PETER GALISON

# Judgment against Objectivity*

## INTRODUCTION: THE BIRTH AND DEATH OF MECHANICAL OBJECTIVITY

Objectivity is a fighting word. It is lambasted, cherished, hunted, defended; it is realism on Monday, certainty on Wednesday, intersubjectivity on Friday, and truth on Sunday. Claims and counterclaims proliferate: the natural sciences are objective; the social sciences want to be; architecture was in the 1920s. Postmodernism corrodes it, and metaphysics may or may not have captured its essence. Amid the cacophony of these discussions, the term loses its sense, and becomes little more than a contested token in battles from the *Methodenstreit* to the Culture Wars. In the midst of such polemics, a reader can be forgiven for having no conception of what might be meant by claims that objectivity still resides in quantum measurement, democratic politics, and statistical certainty.

Erased in such contemporary debates about objectivity is its genealogy within the conduct of the natural sciences. "Objectivity" is historical. As it is used in the physical, medical, and biological sciences, objectivity is deeply, ineradicably, a nineteenth-century category, one bound up with the process of depicting objects. To get at this visual culture where it most directly intersects notions of objectivity, I will focus on methods for visually classifying the "working objects" of science, rather than, for example, graphical representations of higher theoretical structures. I want to put aside the polemical abstractions about which disciplines have or lack "objectivity." I want to ask, instead: How is objectivity *practiced* at the most rock-bottom level; how is objectivity employed and mobilized by those sorting out the "working objects" of science? In particular, what constituted an *objective pictorial rendition* of the natural world—how did objectivity function, at specific times, for scientists who aimed to represent fossils, clouds, stars, elementary particles, bones, and the electrical activity of the brain?

Nowhere does debate over the classification of such objects come into focus so strikingly as in the spectacular literary genre of the scientific atlas. There are atlases of anatomy, atlases of wounds, atlases of cells, atlases of clouds, atlases of elementary particles, atlases of heads, atlases of peoples, and atlases of stars—in fact, there are atlases of almost any collection of studied objects within science. Many of these collections are explicitly called atlases, others handbooks, guides, or catalogues. But binding them together is the aim of representing the basic species of a field of inquiry, usually addressed to practitioners with the aim of helping to codify existing data and to serve as the basis for further research.

The claim that pictorial objectivity as revealed through atlases is a nineteenth-century concept is *not* to say that there was no notion of getting a true picture of nature long before 1800; of course there was. But elsewhere, Lorraine Daston and I have used the history of scientific atlases and their cognate literary forms to argue that one earlier ideal, that of attaining pictorial "truth to nature," had little to do with objectivity, a notion used in something like its current sense by Coleridge.[1] Truth to nature was associated with a set of practices—practices involving massive artistic and scientific intervention by a natural philosopher whose genius vouchsafed the validity of the move to idealize and correct the unreliable appearances of the given. Individual items misled—this particular skull "erred," only the platonized skeleton would reveal the true form of nature. Individual plants, even individual species contained spurious elements, distorted by the oddities of their history and the circumstances of their observation. Look as we might among the objects of the world, they could only suggest the Goethean *Urpflanz* or the ideal skeleton "behind" the visible. This struggle to get at the hidden true picture was not considered at the time to be "objective"; the term emerged only in the nineteenth century and, when it did, in opposition to the artistry of Genius's intervention.

In the nineteenth century—or, more specifically, after about 1830—both the persona of the natural philosopher and the status of pictorial representations of nature shifted. Instead of a transcendental Genius improving or idealizing nature, the desired character of the natural philosopher inverted to one of self-abnegation. Instead of truth to nature, these scientists aspired to let nature "speak for itself" through a set of instrumentalities that minimized intervention, hamstrung interpretation, and blocked artistic license. More saint-like in self-denial than powerful in genial interpretation, the new scientist of the last two thirds of the nineteenth century set aside the pictorial revelation of metaphysical truth per se, and aimed, happily, at an essentially mechanical registration of natural objects as they came. But this paper is not so much about this displacement of the seventeenth- and eighteenth-century *metaphysical image* (held to be "true to nature") and its replacement by the nineteenth-century *mechanical image* (of "objectivity"). Rather, it concerns a second displacement that occurred as the mechanical image itself increasingly yielded to a third representational strategy predicated on judgment: what I will call the *interpreted image*, emerging in the twentieth century. In both transitions, the changing practices of image making were intertwined with shifts in the moral culture of the scientist-author.

In the case of the mechanical image of the nineteenth century in which "objectivity" first came into prominence, the proclaimed association of automatic practices and moral self-denial are rife. Two examples are illustrative. The first is from Percival Lowell, the American astronomer, as he struggled during the first years of the twentieth century to establish the reality of the "canals" of Mars:

> Each drawing was made as if I had never seen the planet before; only twice did I allow myself even to put in afterward the snow accidentally omitted at the time. About fifteen minutes only was allowed in every instance, so that each drawing does not pretend to represent all that could be seen on that night at the telescope. They were meant to get as nearly as possible impersonal intercomparable representations,—scientific data, not artistic delineations.[2]

After the fact, Lowell could see a great deal that he had not put in the pictures; "snow" at the polar ice caps, for example, was plainly absent from his quick sketches (see Figure 1a). But with pride he reported how he (all but twice) resisted the temptation to reinsert the missing matter and, by so suppressing his impulse to improve, guaranteed the objectivity of his representation. These were "scientific data, not artistic delineations," where artistic correction had previously been precisely the guarantor of Truth. Lowell, in essence, argued that while the artistic delineations might be more complete and accurate, succumbing to the siren call of art would doom the objectivity of the project.

With the collaboration of Carl Otto Lampland, Lowell began, not long after these sketches were made, to begin the photographic exploration of the canals. On May 11, 1905, three days after Mars had been in opposition, Lowell and Lampland were able to capture, on film, the fine lines of the planetary surface. "Thus," Lowell proclaimed, "did the canals at last speak for their own reality themselves." Speak they might, but in whispers: only one-quarter of an inch in diameter, Lowell's photographs of Mars were so blurred, gray, and small that, at the time, they could not even be reproduced.[3] Figure 1b shows the pictures as they appeared in his record book in their original blurry but unretouched form. Though one prominent British astronomer, A. C. D. Crommelin, declaimed that "these photographs did a great deal to strengthen my faith in the

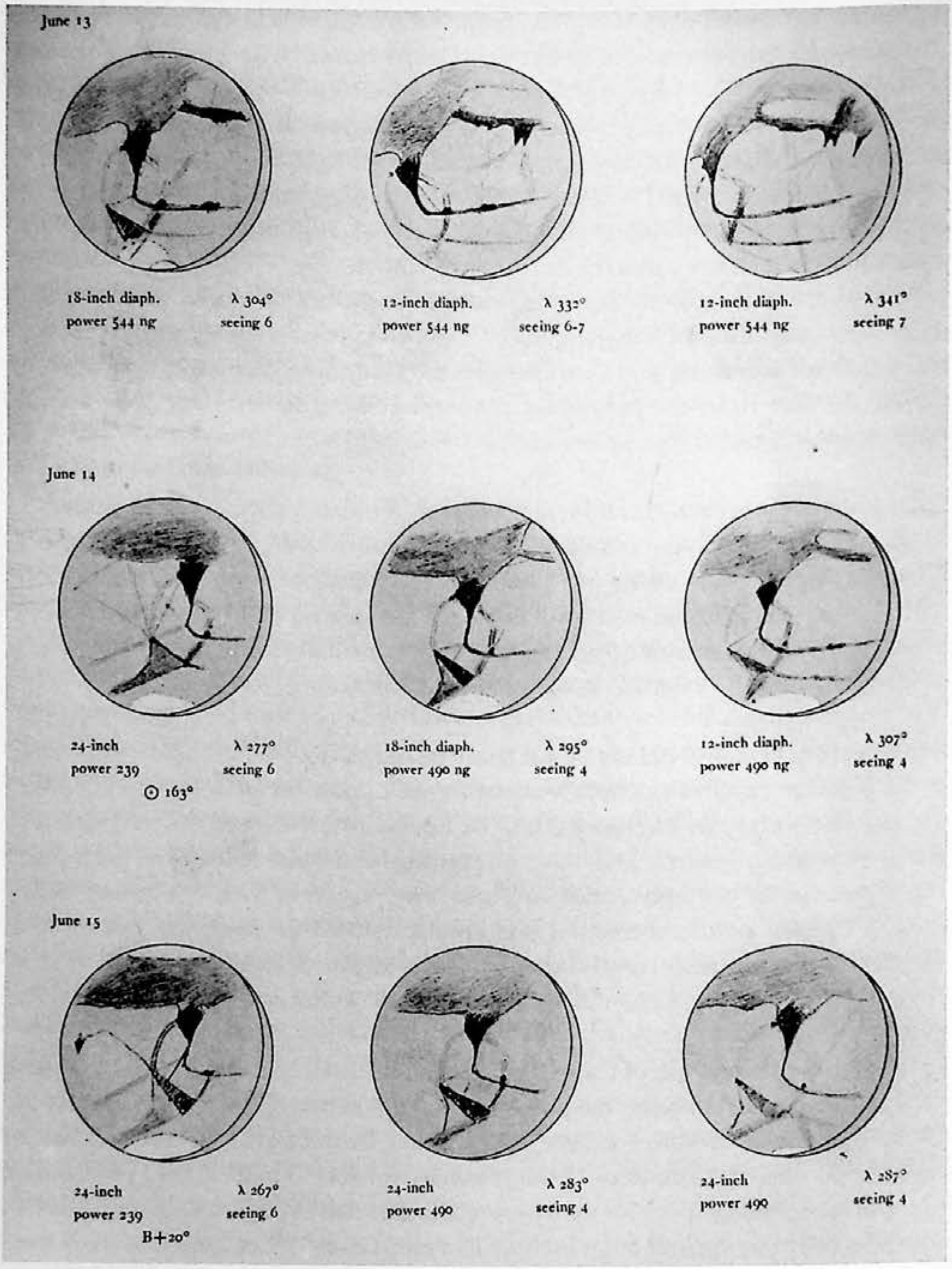


*Figure 1a. Percival Lowell, sketches of Mars showing canals, June 13–15, 1905 (reproduced directly from the record book).*

