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Objectivity versus Truth*

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Abstract: The aim of this paper is to clarify the difference between science practiced in a world divided up into the true and false and that pursued in a world divided up into objective and subjective. Althogh both truth and objectivity broaden into a spectrum of meanings, they do not coincide. This paper traces the fortunes of the rival epistemological virtues of truth and objectivity less through philosophical pronouncements than through the quotidien practies of naturalists. The contrast between a regime a truth and one of objectivity is heavy with concrete consequences for everything from the choice of scientific images to the formation of scientific personae.

Resumen: El propósito de este artículo es clarificar la diferencia entre la ciencia que se practica en un mundo dividido entre lo verdadero y lo falso y la que tiene lugar en un espacio que divide lo objetivo de lo subjetivo. Aunque tanto la verdad como la objetividad se expanden en una gran pluralidad de significados, estos últimos no coinciden. Este artículo traza las fortunas de las virtudes epistemológicas de la verdad y la falsedad no tanto a través de textos filosóficos cuanto en relación a las prácticas científicas cotidianas. El contraste entre un régimen de verdad y otro de objetividad está repleto de consecuencias, desde la elección de imágenes científicas hasta la formación del científico como «personae».

Introduction**

What is the opposite of truth? Falsehood, surely, but what shade of falsehood--lies? errors? fictions? fantasies? Depending on what nuance of falsehood we choose, truth itself takes on a different coloring. Jean-Jacques Rousseau once came perilously close to suggesting that facts were the opposite of truth: «Fictions which have a moral end in view are called parables or fables, and since their aim is or should be to present useful truths in a form which is pleasing to the senses, there is hardly any attempt in such cases to conceal the factual untruth, which is merely the disguise of truth, and the person who tells a fable simply as a fable is not in any sense a liar»[1]. Contrast this with the stern warnings of John Locke approximately a century earlier against the blandishments of metaphor and rhetoric: «all the artificial and figurative application of words eloquence hath invented, are for nothing else but to insinuate wrong ideas, move the passions, and thereby mislead the

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** Unless otherwise noted, all translations are my own. Because this lecture is to be given to a bilingual German-English edition, I have kept German quotations in the original.

1 Jean-Jacques Rousseau, *Reveries of the Solitary Walker* [comp. 1776-78, publ. 1782], trans. Peter France (Harmondsworth: Penguin, 1979), «Fourth Walk,», p. 69.

judgment; and so are perfect cheats:...and where truth and knowledge are concerned, cannot but be thought a great fault, either of the language or the person that makes use of them»[2]. For Rousseau, truth and justice may be used almost interchangeably, hence fictions that serve justice may be truer than facts of indifferent utility; for Locke, truth meant the exact correspondence between ideas and things, and any ambiguities or ornaments that might blur that correspondence were therefore the enemies of truth —in poetry as well as in philosophy[3]. Neither Locke nor Rousseau would have subscribed to a doctrine of multiple truths, yet their disagreement about what is the opposite of truth is emblematic of the range of meanings truth may have, and this in the scant span of a hundred years.

That range is not much narrowed if we restrict ourselves to scientific truth. For the early Fellows of the Royal Society of London, the opposite of truth in natural philosophy seems to have been neither lies nor errors of any simple kind, but rather what Bishop Thomas Sprat, in his 1667 apologia for the Royal Society, derisively called «Romances», i.e. a taste for the «extravagant» or wondrous in nature: «For it will make men inclinable to bend the Truth much awry, to raise a specious Observation out of it....It is like *Romances*, in respect of *True History*; which by multiplying varieties of extraordinary Events, and surprizing circumstances, makes that [true history] seem dull and tasteless»[4]. According to Sprat, it is the «very delight» of such marvel-studded narratives about nature that perverts truth, an echo of Francis Bacon's earlier warning that the pleasures of poetry stand seductively opposed to the sobrieties of truth in both civil and natural history[5]. On this view, it was the Don Quixotes of natural philosophy, not forgers or bumblers, who posed the greatest threat to truth, mistaking windmills for giants, the ordinary for the extraordinary in nature. For late eighteenth-century natural philosophers, however, the temptations to distort the truth tugged in the opposite direction. They worried about the *esprit de système*, which exaggerated the regularity rather than the irregularity of nature. The French comparative anatomist Georges Cuvier, writing critically of the developmental theories advanced by the naturalist Jean-Baptiste Lamarck, compared such systems within which all evidence is forced to fit into «vast edifices [built] upon imaginary foundations»[6]. For both Sprat and Cuvier, the unregulated imagination and its pleasures endangered scientific truth, but in diametrically opposed ways: in the one case by inventing pleasing anomalies and in the other by inventing pleasing order.

