Author's Response

By Peter Galison

David Gooding: The Tacit, The Visual, and the Simulated

DAVID Gooding identifies three crucial lines of argument in *Image and Logic*: the tacit, the visual, and the simulation. He rightly points out that my argument is not by any means that all forms of scientific argument are explicit or algorithmic. Instead, I want greater specification of what counts as "tacit knowledge," When is knowledge not explicit because it cannot, in principle, be made explicit—and then *what* principles are involved? But the sociologist or historian of science should not invoke the tacit to halt inquiry: We *can* know more about reasons for hiding knowledge, for saying less than could be said in the pursuit of proprietary, military, or competitive advantage. Often what appears to be an elliptical expression of technique is so only in one setting. What is unspoken at physics meetings is often articulated in craft, engineering, computer, or instrumentation conferences.

Gooding raises an intriguing and difficult question when he asks after the links that might be forged between *Image and Logic* and the cognitive psychology of visual thought. As he puts it, "we should investigate cognitive capacities which enable the work of the trading zones where practitioners from different cultures exchange meanings and methods, often through the enhancement of already specialised languages". It is worthwhile, I believe, to specify the question further: Already Freud, in his last major theoretical work, *The Ego and the Id,* argued that the visual was more primitive than the verbal: "Thinking in pictures is...only a very incomplete form of becoming conscious. In some way, too, it stands nearer to unconscious processes than does thinking in words, and it is unquestionably older than the latter both ontogenetically and phylogenetically" (Freud 1927, p. 21). Gooding is less concerned with supplanting the visual by the verbal, as he is with the replacement of the visual-analogue by the counting-digital. Taken together, these concerns raise a complex issue that has historical, philosophical, and psychological components: what relations are there between the various *non*-visual capacities: our ability to make use of language, our ability to form propositions, and our ability to count and sort?

Related issues on the boundary between logic and psychology have engaged the likes of Wittgenstein and Quine—and certainly, in various ways, more recent writers in the tradition of artificial intelligence, neural networks, and the broader field of cognitive psychology. As difficult as they are to resolve, I have an additional concern. That is, even if one had a full ontogenetic or phylogenetic picture of a universal and monotonic development from the visual to the digital, there would still remain a gap between them and the historical developments of science. Gooding may be right that the visual is the default or 'natural' mode of cognition ("Humans are naturally analogue devices, capable of interpreting experience without recourse to sorting or counting.") Suppose this were so both in the history of the species and in the history of each individual. It could still be the case—and I would argue, it is in fact the case—that the status of visual and anti-visual reasoning has continually been in flux across the whole of the last 150 years. Neither side has 'won', whether in the theoretical reaches of mathematics or physics or in the domain of experiment or simulation. In other words, phylogenetic and ontogenetic development need not univocally fix the dynamics of history.

That cautionary note taken, I do think there are relevant and intriguing results from cognitive psychology. One thinks here of the long-standing debate between those who view pictorial mental thinking to exist as a specific form of thought and their detractors who take all thought ultimately to be explicable in terms of language. In cognitive psychology Stephen Kosslyn has, for example, shown experimentally that people possess the capacity to rotate images (see Tye 1991). Interpreting such mental manipulations as if the images were translated into the linguistic, resolved propositionally, and then re-translated back into images is awkward and clashes with other laboratory studies. My inclination is to take neither propositional nor visual thought as epistemically prior, but rather to assume that humans possess multiple ways of reasoning. What we absolutely lack, as Gooding points out, is a cognitive psychological gloss of what happens at the boundary between the visual and the non-visual. More generally, we know nothing of the coordinative cognitive processes needed to produce a trading zone.

In the world of microphysics, the joining of visual and non-visual forms of evidence came, in part, through the computer. And the computer entered in a variety of ways: as a form of on-line information sorting, as a mechanism for the distribution of data and control, and, strikingly, through simulation. Gooding points out that there are features of Monte

Carlo simulations that have precedent in seventeenth century thought experiments; that is dead right. Thought experiments function in complex ways invoking at one and the same time, abstractions from lived experience, the use of logical and mathematical discipline. My view that simulation is both new and transformative in the sciences is based on three arguments. 1) *Historically*, the computer-based simulations were seen as a break at the time they occurred, and the issue of whether such work was experimental or theoretical was hotly debated. 2) *Philosophically*, some participants argued on epistemic grounds that the simulations were really part of experiment: no two runs of the simulation were the same (unlike a thought experiment). Others contended, on metaphysical grounds, that simulations were a better representation of physical reality because both Monte Carlos and physical reality were irreducibly stochastic. 3) *Sociologically*, the development of a new class of physicists trained in the methods of simulation left them neither accepted by theorists nor by experimentalists. Simulators and simulations became a *tertium quid* in the scientific world. The Monte Carlo and the digital image are novel forms of demonstration in physics, part of what Gooding has aptly called my 'Heraclitean' view of physics. Indeed, building on *Image and Logic*, I would go out on a limb to say this: 50 years from now, it may well be that the most remarkable feature of a wide range of sciences—from microphysics to astrophysics, from nuclear medicine to seismology, from chaotic systems to number theory—will be seen to have been radically restructured by the controllable image.