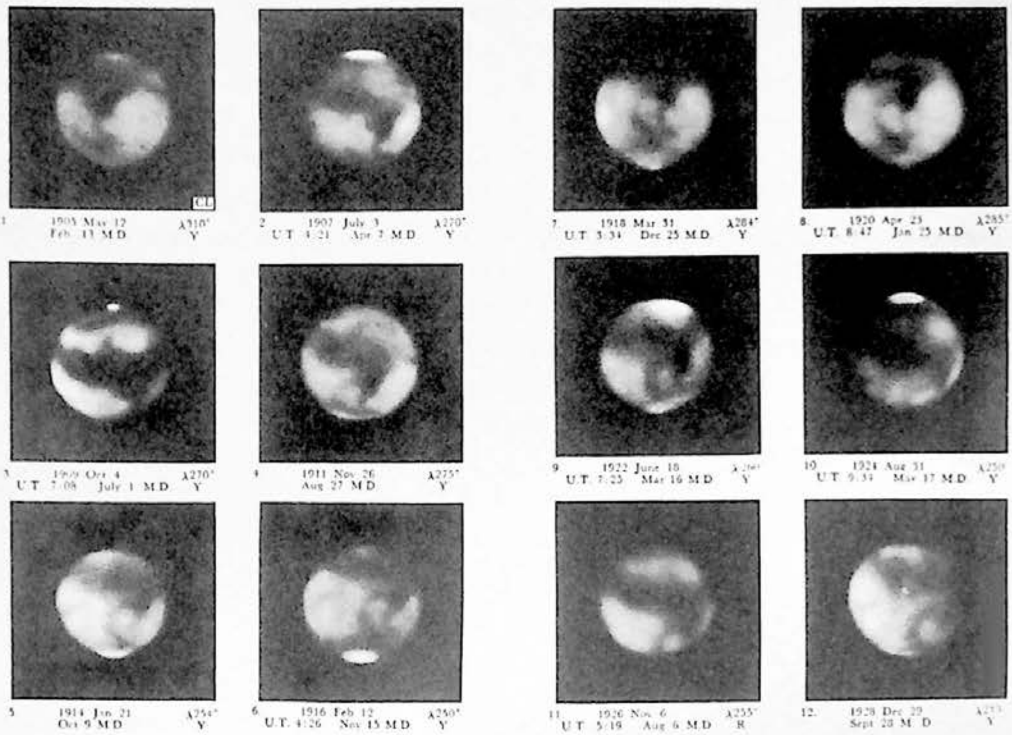

*Figure 1b. Lowell, photographs of Mars, 1905, courtesy of the Lowell Observatory, Tucson, Arizona.*

objective reality of the canals," others looked at the same pictures and were struck by their ambiguity. In response, a desperate Lowell almost succumbed to artistic temptation—he considered having a more neutral party (his friend and fellow Boston scientist, George R. Agassiz) "retouch" the pictures so the canals would be visible in mass reproduction. But Lowell's editors immediately revolted: such retouching would be a "calamity . . . as it would certainly spoil the autographic value of the photographs themselves. There would always be somebody to say that the results were from the brain of the retoucher."[4] This was the classic charge against intervention; Lowell demurred, and in the end, accuracy, completeness, color, sharpness, and even reproducibility was sacrificed. Objectivity would come first.

As Lowell's testimony makes clear, the ideal of removing oneself from the picturing process functioned even in the absence of the photograph. But once photography was available, it served (as Lowell indicated) as a splendid means of breaking that dreaded circle of art, interpretation, and personal predilection. Both before and after Lowell, paeans to the superiority of the photographic over the artistic were commonplaces within scientific discourse. Let one such instance stand in for many. Here is an early-twentieth-century clinical atlas of sectional and topographical anatomy that preserves the nineteenth century culture of mechanical objectivity.

> For the reproduction of their sections Braune and his predecessors were compelled to resort to tracings, hand drawings and engraved lithographic plates, thus introducing a possible dual source of error. In the present work these possible sources of error are entirely eliminated by the introduction of photography throughout. The plates are, therefore, an exact and faithful representation of the original sections. . . . The reason why photography was selected as the medium of recording . . . was . . . due to the fact that it affords the most faithful reflex of the originals, and is altogether free from any intermediate sources of error or of possible idealization from the pen of the artist.[5]

Drawings were erroneous and artists were prone to idealization; photography was faithful, and the photographer would reflect the original in nature. Self-evident criteria of reproduction had altered: where the eighteenth-century atlas maker took it as obvious that idealization was precisely what was called for, by the mid-nineteenth century that very move became anathema.[6]

Objectivity, then, or more specifically *mechanical objectivity*, was not an inextricable component of the atlas-making tradition originating in the sixteenth century, but begins much later in the history of atlas making. Objectivity as it was used at the very center of scientific work had a birth date in the mid-nineteenth century. Moreover, the story of objectivity is a conjoint development, implicating both observational practices and the establishment of a very specific *moral culture* of the scientist. In the first instance, objectivity had nothing to do with truth, and nothing to do with the establishment of certainty. It had, by contrast, everything to do with a machine ideal: the machine as a neutral and transparent operator that would serve both as instrument of registration without intervention *and* as an ideal for the moral discipline of the scientists themselves. Objectivity was that which remained when the earlier values of the subjective, interpretive, and artistic were banished. If the makers of the objective image had a slogan, it might have been: where genius and art were, there self-restraint and procedure shall be.

As we have seen, proceduralism and moral self-abnegation persisted into the early twentieth century, but the pictorial objectivity of atlases soon took another turn. Suddenly one begins to see something only seen in the rarest of late-nineteenth-century atlases—an explicit and repeated call for judgment and interpretation. Within the first third of the twentieth century, both practitioner and practice altered as the self-abnegating scientist and the automatic registration began to yield to judgment, though not at once and not everywhere. This essay explores that turn, beginning with an inquiry into the death of the *mechanical image* and concluding with a sketch of the *interpreted image*. Instead of our imagined nineteenth-century slogan, the twentieth-century atlas writers might say: at the end of procedure begins judgment.

Elements of older strategies for the depiction of nature persist long after new forms emerge. Even after the great efflorescence of atlases espousing nineteenth-century mechanical objectivity, for example, one saw instances of the older, eighteenth-century "truth to nature" that could only be unveiled by genius. Similarly, the death of mechanical objectivity was not sudden. Some atlas writers embraced a vision of mechanical objectivity deep into the twentieth century. This is important: the argument here is *not* that mechanical objectivity suddenly vanishes during the first third of the twentieth century. Rather, it is that during the early twentieth century, the moralized virtue of self-eliminating pictorial practices begins to yield to the moralized virtues associated with active judgment.

To see concretely an instance of the survival of mechanical objectivity, consider, for example, the following excerpt from Henry Alsop Riley's 1960 atlas of the basal ganglia, brain stem, and spinal cord, which perfectly illustrates the goal of mechanical, automatic reproduction safe from interpretation:

> This process [of hand-based illustration], however, makes the illustration a purely selective presentation and therefore the user of the atlas is often uncertain of the exact outline, relations and environs of the structures illustrated. The advantage of a photograph . . . seems to be self-evident. The photograph is the actual section. There is no artist's interpretation in the reproduction of the structures.[7]

For Riley, over-selection was the villain. Allowing the author or artist interpretive autonomy would throw into doubt the reliability of the object depicted. (If, Riley claims, artistic interpretation were to be allowed, then the depiction would become unreliable in its outline, relations, and environs.) Riley contended that hardly anything needed be said to defend the superiority of photographs. So tightly did the photographic image bind itself to the object that he could conclude: "the photograph *is* the actual section." Automaticity welded the image to the object until they stood as one; resemblance became identity.

Still, by this late date in the mid-twentieth century, such an unblinking faith in the photograph could not be sustained completely, and Riley readily conceded that staining was not completely targetable to a specific part of the specimen—his photographs revealed the irregularity of even the best and most technically skilled staining. Alas, even occasional scoring (from cutting) of the samples could be detected. Nonetheless, Riley judged that this photographic procedure ensured that "the accuracy and reliability of the photographs makes up for at times an inartistic appearance,"[8] where being inartistic was a right-handed criticism (rather than a left-handed compliment). Like

Riley, the authors of a 1975 *Hand Atlas* dismissed the artistic in favor of mechanically objective reproduction: "the authors have provided more realistic illustrations by substituting the surgeon's camera for the artist's brush."[9] On the mechanical-objective view, realism, accuracy, and reliability all were identified with the photographic. Nature reproduces itself in the procedurally produced image; objectivity is the automatic, sequenced production of homomorphic images from the object of inquiry to the atlas plate. Photography counted *among* these technologies of homomorphy; its importance was in underwriting the identity of depiction and depicted.

But if mechanical objectivity survived into the twentieth century, it did come to be supplanted across a myriad of scientific fields. My interest is *not* on extra-scientific attacks on objectivity, but rather on the *practices* used *within* laboratory and field inquiry to establish matters of pictorial fact about the basic objects of many scientific fields. For here in the atlases, handbooks, surveys, and guides we are in a central territory of science, far from the speculative frontier of elaborate new theories. In these compendia of pictures the simple (even simplistic) nineteenth-century model of pictures grounded in mechanical objectivity came under the fire of judgment.

## JUDGMENT AGAINST OBJECTIVITY

Starting in the early twentieth century, atlas-making scientists began celebrating their use of judgment and interpretation in the production of systematic images of nature. No longer were scientists lionized for their self-abnegation, and their tools celebrated for the ability to present nature "in her own language." Gone too is the ferocious denial of any peculiarly human assessment of evidence. Suddenly, in field after field, atlas makers articulated a new stance toward representation, one that frankly set aside the hard-won objectivist ideals of absolute self-restraint and automaticity. Listen, for example, to Frederic A. and Erna Gibbs, who launched their compendious *Atlas of Encephalography* (1941) with the proclamation that:

> This book has been written in the hope that it will help the reader to see at a glance what it has taken others many hours to find, that it will help to train his eye so that he can arrive at diagnoses *from subjective criteria*.[10]

Surely there are exceptions to every rule, but let us put it this way: in the hundred-odd-year history of late-nineteenth-century scientific atlases one finds scarce evidence of such an utterance, and few that espouse so explicitly the subjective as a necessary part of scientific depiction.

Could it be that the Gibbses simply did not understand the way "objective" and "subjective" had been deployed by the mechanical objectivists of the previous hundred years? Could they be "talking past" those who deplored the subjective? No, the Gibbses understood full well the pictorial practice of mechanical objectivity. And they emphatically rejected it, as is clear from the continuation of their explanation:

> Where complex patterns must be analyzed, such [subjective] criteria are exceedingly serviceable. For example, although it is possible to tell an Eskimo from an Indian by the mathematical relationship between certain body measurements, the trained eye can make a great variety of such measurements at a glance and one can often arrive at a better differentiation than can be obtained from any single quantitative index or even from a group of indices. It would be wrong, however, to disparage the use of indices and objective measurements; they are useful and should be employed wherever possible. But a "seeing eye" which comes from complete familiarity with the material is the most valuable instrument which an electroencephalographer can possess; no one can be truly competent until he has acquired it.[11]

In this context "indices" and "objective measurements" are closely connected. Fourier transformations, auto-correlations, and other attempts to parametrize the complex spikes and wave patterns of the electroencephalogram were positioned precisely as alternatives to the "subjective" criteria. The Gibbses' vaunted subjectivity is not, however, a return to the long-abandoned "truth to nature." Where in the mid-1800s mechanical objectivity was counterpoised to the genial intervention in nature to platonize, perfect, average, or derive the *Urpflanz* behind the earthly plant, the procedure accompanying interpreted images was to be far different. Instead of Goethean genius, and in place of proceduralist, bureaucratic self-denial, now the scientist invoked *judgment* based on familiarity and experience. The Genius revealed the true image of nature; the trained expert offered apprentices the means (through the "trained" or "seeing" eye) to classify and manipulate.