If we turn from the opposites of truth to those of objectivity, we at first seem to encounter a neater, more compact pair. The opposite of objectivity is subjectivity, as dictionaries in German, French, and English from around the 1830s onward repeatedly testify in their definitions and cross-references[7]. But on closer inspection, the palette of the subjective turns out to be as variegated as that of falsehood. The subjective can refer to individual idiosyncracies that confine one's view to a

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2 John Locke, *Essay Concerning Human Understanding* [1690], ed. Alexander Campbell Fraser, 2 vols. (New York: Dover, 1959), III.x.34, vol. 2, p. 146.

3 Ibid., III.xi.26, vol. 2, p. 164.

4 Thomas Sprat, *History of the Royal Society* [1667], eds. Jackson I. Cope and Harold Whitmore Jones (Saint Louis: Washington University Press, 1958), pp. 90-91.

5 «The use of this feigned history [poesy] hath been to give some shadow of satisfaction to the mind of man in those points wherein the nature of things doth deny it, the world being in proportion inferior to the soul;[...]». Francis Bacon, *The Advancement of Learning* [1605], in Basil Montagu, ed., *The Works of Francis Bacon*, 16 vols. (London: William Pickering, 1825-34), vol. 2, pp. 119-120.

6 Georges Cuvier, *Recueil des éloges historiques lus dans les séances publiques de l'Institut de France* [1819-27], 3 vols. (Paris: Firmin Didot Frères, Fils, 1861), vol. 3, p. 180.

7 On the early history of both words, see Lorraine Daston, «How Probabilities Became Objective and Subjective,» *Historia Mathematica* 21(1994): pp. 330-344.

particular perspective--be it that of a physicist versus that of a chemist, of a Frenchman versus that of a German, or of an earthling versus that of a martian. Or the subjective can refer to a tampering with a given state of affairs, as when a photograph is retouched in the interest of aesthetics, or data is massaged in the interest of a pet hypothesis. To each form of subjectivity corresponds an equally distinctive form of objectivity: the mechanical objectivity that wants nature to be allowed to «speak for itself», or the communitarian objectivity that seeks to eliminate idiosyncracies of individual researchers and research groups. Mechanical and communitarian objectivities possess their own contrasting metaphysics, methods, and morals, which can and do lead to conflicts in the concrete choice of a scientific instrument, protocol, or technique of illustration. The French physiologist Claude Bernard spoke in the name of mechanical objectivity when he instructed the experimenter on how to pose questions to nature: «but as soon as she [nature] speaks, he must be silent; he must affirm what she answers, hear her out until the end, and, in all cases, submit to her decisions»[8]. The American philosopher and geophysicist Charles Sanders Peirce spoke in the name of communitarian objectivity when he equated reality and community in science: «The real, then, is that which, sooner or later, information or reasoning would finally result in and which is therefore independent of the vagaries of me and you. Thus the very origin of the conception of reality shows that this conception essentially involves the notion of *community*, without definite limits, and capable of an indefinite increase of knowledge»[9].

In a series of recent articles, Peter Galison and I have argued for the plurality of objectivity in scientific practice as well as precept, and for the relatively recent emergence of both kinds of scientific objectivity, mechanical and communitarian, in the mid-nineteenth century[10]. I do not intend to repeat those arguments here, though they do serve as background for this paper. Instead I would like to clarify the difference between science practiced in a world divided up into the true and the false and that pursued in a world divided up into objective and subjective. Although both truth and objectivity broaden into a spectrum of meanings when examined closely (especially from the standpoint of their opposites), they at most overlap; they do not coincide. And even their overlap turns out to be surprisingly small, limited to the metaphysical overtones of objectivity as a God's-eye-point-of-view. The phrase «objectively true» is not redundant. In order to throw the contrasts between the ideals and practices of scientific truth and of scientific objectivity into relief, I will focus on examples drawn from several scientific disciplines in the eighteenth and nineteenth centuries. My aim is to trace the fortunes of the rival epistemological virtues of truth and objectivity less through philosophical pronouncements than through the quotidien practices of naturalists, although I will occasionally call upon a philosopher as articulate witness to those practices. In drawing a contrast between the truth-oriented sciences of the mid-eighteenth and early nineteenth centuries, and the objectivity-oriented sciences of the mid- and laste nineteenth-centuries, I do not

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8 Claude Bernard, *Introduction à l'étude de la médicine expérimentale* [1865], ed. François Dagognet (Paris: Garnier-Flammarion, 1966), p. 53.

9 Charles Sanders Peirce, «Consequences of Four Incapacities,» [1868] in *Writings of Charles Sanders Peirce. A Chronological Edition*, ed. Christian J. W. Kloesel et al., 4 vols. (Bloomington: Indiana University Press, 1982-86), vol. 2, p. 239.