2. McKinney. Technological Constructivism.

Though they come from rather different traditions, William McKinney and David Gooding both conclude that my position might be described as a non-relativist constructivism. "Physical knowledge," McKinney concludes, "emerges as a technological construction in *Image and Logic*", and elsewhere "Galison's historical studies paint a . . .picture of science [that is] technological in its very essence." I agree, with two qualifications.

First, my understanding of the technological is that tools both shape and carry values and meanings. In the example that McKinney cites of the competing reading regimes, this point is clear. Alvarez considers the human-machine hybrid to be a guiding value governing all aspects of his laboratory from the practice of the lowest-paid scanner to the head physicist. At *every* level, Alvarez aims to mix machines (conducting repetitive algorithmic tasks) with the peculiar pattern-recognition capabilities of humans. This is, in part, a social-historical commitment, a piece of Taylorised workplace strategy to maximise production (pictures scanned per employee-hour). It is also a commitment to a view about human nature—that machines will remain inferior to people in their capacity to recognise the unexpected, the quirky, the discovery. And it is, at the same time, a view about physics—that the important work lying before us is not in marginally better error bars on the masses of known particles, but rather the completely unknown animal, the entity or phenomena Alvarez dubbed the "zoo-on."

This example illustrates the way in which the social, the epistemic, and the cultural enter into the account: technology embodies at once social relations, conditions of demonstration, and guiding values. The 'cognitive' is not manipulated by the 'social' the way iron filings are shoved about by a bar magnet: the social is not a pure, external force lurking outside science, acting by some unseen mechanism. In this sense the narrative of *Image and Logic* is both socio-cultural *and* aimed at demonstrating the technological assembly or construction of knowledge. It is not *reductively* social (or technologically determinist).

There is a further point of clarification. McKinney (and Don Ihde in his very thoughtful book, *Instrumental Realism*, and see especially Chapter 5) characterises my work as taking science to be embodied in technology. I agree with this, but perhaps differ from some others categorised this way (such as Hacking) insofar as I do not take experimental or instrumental technologies to be epistemically prior to theoretical practices. Put it this way: on my view the problem with a history of science periodised by theory was *not* that it would have been better to periodise by experiment. Rather, what we need is a recognition that physics is multiply periodised—that the epistemic practices of experimenters, instrument makers *and* theorists do not march in lockstep (Galsion 1987, Chapters 1 and 5 and *I&L*, Chapters 1 and 9). It is the partial autonomy of these different subcultures (and the material and linguistic practices they employ) that creates the need for the trading zones that hold them in partial, sometimes tense, contact.

3. *Marks: Marx, Alienation, and the Labour History of Physics*

In pointing to the labour history of the laboratory found in *Image and Logic*, Marks has identified one of its critical axes. Socially, architecturally, culturally, the book is an account of the changing ethos of the laboratory, epigrammatically a halting, difficult path from craft shop to cottage industry, to factory, to multinational industry. For even as techniques were handed from cloud chamber through emulsions to film to bubble chamber to time projection chamber, the nature of labour in the laboratory altered dramatically. C.T.R. Wilson was an expert glass-blower and an adept at photography; he wrote papers, conducted experiments in railway tunnels, on mountain tops and in rooms off the corridors of the Cavendish. Wilson's student, Cecil Powell, organised his 'scanning girls' into piecework teams. Luis Alvarez, by contrast, structured his laboratory explicitly on the wartime factory-laboratories he contributed to at Los Alamos and the MIT Rad Lab. And when laboratories at the scale of the Superconducting Supercollider and the Large Hadron Collider were contemplated, remote control, data distribution networks, and multiply-cantered administration linked the production of their product more closely to the administrative/labour structure at General Dynamics than to older factory-style labs.