Some twenty years later—in a 1950 preface—the Gibbses produced a new edition of their 1941 *Atlas*, expressing the same anti-objectivist sentiment in somewhat different language:

> Experimentation with wave counts . . . and with frequency analysis of the electroencephalogram . . . indicate that no objective index can equal the accuracy of subjective evaluation . . . if the electroencephalographer has learned to make those significant discriminations which distinguish between epileptic and non-epileptic persons. Accuracy should not be sacrificed to objectivity; except for special purposes analysis should be carried on as an intellectual rather than an electromechanical function.[12]

"*Accuracy should not be sacrificed to objectivity.*" In this astonishing statement—astonishing from the perspective of mechanical objectivity—we see the epistemic footprint of the new, mid-twentieth century's regime of the interpreted image. How different this is from the reverse formulation of mechanical objectivity: that objectivity should not be sacrificed to accuracy. One thinks here of Erwin Christeller's insistence in his *Atlas der Histotopographie gesunder und erkrankter Organe* (1927) that "[it] is obvious that drawings and schemata have, in many cases, many virtues over those of photograms. But as means of proof and objective documentation to ground argumentation [*Beweismittel und objektive Belege für Begrunde*] photographs are far superior."[13] In the search for such *objektive Belege*, advocates of mechanical objectivity, roughly from the 1830s to the 1920s, were willing to sacrifice the color, sharpness, and texture of scientific representations for a method that took the brush from the artist's hand and replaced it with instruments. In his time, Lowell's tiny, blurry, black-and-white photographs counted for more than artistic renderings, even if the latter would have been sharp, complete, reproducible, and in color. For advocates of judgment like Gibbs and Gibbs, it was equally obvious that the "autographic" automaticity of machines, however sophisticated, was no longer an acceptable substitute for the professional, practiced eye.

In their radical devotion to mechanical means and their protestation of innocence against the charge of intervention, one senses in nineteenth-century atlas writings a certain defensiveness, a nervousness before the charge that the phenomena were not actually out there, but instead were the mere projections of desires or theories. For Gibbs and Gibbs, that acute anxiety is not present; the idea that the phenomena might be a "mere projection" is simply absent. At one level, this transition from a strident objectivism to a confident culture of scientific judgment should not surprise us. We know from a wide variety of excellent studies that throughout Europe and the United States, the mid to late nineteenth century was precisely the period of maximal scientific institution building, the time when amateur societies coalesced into major state and privately financed fixtures.[14]

These last decades of the nineteenth century were, institutionally, years of transition, during which the persona of the scientist was itself shifting. On the outside, the weighty buildings of the new scientific buildings were hybrids, crossed between neoclassicism and nineteenth-century factory design. Inside the walls, and in the self-image of the investigators themselves, the interior world celebrated the values associated with precision, accuracy, and self-abnegation.[15] In this period of rapid institutional expansion and reformulation of the role and proper comportment of the scientist, it is perhaps not surprising that while these new investigators aim for the durable results of exactness, they were still defensive about their new status. (Should one already call them professionals, trained experts, or following Timothy Lenoir, *Bildungsburger*?) Even as the great brick and stone buildings arose across Berlin, London, Washington, and Paris, laboratory scientists embarked on a nearly fanatical effort to establish their bona fides (in which the epistemology of mechanical objectivity played a part). Only after the institutions themselves had completed the bulk of their construction and the category of the investigator had stabilized does one see the emergence of the more assured ethos that characterized trained experts (with their epistemology of learned judgment).

Reading on in the Gibbses' 1941 *Atlas*, one finds, too, a contrast that would have been unimaginable within the earlier atlas-writing tradition of mechanical objectivity: they oppose an "intellectual" approach to one that is (electro)mechanical. Such a clash again signals a changed vision of *who* the scientist is. No longer most admired for a saint-like (or bureaucratic) self-restraint or an ability to become part of a machinic order transmitting nature undistorted, the scientist now emerged as an intellectual. Neither the eighteenth-century Genius nor the nineteenth-century lay ascetic, the scientist of the twentieth century entered as expert, with a trained eye that could perceive patterns where the novice saw nothing. The "practiced eye" emerges, for example, in geology as well—in atlases, for example, such as Oelsner's 1961 mineralogical study that trained the budding geologist to sort microscopic ore samples. Reflectivity, Oelsner noted, depends crucially on the polishing of the surface, so "beginners using it can often make gross errors." Color too is susceptible to "remarkable misinterpretations" until the neophyte has acquired a "very experienced eye."[16]

Emphasizing the activity demanded of the picture user, the Gibbses went on to liken the development of skills needed to "read" an encephalogram to those required to read a new language bearing an unfamiliar alphabet and a different script. True, they acknowledge, encephalography is not simple to master, but with three months of practice, they promised 98 percent accuracy by an average person.[17] The expert (unlike the Genius) can be trained; and (unlike the machine) the expert is expected to learn—to read, to interpret, to draw salient, significant structures from the morass of uninteresting artifact and background. As another encephalographic atlas (from 1962) put it, "the encephalogram remains more of an empirical art than an exact science."[18] Strikingly, this advocate contrasts empirical art with exact science. Here, the "empirical art" does several things: it first identifies that portion of the wave train that is "regular"—unlike automatic methods that ploddingly must examine each fragment, the eye quickly assesses some portion of the signal as "regular" or "typical." Second, the unaided eye finds "patterns" (which the author inserts into quotation marks). In part, this frank admission of the craft nature of encephalogram reading ties the debate over objectivity to the practice of clinical medicine. But the supplanting of the automatic by the judgmental extends so far beyond the clinical—into the domains of geology, particle physics, and astronomy—that one cannot rely on the specific history of medicine to account for the new emphasis on an active reader.

Before leaving the call of these atlas writers for an "empirical art," return for a moment to the analogy used by the Gibbses in 1941 for their new judgment-based reading: the practice of making a distinction between an Eskimo and an Indian by an un-self-conscious process of totalistic recognition. In this ethnological simile one has a theme that emerges quite widely, not only through Gestalt psychological concern with holistic cognition, but through the wider (and not unrelated) preoccupation with matters of race in the 1930s and 1940s.[19] Judgment as an act of perception and cognition seems to be associated with a picture of reading that is both antialgorithmic and antimechanistic. Judgment in some sense stands as opposed to a fragmented building-up, to a mechanistic assemblage, and to an automated, protocol-driven set of procedures. Judgment must be acquired laboriously, but it is a labor of a very different sort from that of the nineteenth-century mechanical objectivist. Interpreted images carried force not through the labor behind automation, self-registration, or absolute self-restraint, but through the expert training of the eye.

Consider an atlas located (literally) light years from the human brain, W. W. Morgan, Philip C. Keenan, and Edith Kellman's *An Atlas of Stellar Spectra* from 1943. (See Figures 2a and 2b.) Here the authors set out a classification of stars in the 8–12 magni-

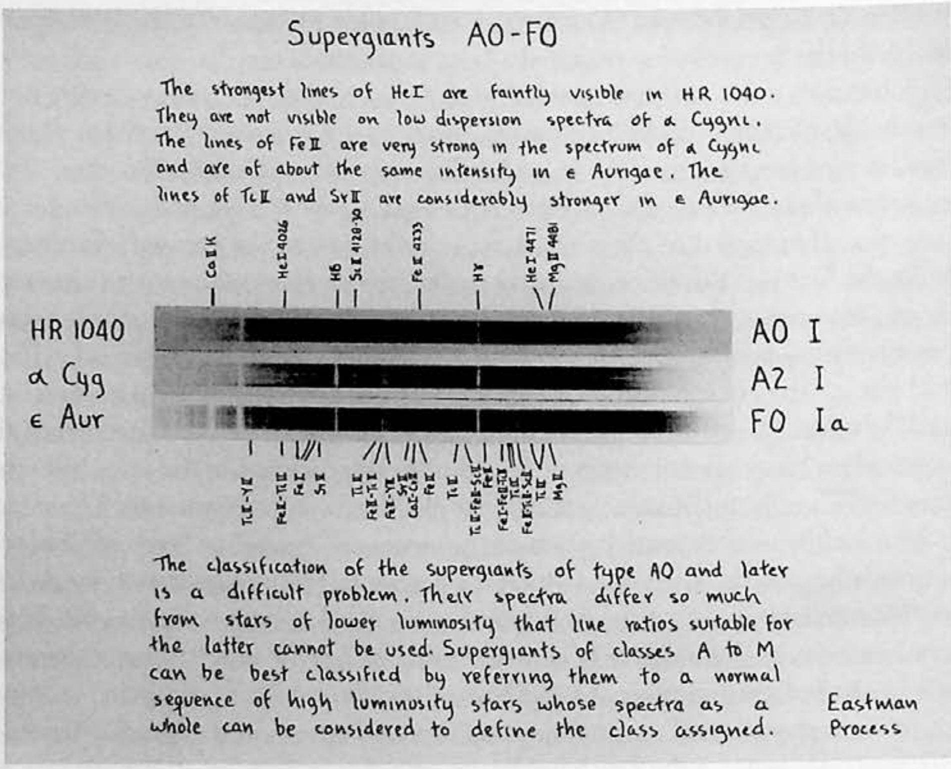


*Figure 2a.* *"Supergiants AO-FO," plate 20 from Morgan, Keenan, and Kellman,* An Atlas of Stellar Spectra *(Chicago: University of Chicago Press, 1943).*

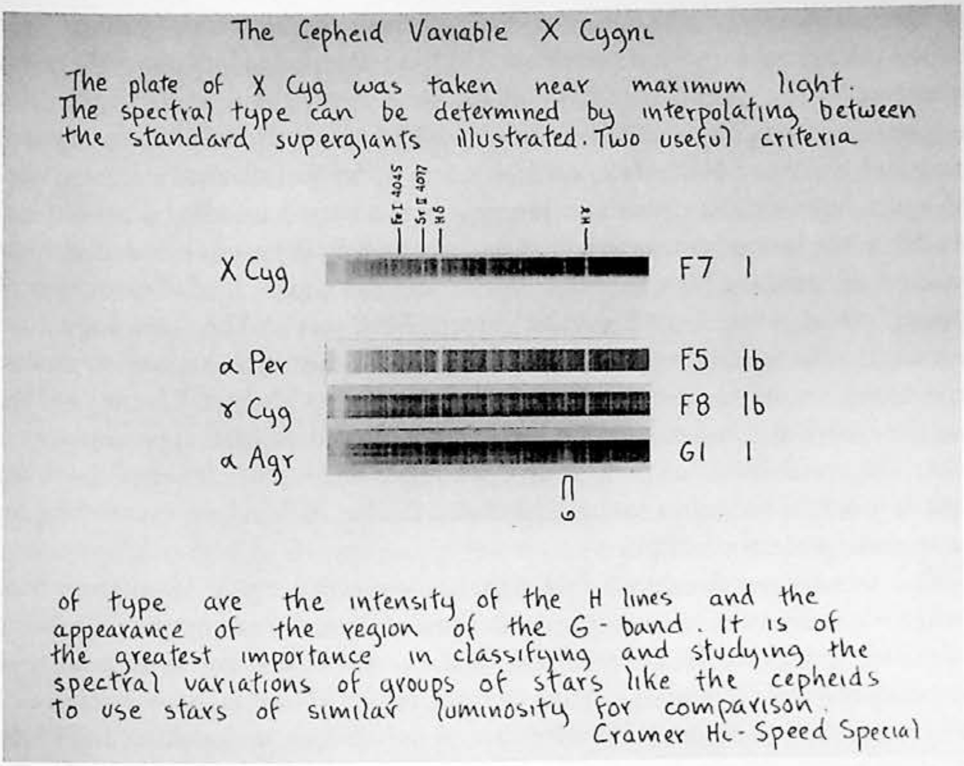


*Figure 2b.* *"The Cepheid Variable X Cygni," plate 39 from the same atlas.*

tude range based on their spectra. The work was carried out with a one-prism spectrograph attached to a 40-inch refracting telescope. Plates were then sorted according to a two-dimensional system: on one axis stood the spectrum (based, for example, on the intensity of the hydrogen lines), yielding the *star type* (O, B, A, F, G, K, M, R, N, S), and on the other axis stood the luminosity (ranked by class I–V, progressing from the dimmest to the brightest). In practical terms, the astronomers first determined a rough type, an "eyeball" estimate of the category of a given spectrum—say B2, a variant of the B-type. Second, using parallax measurements to fix the distance to the star, they found the star luminosity. With the luminosity in hand they could then compare the candidate star spectrum with previously established spectra of similar luminosity. Matching the candidate spectrum against previously sorted spectra for B1, B2, and B3 then fixed the precise classification, which might well not be B2 after all, but rather B1 or B3 (the final classification rarely differed from the rough estimate by more than that).