10 Lorraine Daston and Peter Galison, «The Image of Objectivity,» *Representations* no. 40 (Fall 1992): 81-128; Lorraine Daston, «Objectivity and the Escape from Perspective,» *Social Studies of Science* 22(1992): 597-618; Peter Galison, «Judgment After Objectivity,» in Caroline A. Jones and Peter Galison, eds., *Picturing Science, Producing Art* (New York: Routledge, 1998), pp. 327-259; Lorraine Daston, «The oralized Objectivities of Nineteenth-Century Science,» in Wolfgang Carl and Lorraine Daston, eds. *Wahrheit und Geschichte. Jahrbuch der Akademie der Wissenschaften zu Göttingen*, (Göttingen: Vanderhoeck & Ruprecht, 1999), pp. 78-100.

claim to judge whether the one was more «true» or the other more «objective» in a normative sense. If one uses these words as trans-historical honorifics, it is possible to find examples of each in the study of nature in almost any time and place, just as it is possible, by repressing historical context and connection, to find anticipations of evolutionary theory in the fragments of the pre-Socratic philosophers. Instead, I want to examine the honorifics «true» and «objective» themselves as historical entities, that come into being and pass away. I hope to show that the contrast between a regime of truth and one of objectivity is heavy with concrete consequences for everything from the choice of scientific images to the formation of scientific personae.

The Truth of Types

When Rousseau opposed the moral truth of parables to mere factual accuracy, he was not speaking *avant la lettre* of C.P. Snow's Two Cultures, the humanities versus the natural sciences. Rousseau himself was a passionate botanist, who in the same *Reveries* in which his reflections on truth appeared described his outings «with a magnifying glass in my hand and my *Systema Naturae* [of Linnaeus] under my arm», and rhapsodized about «every grass in the meadows, every moss in the woods, every lichen covering the rocks--and I did not want to leave even one blade of grass or atom of vegetation without a full and detailed description»[11]. This may sound like a verbal version of Albrecht Dürer's meticulously detailed aquarelle, *Das Rasenstück*, but Rousseau's ultimate ambition was not to record the variety of the plant world in its minutest particularities, but rather «to follow the intricate working of these living mechanisms, to succeed occasionally in discovering their general laws and the reason and purpose of their varied structures, and to give myself up to the pleasure of grateful admiration of the hand that allows me to enjoy all this»[12]. Botanical truth lay no more in the bald appearances of plants than moral truth lay in the unvarnished facts of human actions.

Rousseau's views (and his natural theological motives) were shared not only by the majority of eighteenth-century botanists, but also by contemporary practitioners of other descriptive sciences, such as anatomy, conchology, entomology, anthropology, and geodesy. These were the sciences of the trained eye, accustomed by years of experience to distinguish the essential from the accidental, the normal from the pathological, the typical from the anomalous, the variable from the constant. In principle, this was just as much a problem for the observational astronomer as for the field naturalist. The astronomers who tracked comets with telescopes were plagued by observations that strayed from any smooth path; hence the dilemma, made explicit by Astronomer Wilhelm Olbers in an 1827 letter to the mathematician Karl Friedrich Gauß of whether to edit the logbook of observations to select only the best values, eliminating outliers that could possibly be due to observational error. In the context of a mathematical theory of observational error, Pierre-Simon Laplace imagined underlying constant causes like the universal law of gravitation upon which were superimposed a swarm of variable causes, ranging from the quality of the scientific instrument to the incalculable perturbations due to three or more massive bodies[13]. Although few naturalists

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11 Rousseau, *Reveries*, (as note 1) «Fifth Walk,» p. 84.

12 Ibid., «Seventh Walk,», p. 115.

13 Zeno J. Swijtink, «The Objectification of Observation, in: Lorenz Krüger et al. (eds.), The Probabilistic revolution, Vol. 1: Ideas in History (Cambridge Mass.: 1987), pp. 261-286, on p. 278; Pierre-Simon Laplace, *Essai philosophique sur les probabilités* [1814], in *Oeuvres complètes de Laplace*, 14 vols. (Paris: Académie des Sciences, 1878-1912), vol. 7, pp. XLVIII-XLIX.

followed Laplace in developing a metaphysics of variability, they concurred that their task was to extract the truths of nature from the welter of confusing appearances.

There is of course an audibly Platonic ring to the language of truth somehow underlying appearances, noumena undergirding phenomena. This language of hidden simplicity under manifest complexity was as well-suited to the sciences of stars and crystals as to those of plants and insects. But naturalists could additionally appeal to an Aristotelian language of organic form, as real or more real than the observable individuals of this or that species of orchid or moth. This was a truth unlikely to be revealed by recourse to smooth curves and mathematical equations; it was instead a truth of synthetic perception, of the ability to detect a common form uniting many individual exemplars of a kind. In his morphological and methodological writings Johann Wolfgang Goethe called such truths «archetypes» or «pure phenomena»: «Es gibt, wie ich besonders in dem Fache das ich bearbeite oft bemerken kann, viele empirische Brüche, die man wegwerfen muss um ein reines konstantes Phänomen zu erhalten;...Um es [das reine Phänomen] darzustellen bestimmt der menschliche Geist das empirische Wankende, schließt das zufällige aus, sondert das Unreine, entwickelt das Verworrene, ja entdeckt das Unbekannte»[14].

