. 253.

idealism because multifactorial Mendelian inheritance explained the phenotypic results by introducing factors whose presence was not directly deducible from the observable results.

The aim of this paper will be to show that neither simplicity nor realism played a role in this debate about the stability of the hereditary units. Both East and Castle appealed to simplicity, but there was no way of deciding which scheme was the most simple. The issue of the reality of genes also could not decide the issue because East was not the antirealist that Castle said. East did believe in the existence of genes. Therefore, the contemporary appraisal of East as an instrumentalist is not correct. I will analyze East's pattern of reasoning and argumentation and show that he thought genes were material entities in the germ cells. I conclude that the choice between modified and modifer genes was determined empirically, and not by commitments to simplicity or realism.

In discussions on realism and simplicity it is necessary to pay attention to the scientists' work, and not only to their words, in order to assess the role that philosophical criteria play in the evaluation of scientific hypotheses. Many important issues in the philosophy of science can be discussed intelligently only with reference to the history of science, but there is a danger in taking arguments seriously just because they are presented as such. Very often appeals to philosophical criteria like simplicity, realism, etc., are put forward only to score points in a debate. These criteria can be used as rhetorical devices while playing no substantive role in the scientist's work. Therefore, scientists' words cannot always be taken at face value. In the case at hand, to defend his position on the instability of the hereditary units, W. E. Castle appealed to realism and simplicity. In this paper, I will argue that these criteria did not play any role in the outcome of the important debate over the stability of genes.[5]

2. Realism and Instrumentalism. Genes as Hypothetical Factors

East defended the introduction of as many factors as necessary to explain the breeding results. Castle opposed this strategy and accused East of postulating what he called 'merely subjective' entities to widen the scope of the Mendelian rule of segregation. Castle claimed to attribute material reality to the genetic units.[6] Castle's criticism gives the impression that East was an

[5]On the rhetorical dimensions of scientist's pronouncements about method, see the papers in J. A. Schuster and R. R. Yeo (eds), *The Politics and Rhetoric of Scientific Method* (Dordrecht: D. Reidel, 1986). Also M. Mulkay and G. Nigel Gilbert, 'Putting Philosophy to Work: Karl Popper's Influence on Scientific Practice', *Philosophy of the Social Sciences* **11** (1981), 389–407.

[6]W. E. Castle, 'Multiple Factors in Heredity', *Science* **39** (1914), 688. See quotation corresponding to footnote 20 of this paper.

instrumentalist, whereas Castle was looking for not only a useful scheme to explain certain facts, but for the real mechanism in nature responsible for them.

Castle's interpretation of his results with the hooded rats was that the unit-character responsible for the hood and the stripe on the back had changed. A detailed analysis of his work shows that he used the word 'unit-character' to refer to the genetic units.[7] The multiple factor hypothesis explained the gradual variation of the quantitative characteristics by pointing to the combination of the independent effects of many genetic units. There was no need to appeal to a blending of the genetic factors. However, Castle was extracting conclusions about the genetic material based on phenotypic evidence. East stated the necessity of first clarifying the meaning of the term *unit-character*:

> If one describes a unit character as the somatic expression of a single genetic factor or hereditary unit, he at once gets into trouble. As the factor and not the character is the descriptive unit, a unit factor may affect a character but that character may never be expressed except when several units cooperate in ontogeny. I should prefer to disregard the word character therefore in formulating the problem.[8]

It is important to note that East was not proposing to reject the factors — the hypothetical non-observable entities — but rather, to reject the concept of

[7]The conclusion that a unit-character had changed amounted to, in Castle's view, the view that the germinal factor (or gene) had changed. For the confusions and misunderstandings introduced by Castle's use of the term 'unit-character', see my 'The Importance of the Genotype-Phenotype Distinction in the Early Days of Genetics: An Analysis of the Unit-Character Fallacy as Exemplified in W. E. Castle's Work' (unpublished manuscript).

[8]E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *American Naturalist* **46** (1912), 645. For the work of East consulted for this paper, see: E. M. East, 'The Distinction between Development and Heredity in Inbreeding', *American Naturalist* **43** (1909), 173–181; 'A Mendelian Interpretation of Variation that is Apparently Continuous', *American Naturalist* **44** (1910), 65–82; 'Notes on an Experiment Concerning the Nature of Unit Characters', *Science* **32** (1910), 93–95; 'The Role of Hybridization in Plant Breeding', *Popular Science Monthly* **77** (1910), 342–354; 'The Role of Selection in Plant Breeding', *Popular Science Monthly* **77** (1910), 190–203; 'The Genotype Hypothesis and Hybridization, *American Naturalist* **45** (1911), 160–174; 'The Mendelian Notation as a Description of Physiological Facts' *American Naturalist* **46** (1912), 633–655; 'Mendelian Formulae' (Review papers by T. H. Morgan, W. E. Castle and R. A. Emerson), *Zeitschrift fur induktive Abstammungs- und Vererbungslehre* **12** (1914), 157–159; 'Johannsen on Genetics', *Botanical Gazette* **57** (1914), 3; 'The Chromosome View of Heredity and Its Meaning to Plant Breeders', *American Naturalist* **49** (1915), 457–494; 'As Genetics Comes of Age', *Journal of Heredity* **13** (1922), 207–214; 'Mendel and his Contemporaries', *Scientific Monthly* **16** (1923), 225–236; 'The Concept of the Gene', Presented at the International Congress of Plant Sciences, Section of Genetics, Ithaca, New York, 19 August 1926; 'Biology and Human Problems', in E. M. East (ed.), *Biology in Human Affairs* (New York: Whittlesey House, 1931), pp. 1–26; 'Heredity', in East (ed.), *Biology in Human Affairs*, pp. 163–196; 'The Nucleus-Plasma Problem', *American Naturalist* **63** (1934), 289–303 and 402–439; East and H. K. Hayes, 'Inheritance in Maize', *Connecticut Agricultural Experiment Station Bulletin* **167** (1911), 1–141; East and H. K. Hayes, 'The Improvement in Corn', *Connecticut Agricultural Experiment Station Bulletin* **168** (1911), 3–21; East and R. A. Emerson, 'Inheritance of Quantitative Characters in Maize', *Nebraska Agricultural Experiment Station Research Bulletin* **2** (1913), 1–20; East and D. F. Jones, *Inbreeding and Outbreeding* (Philadelphia and London: J. B. Lippincott Company, 1919). See also Donald F. Jones, 'Edward Murray East (1879–1938)', *Biographical Memoirs; National Academy of Sciences of the U.S.A.* **22** (1944), 217–242; and W. B. Provine, 'E. M. East', *Dictionary of Scientific Biography'*, *op.cit.*, note 2, vol. 4, pp. 270–272.