The process of identifying a star as, say, a B2 class V star might seem purely routine, the kind of sorting that might just as well be effected by an automatic system. Not so, said Morgan, Keenan, and Kellman: "There appears to be, in a sense, a sort of indefiniteness connected with the determination of spectral type and luminosity from a simple inspection of a spectrogram. Nothing is measured; no quantitative value is put on any spectral feature. This indefiniteness is, however, only apparent."[20] Here is an interesting and important claim: *the qualitative is not, for being qualitative, indefinite*. Again and again, one sees this cluster of terms now in the ascendent: what is needed is the subjective, the trained eye, and an empirical art, an "intellectual" approach, the identification of "patterns," the apperception of links "at a glance," the extraction of a "typical" sub-sequence within a wider variation. Reflections like these point to the complexity of judgment, to the variously intertwined criteria that group entities into larger categories defying any simplistic algorithms. But for Morgan, Keenan, and Kellman the complexity and nonmechanical nature of this identificatory process does not vitiate the possibility of arriving at an appropriate and replicable set of discriminations. It may take judgment to sort a B1 from a B2, but such judgments can be unmechanical *and* perfectly definite.

What the observer does, according to the authors, is to combine a variety of considerations: the relative intensity of particular pairs of lines, the extension of the "wings" of the hydrogen lines, the intensity of a band, "even a characteristic irregularity of a number of blended features in a certain spectral region." None of these characteristics could be usefully quantified ("a difficult and unnecessary undertaking"). The root problem is one that has long vexed philosophers: "In essence the process of classification is in recognizing similarities in the spectrogram being classified to certain standard spectra."[21] Of what do these "similarities" consist?

Recognition cannot be grounded in the application of algorithmically fixed procedures; any such attempt would be cumbersome at best, and at worst, would ultimately fail. Our stellar spectroscopists continue with the by-now-familiar appeal to the physiognomic Gestalt:

> It is not necessary to make cephalic measures to identify a human face with certainty or to establish the race to which it belongs; a careful inspection integrates all features in a manner difficult to analyze by measures. The observer himself is not always conscious of all the bases for his conclusion. The operation of spectral classification is similar. The observer must use good judgment as to the definiteness with which the identification can be made from the features available; but good judgment is necessary in any case, whether the decision is made from the general appearance or from more objective measures.[22]

Note that, like the Gibbses, these star atlas authors contrast judgment with objectivity, where objectivity is used quite clearly in the sense of mechanical objectivity: fixed, specifiable criteria of evaluation. But, for both twentieth-century picture classifiers, "mere" objectivity was insufficient. Good judgment could be predicated on no such hard and fast rules of engagement.

Classifying (judging) by luminosity was by no means simple, and illustrates the complex way in which judgment had to be deployed. Certain lines or blends of lines might serve as a basis for calibrating stars relative to a standard in one spectral group; in another it might be useless—the lines might vary hardly at all. Dispersion in the spectrogram—the spreading of spectral lines on the plates—also varied for different spectral types. So long as one used plates of low spectrographic dispersion, hydrogen lines varied with absolute magnitude in stars of type B2 and B3. In high-dispersion plates that separated the "wings" (outlying portions of the broadened spectral line) from the central line, the wings were frequently no longer visible. And since it is these wings that vary with the absolute magnitude, when they are not visible the remaining line looks much the same whether the star it issues from be a dwarf or a giant. Conversely, there are lines visible in the high-dispersion plates that are invisible at lower dispersion. According to the stargazing spectroscopists: "These considerations show that it is impossible to give definite numerical values for line ratios to define luminosity classes. It is not possible even to adopt certain criteria as standard, since different criteria may have to be used with different dispersion." Variations like these made it impossible to specify a one-size-fits-all rule by which to classify: "the investigator must find the features which suit his own dispersion best."[23]

One has here a subtle and interesting confluence of phenomena: on the side of the spectra themselves there is variation that precludes naive rule following. On the side of the observer, there is a peculiarly human ability to seize patterns, and therefore to classify even when our algorithmic forms of reasoning fail. Subjectivity becomes an important feature of classification because the objects do not hold universal essential properties and because it is within our species' nature to be able to classify them univalently.

In sum, Morgan, Keenan, and Kellman draw our attention to four features of judgment: First, they emphasize that classification involves the establishment of similarity relations, and that these similarity relations (e.g., of luminosity) cannot be specified in terms of a fixed set of standard criteria (e.g., line-intensity ratios given for all spectral types). Second, the evaluative process of studying stellar spectra (like the evaluation of "race") is not necessarily a conscious one. With a glance, in a flash of recognition, one sees that a star is "racially" a B-class rather than an F-class entity. Third, the cognitive process at work in interpreted images is represented as holistic, and it is precisely this holism ("decision made from . . . general appearance") that stands in contrast to the "objective measures" of mechanical images (which were piecemeal as well as mechanical). Fourth and finally, nothing in the process of judgment is necessarily vague or indefinite—it is an error, they argued, to suppose that quantitative measures (even were they applicable) are the only way to a determinate classification. All four of these distinguishable features of judgment seem to be captured by the authors' racial-facial simile, and its contrast to quantitative and algorithmic assessment. (Though further discussion would take us too far afield, note that Wittgenstein, too, introduced his version of the racial-facial metaphor, "family resemblance," in the early 1930s precisely to capture a judgment-based, non-mechanical conceptual grouping.)

It might be thought that the atlases that foregrounded judgment differed in subject matter from earlier ones grounded in mechanical objectivity. Perhaps (it might be thought) it was just the twentieth-century material itself that in some way *demanded* judgment, where the subject matter of the nineteenth century required the objectivity of machines. This cannot be the case. There are nineteenth-century X-ray atlases that aspired to mechanical objectivity, and twentieth-century X-ray atlases that relied on judgment; there were anatomical atlases of mechanical objectivity and there were altogether comparable twentieth-century anatomical atlases predicated on judgment. Stellar spectra atlases provide a perfect instance of this continuity of topic and sharp break in the mode of categorical classification. For as we have seen, the Morgan, Keenan, and Kellman atlas argued for judgment over objectivity, root and branch. Strikingly, the atlas that Morgan et al. *explicitly* identified as a direct predecessor was that of the Henry Draper Catalogue of 1918, a volume that quintessentially espoused the image-making goals of mechanical objectivity. To make the contrast as sharp as possible, it is worth pausing for a moment to consider that predecessor volume.

The stunning Henry Draper Catalogue included the classification of some 242,093 spectra from 222,000 stars. It was an opus designed from the outset to last forever: the preface even assured the reader that "various authorities" expected the printing paper itself to be "practically permanent." Edward Pickering (director of the Harvard Observatory) began his preface: "In the development of any department of Astronomy, the first step is to accumulate the facts on which its progress will depend." Nowhere did he expound on judgment as necessary to classify the spectra or on the absence of universal criteria of selection, or on the role of preconscious cognition. On the contrary. Pickering's preface to the Henry Draper Catalogue celebrated the use of scientific management and mechanical objectivity. These were so "automatic" that they were held to be suitable for a replaceable set of hardworking (female) assistants of whom an "average" of five were at work at any given time over four years.[24]

The practice of employing women to do astronomical calculation and classification can be, and has been, read as a labor-historical chapter in workplace history.[25] There are, it seems to me, two further elements that bear on the epistemic status of facticity itself. First, in nineteenth-century mechanical objectivity the very possibility of employing "unskilled" workers served as a tacit guarantee that these data were not the figment of a scientist's imagination, or the results of a preexisting philosophical commitment. In this respect, the workers were identified with the machines, and like the machines in their "emptiness" they offered a transparency through which nature could speak.[26] Second, beyond their supposed "lack of skill" women workers were presumed to offer a "natural" predilection away from the grand speculative tradition. Occasionally, in the context of mechanical objectivity, this presumption conveyed the highest praise. Annie Cannon, who coauthored the great Henry Draper Catalogue with Edward Pickering, was hardly a "mere" computer—it was she who modified and rearranged the older star spectrum classification (A, B, C, etc.) into the long-lived Harvard system of spectral classification. It was also Annie Cannon who showed how these species could be rearranged to display the spectra in a continuous fashion. But it was precisely for her deliberate abstinence from theorizing that she was esteemed by her contemporaries, as is clear from the characterization of her written the year of her death in 1941: "Miss Cannon was not given to theorizing; it is probable that she never published a controversial word or a speculative thought. That was the strength of her scientific work—her classification was dispassionate and unbiased."[27]

Both the Henry Draper Catalogue of 1918 and Morgan et al.'s 1943 atlas on the same subject handled stellar spectra. But where the later authors saw the irreducible need for judgment, Pickering, Cannon, and their epistemically nineteenth-century staff had viewed their ideal atlas as planted on the firm ground of scientific management and mechanical objectivity. So despite Morgan et al.'s use of the Draper catalogue—despite their similarity of subject—the framing of the two projects was quite different. Here and elsewhere, in domain after domain, objectivity, facticity, and scientific management yielded to a new world of sorting nature in which judgment, subjectivity, artisanal practice, and theory were heralded as vital to the scientific project of visual classification.

Atlases of the mid- to late twentieth century, unlike those of the mid-nineteenth, began to be explicit about the need for subjectivity, as in the atlas of *Normal Roentgen Variants that may Simulate Disease* (1973): "The proof of the validity of the material presented is largely subjective, based on personal experience and on the published work of others. It consists largely of having seen the entity many times and of being secure in the knowledge that time has proved the innocence of the lesions."[28] Such a spectrum of the normal required exquisite judgment and extensive clinical training. It built on the famous 1939 treatise of Rudolf Grashey (*Typische Röntgenbilder vom normalen Menschen*), an early call for interpreted images, by means of which the author sought to empart to his readers a sense of the limits of the normal. To Grashey, photographs were "*Steckbriefe*" (wanted posters) that told the radiologist where the territory of the pathological began.[29] Again, one sees interpreted exemplary images analogized to the recognition of a suspect, "other" face.