What Goethe theorized, a myriad of less contemplative naturalists of the eighteenth and early nineteenth centuries practiced: they sought to condense and integrate a legion of individual impressions into a «true» represenation, in both words and images, of the natural kind in question. The Göttingen anatomist Albrecht von Haller exclaimed over the variety of arteries, which defied uniform description or even comprehensive naming. Only the experience of dissecting many corpses could instruct the anatomist in what was typical for the «perfect human body», what deviant. And even his own prodigious labors and patience had not sufficed to compare all the diverse branchings of the arteries in one body with those of others so as to eliminate the singular[15]. In such cases, the judgment of the naturalist and the art of the illustrator lay in knowing what to omit from a woodcut or engraving, as well as from the description of the natural object in question. Linnaeus exhorted his fellow botanists to eliminate all variable aspects of plants, such as color, from both specific characters and illustrations: «How many volumes have you [other botanists] written of specific names taken from colour? What tons of copper have you destroyed in making unnecessary plates? What vast sums of money have you enticed fraudulently, as it appears, from other men's pockets, the purchasers to wit, on the strength of colour alone?» According to Linnaeus, the satisfactory botanical illustration «represents the plant as it were in a mirror»--but a mirror that filtered out all features except «Number, Shape, Position, and Proportion»[16].

It was the rule for anatomists and naturalists to supervise their artists and engravers closely, so that naturalism--the depiction of an individual specimen in all its peculiarities, exactly as it appeared to the eye--did not overwhelm the realism of the type. The German anatomist Samuel Soemmerring, himself a gifted draughtsman, argued in his *Abbildungen des menschlichen Auges* (1801) that since even «die beste Abbildung in Rücksicht der Feinheit und Mannigfaltigkeit die Natur nie erreicht... so ist wohl nichts billiger, als daß man dieses wenigstens so nahe als möglich der Natur zu bringen

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14 Johann Wolfgang Goethe, «Erfahrung und Wissenschaft,» [comp. 1768], in *Goethes Werke* («Hamburger Ausgabe»), 14 vols., vol. 13: *Naturwissenschaftliche Schriften I*, ed.Dorothea Kuhn and Rike Wankmüller, 8th ed.[1981] (Munich: C.H. Beck, 1994), pp. 23-24.

15 Albrecht von Haller, *Icones anatomicae*, 2 vols. (Göttingen: B.Abram Vandenhoeck, 1756), vol. 2, Fasciculus V, f. A2.r-v.

16 Carl von Linnaeus, *The 'Critica Botanica' of Linnaeus* [1737], trans. Sir Arthur Hort, rev. by Miss L. Green (London: Ray Society, 1938), Aphorisms 266, 282; pp. 139, 161-162.

trachtet [...]»[17]. The nineteenth-century historian of botany Lodolf Treviranus insisted on the responsibility of the scientist to monitor the artist in every detail: «Die Zeichnung daher [...]muß nicht nur die Umrisse aufs genauste ausdrücken, sondern auch die Form und Richtung des Stengels, die Knoten, die Lage und Richtung der Haare; besonders aber muß sie das Geäder der Blätter charakteristisch darstellen und deshalb nie ohne die Aufsicht eines der Wissenschaft Kundigen gemacht werden»[18]. Julius Schleiden, pioneer of botanical microscopy, was scathing in his criticisms of a colleague, Heinrich Friederich Link, who had allowed his draughtsman to conduct observations «ganz allein»; the result was, according to Schleiden, drawings that confused the reader «durch lauter falsche Anschauungen»[19]. Some naturalists went so far as to speak of their artists as «tools», whose every pencil stroke was to be monitored and corrected in the name of scientific accuracy. Although some naturalists employed celebrated artists as their illustrators, as Oxford botanist John Sibthorp engaged Ferdinand Bauer for the *Flora Graeca* (1806), it was more common for the relationship between naturalist and artist to approximate that of master and domestic servant. Some naturalists trained their own artists while they were still children, in order to form their style completely to the standards set by the naturalist: the English conchologist Thomas Martyn recommended boy artists from the lower classes on grounds of pliability and cheapness[20]; the French entomologist René Antoine Réaumur lodged a young man «chés moi» and had him specially instructed to draw insect specimens[21].