unit-character because it assumed an unwarranted one-to-one relationship between phenotypic traits and genetic units and it obscured whether one was referring to the phenotypic or genetic level.

East played an important role in the rejection of the unit-character concept. But his role is misinterpreted when it is maintained that East threw out the baby with the bathwater. For example, the philosopher David Hull claims that East's position is 'an outright denial of theoretical entities'.[9] Similarly, E. A. Carlson asserts that '... in discarding the unit-character, East substituted the gene in its undefined form. To East, the gene was only a concept "completely free of any hypotheses" as Johannsen had stated, and void of any physical reality'.[10]

'Free from any hypothesis', however, does not mean the same as 'devoid of physical reality'. The fallacy of charging an author with an antirealist view because of his refusal to accept any speculative hypothesis about the nature of the referent of a theoretical term is widespread. It is common in the interpretations of Johannsen's writings. Johannsen introduced the word 'gene' and constantly emphasized that he was not assuming any particular structure for the genes. From these remarks, many historians and philosophers have concluded that Johannsen did not believe in the existence of genes. They have failed to understand the context of Johannsen's remarks. He took the word 'gene' from De Vrie's 'pangenes,' a term derived from Darwin's term pangenesis. Johannsen maintained the last part of Darwin's word, but wanted to eliminate any particular resemblance it had to its ancestors as well as to any morphological view of the genes, such as Weismann's. Johannsen did not want to make any assertion about the gene's nature, but this did not imply a denial of its existence.

East did not deny the physical reality of the genetic units in the gametes. Therefore, it is incorrect to label him as instrumentalist, idealist, or antirealist.[11] A contextualized analysis of his assertions about genes shows that he did believe in the existence of genes.

The main text on which commentators base their understanding of East as an antirealist is the following:

> As I understand Mendelism, it is a concept pure and simple. One crosses various animals or plants and records the results. With the duplication of experiments under comparatively constant environments these results recur with sufficient definiteness to justify the use of a notation in which theoretical genes located in the germ cells replace actual somatic characters found by experiment ... Mendelism is

[9]D. Hull, 'The Operational Imperative: Sense and Nonsense in Operationism', *Systematic Zoology* **17**, 444.

[10]E. A. Carlson, *The Gene: a Critical History* (Philadelphia: Saunders, 1966), p. 29.

[11]Idealism and instrumentalism are not identical positions, but in this context both positions were attributed to East to argue that East did not believe in the existence of the genetic factors used to explain breeding ratios, which can be better characterized as antirealism.

> therefore just such a conceptual notation as is used in algebra or in chemistry. No one objects to expressing a circle as $x^2+y^2=r^2$. No one objects to saying that $BaCl_2 + H_2SO_4 = BaSO_4 + 2HCl$. No one should object to saying that DR + RR = 1DR + 1RR. ... a factor, not being a biological reality but a descriptive term, must be fixed and unchangeable. If it were otherwise it would present no points of advantage in describing varying characters.[12]

To analyze carefully East's position, let us proceed piecemeal. The word 'factor', where the text reads 'a factor, not being a biological reality but a descriptive term', carries a footnote that explicitly says:

> I hope this statement is not confusing. The term factor represents in a way a biological reality of whose nature we are ignorant just as a structural molecular formula represents fundamentally a reality, yet both as they are used mathematically are concepts.[13]

It is then important to analyze these statements in their proper context. This large quote forms the first paragraph of an article written by East in 1912 in response to a series of articles by Castle published that same year. The issue in dispute between East and Castle was the stability of the genes, and the possibility of their blending in the gametes. Thus, the question was not the existence of the genes, since both parties took the reality of the genes for granted when they were discussing whether these entities had a particular property, in this case, whether they were stable or modifiable through contact with other genes.

As noted before, to account for his results with the hooded rats, Castle argued that when the genes were together in the gametes, they sometimes modified each other, i.e. they blended, and, therefore, they did not segregate as stated by Mendel. Around 1912, Castle adopted a radical position regarding his theories of contamination and modifiability or unit-characters. In 'The Inconstancy of Unit-characters,' he criticized the multiple factor hypothesis arguing that characters vary and that 'I for one will be content with the admission that variation is as continuous as water and will not press the argument against discontinuity into realms of the ultimate.[14] Here, Castle was talking about the phenotypic level. However, very often he was referring to the genetic level, and he progressively generalized the results found in hooded rats to the inheritance of other characters: 'I have yet to meet with a unit-character which is not both variable and modifiable. It is only by closing one's eyes to minor variations that one can see gametic purity in heredity'.[15]

[12]East, 'The Mendelian Notation as a Description of Physiological Facts', *American Naturalist* **46** (1912), 633.

[13]*Op.cit.*, note 12, 634.