All this Marks captures perfectly. Yet I demur at the characterisation of alienation as ever increasing, a post-lapserian decline from an Edenic state of wholeness. First, one of the points of *Image and Logic* is that each generation of physicists saw an alienating gulf between itself and its progeny. Big science is always shifting in its definition—the experience of alienating scale emerges in conditions of *change*, rather than in response to any *particular* size. It is not that physics was 'craft-like' at 20 collaborators and 'big' at 50. Even in the time of Wilson, some people bought cloud chambers ready made; some bought already prepared glass containers; some employed technicians. There was no 'pure' state of physics in a time before the fall—in fact to a generation of physicists now engaged in collaborations of 2500 Ph.D.'s, the fixed target teams of 30 look positively personal; to Alvarez's competitors his group of 30 looked like the end of individualism.

Second, there are sudden, unexpected departures from a monotonically increasing form of alienated labour—who would have suspected, at its inception, that the computer would make it possible for a graduate student in Montana to write his own form of data reduction and find a new effect in an experiment conducted at the Swiss-French border by a collaboration of Japanese and European physicists? So yes: labour history and its accompanying form of alienation mark both the technical and physical dimensions of scientific work. But that labour history is neither post-lapserian nor monotonic.

Marks furthermore asks if the history of alienation that I track from the craft-bound cloud chamber to the Time Projection Chamber would be disrupted by embedding Wilson's work in an 'outside' corporate history. Crucially, my argument depends precisely on locating the chamber in a wider history, in fact on un-doing a form of historical narrative that depends on the internal/external division. Let me explain. If you go into the Royal Society archives and order the Wilson notebooks, you get two sets, an "A" series and a "B" series, categorised by whether the studies are of "meteorology" or of "physics." So even before one begins to read, a separation is offered between an 'inside' of physics and an 'outside' of meteorology. But it is precisely *against* such a reading that I have tried to argue: Wilson was always moving back and forth between the two domains, and in the critical years between his first cloud chamber and his publication of track photographs in 1913, he was, in fact, doing both simultaneously. His search was to find the elementary drop, the drop of rain that could form around a single ion. At one and the same time he was revealing the position of the individual carrier of charge *and* the simplest raindrop. And dust and rain were important to late nineteenth century Victorians not just for the fishing industry (no mystery why fishmongers supported the Ben Nevis weather observatory), but also—as one needn't tell Harry Marks—for a variety of public health measures. In the intercalated stories I have to tell in *Image and Logic*, physics is always already embedded in a world wider than that contained in narrow confines of a review article. But the *nature* of that embedding changes in startling, often unpredictable ways. And "embedded in" does not mean "reducible to."

Such a critical stance towards the traditional inside/outside division expands the cast of characters encountered on the historical stage. Scanners, industrial chemists, cryogenic engineers, radar designers, and nuclear weaponeers become part of the physics world. And conversely, by attending to these others, the links between laboratories and between laboratories and the wider world become manifest: cryogenic engineers moved lock, stock, and barrel with their hydrogen liquifiers from Boulder (at the National Bureau of Standards) to Eniwetok (for H-bomb testing) to Berkeley (for the assembly of the 72″ bubble chamber). *Engineers*, not physicists, mediated between the machines at Brookhaven's, CERN's and Berkeley's particle physics labs. Sometimes the mutual relations between these various groups are complementary, sometimes competitive, sometimes bitterly conflictual. But it is here that I aimed to turn my historical spade, in the mix of labour history, epistemology, and material culture—rather than in the classical division between 'internal' and 'external' explanation.

4. Hughes: Culture and Contingency

Jeff Hughes raises the issue of contingency, one of the deep-lying threads running through the entirety of *Image and Logic*. I have a somewhat unconventional view that is at right angle to the scale on which Hughes seems to want to plot my position. The first standard view is that the contextualisation of physics makes it epistemically arbitrary, at any given time best understood as a conceptual scheme dependent on its social surround, and therefore supportive of a philosophical relativism. The second view, a negative version of the first, is that science is *dis*associated from context and that this autonomy militates for a form of metaphysical realism. Despite mutual antagonism these two characterisations are, in fact, closely related to one another: both presuppose a sharp demarcation line between the scientific and the social. In *Image and Logic* I aim to develop a *more* dimensional contextualism that is tied to an *anti*-relativism.