To read with twentieth-century eyes the accounts of these naturalists concerning the infinite pains they took to discipline their artists and select their specimens in order to guarantee maximum fidelity to nature is to experience a certain dizzying double vision, in which two images seem not quite to match up with one another. On the one hand, there are the endless protestations of accuracy, of illustrations drawn exclusively from nature, of precautions taken to insure the exactitude of the most minute detail. On the other hand, there are--in the next sentence, by the same authors--reassurances that the illustrations have been duly corrected, even perfected by the vigilant naturalist, to counteract the artist's regrettable tendency to draw exactly what he or she sees. Consider the case of the Leiden anatomist Bernhard Albinus. In his remarkable *Tabulae sceleti et musculorum corporis humani* (1747), Albinus spared no expense or pains to insure the absolute integrity of his full-page engravings of the human frame. He engaged the renowned artist Jan Wandelaer, not only because of the elegance of Wandelaer's style but also because he could engrave as well as draw the images, thereby minimizing copying errors. Despite his respect for his artist's skill, Albinus asserts that Wandelaer «was instructed, directed, and as entirely ruled by me, as if he was a tool in my hands, and I made the figures myself». Albinus erected a system of two grids, one at forty rhenish feet, the other at four, so that Wandelaer could draw the skeleton quadrant by tiny quadrant, keeping the proportions of each part exact in relation to the whole. Yet this meticulous concern for accuracy did not conflict in Albinus' mind with the selection of his show specimen--«of the male sex, of a middle stature, and very well-proportioned»--and then improving it further in the illustrations: «As therefore painters, when they draw a handsome face, if there happens to be any blemish in it, mend

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17 Quoted in Ludwig Choulant, *Geschichte der anatomischen Abbildung* (Leipzig: Rudolph Weigel, 1852), p. 132.

18 Lodolf Treviranus, *Die Anwendung des Holzschnittes zur bildlichen Darstellungen von Pflanzen* [1855] (Utrecht: W. de Haan, 1949), p. 2.

19 Julius Schleiden, *Die Botanik als induktive Wissenschaft* (Leipzig: W. Engelmann, 1845), p. 105.

20 Thomas Martyn, *Le Conchyliologiste universel* [1784-87] rev. J.C. Chenu (Paris: A. Franck, 1845), pp. 8-9.

21 René Antoine Ferchault de Réaumur, *Mémoires pour servir à l'histoire des insectes*, 6 vols. (Paris: Imprimerie Royale, 1734-42), vol. 1, p. 54.

it in the picture, thereby to render the likeness more beautiful; so these things which were less perfect, were mended in the figures, and were done in such a manner as to exhibit more perfect patterns; care being taken at the same time that they should be altogether just»[22].

To the twentieth- (or even late nineteenth-) century reader, this sounds confusingly as if Albinus had at once reverently respected the facts of his subject and also shamelessly tampered with them. I submit that this wavering double vision measures the distance between a regime of truth and one of objectivity. Albinus perfected his skeletons in the service of truth, but in defiance of what later scientists, including anatomists, would call objectivity. For Albinus and his contemporaries, the truth of nature was revealed not only by close but also by wide observation. A definitive image in anatomy, botany, or entomology was not a naturalistic rendering of any individual, but a composite image based upon numerous observations of the same natural kind. The metaphysics that grounded such practices of description and representation was a motley one, compounded of Platonic idealizing, Aristotelian natural kinds, and a heavy dose of natural theology. But for the ideals and practices of naturalists committed to truth, metaphysics was less significant than the personal qualifications of the naturalist. Sensory acuity, strong memory, and above all judgment made the eminent naturalist. Judgment detached the characteristic from the aberrant, judgment integrated multiple impressions into a single image, judgment perfected the flawed specimen, judgment achieved truth to nature. Falsehood sprang from inexperience and unripe judgment--as when Linnaeus reproached the French botanist Joseph Tournefort for having needlessly multiplied species of flowers, «93 Tulips (where there is only one) and 63 Hyacinths (where there are but two)»[23]. The persona of the naturalist resembled that of the sage, seasoned by long experience and respected for sound judgment. Within the regime of truth to nature, the brilliant young prodigy among naturalists was as rare as the monster among archetypes.

The Objectivity of Appearances

The modern philosophical career of the objectivity/subjectivity pair begins in earnest with Kant's three critiques in the late eighteenth century, but its scientific career begins considerably later, in the 1850s and thereafter. The philosophical history of the words is an intricate one, with twists and turns peculiar to German, English, and French, as each language community grafted the Kantian vocabulary onto indigenous philosophical traditions. I cannot retrace that convoluted reception history here, although it is full of interest. Instead, I will contrast the regime of truth and falsehood with that of objectivity and subjectivity in the practice of the sciences, using examples from optics, anatomy, and history.

In neither nineteenth-century usage nor our own does the domain of objectivity coincide with that of truth, nor the domain of subjectivity with that of falsehood. Claude Bernard for example assigned the eternal truths of logic and mathematics to the realm of the subjective, and considered the «objective truths» provided by the experimental method, to be «only relative to the number of experiments and observations that have been made»--a new result might conceivably falsify them[24]. Although Hermann von Helmholtz identified the objective with the real, he did so in a sense that

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22 Bernhard Siegfried Albinus, *Tables of the Skeleton and Muscles of the Human Body* [1747], trans. from the Latin (London: John and Paul Knapton, 1749), «An Account of the Work,», sigs. a-c.