[14]W. E. Castle, 'The Inconstancy of Unit-Characters', *American Naturalist* **46** (1912), 359.

[15]W. E. Castle, 'Some Biological Principles of Animal Breeding', *American Breeders' Magazine* **3** (1912), 279.

According to Castle, genetic units modified each other. By selecting for the extreme modifications a researcher could end up with completely new units as the result of repeated selection in a particular direction. Castle maintained that the unit-characters, meaning the factors, hereditary units or genes, were not stable. The rules discovered by Mendel in his experiments with peas were not universal; only the qualitative, discrete characteristics of organisms were produced by genetic units that segregated without contaminating each other following Mendel's scheme.

East disagreed profoundly with Castle's conclusions. He defended the stability of the genetic factors. But when East adopted the Mendelian scheme in the explanation of the inheritance of quantitative characters he accepted the assumption that several genes could influence the same phenotypic trait. He believed that even if one did not know the nature of the genetic factors, he could postulate their existence to interpret our breeding results. Because he adopted this strategy, Castle, and the contemporary commentators on East's work, took him as an antirealist. This, however, is an incorrect reading of East's position.

The argument presented by East in his 1912 reply to Castle is a complex and somewhat ambiguous one. He distinguished between two ways of understanding the genetic factors: as a descriptional notation and as a biological reality. He pointed out in the footnote mentioned before that as a biological reality the factor or gene was an entity of unknown nature localized in the germ cells. Used as a notation to describe the facts of inheritance, the factor was a concept like a number or a chemical formula.

Historians have understood that East was defending the use of the gene only as a mathematical artifice to help in making predictions. This is not accurate. According to East, there were two ways of treating the gene, or any theoretical entity: as a mathematical concept or a physical reality. He did not say that either of these ways was incorrect. He argued that to account for the hereditary patterns found in breeding experiments, one needed to use only the 'formal' gene, this is, a concept of the gene that did not include an intrinsic characterization. He added that one should be consistent in what one meant by the formal gene:

> As a mathematical concept it is the unit of heredity, and a unit in any notation must be stable. If one creates a hypothetical unit by which to describe phenomena and this unit varies, there is really no basis for description. He is forced to hypothecate a second fixed unit to aid in describing the first.[16]

East was talking here about the concept of the gene needed in breeding experiments. By crossing different organisms, researchers obtained certain ratios that found their 'ultimate' explanation in genetic elements. East noted

[16]E. M. East and D. F. Jones, *Inbreeding and Outbreeding*, *op.cit.*, note 8, p. 77.

that these ultimate elements must be stable if they were to be the explanatory basis of our descriptions. The gene was introduced as the hypothetical hereditary unit determining a phenotypic difference. If in another experiment what one took as the unit of inheritance was different, there was no basis for the description of the phenotypical level. Thus, when a hypothetical entity is used to give account of the observational level in a mathematical model it has to be taken as a stable unit. In this sense, the hypothetical entities are being used as instruments to systematize our data and make predictions. But at this level one does not find an answer about the physical stability of the unit hypothesized as stable. East continues:

> The point at issue in this connection may be explained as follows: characters do vary from generation to generation, and the question to be decided is, how much of this variation is due to the recombination of factors (considered now as physical entities) and how much is due to change in the constitution of the factors themselves. The obvious way to determine such a matter is first to appeal to Nature and see whether it is possible for characters to have a long period of stability.[17]

In other words, East points out that it is useful to distinguish between two uses of theoretical concepts: as instruments and as terms purportedly referring to real entities. To be an instrumentalist is to admit only the first aspect of theoretical terms. East, however, thought that we could use them also to refer to unobservable entities. We can use a theoretical term merely as a notation, or as a mark for a physical entity. Sometimes, we can use it as both, depending on what nature indicates about the hypothetical existence of the putative referent. What does nature tell us in the case of the genetic factors? That the term gene can be also used to refer to a physical entity, which also turns out to be stable.

> Characters are remarkably stable. They do change, but they change so rarely that a more useful purpose is served by identifying the physical unit factor with the mathematical factor unit, than to assume without justification that the physical factor is constantly changing and must be described by complex mathematical formulae using other hypothetical units having no warrant for a physical existence.[18]

Thus, we introduce a theoretical concept because it is useful for referring to a hypothetical entity. Just as in mathematical notation, the denotation of this concept should remain stable; otherwise, it is of no use for the descriptive purpose for which it was introduced. Through empirical research, we can discover more and more things about the physical referent. And, if these investigations are fruitful, we may assume with a high degree of confidence that the entities whose existence we assumed at the beginning are real. East began by treating theoretical concepts as guides for explaining certain facts, which they undoubtedly are. But, later on he believed one could assert, with

[17] *Ibid.*
[18] *Ibid.*, p. 78

good reason, the physical status of the entities that the terms represent and, perhaps, discover their true nature. In the present case, it did not make sense to establish the separation and to retain only the instrumental character, the operational value of the concept of *factor*. East said:

> For these and other reasons which might be given, could further space be devoted to the subject, we believe there should be no hesitation in identifying the hypothetical factor unit which the physical unit factor of the germ cells.[19]

Not understanding East's complete view, Castle only focused on East's assertion about the notational aspect of the factors and accused East of being an antirealist:

> The question whether Mendelian factors are constant or inconstant has been discussed from different points of view by my colleague Dr. East and myself . . . he maintaining their constancy on the ground that they are subjective merely, while I have thought it necessary to assume for them an objective existence in the germ-cell, and am unable to discover any evidence of their constancy from the behaviour of germ-cells.[20]

Castle's presentation of his differences with East is deceiving. First, East had not said that the factors were merely subjective. Second, it was Castle himself who did not accept the reality of constant factors, as can be seen in his criticisms of the multiple factor hypothesis:

> Practically everyone has now abandoned the idea of '*gametic purity*', but the idea of purity has been shifted from the characters which can be seen to vary, to *factors* which may be imagined to be invariable, though they can not be seen.