Hughes quotes this passage from *I&L*: "the subatomic world was suddenly rendered visualizable—and consequently took on a reality for physicists that it could never have obtained from the chain of inferences that had previously bolstered the corpuscular viewpoint". Hughes protests that the experimental results are selected, and that this selection process argues against realism. Two points. First, the argument I am making about the reality of particles is *historical*, not transhistorical: in showing how it was that it became a standard trope for physicists (and philosophers) to cite the cloud chamber tracks either as the best evidence for microphysical realism or as the evidence against which one would have to argue to defend an anti-realism. Eddington says we 'almost see' the proton passing through the cloud chamber—Born contends that cloud chamber evidence for the proton is better than our eye-witness evidence of a man being shot: in the former case we see the trajectory of the particle while the bullet moves too swiftly to see. When George Thomson tells Wilson that Wilson had made the subatomic world *real and visible* he, like the others, is linking the visible with the real. As an historical matter, there is no question that for a great number of early twentieth-century physicists this new complex of techniques did, in fact, render the atomic simultaneously visualisable and real.

Because the cloud chamber made possible such statements about the reality of protons, electrons, and later nuclei, pions, mesons and other subatomic entities, I take the material culture of cloud-chamber work to be what one might call a condition of experimental possibility. It is not the only such condition. But it did open up a class of argumentation that had not been prevalent in quite the same way before it. And it is for precisely this reason that I chose in *Image and Logic* to explore the historical circumstances that enabled the cloud chamber to be built—and then understood in the way it was. *That* is why I wanted to show how Wilson was thoroughly embedded in the Victorian world of mimetic experiments, *that* is why it was important to show his background in late 19th century Victorian photography.

Image and Logic is after the conditions (technological, socio-cultural, epistemic) that made such disputes possible. Here (in *I&L*) I wanted to capture how particular *routines* of demonstration were canonised—the establishment of technologies of the real. It is true I do not see radical contingency in the resolution of particular disputes in microphysics; it is *not* a flip of the coin whether the Z boson stands or falls. But that is because I take the conditions of demonstration to be so profoundly rooted in the material and theoretical cultures of this century.

Technologies of the real include the cloud chamber, the emulsions and the bubble chamber; but also the routinisation of "logic" demonstrations where fragmentary pieces of many tracks are assembled into arguments. Technologies of the real can also include hybrid entities, such as Dalitz plots that in the days of cosmic rays often were a visual display of *many different* experimental groups' results. And these technologies include mechanisms with no physical apparatus *per se*, such as Monte Carlo demonstrations. It may be therefore that Hughes is looking for an important topic (disputed phenomena) in the wrong book: my concern here is not with the further adumbration of experimental clashes over the reality of objects, but rather with the criss-crossed roots of the conditions under which we judge technologies to certify the real. Explicating the spatial and cultural extent of these conditions takes us from the mathematics of randomness to the radar mechanisms of finding low-flying aircraft; from stochastic methods of calculating neutron diffusion in nuclear weapons to dust surveys in Victorian England. Indeed, it is precisely the deep imbrication of microphysics in the manifold cultures of the twentieth century that leaves me so unsympathetic to the Hobson's choice between top-down or bottom-up narratives, between internal inquiries and external exposes.

So it is that when Hughes wonders if I "assume the phenomena" I would say no, I *bracket* the phenomena. My interest is in the regularisation of methods. When Hughes ask if I am committed to the "epistemological pre-eminence of microphysics," I would point to the cover of the book, a diagram of nuclear weapons' design management from the midst of the second world war. At that moment, in the midst of fierce internal battles, theorists and experimentalists were plunged into a world in which they were forced to sit as equals with chemists, engineers, and industrialists. The marks of that zone of exchange are still stamped on the fabric of science half a century later.

5. Chalmers: Intercalation and Progress

Alan Chalmers raises two important questions: 1) Does the picture of intercalated practices presented in *Image and Logic* apply where there is no sociometric separation of the subcultures? And 2) Is there a sense in which the picture of intercalated practice can be reconciled with a notion of progress?

The limits of intercalation. I am sympathetic to Chalmers' worry that there may *never* have been a completely isolated knowing subject. In fact, what struck me in writing *Image and Logic* is that the past integrity of the scientific subject was always seen as just west of the sunset, painfully lost to the present. This suggests that it is deceptive to identify "big science" with the passing of a threshold or the attainment of a particular state of affairs (budgets in the millions of dollars, personnel size in the hundreds, or organisational infrastructures that include middle management). Instead, it may be that the experience of big science—with its alternating nostalgia, triumphalism, and despair, arises at moments of change, and it is the experience of *dis*location from a technologically, epistemically, and socially stable form of research that raises hackles, alienation and anxiety. To find the periods of powerful reaction against big science we ought to look for moments when scientists are abruptly excluded from a work structure that was once theirs—in other words, we need to search for historical transitions in work structure and coordination rather than absolute states. Not complexity *per se*, but complexification.