23 Linnaeus, *Critica Botanica*, (as note 16) Aphorism 259, p. 122.

24 Bernard, *Introduction*, (as note 8) p. 62.

diverged significantly from the earlier meaning of truth to nature. Addressing the 1865 Innsbruck meeting of the Deutsche Versammlung Naturforscher und Ärzte, Helmholtz described natural laws as «eine fremde Macht, nicht willkürlich zu wählen und zu bestimmen in unserem Denken...So tritt uns das Gesetz als eine objective Macht entgegen, und demgemäß nennen wir es Kraft»[25]. For naturalists like Albinus or Linnaeus, the false had not been the arbitrary imposition of human will upon nature, but rather the variable, accidental, or aberrant aspects of nature itself. When Linnaeus railed against specific names that do not distinguish a plant from others in its genus as «false»[26], he meant that such names failed to capture the true essence of the plant, not that they were emanations of the overweaning will. It is more than ironic that Helmholtz chose as his example of such willful impositions «verschiedene Systeme der Thiere und Pflanzen», like the classificatory system of Linnaeus.

The extent to which parsing knowledge into objective and subjective rather than into true and false transformed late nineteenth-century science is revealed perhaps most dramatically in the discipline to which Helmholtz himself made seminal contributions, sensory physiology. In eighteenth-century optics, certain visual effects, such as colored shadows, were investigated in the same manner as phenomena like refraction, as in Joseph Priestley's *The History and Present State of Discoveries Relating to Vision, Light, and Colours* (1772). That this apparent conflation of subjective and objective phenomena was not simply a failure to distinguish physiological from physical effects is shown by the French chemist E. Chevreul's *De la loi du contraste simultané des couleurs* (1839). As director of the Gobelins tapestry factory, Chevreul had investigated the visual effects of juxtaposing certain colors, and concluded that constant laws governed how, for example, the color yellow was differentially perceived next to violet than next to red. Chevreul was perfectly clear that these laws had nothing to do with the chemical--we would say objective--composition of dyestuffs, but he insisted that they were laws nonetheless, constant for all observers everywhere:» After having assured myself that the preceding phenomena were constant for my vision, when it was not fatigued, and that a few other persons accustomed to judging colors viewed them as I [did], I sought to assemble them into an expression general enough so that one could predict the effect of two juxtaposed colors on the organ of vision.»[27]. Chevreul judged what we could call subjective phenomena to be as law-like and as universal as those of physical phenomena, at least when judged by an expert. In his *Handbuch der physiologischen Optik* (1867), Helmholtz was considerably less confident about the possibility of generalization from perceptual experiments, warning the reader that «möglicherweise vieles, was er in den folgenden Kapiteln etwa neues finden wird, auf individuellen Eigenthümlichkeiten meiner eigenen Augen beruhen mag»[28]. Helmholtz emphasized not only the variability of perceptual phenomena, but also the gap between subjective perception in general and the objective world. In the realm of color perception, for example, he believed that sensory physiology had shown that «keinerlei Art von physikalischer Gleichheit der subjektiven Gleichheit verschieden gemischter Lichtmengen von gleicher Farbe entspricht»[29].

Chevreul still worked within a regime of truth, in which falsehood could be blamed on the world as well as upon the investigator, and in which color perception was as likely a candidate for universal laws, expressed algebraically, as chemical reactions were. Expert judgment --his own and

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25 Hermann von Helmholtz, «Ueber das Ziel und die Fortschritte der Naturwissenschaft,» [1869], in: Id., *Vorträge und Reden*, 2 vols. (Braunschweig: Friederich Vieweg und Sohn, 1896), vol. 1, pp. 375-376.

26 Linnaeus, *Critica Botanica*, (as note 16) Aphorism 257, p. 117.

27 Eugène Chevreul, *De la loi du contraste simultané des couleurs* (Paris: Chez Pitois-Levrault et Ce., 1839), p. 14.

28 Hermann von Helmholtz, *Handbuch der physiologischen Optik* (Leipzig: Leopold Voss, 1867), p. 440.

29 Hermann von Helmholtz, «Ueber das Ziel,» (as note 25) p. 393.

that of a few other trained observers--sufficed to warrant the stability and scope of these laws. The next generation of scientists by no means abandoned the project of a sensory physiology that sought lawlike regularities in perception, but these scientists drew a sharp line between subjective and objective phenomena. Subjective phenomena were not «false»--much of sensory physiology was dedicated to their investigation--but they were by definition rooted in the individual, and therefore of restricted generality. Whereas Chevreul and his predecessors had assumed that an investigation of visual phenomena would yield the same kinds of generalizations that the investigation of light wavelengths had, Helmholtz and his colleagues worried about the incorrigible inter-individual variability that was part and parcel of the subjective. Moreover, whereas Chevreul had envisioned a science of formalized relationships among visual appearances--for example, the mutual modification of red and green--later sensory physiologists instead attempted to correlate wavelengths with impressions, the objective with the subjective. Johann König's 1891 graph neatly illustrated both points: it plotted subjective brightness as a function of objective light wavelength and intensity; and the curves were explicitly for König's own eye[30].