In particle physics one finds the same kind of argument as that advocated by the X-ray master Grashey: the atlases are there to teach the range of what is known in order to highlight the unusual. In physics, however, the "pathological" becomes the

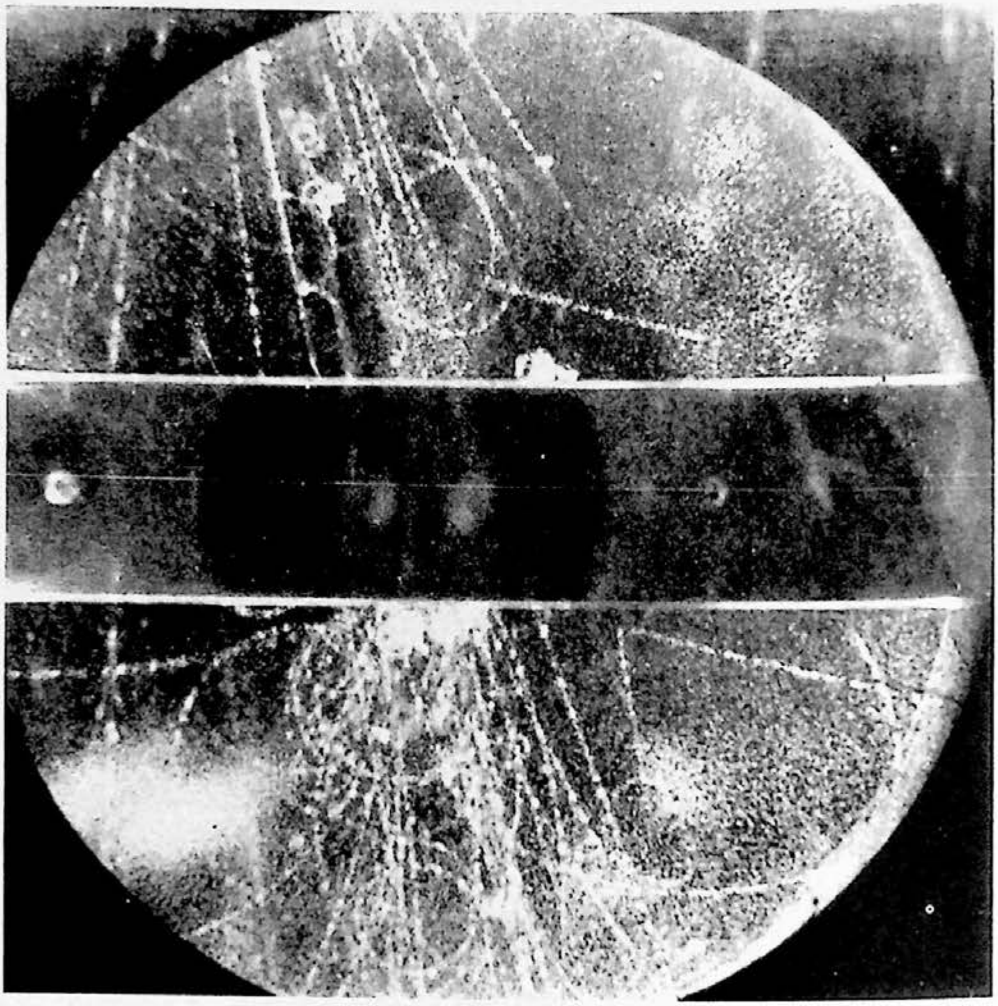

*Figure 3.* *"The First V-Particle," plate 103 from Rochester and Wilson,* Cloud Chamber Photographs *(New York: Academic Press, 1952).*

rare and unknown species of particles, and the "normal" becomes the known instances of particle production and decay. P. M. S. Blackett, one of the great cloud-chamber physicists of British physics, authored the foreword to George Rochester's 1952 *Cloud Chamber Photographs* (see Figure 3), in which he put it this way:

> An important step in any investigation using [the visual techniques] is the interpretation of a photograph, often of a complex photograph, and this involves the ability to recognize quickly many different types of sub-atomic events. To acquire

> skill in interpretation, a preliminary study must be made of many examples of photographs of the different kinds of known events. Only when all known types of event can be recognized will the hitherto unknown be detected.[30]

Learning to recognize the novel was a matter of training the eye, whether to pick malignant lesions from normal variations, or to extract a kaon from a background of pions.

Whether one was dealing with pions, skulls, hands, lesions, stellar spectra, heartbeats, or brain waves, the problem was the same. Automatically sorted pictures, by themselves, were no longer enough. According to an increasing number of mid-twentieth century atlas makers, more than mechanical images were needed. Only *interpreted* images—interpreted through creative assessment, unconscious pattern recognition, guided experience, and holistic perception—could be made to signify. Only through individual, subjective, creative judgment could pictures transcend the silent obscurity of their raw form. Only the judging eye could pluck the pathological lesion or the previously unseen meson from the tangled pictorial world of "normal variations."

## THE ART OF JUDGMENT

Bearing in mind the twentieth-century demand for judgment of images—from electroencephalograms to stellar spectra—one can now come back to our (by now) long-familiar relation of surgeon to medical artist. But where, in the 1800s, our surgeons swore that they policed every line, every dab of color for accuracy, or sought the photographic as an explicit means of avoiding the need for such surveillance, after the 1920s one begins to uncover a very different relation between scalpel and sketch. Here, in a 1968 *Atlas of Precautionary Measures in General Surgery*, Ivan Baronofsky reports, without apology, on the active measures taken by "his" illustrator, Daisy Stilwell, "one of the finest artists in the medical field." He adds: "This accomplishment might be sufficient were it not for the fact that Miss Stilwell is a superb interpreter. It would have been simple for her merely to act as a camera, but instead she brought out the features that justified the picture."[31] In the nineteenth century, being likened to a camera had been the highest praise. The artist's autonomy and interpretive moves were powerful threats to the representational endeavor, threats the camera alone could quell. For Baronofsky, being a "mere" camera carried only opprobrium. To be able to *interpret* was the key; judgment made it possible to sort the significant elements that "justified the picture" from the background. Mere camera-enabled naturalism was too blunt to reveal what the atlas makers and readers wanted to see.[32]

Baronofsky was not alone. John Madden's 1958 *Atlas of Technics in Surgery* did not hesitate to underline just how far representation stood from the surgical theater: "In illustrations, the incisions never bleed and the clamps and ligatures on the cystic and superior thyroid arteries never unlock or slip off. Furthermore, postoperative complications do not occur and there are no fatalities." Bloody incisions and slipping ligatures were the human side of the operating room, hospital-floor pragmatic realism barred from a representational realism founded on judgment:

> In the preparation of the Atlas the importance of having the medical artist present at each operation was stressed. It is only in this way that one may obtain in the illustrations anatomic realism and the creative interpretation of the artist. Only those operations that were witnessed by the medical artist are depicted.[33]

In pursuit of this "anatomic realism," the artist would sometimes witness three or four surgical procedures, with the goal of obtaining a logical visual exposition with no "jumps." To obtain that realism, Madden (like Baronofsky) was perfectly willing to eschew the mechanical objectivity of the camera, and was more than willing—enthusiastic, even—about the adoption of the "medical artist" whose "creative interpretation" offered an accuracy, *a realism*, that more automatic procedures could not match.

No policing of the artist, it seemed, was desirable in these various twentieth-century atlases. (How different Madden and Baronofsky are from Johannes Sabotta, whose famous turn-of-the-century work, *Atlas and Textbook of Human Anatomy*, denounced woodcuts as not "true to life" precisely because they left "entirely too much to the discretion of the wood engraver"—a discretion that photomechanical reproduction would stop cold.)[34] As Wittgenstein, Madden and Baronofsky insisted, it was just the artist's ability to extract the salient that rendered a depiction useful.

The identification of the salient by the self-confident anatomist, surgeon, or scientific illustrator is far from the metaphysical "truth to nature" image extracted by Genius. Goethe, Cruveilhier, Alabinus, and Soemmerring never had as their aim the use of exaggeration or highlighting to facilitate recognition, classification, or diagnosis—they were after a truth obscured by the imperfections of individual appearance. Emphasis in the interest of operational success is a long way from perfection in the interest of metaphysical truth.

One 1954 atlas celebrated the choice to maintain drawings over actual X-ray photographs in pursuit of this operational and diagnostic truth:

> The publisher has done well to retain the original illustrative sketches. A drawing can show so much better the features one is trying to emphasize than the best chosen original roentgenogram. And of course it is such ideal abstractions of sought-for morbid changes that one carries in one's mind as one searches the fluoroscopic screen for diagnostic signs.[35]

Interpolation, highlighting, abstraction—all were subtle interventions needed to elicit meaning from the object or process and to convey that meaning in the representation itself. The images of judgment are neither those of truth to nature nor those of mechanical objectivity.

Even where the object itself is as unchanging as the visible face of the moon, accurate representation was a task of monumental difficulty long after the development of the camera. In 1961, V. A. Firsoff published his *Moon Atlas*, and the difficulties of extracting realism from the vagaries of moment-to-moment astronomical appearances were all too apparent. Judgment, individual judgment, could not be eliminated:

> Nobody who has not himself attempted to map the Moon can appreciate the difficulties involved in such a programme. The lights and shadows shift with the phase and libration and can alter the appearance, even of a clear-cut formation, almost beyond recognition. Thus every region has to be studied under different illuminations and a true picture of the surface relief built up step by step. To some extent the result must needs be one of individual judgment.[36]

Representation need not be homomorphic.[37] That is, the pictures we construct from the world need not correspond in form to something one has seen—or even could see were one to be somewhere else (or even were one to be much bigger or smaller than our given human size). Population density maps, for example, use the visual to express a phenomenon that might otherwise have been presented in tabular form. For the physical sciences such non-ocular representations as tables serve frequently in all branches of theoretical and experimental work, and such illustrations are often the highly processed output of a computer that has not only stored reams of data but manipulated them in controllable ways. When Robert Howard et al. composed their *Atlas of Solar Magnetic Fields* in 1967 (see Figure 4), they had to *choose* how much to "smooth" the data as they grappled with different observations. Even here, in this most physical of the sciences, the role of objectivity is frankly contested by a resurgent subjectivism tied to the twentieth-century emphasis on judgment and interpretation:

> Considerable experience in the handling of the magnetograms has made us cautious in our approach to their interpretation, but for those unfamiliar with the instrument the variation in the quality of the observations can be a great handicap. For this reason we decided that the best way to make the information available was in the form of synoptic charts, which represent a somewhat smoothed form of the data.
>
> Inevitably many decisions had to be made concerning what were or were not real features on the magnetograms. Naturally there is a certain *subjective* quality to these charts.[38]

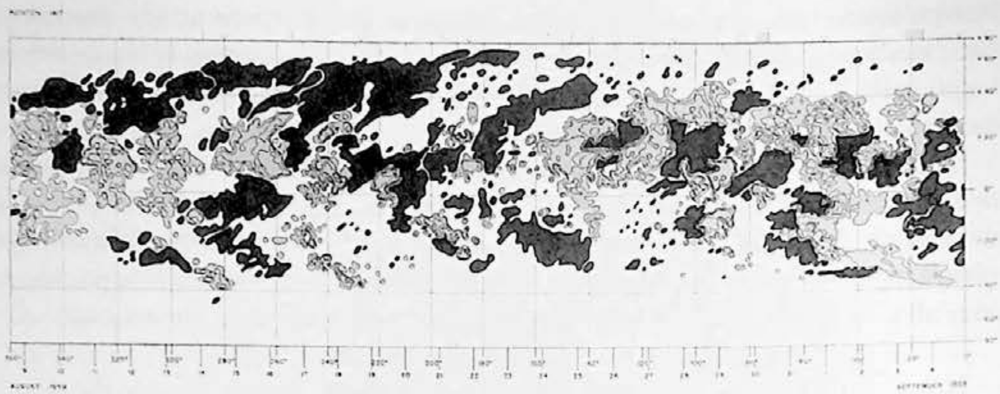

*Figure 4.* *"Rotation 1417, August-September, 1959," from Howard et al.,* Atlas of Solar Magnetic Fields, *1967. Courtesy of the Observatories of the Carnegie Institution of Washington, D.C.*

Decisions are active, and as such would have had no place within the fundamentally passive category of nineteenth-century mechanical objectivity.