And,

> What ground is there, then, for supposing that in a case where no factors are demonstrable, such factors are *invariable*? This is like supposing that the moon is made of cheese and that further this cheese is *green*.[21]

Castle's own words highlight, by contraposition, East's realism. East did not need to see the factors to accept their existence and infer their stability. He was one of the discoverers and strong defendants of the multiple factor hypothesis, a hypothesis that explained the observable results by postulating unobservable entities. To maintain that East did not believe in the existence of genes is a misreading of his separation between the formal gene used as a notation in breeding experiments and the biological gene, which cytological studies later showed to be in the chromosomes.

The passage quoted by D. Hull in which East says 'We have a good right therefore to poke our characters into the germ cell and to pull them out again

[19] *Ibid.*
[20] W. E. Castle, 'Multiple Factors in Heredity', *Science* **39** (1914), 688.
[21] W. E. Castle, 'Pure Lines and Selection', *Journal of Heredity* **5** (1914), 94 and 95.

if by so doing we can develop — not a true conception of the mechanism of heredity — but a scheme that aids in describing an inheritance',[22] must be understood in the context of East's discussion with Castle. East directly replied to Castle's recommendation: 'let us in no case introduce more factors into our hypotheses than can be shown actually to exist'.[23] East's reply, however, was that one did not need irrefutable proof of the existence of those factors to introduce them as a working hypothesis.

Furthermore, it should be underscored that Castle and East debated not the existence of the factors in general, but the validity of the assumption that there exists a single factor for controlling a phenotypic unit. Thus, the question for debate was not the 'reality' of the genes. What was in dispute was whether the evidence warranted the introduction of various factors to explain a phenotypical trait. Castle believed that such a hypothesis was not warranted because at the empirical level there was no evidence for the existence of more than one gene. East was opposed to Castle's skepticism and maintained that it was legitimate to introduce more factors if they helped to explain the observable phenomena.

At this point East might still be accused of being an instrumentalist. However, the previously quoted words were written after East clearly differentiated between the notational factor and the biological factor and pointed out that he was going to talk about the first. Later on, he told us that if in using theoretical concepts nature responds in a favorable manner, then this should be interpreted as an indication that we are truly getting our hands on real entities, an indication that these hypothetical entities have a physical basis. In the instrumentalist position, theoretical concepts help to systematize and to organize theories, and to make predictions. But no matter how well they play these roles, it is not legitimate to infer the existence of the the entities denoted by the theoretical concepts. In contrast, in East's position, if the theoretical terms prove to be fruitful, then they should be accepted as referring to material entities, and, therefore, they are concepts with which we can describe and analyze nature.

The long paragraph quoted at the beginning of this section and discussed as the source of the antirealist reading of East was used by East in an almost identical way several years later in a book written with D. F. Jones. In this work, East lamented that the confusion between theory and fact in the representation of the hereditary mechanism led to a regrettable controversy over the important issue of the stability of the genetic factors. The second version of the misleading text is more explicit than the original. It is reproduced here to show the thread of East's argument:

[22] *Op.cit.*, note 12, 634.
[23] W. E. Castle, 'Yellow Mice and Gametic Purity', *Science* **24** (1906), 280.

> The relation between fact and theory in the Mendelian conception of inheritance is this: various kinds of animals and of plants were crossed and the results recorded. With the repetition of experiments under comparatively constant environments these results recurred with sufficient regularity to justify the use of a notation in which theoretical factors or genes located in the germ cells replaced the actual somatic characters found by experiment. Later, the observed behavior of the chromosomes justified localizing these factors as more or less definite physical entities residing in them. Now the data from the breeding pen or the pedigree culture plot and the observations on the behavior of the chromosomes during gametogenesis and fertilization are facts. The factors are part of a conceptual notation invented for simplifying the description of the breeding facts in order to utilize them for purposes of prediction, just as the chemical atom is a conception invented for the purpose of simplifying and making useful observed chemical phenomena. *As used mathematically, both the genetic factor and the chemical atom are concepts, but biological data leads us to believe that the term factor represents a biological reality of whose nature we are ignorant, just as a molecular formula represents a physical reality of a nature yet but partly known.*[24] [emphasis added]

East clearly is not being an instrumentalist, rather he is making an explicit realist manifesto. The argument, and even the words, are almost the same used in 1912, although by 1919 he was able to be more specific about the localization of the factors or genes. The theoretical term 'factor' was introduced to simplify descriptions of breeding results, but both genetic and cytological advances allowed the identification of genes with chromosomes. Thus, the theory of the gene unified the facts of heredity and showed that quantitative and qualitative inheritance follow the same rules, proving the universality of Mendelian segregation.

East was definitely not an antirealist. For him, theoretical terms could be used either as a useful notation or as purporting referential devices, or as both. In the case of the gene, geneticists did not need to worry about the biological gene, but cytological research had shown that the useful notation in genetics did refer, in fact, to a physical structure in the cell. When the Mendelian scheme was rediscovered in 1900 nothing was known about the genes. At this point, it was not clear whether there was a real referent for the newly introduced theoretical term. However, the cytological research carried out in the first two decades of the 1900s proved that there was a biological gene that could be identified with the formal gene.

Only after subjecting a theory to a trial period can one make definite statements regarding the status of the entities it postulates. For an instrumentalist theoretical concepts are nothing but mere tools for making predictions. For the realist, after a period of testing, hypothetical entities can acquire full standing in our ontology. Despite the fact that East knew nothing about the chemical or physical nature of the gene, he had not doubt that to discover it

[24] *Op.cit.*, note 16, p. 76.

was only a question of time and technical progress. He actually thought that the testing period for the gene had been extremely long. East did not doubt the gene's citizenship in our ontological world.