If we turn to disciplines in which illustration played a central role, like embryology, the contrast between old and new practices is even more striking. One example may suffice to outline this contrast. In the 1870s the Leipzig embryologist Wilhelm His began a series of attacks on Ernst Haeckel's use of embryological evidence, in particular drawings of embryological development, to support Haeckel's thesis that ontogeny recapitulates phylogeny. His accused Haeckel of smuggling his theoretical prejudices into the illustrations (drawn by Haeckel himself in some instances), which were intended to show the continuity of embryological forms across species, and came perilously close to calling Haeckel a liar: «Ich selbst bin im Glauben aufgewachsen, daß unter allen Qualificationen eines Naturforschers Zuverlässigkeit und unbedingte Achtung von der thatsächlichen Wahrheit die einzige ist, welche nicht entbehrt werden kann»[31]. Haeckel responded explosively, pointing out that his illustrations were not intended as «'exacte und vollkommen naturgetreue Abbildungen', wie sie *His* verlangt, sondern [...] Abbildungen, welche nur das Wesentliche des Gegenstandes zeigen und das Unwesentliche fortlassen». To call such illustrations «inventions», much less lie, was, according to Haeckel, to drive all ideas out of science, leaving only facts and photographs: «Völlig tadelfrei und tugendrein ist nach *His* (und vielen anderen 'exacten' Pedanten) demgemäß nur der Photograph»[32].

In his indignation Haeckel of course exaggerated His's obsession with the bare facts; His actually acknowledged the utility of drawings as well as photographs in scientific illustration. But he considered drawings always to contain «subjective Elemente», sometimes advantageous and sometimes not, whereas «die Photographie den Gegenstand mit allen seinen Einzelheiten, auch den zufällig vorhandenen wiedergibt, gewissermaßen als Rohstoff, dafür aber die absolute Treue garantirt.» More revealing than this bald opposition between drawing and photograph was His's elaborate method of making drawings, employing a drawing prism and stereoscope so as to project an image upon the drawing surface, which was then traced. These tracings of microscopic cross-sections were then subjected to a painstaking process of checking against finely lined graph paper

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30 E.G. Boring, *Sensation and Perception in the History of Experimental Psychology* [1942] (New York: Irvington, 1970), p. 179.

31 Wilhelm His, *Unsere Körperform und das physiologische Problem ihrer Entstehung* (Leipzig: F.C.W. Vogel, 1874), p. 171.

32 Ernst Haeckel, *Anthropogenie oder Entwicklungsgeschichte des Menschen* [1874], 4th ed., 2 vols. (Leipzig: Wilhelm Engelmann, 1891), pp. 858-860.

and against one another to ascertain the exactness of the proportions. Any amendments or idealizations of the drawings or models obtained under this system of multiple controls, in the style of Albinus, His equated with «bewußten Pfuscherei»[33]. Where Albinus and his contemporaries had understood it to be their scientific duty to improve drawings executed under strict constraints of empirical exactitude, His condemned any such intervention in drawings as tantamount to fraud. When Haeckel followed older usage in using his drawings to extract «the essential», or what he believed to be the true idea hidden beneath false appearances, His indicted him for sinning against objectivity. The point here is not to settle the scientific issues that divided Haeckel and His — subsequent research confirms and corrects both on individual claims— but rather to show the clash of the regime of truth with that of objectivity at the level of scientific practices —the freehand drawing versus the meticulous tracing or photograph— as well as that of ideals.

Conclusion: The Price of Progress

The confrontation between Haeckel and His suggests that the regime of objectivity did not stamp out that of truth. But the pitch and volume of the controversy, in which both sides believed personal and professional integrity was at stake, indicates how uneasily the two regimes co-existed within a single research community. Nor was the tension between the two regimes restricted to the natural sciences: recall Friederich Nietzsche's tirade against «objectivity» in history, his opposition of objectivity on the one hand, to justice and truth on the other. Nietzsche sneered at the «historical virtuosi» (he surely meant Leopold Ranke and his followers) who clung to the superstition of objectivity: «sollten sich in jenen Momentum die Dinge gleichsam durch ihre eigene Tätigkeit auf einem reinen Passivum abzeichnen, abkonterfeien, abphotographieren?»[34] The very vehemence of Nietzsche's and Haeckel's tone plants the suspicion that they were fighting a battle they feared to be already lost.