Gerhart Schwarz (from the Chronic Disease Center of New York Medical College), collaborating with Charles R. Golthamer (van Nuys, California), also had an active, artistic conception of pictorial production. Together these two radiologists teamed up to produce a 1965 Röntgen atlas of the human skull. By this time, the authors argued, the discipline had advanced to the point where familiarity with normal skull radiology could be simply assumed as background knowledge: now radiologist, orthopedic surgeon, dental surgeon, neurologist, neurosurgeon, otolaryngologist, and forensic specialist needed not normality but the variants and pseudo lesions that could "vex" even the expert. Several simultaneous demands made the task complex: first, Golthamer and Schwarz wanted not a "facsimile" but "a theoretical composite of many different skulls, containing more than *one hundred* variants and pseudo lesions on each printed plate." Second, the authors insisted on prints at least of actual size, some even larger than life. These two constraints, coupled with the "profusion of nature's variants" promised to overwhelm any possible text. What to do? "It was then that Dr. Golthamer suggested that we might reproduce all radiographs by hand." *Even though the X rays already existed, drawings would be created.* It was a move unimaginable seventy-five years earlier: after the hard-won struggle to extract a photograph of Mars, can one conceive of Lowell reverting to a hand-produced image when he had a photograph available? Realism (in this mid-twentieth-century context) did not aim at the reflexive correspondence of nature with reproduction, but rather at the half-tone drawing that *interpreted* particular radiographs.[39]

Golthamer, though he was (on his own account) "an expert painter with many awards to his credit" could not produce a "sufficiently realistic" rendering, nor could Schwarz. Finally, with the aid of the art department director of the College of Physicians and Surgeons, they met with success; the volume represented the combined efforts of two other artists (Helen Erlik Speiden and Harriet E. Phillips). Once the artistic technique (and artist) had been perfected, a more subtle set of concerns arose, issues that get at the very heart of the problem of objectivity in its struggle with judgment:

> The question as to how true to nature the image should be arose for more than one reason. Our initial intention was to make the plates look as "natural" as possible, depicting the normal variant, or pseudo-lesion, as true to its appearance on an actual radiograph as the artist's skill could achieve it. However, after our first plate had been drawn in this manner, we came to realize that painstaking copying of nature was not the purpose of drawings in an anatomic atlas. In many instances, a normal variant, depicted "naturally," remained invisible except to the trained eye of a specialist who was familiar with the lesion to begin with. Reading the completely "natural" plates turned out to be an exercise in "rediscovering" lesions, rather than viewing them. Since a laborious search for lesions in an atlas was surely neither desirable nor practicable, this "natural" manner of graphic presentation would have missed the point altogether. We became convinced that our atlas would gain proportionately in usefulness the more each lesion could be made to look so obvious that a reader would recognize it instantly and without effort.[40]

To bring out the pseudo lesions, the authors depicted *foramen lacerum* "naturally" subdued, and emphasized normal variants and pseudo lesions by "slight optical distortion." "The lesson we learned in preparing the plates for the atlas was that nature may be depicted realistically only by setting off the uncommon and unusual against the background of the 'natural' and common."[41]

If ever one needed evidence that mechanical objectivity had broken down it is here: the enemy of the "natural" (Schwarz and Goldhamer's term) had become the "realistic" (see Figure 5). The real emerged from judgment, and the mechanical transfer of object to representation may well be natural, but the natural was no longer desired. Differing both from the genial improvement of the found object and from the objectivist's mechanical reproduction of the found object, the *interpreted image* is something new. Manipulated to build on the natural, but to bring out features through understanding, the twentieth-century image embodies professional experience; it is the pictorial presentation of the trained eye. A new form of scientific visualization is photographed, painted, and written across this saga of X-rayed lesions.

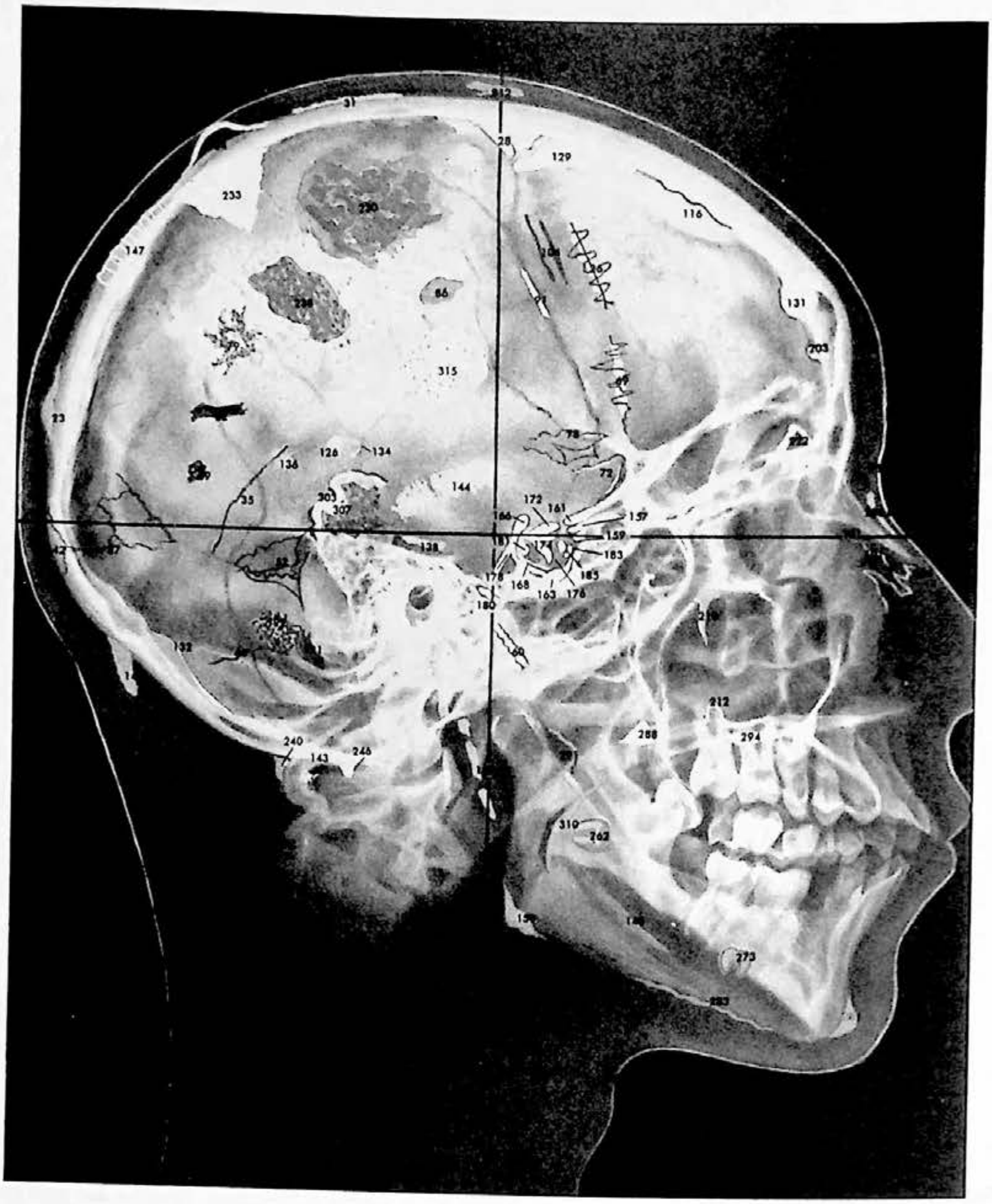


*Figure 5. "Realism vs. Naturalism," plate 1 from Schwarz et al.*, Radiographic Atlas of the Human Skull *(New York and London: Hafner Publishing Company, 1965).*

Here, in the already interpreted image of Figure 5, realism is redefined as one that forcefully takes already existing photographs and replaces them with artwork; a realism explicitly positioned *against* the automaticity of unvarnished photographic naturalism, *against* mechanical objectivity. In making this claim, Schwarz and Golthamer re-situated the nature of depiction; the whole project of nineteenth-century mechanically underwritten naturalism suddenly seemed beside the point. As they arrived at the golden fleece of mechanical objectivity, the purely natural depiction, it proved to be woven of fools' gold. For the image to be purely "natural" was for it to become, *ipso facto*, as obscure as the nature it was supposed to depict. Only by surfacing the oddities against a visual background of the normal could anyone learn anything from the sum of Schwarz and Golthamer's vast labor of compilation.

Golthamer and Schwarz wrote, disarmingly, that it was only after excruciating efforts to depict nature as it was, that they "discovered" the "purpose" of their atlas. Looking back, I would put their concern differently, for what they had discovered was qualitatively unlike the unearthing of a new fossil or the recognition of a never-seen star. Theirs was just as surely a discovery, but one that turned inward to reconstruct not only the kind of evidence they would allow, but the kind of persona that they themselves would need to be. Instead of transparent vehicles for the transport of forms from nature to the reader, the scientist aspired to another ideal, one in which an expert eye counted for more than a mechanical hand. To understand the "discovery" Golthamer and Schwarz had made—to see it repeated over and again as judgment displaced objectivity—is to see just how impossible the interpreted image would have been in the age of mechanical objectivity.

## CONCLUSION: PERSONAE AND PRACTICES

The changing ideals of objectivity reformed both pictorial practice and the scientific persona itself. As such, objectivity exists within history and not outside it. And within the history of the natural sciences, the objective image was never a mere synonym for Truth, Certainty, or Consensus. Instead, the objective (mechanical) image stood at a singular moment in the dynamic and contested history of the image, wedged between a pre-nineteenth-century "truth to nature" and a twentieth-century call to judgment. Put otherwise: the scientific image has, historically, been structured to bring forward a variety of often incompatible virtues—mechanical objectivity carried some, but not all of these virtues, and even those it did capture remained primary for a finite time. The pre-nineteenth-century image of a Goethe, Cruveilhier, Albinus, and Soemmerring aimed (in different ways) to depict a world behind the appearances, a *truth to nature*. The resulting tableaux were intended to be better, higher, more universal than anything nature actually made: they revealed a Truth otherwise obscured, and so were truly *metaphysical images*. Not just anyone could pull back the curtain of unstable appearances to reveal these metaphysical images; it was only the Genius who could extract a form more perfect than the best objects we find this side of our sensory limits.

The nineteenth-century machine ideal, by contrast, made pictures into objects of manufacture. Automaticity aimed to secure the identity of the mechanical image with the entity depicted; the mechanical militated against just the kind of intervention that had been celebrated a generation earlier. If the vocabulary of discipline, management, and policing arrived in force during this period, it is precisely because control of the *mechanical image* is factory-like, with an emphasis on the astonishing regularity that such discipline was supposed to produce. Taking place against the background of the mid-nineteenth-century romance with manufactured objects, image technologies instantiated the valued ability to produce identical things.[42] The modernity of manufacture, the dynamics of control, and scientific labor management all figured in the nineteenth-century mechanical image. Self-denial, self-restraint, and supervision were the moral correlates of such production, and they reinforced and affirmed both the social and epistemic rightness of this new way of re-presenting nature. In such a world, Genius necessarily played a distinctly secondary role, entering, if at all, not in the establishment of the ground level "facts" of the matter, but rather in higher-level theoretical constructions out of these facts.