But, Chalmers rightly continues, how ought we treat circumstances in which there is no sharp sociometric separation between subcultures—where, for example, there is no sociological distinction between those prosecuting experiment and those engaged with theory? In such circumstances, is any talk of local coordination, trading zones, and interlanguage of use? In part, the answer is historically contingent: *no* (no relevance) if clusters of practices do not carry a significant degree of autonomy from one another; *yes* if such partial autonomy obtains. If we say, for example, that Maxwell was both participating in a mathematical culture and in an empirical one, one would want to know: Is there a distinct mathematical network (via Tait, for example) that is pursuing aspects of geometry in a partially distinguishable fashion from the use of mathematics in Maxwell's exchanges with instrument makers and other experimenters?

It may be, for example, that some 19th century experimenters are borrowing from mathematicians, instrument makers and experimentalists but that the combination of these practice clusters do not take place in bi- or multi-lateral exchange, but rather within a single person. What then? Either the language coordination process "within" an individual resembles the public dynamic of coordination or not. If not, if the individual pieces together languages internally in a way wholly different from what happens between people, then we are pushed perilously close to supposing that there is a private language with all the notoriously difficult (Wittgensteinian) problems associated with such a move. Considerations like these suggest that it may prove useful to think about interlanguage coordination even where the subcultures are not entirely distinct—*if and only if* there are partially autonomous clusters of practice that are being set in coordination.

The Nature of Progress. Chalmers, invoking Gaston Bachelard, has in mind a retrospective notion of progress. That is, he supposes that the present can reformulate the past in such a way as to see present theories as overcoming obstacles that were previously blocking the path. This notion certainly seems more sensible than any single criterion that aims to capture an asymptotic approach of the sequence of physical theories towards a future, ideal state of knowledge. It also brings to mind Thomas Kuhn's distinction between "progress from," within a paradigm, which Kuhn accepted, from "progress to", which Kuhn rejected, (Kuhn 1971, p. 170). Chalmers goes on to ask: in what sense do twentieth century particle physicists—theoreticians, instrument makers, and experimentalists—see themselves as contributing to a progressive common enterprise?

I would make three points. First, even when recast into a retrospective framing of progress there are often *separate* notions of progress alluded to within the various subcultures. Accelerator physicists from the 1930s to the end of the twentieth century have quantitative narratives of progress in terms of higher energy beams from 1 MeV to a thousand MeV to a million MeV to a billion MeV; or to higher beam luminosity or to the production of ever larger numbers of particular rare particles. Theorists have other ways of recounting the progress of the field: increasing symmetries, decreasing number of fundamental entities, integration of different forces. Experimentalists sometimes refer to the increasing quantity of information stored and processed, or the ever-decreasing distance scale of the world that is accessible to inquiry.

Second, it is often local coordination of subcultures that makes possible the coordination of sequences *within* as well as *between* subcultures. Einstein, Poincaré and Abraham used the relation of their theories to high-velocity electron experiments by Kaufman to assess the relation of these various theories to one another. It is precisely the intercalation of the different clusters of practice that makes possible the construction of a retrospective vision of progress. Reliance on conceptual schemes (paradigms, programmes and the like) has led some sectors of science studies to embrace a form of relativism that has made it incomprehensible how working scientists could experience their world as continuous with the past and with other branches of science. The twentieth century quantum field theorist does not see her 'field theoretical electron' as utterly disjoint from the 'chemist's electron' next door, or Millikan electron scattered a generation earlier.

Third, even the integrated, retrospective narratives of progress have limits—points in the past beyond which the story simply begins to fall apart. Without great cracking of narrative bones, it is very hard to begin a story of the progressive unification of forces with reference to medieval natural philosophy. The reason for this, I suspect, can be understood as follows. On the picture of subcultures and trading zones, the meaning of terms are worked out in areas of high density clusters (such as the subculture of theoretical particle physics) and rarefied zones in which the interlanguage is more sparsely linked to other terms. Because the physicist and the chemist share such interlinguistic zones (in physical chemistry and chemical physics) there are passages between the meanings, though the meanings themselves are not identical. Chemistry is, in other ways, linked to biology and to many other fields both past and present. But the fact that there are links from A to B, from B to C, and from C to D does not mean that any significant common meanings *necessarily* lie between A and D. So it may be useful and historically correct to identify a host of shared meanings between medieval and late-Renaissance (Galilean) mechanics; and similarly between Galilean and mid-nineteenth century mechanics, but *not* useful to look for shared meanings and practices between Occam and Ostwald.

In short: the notion of a trading zone ought both support a notion of a retrospective, intercalated notion of progress *and* account for the limits to the reach (conceptually and socio-culturally) of any particular progressive narration of history.

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