How did the regime of objectivity come to rival and nearly supplant that of truth in the mid-nineteenth-century sciences? The physiologist Rudolf Virchow's 1872 address to the fiftieth anniversary of the Versammlung Deutscher Natuforscher und Ärtzte provides some clues. Virchow distinguished sharply between the «freedom of science» and the «freedom of scientific teaching», and set strict standards for the latter. Although researchers should be free to pursue their most obscure hunches and wildest speculations, scientific doctrines taught in the classroom must meet the highest standards of proof. Premature synthesis, reasoning by analogy, and even induction over cases had no place among the *Lehrsätze* professed in the lecture hall. Qua teacher, the scientist must strive to increase the proportion of the «objective» to «subjective» in his lectures. Virchow admitted that this was hard work, and that complete success was probably impossible:

> «Ich gehöre jetzt so ziemlich zu den ältesten Professoren der Medizin, ich lehre nun mehr als 30 Jahre meine Wissenschaft und ich darf sagen, ich habe in diesen 30 Jahre ehrlich an mir gearbeitet, um immer mehr von dem subjectiven Wesen abzuthun und mich immer mehr in das objective Fahrwasser zu bringen. Nichts desto weniger bekenne ich offen, daßs es mir nicht möglich ist, mich ganz zu entsubjektiviren [...] aber ich sage, wir müssen uns die

33 Wilhelm His, *Anatomie menschlicher Embryonen* (Leipzig: F.C.W. Vogel, 1880), pp. 6-12.

34 Friederich Nietzsche, «Vom Nutzen und Nachteil der Historie für das Leben,» [1874] in: Id., Unzeitgemäße Betrachtungen [1873-76], ed. Peter Pütz (Munich: Goldmann, 1992), pp. 111-112.

> Aufgabe stellen, in erster Linie das eigentlich thatsächliche Wissen zu überliefern, und wir müssen den Lernenden jedesmal sagen, wenn wir weitergehen, 'dieses ist aber nicht bewiesen, sondern das ist meine Meinunng, meine Vorstellung, meine Theorie, meine Speculation'»[35].

This restraint ——Virchow called it *Resignation*—— would, he hoped, protect science from public scepticism. Although Virchow's immediate targets were Haeckel's speculations about ensouled atoms and Carl Friderich Naegeli's theories of spontaneous generation, he was more broadly concerned with the lessons of the history of science, particularly recent science, in which one highly plausible and empirically well-supported scientific theory after another had been toppled by a rival. The public had begun to lose its faith in science, and to turn upon scientists: «Da beginnen dann die Vorwürfe; ihr seid ja selbst nicht sicher; eure Lehre, die heute Wahrheit heisst, ist morgen Lüge; wie könnt ihr verlangen, dass eure Lehre Gegenstand des Unterrichts und des allgemeinen Bewusstseins werde?»[36] Virchow preached scientific restraint in order to safeguard scientific authority.

Virchow was not alone in his worries about the ephemeral nature of alleged scientific certainties. If many late nineteenth-century scientists had renounced their conviction that their theories corresponded to the deep truths about nature or even that their theories would ultimately converge upon that truth, it was because they had seen how these theories suceeded one another at indecently short intervals. Theories encompassed ever more phenomena, the precision of predictions sharpened steadily, science-based technology expanded and flourished, but successive insights about the deep structure of nature were as likely to contradict one another as to converge. As the Austrian physicist Ernst Mach remarked, history of science taught the Heraclitean lesson of *panta chorei*, for the revolutions of science were no longer permanent but rather perpetual: «in der That, wenn man aus der Geschichte [der Wissenschaften] nichts lernen würde, als die Veränderlichkeit der Ansichten, so wäre es schon unbezahlbar.[... Versuche den schönen Augenblick durch Lehrbücher festzuhalten, sind stets vergebliche gewesen. Man gewöhne sich also bei Zeiten daran, dass die Wissenschaft unfertig, veränderlich sei»[37]. The French mathematical physicist Henri Poincaré was so dizzied by the pace of theoretical and experimental change in the field of electrodynamics that he refused to hazard even a guess as to the outcome, lest events take yet another turn «between the day when I give this book to the publisher and that when it appears at the bookstore.»[38]. By the second half of the nineteenth century the pace of scientific progress had become vertiginous, and scientists could no longer expect the truths of their student days to survive until the first call to a professorship came their way. The ascendance of the regime of objectivity can be seen as a desperate attempt to salvage a small but durable core of facts from the floodtide of scientific progress. Objectivity is not only distinct from truth; it is a retreat from truth.

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35 Rudolf Virchow, «Die Freiheit der Wissenschaften im modernen Staatsleben,» [1872] *Amtlicher Bericht über die Versammlung Deutscher Naturforscher und Ärtzte* 50(1877): 65-77, on pp. 74-75.

36 Ibid., p. 73.

37 Ernst Mach, *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Kraft* [1872], 2nd ed. (Leipzig: Johann Ambrosius Barth, 1879), p. 3.

38 Henri Poincaré, *Science et l'hypothèse* (Paris: Flammarion, 1902), p. 281.