Though judgment, like truth-to-nature, stood in opposition to mechanical objectivity, judgment and truth-to-nature are far from identical. The atlas author of the twentieth century is a more adept version of the *reader*, not a debased echo of the Genius. To the reader-apprentice of the twentieth century, there was no need to rely on the guiding Genius's qualitatively different sensibility. The Gibbses may have been more familiar with the erratic markings of an EEG than the advanced medical student or up-to-date doctor, but the EEG reader is promised 98 percent reading accuracy in twelve short weeks. No part of the self-confidence displayed here is grounded in genius; the self-confident trained experts (doctors, physicists, astronomers) ground their knowledge in guided experience, not special access to reality. (Imagine Goethe promising his readers the ability to construct the *Ur-Formen* of nature after a Gibbs-like high-intensity training course.) Nor are the *interpreted images* that are products of judgment to be likened to the metaphysical images of an earlier age. Explicitly "intellectual," the new depictions not only invited interpretation once they were in place, they built interpretation into the very fabric of the image—but they did so as an epistemic matter. Theirs were exaggerations meant to teach, to communicate, to summarize knowledge, for only through exaggeration (so advocates of the interpreted image argued) could the salient be extracted from the otherwise obscuring "naturalized" representation. The extremism of iconography generated by judgment is there to allow the initiate to learn to see and know, not to display the ideal world behind the real one.

Here, in summary form, is the set of dualisms presented by judgment advocates (in their own terms), ranged against corresponding aspects of mechanical objectivity.

| **objectivity** | **judgment** |
|---|---|
| objective | subjective |
| exact science | empirical art |
| conscious classification | unconscious classification |
| reliance on "indices" | seeing eye |
| (electro)mechanical | intellectual |
| quantitative | qualitative |
| universal rules | individual judgments |
| re-production | interpretation |
| shared properties | family resemblances |

With this set of contrasts in mind, it becomes possible to summarize the three regimes in which pictorial compilations have been embedded. Images—even images as apparently similar as those found in the atlases of science and medicine—turn out to be radically different entities under the three regimes that roughly covered the three periods of pre-mid-nineteenth century, mid-nineteenth century to the early twentieth century, and the last two thirds or so of the twentieth century. The *metaphysical image*, revealing the essence behind the appearance, mediates between the Genius and an audience that learns from the metaphysical images, but will never become the genial author himself.[43] By contrast, the objective, *mechanical image* is produced by scientists committed to the role of a stoic, and, in this resolve, determined to become transparent to nature, a copying mechanism with the affective disengagement of the technical manufacturer. Third and finally, the *interpreted image* is produced not by a moral culture of "towering Geniuses" or neutral, self-abnegating bureaucrats, but by self-confident experts, who trust the trained eye more than master philosophical systems or the automatic conveyance of pictures. While the Genius used the metaphysical image to reveal truth, the technocratic objectivist became a transparent medium for nature to image itself, and the trained expert created images that brought conditioned experience and judgment to the edification of initiates. In the sense used here, "trained expert" designates not so much an initiate into a secret set of skills, but a potential "everyman" who will come in greater or lesser measure to exercise correctly the "experienced eye." "One day," the twentieth-century apprentice could say of the interpreted images of science (as the admirer could never say of the Genius), "I will see like that."

Given the historicity and the contingent nature of these regimes of scientific images, it strikes me as rather doubtful that *the* role of scientific representation can be located. Michael Lynch, for example, maintains that scientific representation is *about* selectivity and mathematization. By contrast, we can see such an assertion as a frequently heard voice of a particular epoch (that of the interpreted image), in which manipulation and restructuring of images was taken not only to be acceptable but praiseworthy. Within the ideal of mechanical objectivity, such intervention was heresy of the worst sort. Recall Henry Alsop Riley's denunciation of drawing as a "purely selective presentation" able to illustrate "anything that the author wishes."[44] Selectivity and mathematization are modes of manipulation that themselves exist within a larger framework of judgment, and judgment within and of images is a *historically specific* form of object classification.

Genius to manufacturer to trained expert; metaphysical image to mechanical image to interpreted image. This epigram, necessarily schematic, joins the epistemological history of the image to the characterological history of the author-scientist. Along with this conjoint history comes a reshaping of the presupposed audience for the image. For different reasons, both the metaphysical and mechanical image presuppose an epistemic passivity on the part of those who see the images: the metaphysical image is self-contained because it is an image of a revealed truth otherwise hidden, and the mechanical image is self-contained because it "speaks for itself" (or for nature). But the interpreted image demands more from its recipient, explicitly. The often-repeated refrain that one needs to learn to *read* the image actively (with all the complexity that reading implies), shifts the assumed *spectator* into an assumed *reader*.

Taken together, these changes in author-artist, reader, and image track a profound shift in the status of the basic low-level objects that make up the disciplinary "facts" of the special sciences. Temporally, the start time for mechanical objectivity appears to sit squarely in the nineteenth century, not in the seventeenth. Spatially, this restructuring of figuration violates national boundaries—our history would only awkwardly separate developments in Germany from those in France, England, or the United States. Should one then speak of an "American-European context" that emerged in the twentieth century, as an explanation that would depict these changes in image making and image understanding as epiphenomenal?

One such approach might involve the invocation of a kind of technological determinism: the shift to objectivity merely reflected the adoption and dissemination of the photographic techniques emerging in the mid-nineteenth century. But this puts the cart before the horse. The ideal of mechanical objectivity could be and indeed was put into practice well before photography became widespread and certainly long before photography entered the atlas-making business. Through policed artistic work, tracing, copying, and the cameras lucida and obscura, film itself entered into an already-existing praxis of mechanical objectivity, and enhanced it.

At the same time, there is a political dimension to the shift from mechanical objectivity and toward judgment but, I suspect, one different from two popular conceptions. The first political reading, articulated frequently in science studies, is that a sociologically glossed Wittgenstein shows science to be now and to have always been a judgment-governed rule-defying activity. Without explicit protocols of action and inference, science is seen as stripped of its authority to make realist claims about the world. In much current work within science studies, judgment comes to stand for the political left, a rebuttal to the ineradicable conservatism of a realistically interpreted science. Against this emerges the second political reading, articulated ever more stridently by conservative critics of science studies, that science is now and has always been an objective reality-reflecting activity. With its strict, universally applicable methods, mechanical objectivity defies the arbitrariness of the subjective, and reflects nature directly. Objectivity in this world is the province of the political right, a refutation of the ineradicable irrationalism of leftist or poststructuralist claims. *Umgekehrt*, as Marx liked to say in his favorite one-word sentence. Both views have their politics ahistorical and backward.

To the extent that mechanical objectivity has a political valence, it would seem to be closer to that of a nineteenth-century, European, technically oriented bureaucrat, a mostly German liberalism that characterized the *Bildungsburger*: cultured in specific ways, above party politics, perhaps, but committed, above all, to a stabilizing program of trade, technological advance, and national unity.[45] To the extent that judgment took on a political valence, it would find its most articulate spokesmen not among firebrand leftists, but among mid-twentieth century *conservatives*: Wittgenstein, as he opposed (and was detested by) the left wing of the Vienna circle, Michael Oakeshott and his followers, who saw judgment as the antidote to an excessive left-wing rationalism, and Michael Polanyi as he struggled to find a place for faith and cultural elitism within modern science. I am afraid, therefore, that I see neither mechanical objectivity nor judgment as salvational moments in a political philosophy read out of science. Of course there are stunning scientific achievements grounded in both the mechanical objectivity of the last century and judgment-emphasizing classifications of this. But in the end, *politically*, I find the opposition between late-nineteenth-century bureaucratic European liberalism and mid-twentieth-century cultural-political conservatism to be a claustrophobic choice indeed. Reading politics out of scientific practice turns out to be a very untransparent affair.

But however one glosses the shifting self-conception of the scientist and the images that accompany it, two points emerge from this story of pictorial practice. First, mechanical objectivity, the nineteenth century's vision of a rock-bottom facticity for the objects with which science works, is a time-specific, hard-won, and contingent category. To depict only what was actually seen meant sacrificing the universalism and truth of the metaphysical image; to rely on photographs often meant abandoning color, accuracy, reproducibility, clarity, even usability on the altar of this mechanical conception of re-production. Second, if the example of the past is any guide, we might do well not to raise twentieth-century judgment as the new standard, or the always present flag of our epistemological continent. Perhaps we should not take it for granted that the metaphysical and mechanical images were "mistaken" and the interpreted image has finally and permanently got it right. We enjoy—both in science and in science studies—re-killing the proceduralism of mechanical objectivity, the way the mechanical objectivists danced on the grave of interventionist genius. But perhaps we are not at the end of the history of image making. Is it too historicist to see the celebration of judgment over mechanical objectivity as historically rooted in the practices of the new academic scientist, philosopher, historian, and sociologist, as its predecessor image techniques were in earlier versions of the natural philosopher?

## Notes

*For many helpful comments and suggestions, I would like to thank James Conant, Lorraine Daston, Clifford Geertz, Caroline Jones, Hilary Putnam, and Joan Scott.

1. Lorraine Daston and Peter Galison, "The Image of Objectivity," *Representations* 40 (1992): 81–128.
2. Percival Lowell, *Drawings of Mars* 1905 (Lowell Observatory, 1906), foreword, n.p.
3. Lowell, *Mars and its Canals*, cited in William Hoyt, *Lowell and Mars* (Tucson: University of Arizona Press, 1976), pp. 179, 182–85. As Hoyt notes, the pictures that were reproduced (in the *New York Times*, *Scientific American*, *Popular Astronomy*, *Knowledge and Scientific News*) all failed to show the lines of the canals; one journal's pictures did: the Scottish Review. Lowell was sufficiently concerned by this fiasco that he personally brought the original photographs to show some of the more prominent astronomers.
4. Hoyt, *Lowell and Mars* (1976), pp. 185, 195–96.
5. Richard J. A. Berry, *A Clinical Atlas of Sectional and Topographical Anatomy* (New York: William Wood and Company, 1911), pp. 2–7. Reference is to Wilhelm Braune, *Topographisch-Anatomischer Atlas nach Durchschnitten an gefrorenen Cadavern* (Leipzig: Veit, 1872).
6. The transition from truth-to-nature to mechanical objectivity is discussed in much greater detail in Daston and Galison, "Image of Objectivity," *Representations* 40 (1992): 81–128.
7. Henry Alsop Riley, *An Atlas of the Basal Ganglia, Brain Stem and Spinal Cord* (New York: Hafner Publishing Company, 1960), p. viii.
8. Ibid.
9. Moulton K. Johnson and Myles J. Cohen, *The Hand Atlas* (Springfield, Ill.: Charles C. Thomas, Publisher, 1975), p. vii.
10. Frederick A. Gibbs and Erna L. Gibbs, *Atlas of Encephalography* (Cambridge, Mass.: Lew A. Cummings Co., 1941), preface, n.p., emphasis added.
11. Ibid.
12. Frederic A. Gibbs and Erna L. Gibbs, *Atlas of Electroencephalography*, vol. 1, *Methodology and Controls* (Reading, Mass. : Addison-Wesley Publishing Company, Inc., 1951, 1958), pp. 112–13.
13. Erwin Christeller, *Atlas der Histotopographie gesunder und erkrankter Organe* (Leipzig: Georg Thieme, 1927), cited in Daston and Galison, "Image," *Representations* 40 (1992), fn. 68.
14. In Germany, for example, as the work of David Cahan, Timothy Lenoir, Russell McCormmach, Kathryn Olesko, and others has shown, it was during this period that the university physics laboratory came into existence, along with Ordinarius professorships and the institutionalization of research at the university. From 1830 to 1848, institutes and laboratories multiplied and expanded in the heat of enthusiasm and imperial competition: e.g., Göttingen (1833–34), Leipzig (1837), Berlin (1843), and Königsberg (1847); from the 1840s onward, staffing of the institutes began to include the full complement of *Extraordinarien*, *Privatdozenten*, and *Assistenten*. Finally, after 1848 the whole of this expanding state-supported infrastructure began to shift in its orientation from teaching toward research, and to expand numerically. From 1870 to 1920 some twenty-three new research institutes launched their operations; Cahan, "Institutional Revolution," *Historical Studies in the Physical Sciences* 15, 2 (1985): 1–65, on 20–21. Laboratory-based medicine burgeoned during these years as well. See,