In discussing his interpretation of the experiments with hooded rats Castle recoursed to simplicity, arguing that his scheme was the most parsimonious, whereas the multiple factor hypothesis introduced *ad hoc* entities. Now I will show that the criterion of simplicity did not play a substantial role in the controversy at hand.

3. The Complexity of Simplicity

Parsimony, simplicity, Occam's razor, and unification are some of the different ways of expressing the same idea: minimality. To argue for simplicity in the context of scientific reasoning amounts to a defense of economical solutions over inflationary ones. Nevertheless, in spite of the many attempts to clarify the nature of simplicity, there exists neither a well defined and commonly accepted conception of what simplicity is nor an extensive study of its role in science.[25]

There are two contexts in which simplicity is said to play a role: in the construction of hypotheses and in the choice between alternative hypothesis, i.e. in the context of discovery and the context of justification. For example, K. Schaffner has presented two case studies, one in physics, involving the choice between Lorentz's and Einstein's theories, and the other in biology, involving the development of regulatory genetics, in which he argues simplicity played an active role. In his 'Logic of Discovery and Justification in Regulatory Genetics', Schaffner argues for the existence of certain considerations that serve both as generators of new hypotheses and as constraints on the articulation of new model types and new details within model types. Along with experimental adequacy and theoretical context sufficiency, he lists simplicity and what he calls a principle of the unity of fundamental biological processes. Schaffner recognizes that this last principle can be constructed as a case of simplicity, but

[25]Simplicity is a philosophical topic to which I can not do justice in this paper. To sort out the different types of simplicity referred to by philosophers and see how they are judged to contribute to the evidential support of a theory would require a book-length discussion. Thus, I will only give some references to works that deal with these issues: K. Friedman, 'Empirical Simplicity and Testability', *British Journal for the Philosophy of Science* **23** (1972), 25–33. N. Goodman, 'The Test of Simplicity', *Science* **128** (1958), 1064–1069. N. Maxwell, 'The Rationality of Scientific Discovery', *Philosophy of Science* **41** (1974), 123–153; 246–295. W. Quine, 'Simple Theories of a Complex World', in *The Ways of Paradox and Other Essays* (New York: Random House, 1966), pp. 242–246. H. Reichenbach, *Experience and Prediction* (Chicago: University of Chicago Press, 1938). Roger D. Rosenkrantz, *Inference, Method and Decision* (Dordrecht: D. Reidel, 1977). E. Sober, *Simplicity* (Oxford: Oxford University Press, 1975). E. Sober, *Reconstructing the Past: Parsimony, Phylogeny, and Inference* (Cambridge: Bradford/MIT Press, 1988). B. v. Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980).

prefers to differentiate it on the basis that it has generating capacity while simplicity acts more as a constraint. Thus, simplicity, defined as the desideratum to minimize properties and relations whose existence has been independently corroborated, is viewed as acting primarily as a constraint and not a generator of new hypotheses. Although Schaffner has analyzed a single case, he believes that simplicity has exercised an important constraint on the development of new theories in biology.

His case study in physics suggests that simplicity was used as a consideration in the selection of two competing hypotheses. He argues that the elimination of the ether made Einstein's theory simpler than its competitor and, thus, more attractive. Leaving aside the problems with his interpretation of these historical examples, we can take his ideas as an example of the belief that simplicity plays an active role in the dynamics of scientific development.[26]

In principle, the defence of simplicity need not be made both at the level of hypothesis building and the hypothesis selection. E. A. Carlson, writing on the history of genetics, points out the explicit use of simplicity by many of the geneticists involved in the development of the theory of the gene, and, sometimes, he even explains progress by the use of simplicity or Occam's razor. Nevertheless, in the conclusion of his deservedly classic book *The Gene*, he talks about the limitations of Occam's razor. He correctly states that many 'losers' made use of the principle of simplicity: Castle to disprove the existence of modifying factors and the interference hypothesis; Bateson to justify his 'presence and absence' theory against the model advocated by the *Drosophila* group; Goldschmidt to attack the hypothesis of position effect; Stadler to advocate breakage as the exclusive mechanism of radiation mutagenesis in plants and animals; and Pontecorvo to interpret all multiple allelic series as cistrons.

However, Carlson allows the possibility that Occam's razor might be useful in the construction of one's own model, in spite of denying it any scientific merit in cases of conflict or controversy because

> it avoids the need for an exploration of the different levels of experimental analysis; it evades a critical study of the predictability of different model systems, and it

[26]See K. F. Schaffner, 'Outlines of a Logic of Comparative Theory Evaluation with Special Attention to Pre- and Post-Relativistic Electrodynamics', *Minnesota Studies in the Philosophy of Science* **5** (1970), 311–353, and 'Logic of Discovery and Justification in Regulatory Genetics', *Studies in History and Philosophy of Science* **4** (1974), 349–385. Schaffner (1974) differentiates between three dimensions of relative simplicity: fitness, ontological simplicity, and system simplicity. It is not necessary here to enter into the details of his proposal, the point for our purposes is that he presents a view in which simplicity is said to play an active and important role in science. Also, G. Buchdahl, in his 'History of Science and Criteria of Choice', *Minnesota Studies in the Philosophy of Science* **5** (1970), 204–229, divides the factors that appear to determine the acceptability of scientific hypotheses into three categories: conceptual explication, constitutive articulation and architectonic determination. One of the components of the latter — along with preferred explanation types and consilience — concerns regulative ideas and maxims that include simplicity and economy (also considerations of an aesthetic nature, continuity, etc.) Buchdahl

> evades the need for the comparative examination of a genetic principle in several organisms and in more than one experimental design.[27]

He therefore argues against the use of simplicity in the context of justification, but does not criticize its use in the context of discovery.

I will try to show that, whether in the context of discovery or the context of justification, scientists do not always make use of the tools that they argue they are using. Sometimes this occurs because they employ an argument as a rhetorical device to score points against a competitor. Sometimes because the criterion they argue for, in this case simplicity, is not capable of playing the expected role. Thus, the problem is not to find out whether scientists appeal to simplicity, not even to explain why they may do so, because many pragmatic considerations could be adduced here. The point is to analyze whether the principle of simplicity can do what it is supposed to be doing, namely, to adjudicate between alternative hypotheses, and whether it actually does it.

The questions to answer are: can simplicity serve as a criterion to choose between competing hypotheses either in the construction of one's own model or in the choice between alternative theories? And, does simplicity, stated as a criterion of minimality, offer us a solution in conflict situations? I argue that in situations of choice, simplicity often cannot play a significant role for two reasons. First, one rarely comes across theories that are simpler than their rivals in all respects. If one theory is simpler than another only in some respects, the need to explain why one should pay attention to certain respects and not others brings us back to the empirical realm, i.e. to questions about the substantive claims of the hypotheses. Second, since the simplicity of a theory is not a sufficient reason for its correctness, as the history of many

believes that these maxims play a role in the acceptance and rejection of scientific hypotheses (see p. 213).

[27]Carlson, *op. cit.*, note 10, p. 251. William Bateson appealed to simplicity to defend his presence–absence hypothesis with the argument that, although Mendel had thought of 'a definite something' both for the dominant and the recessive character, 'it is however evidently simpler to imagine that the dominant character is due to the presence of something which in the case of the recessive is absent'. See his *Mendel's Principles of Heredity* (Cambridge: Cambridge University Press, 1909), p. 135. K. Pearson was scientifically and philosophically a strong advocate of simplicity, as can be seen in his *The Grammar of Science*, 2nd edn (London: Black, 1900). Likewise, R. Goldschmidt always argued to favor the most economical solutions. In his 'Different Philosophies of Genetics', *Proceedings of the 9th International Congress of Genetics* (1954), pp. 83–99, he asserts that the words of Willard Gibbs, 'One of the principal objects of theoretical research in any department of knowledge is to find the point of view from which the subject appears in its greatest simplicity', had guided his work in genetics (p. 99). Goldschmidt actually favored Castle's side in the controversy about the stability of factors for simplicity reasons, as G. Allen has noted: 'The concept of modifying factors was spurious, a *deus ex machina* which did not seem to fit the facts'. See G. Allen, 'Opposition to the Mendelian-Chromosome Theory: The Physiological and Development Genetics of R. Goldschmidt', *Journal of the History of Biology* 7 (1974), 49–92. He also appealed to simplicity to support his own conception of the continuum model of the chromosome. My claim is that what is seen as 'the point of view from which the subject appears in its greatest simplicity' depends on empirical considerations, and, thus, on the adequacy, and not simplicity, of our conception.

simple and rejected theories shows, the best bet in situations of conflict would be to suspend judgment, rather than support one theory on considerations of simplicity alone. Thus, simplicity, very often, is either a cover for empirical assumptions about nature or an aesthetic desideratum with no relevant role in scientific advances. Furthermore, in many situations there is no clear-cut way of deciding which is the most simple or economical theory.

Taken as a rule to choose between alternative competing hypotheses, ontological simplicity tells one to adopt the explanation that introduces fewer kinds of entities or processes. One explanation is preferable to another when it postulates fewer entities or processes. The definition presents an initial difficulty: the principle of simplicity could be unproblematic, if it asked for the theory with the fewest entities *and* processes, but, sometimes, for example in the case debated between East and Castle, the postulation of entities is avoided at the cost of introducing new processes and *vice versa*.

How did simplicity come about in the discussion on the stability of factors? The situation in the Castle–East argument was the following: there was a set of phenomena in need of explanation, namely the pattern of inheritance of quantitative characters and, more specifically, the results obtained in Castle's selection experiments with hooded rats. East's and Castle's theories could account for these phenomena equally well. Empirically, there seemed to be no reason to choose between variable factors and the multiple factor hypothesis. How to choose? Castle referred to simplicity:

> To sum the matter up, it is certain that unit-characters exist, but it is equally certain that the units are capable of modification; gametic segregation certainly occurs in some cases (Mendelian inheritance), it does not occur in others (blending inheritance); factors of characters certainly exist, when characters are demonstrably complex and result from the coexistence of two or more simpler ones, as, for example, a purple pigmentation due to coexistence of red and blue chloroplastids in plants. But let us in no case introduce more factors into our hypotheses than can be shown actually to exist.[28]

Scholars have accepted Castle at his word: 'Castle adopted the unitary hypothesis as the better interpretation because it involved fewer assumptions'.[29] Should we, then, in the name of simplicity, favor Castle's interpretation over that of East? If we are going to apply a minimalist criterion, then we should expect Castle's hypothesis to be really simpler than the alternative one. But, was this so?