e.g., Timothy Lenoir, "Laboratories, Medicine and Public Life in Germany, 1830–1849: Ideological Roots of the Institutional Revolution," in *The Laboratory Revolution in Medicine*, ed. Andrew Cunningham and Perry Williams (Cambridge: Cambridge University Press, 1992), "Science for the Clinic: Science Policy and the Formation of Carl Ludwig's Institute in Leipzig," in *The Investigative Enterprise: Experimental Physiology in 19th-Century Medicine*, ed. William Coleman and Frederic L. Holmes (Berkeley and Los Angeles: University of California Press, 1988), and "Social Interests and the Organic Physics of 1847," in *Science in Reflection*, ed. Edna Ullman-Margalit (Dordrecht: Kluwer Academic, 1988).

15. On precision see M. Norton Wise, ed., *The Values of Precision* (Princeton: Princeton University Press, 1995).
16. Finally, most difficult of all, comes paragenesis, the unraveling of the order of growth—and it is to this aim that O. Oelsner, *Atlas of the Most Important Ore Mineral Parageneses Under the Microscope* (Oxford, London, Edinburgh, New York, Paris, and Frankfurt: Pergamon Press, 1961 [German], 1966 [English]) directs his work, see pp. v–vi. The atlas trains the eye by depicting microscopic samples and providing worksheets that the reader superimposes on the picture to provide keys to interpretation.
17. Frederic A. Gibbs and Erna L. Gibbs, *Atlas of Electroencephalography*, vol. 1, p. 113.
18. Hallowell Davis, introduction to William F. Caveness, *Atlas of Electroencephalography in the Developing Monkey* (Reading, Mass., Palo Alto, London: Addison-Wesley Publishing Company, Inc., 1962), p. 2.
19. On holism and politics before and during Nazism, see Anne Harrington, *Hunger for Wholeness: Holism in German Culture, from Wilhelm II to Hitler* (Princeton: Princeton University Press, 1996), and Mitchell Ash, *Gelstalt Psychology in German Culture, 1890–1967: Holism and the Quest for Objectivity* (Cambridge: Cambridge University Press, 1995).
20. W. W. Morgan, Philip C. Keenan, and Edith Kellman, *An Atlas of Stellar Spectra* (Chicago: University of Chicago Press, 1943), p. 4.
21. Ibid., p. 5.
22. W. W. Morgan, Philip C. Keenan, and Edith Kellman, *An Atlas of Stellar Spectra*. In twentieth-century medicine one often sees "clinical judgment" opposed to the view that nature can speak for itself. For example the author of an atlas on electrocardiograms argues that traces cannot replace clinical judgment. See J. Riseman, P-Q-R-S-T, *A Guide to Electrocardiogram Interpretation*, 5th ed. (New York: Macmillan, 1968).
23. Ibid., p. 6.
24. Edward Pickering, *Harvard Observatory Annals* 91 (1918), preface, pp. iii–iv.
25. On women astronomical workers and their often low-paid status within the observatory, see, e.g., Margaret Rossiter, *Women Scientists in America: Struggles and Strategies to 1940* (Baltimore: Johns Hopkins University Press, 1982); also Londa Schiebinger, *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge: Harvard University Press, 1989); on the Harvard Observatory see Pam Mack, "Women in Astronomy in the United States, 1875–1920" (unpublished senior thesis, 1977). On women scanners in the high-energy physics laboratory, see Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), esp. chapters 3 and 5.
26. On the management and "correction" of workers by use of the personal equation, see Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context* 2 (1988): 115–45; also Schaffer, "Babbage's Intelligence: Calculating Engines and the Factory System," in *Critical Inquiry* 21, 1 (1994): 201–27. On the notion of the "unskilled" generally and in particular about the mostly women workers who reduced the nuclear emulsion photographs in the 1940s and 1950s see Galison, *Image and Logic*, Chapter 3.
27. Owen Gingerich, "Cannon, Annie Jump," in *Dictionary of Scientific Biography*, ed. Charles C. Gillispie (New York: Charles Scribner's Sons, 1980), vol. 3, p. 50. Katherine Heramundanis, ed., *Cecilia Payne-Gaposchkin: An Autobiography and Other Writings* (Cambridge: Cambridge University Press, 1984).

28. Theodore E. Keats, *Normal Roentgen Variants that may Simulate Disease* (Chicago: Year Book Medical Publishers, Inc., 1973), p. vii.
29. Rudolf Grashey, *Typische Röntgenbilder vom normalen Menschen* (Munich, 1939), cited in Daston and Galison, "Image," *Representations* 40 (1992): 81–128, on 105–07.
30. P. M. S. Blackett, foreword to G. D. Rochester and J. G. Wilson, *Cloud Chamber Photographs of the Cosmic Radiation* (New York: Academic Press, Inc. and London: Pergamon Press Ltd., 1952), p. vii.
31. Ivan Baronofsky, *Atlas of Precautionary Measures in General Surgery* (St. Louis: C. V. Mosby Company, 1968), p. x.
32. The problem of what we "see" raises a fundamental and exceedingly subtle point. Wittgenstein distinguishes between judgments that are in some way so close-lying to our perceptions that the insertion of a distance between what we perceive and what we judge is absurd. Can one really say "I *know* I am in pain" without making a joke? (L. Wittgenstein, trans. G. E. M. Anscomb, *Philosophical Investigations* [New York: The MacMillan Co., 1958], par. 246). This is to be contrasted with the expert judgment, e.g., in algebraic extensions of a series. This distinction between a kind of basic and expert judgment lies deep in Wittgenstein, as Cavell has shown (*The Claim of Reason* [Oxford: Clarendon Press, 1979], esp. chapters 3 and 4.) But in the case of scientific figuration the radical splitting between "seeing" and "seeing as" leads, I believe, to much confusion.
33. John L. Madden, *Atlas of Technics in Surgery* (New York: Appleton-Century-Crofts, Inc., 1958), p. xi.
34. Johannes Sabotta, *Atlas and Text-Book of Human Anatomy* (Philadelphia, 1909), cited in Daston and Galison, "Image of Objectivity," *Representations* 40 (1992): 101.
35. E. A. Zimmer, *Technique and Results of Fluoroscopy of the Chest* (Springfield, Illinois: Charles C. Thomas, Publisher, 1954), pp. v–vi.
36. V. A. Firsoff, *Moon Atlas* (London, Auckland, and Johannesburg: Hutchinson & Co., 1961), introduction, p. 7.
37. Elsewhere (*Image and Logic: A Material Culture of Twentieth-Century Physics*), I have contrasted technologies that are homomorphic (retaining the *form* of that which is represented) with those that are homologous (retaining logical relations within that which is represented).
38. Robert Howard et al., *Atlas of Solar Magnetic Fields* (Washington, D.C.: Carnegie Institution, 1967), pp. 1–2, emphasis added.
39. Gerhart S. Schwarz and Charles R. Golthamer, *Radiographic Atlas of the Human Skull: Normal Variants and Pseudo-Lesions* (New York and London: Hafner Publishing Company, 1965), n.p.
40. Ibid.
41. Ibid.
42. One thinks here of Maxwell's reflections on Herschel's consideration of atoms, manufactured goods, and God. Maxwell identified the three categories of usefulness of identical manufactured objects: cheapness, serviceableness, and quantitative accuracy. "Which of these was present to the mind of Sir J. Herschel we cannot now positively affirm . . . though it seems . . . probable that he meant to assert that a number of exactly similar things cannot be each of them eternal and self-existent, and must therefore have been made, and that he used the phrase 'manufactured article' to suggest the idea of their being made in great numbers" (W. D. Niven, ed., *The Scientific Papers of James Clerk Maxwell* [New York: Dover, 1965], p. 484). On the culture of manufacture in Victorian England, see Maxine Berg, *The Machinery Question and the Making of Political Economy, 1815–1848* (Cambridge: Cambridge University Press, 1980).
43. Indeed, throughout the late eighteenth century, Genius was a contentious term; and for the older Goethe it was inextricably bound with the rare ability to discover and interpret the hidden laws of nature. There was nothing democratic about it, none of the genius in every man that characterized Fichte's position, for example. English writers, though they differed in certain respects, also emphasized that the Genius could discover laws otherwise hidden: the Genius offered rules to which others conformed. Strikingly, Alexander Gerard's *Essay on Genius* (1774) explicitly contrasted genius with judgment: judgment was a moderating aspect of affect that controlled and directed otherwise ram-

pant imagination. Judgment alone was never genius. Such remarks show two things: first, that the creative action of the genius was not (primarily) the exercise of judgment, and second, that judgment understood in the eighteenth century as capacity for moderation was *not* judgment understood in the twentieth century as non-algorithmic assessment.

On the role of genius, see Simon Schaffer, "Genius in Romantic Natural Philosophy," in A. Cunningham and N. Jardine, *Romanticism and the Sciences* (Cambridge: Cambridge University Press, 1990), pp. 82–92; Myles Jackson, "Genius and the Stages of Life in Eighteenth-Century Britain and Germany," in *Les Ages de la Vie en Grande-Bretagne au XVIIIe Siècle*, ed. Serge Soupel (Paris: Presses de la Sorbonne Nouvelle, 1995), pp. 35–46; Richard Yeo, "Genius, Method, and Morality: Images of Newton in Britain, 1760–1860," *Science in Context* 2 (1988): 257–81. On Gerard see Jackson, "Genius and the Stages of Life," pp. 38–39.

44. Michael Lynch, "The Externalized Retina: Selection and Mathematization in the Visual Documentation of Objects in the Life Sciences," in *Representation in Scientific Practice*, ed. M. Lynch and S. Woolgar (Cambridge, Mass.: MIT Press, 1990), pp. 153–86.

45. In the German case, one thinks here of the older literature by Fritz K. Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890–1933* (Cambridge: Harvard University Press, 1969) and Fritz Stern, *The Politics of Cultural Despair: A Study in the Rise of the Germanic Ideology* (Berkeley and Los Angeles: University of California Press, 1961), both of whom explore the character of the new bourgeois educated elite. More recently, in a series of important articles, Timothy Lenoir has sought to displace the very category of the "professional" (which he finds both post hoc and too narrow) and replace it for the late nineteenth century with the *Bildungsburger*—with its connotations both of a certain kind of career *and* cultural engagements. See, e.g., Lenoir, "Laboratories," "Ideological Roots," and "Science for Clinic," all cited above in note 14.