As it happened, the opposition also appealed to simplicity. Thus, in response to the statement by Castle that 'The conclusion seems to me unavoidable that

[28]Castle, 'Yellow Mice and Gametic Purity', *Science* **24** (1906), 280.
[29]Carlson, *op. cit.*, note 10, p. 26.

in this case selection has modified steadily and permanently a character unmistakably behaving as a simple Mendelian unit,'[30] East said:

> This conclusion, from the writer's standpoint, is not only avoidable, but *unnecessary*. No direct or implied denial of these facts is made, but a shift is made in the point of view. It seems to me a logical necessity that hypothetical units used as measurement or descriptive standards be fixed. *The problem to be solved is the simplest means of thus expressing the facts.* If the most definite characters — i.e., certain pure-line homozygotes — are sufficiently constant in successive generations to be expressed by a fixed standard, well and good. The whole heredity shorthand is then *simple*.[31]

And again:

> It is the expressed character that is seen to vary; and if one can describe these facts by the use of hypothetical units theoretically fixed but influenced by environment and by other units, *simplicity of description is gained.*[32]

Although East put his point in terms of simplicity of description, it is important to note that he is referring to ontological simplicity. This can be seen when he expressed his view about Mendelian genetics (including also quantitative genetics): 'It is a very *simple* conception of heredity, moreover, for it allows a multitude of individual transmissible differences with the assumption of a *very few* factors'.[33]

By using stable factors whose effects combine in their expression, East was able to explain both qualitative and quantitative characters with the same scheme. This unification in the description of heredity was a gain in simplicity. On the one hand, he had to introduce as many modifying genes as necessary to explain the segregation ratios found in the experiments. On the other hand, he had a unitary system of description. In Castle's scheme, there was no need for a new type of entity, the modifier gene, if it was assumed that the hereditary units responsible for the character varied and, thus, did not follow the Mendelian rule of segregation. Thus, he had to introduce two types of processes, Mendelian inheritance and blending inheritance, to account for differences in the observable facts of heredity. What would simplicity choose here: *fewer entities or fewer processes*? Simplicity simply cannot decide.

A criterion of unification favored East's scheme because it allowed one to give an account of all phenomena of inheritance by the postulation of only one mechanism, but it required numerous token factors. A criterion of minimality in regard to types of entities favored Castle's theory because it did not require one to introduce a new type of factor, i.e. modifier genes. Simplicity could not help in this conflict because there was no way of establishing which theory was

[30]Castle, 'The Inconstancy of Unit-Characters', *American Naturalist* **46** (1912), 356; quoted by East, *op. cit.*, note 12, p. 647.

[31]East, *op. cit.*, note 12, p. 647.

[32]*Ibid.*, p. 651.

[33]*Ibid.*, p. 649.

the most parsimonious one in *all* respects. One could defend that the introduction of a new type of entity was necessary. Or one could say that this type of entity was not needed. A this point it became necessary to discuss the adequacy of the experimental material, the set-up of the experiment and the validity of the results obtained and the conclusion drawn. Thus, support for the conclusion was not based upon a general and abstract criterion of simplicity, but derived from specific empirical considerations.

As it happened, the main criticism of Castle's conclusions was not that his theory was complex. Geneticists pointed out his unwarranted inferences from the phenotypic to the genotypic level (Muller, Pearl); the lack of reliable data, i.e. individual pedigrees, to answer the question of the effectiveness of selection on the genetic factors (Hagedoorns); the confusions created by his use of the term 'unit-character' (Hagedoorns, MacDowell, East); the lack of any other experiments in genetics supporting his conclusions since the experiments carried in other organisms disconfirmed Castle's hypothesis (Sturtevant). These were the real reasons why the scientific community found the multiple factor hypothesis more plausible than the idea of unstable factors that selection could modify.[34] The final verdict came when a crucial experiment suggested by Sewall Wright (then Castle's graduate student at the Bussey Institution) was carried out at Castle's laboratory. The extreme grades obtained in both the plus and minus series were crossed with wild-type rats. The progeny were rats converging towards the standard pigmentation that eventually came closer and closer to the normal hooded rats. This result could be explained by the existence of modifiers for the hooded pattern, modifiers that had been accumulated in the plus series and eliminated in the minus one. These results, and not considerations of simplicity, decided the outcome on the question of the stability of factors.

4. Concluding Remarks

The polemic between East and Castle is interesting historically because it shows an important aspect in the development of genetics at the beginning of the century. During the first decade of genetic research there were many discussions in which conceptual and empirical issues were tightly intertwined.

[34]See A. L. Hagedoorn and C. Hagedoorn, 'Studies on Variation and Selection', *Zeitschrift für induktive Abstammungs- und Vererburngslehre* **11** (1913), pp. 147 and 162. A. L. Hagedoorn and C. Hagedoorn, 'Selection in Pure Lines', *American Breeders' Magazine* **4** (1913), 165–168. E. C. MacDowell, 'Piebald Rats and Multiple Factors', *American Naturalist* **50** (1916), 739. H. J. Muller, 'The Bearing of the Selection Experiments of Castle and Phillips on the Variability of Genes', *American Naturalist* **48** (1914), 567. R. Pearl, 'Seventeen Years Selection of a Character Showing Sex-Linked Mendelian Inheritance', *American Naturalist* **49** (1915), 608. A. H. Sturtevant, 'An Analysis of the Effects of Selection', *Carnegie Institute of Washington Publications* number 264 (1918).

Thus, the study of this period is important both to understand the evolution of the concept of the gene and to analyze the way scientific thinking itself evolves, i.e. the dynamics of scientific thought.

Castle rejected multiple factors because the phenotypic evidence did not show the existence of more than one factor. Actually, the observable evidence did not even show the existence of one factor. Even in the case of qualitative inheritance, the existence of factors was hypothetical, and its plausibility derived from the usefulness of the Mendelian scheme. By the same standards, East maintained, to bring quantitative inheritance under the scope of Mendelian segregation, one needed to show only that the Mendelian notation also adequately described the breeding results in cases of quantitative inheritance.[35]

This did not mean, though, that factors were merely subjective or that East was an instrumentalist. East was making a distinction between the Mendelian scheme as a notation of breeding results and the physical mechanism responsible for these results. His contention was that if in the case of qualitative characters we accepted the Mendelian laws without demanding a conclusive proof of the reality of the factors, we should not raise our standards in the case of quantitative inheritance. For East, quantitative and qualitative inheritance were manifestations of the same phenomena and, as such, had to be explained by a common cause. A law of inheritance that could only account for the inheritance of a certain type of character was seriously faulty. Thus, the Mendelian scheme had to be able to explain the inheritance of quantitative characteristics or be discarded. Since it explained the inheritance of qualitative characters, East thought science should strive to explain quantitative inheritance with it. A good research strategy is to look for a common cause when we are dealing with similar phenomena.

[35]In 'The Inheritance of Quantitative Characters in Maize', *Nebraska Agricultural Equipment Station Research Bulletin* **2** (1912), 5, in a footnote following the title, East and Emerson specify: 'The prevailing Mendelian terminology is followed in this paper, but it must not be assumed that the writers regard Mendelian formulae as other than a helpful descriptive shorthand convenient for describing breeding facts. Hypothetical germ cell factors are substituted for somatic characters because they are useful in exactly the same manner that hypothetical formulae are useful in describing chemical reactions. To establish the contention that quantitative characters are essentially Mendelian in their inheritance, therefore, it is only necessary to show that the notation adequately describes the breeding facts.' We must examine the context of these words. East is going to put forward — together with Emerson — an hypothesis to explain the inheritance of quantitative characters. He is aware that one of the main objections to the multiple factors hypothesis is that the breeding ratios do not prove the existence of various factors. East is arguing here that in the description of qualitative inheritance we do not have proof of the existence of one factor, but only a notation useful in describing the breeding ratios. Therefore, this is also what we are allowed to expect from his hypothesis on quantitative inheritance: an accurate description of the breeding ratios. For East, breeding facts fell all under one category and the question was whether the Mendelian notation could describe them adequately. If qualitative inheritance was Mendelian, quantitative inheritance had to be too (or neither of them; see his 1912 article). And, because the Mendelian notation is able to account for both types of heredity, we should treat the theoretical terms used in it as referring to real entities.

Nevertheless, this strategy is not always easy to apply. Very often scientists realize that two phenomena are similar only *after* discovering that they have a common cause. By focusing on transmission, East saw the heredity of qualitative and quantitative characters as an expression of the same phenomena. Therefore, he supposed they had a common cause and thus believed they could be described by the same system of description. Castle, on the other hand, focused on expression, on the effects of inheritance, and saw qualitative and quantitative inheritance as different phenomena. I am not claiming that these authors had this distinction in mind. But Castle, by focusing on the observational level, was actually seeing the effects of inheritance, whereas East, by focusing on the factors, was looking at the mechanism of transmission. At the causal level, both types of inheritance are similar precisely because they have a common cause. At the observational level, they were constituted as different phenomena. Castle did not apply a criterion of unification because he did not see them as similar effects. Sometimes, it is only when we know that two sets of phenomena can be explained by a common cause that we come to see them and group them as similar.

East clearly separated the descriptive level from the physical one, but he also argued that, given the physiological and cytological results reached, one could identify the notational factors with the physical factors carried by the chromosomes. In my opinion, Castle accused East of taking an easy path by introducing subjective factors as a defence of any criticism of his own position.

The hypothetical character of East's factors was both misunderstood by Castle and by the historical exegesis that erected East as the prototype of the instrumentalist scientist. For East, first there was a hypothetical scheme that operated with theoretical unit-factors as hereditary units, i.e. there was a hypothetical genetic level. This scheme was able to explain the facts observed in breeding experiments with qualitative characters, as Mendel himself proved. By further investigations, scientists found that the same scheme was able to explain the inheritance of quantitative characters. Given that the Mendelian conception was able to account for such a wide range of phenomena, it had to be capturing the real mechanism in nature. Thus, one could identify the units of the notational description with the physiological units postulated in the chromosomes. The original hypothetical factors turned out to have a solid biological basis.

In defending their views in this highly controversial period of genetics, scientists appealed very often to extra-empirical considerations. For example, in the discussion concerning the Mendelian notation, with contributions from important geneticists including T. H. Morgan, W. E. Castle, and R. A. Emerson, all of them appealing to simplicity, one could conclude that this criterion influenced the choice of symbolism preferred by each one of these researchers. Analysis shows that, contrary to the scientist's rhetoric, simplicity could not play any role because it was really not possible to decide which one was the most simple notation, expecially if we want a simple *and* adequate notation.[36]

In the East–Castle debate, what appears to be a methodological issue was actually a question of empirical evidence for the nature of the genotype. Neither realism not simplicity played a substantive role in the discussion about the stability of the genetic factors. I take this case to support the more general thesis that the adequacy of a hypothesis is determined empirically, not by appeals to simplicity or to commitments regarding instrumentalism or realism. Simplicity, I have argued, is a problematic criterion, and more work needs to be done to assess its role in scientific development. Epistemological positions — like realism and instrumentalism — no doubt can influence one's work in science, but we are far from knowing how much and in what ways they do. Furthermore, I have argued for the need to look at scientific work, not only rhetoric in analysis of philosophical issues in the history of science. Sometimes, in the history of science, criteria presented as methodological rules are nothing but rhetorical devices. Historical exegesis, then, should be aware of these window-dressing devices, and separate them from substantive methodology.

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[36]T. H. Morgan, 'Factors and Unit Characters in Mendelian Heredity', *American Naturalist* **47** (1913), 5–16, and 'Simplicity versus Adequacy in Mendelian Formulae', *American Naturalist* **47** (1913), 372–374; W. E. Castle, 'Simplification of Mendelian Formulae', *American Naturalist* **47** (1913), 170–182; and R. A. Emerson, 'Simplified Mendelian Formulae', *American Naturalist* **47** (1913), 307–311.