What can particle physicists count on?

Peter Galison, *Image and Logic: A Material Culture of Microphysics*. Chicago: The University of Chicago Press, 1997. Pp. xxv + 955. US$34.95 PB.

Author's Outline

STAMPED into the physics laboratory are the imprints of the world: cloud chambers drawn from weather stations, armour plating blow-torched out of scrapped warships and bolted into spark chambers, radiation-hardened detector electronics from defence technology, x-ray film turned into cosmic-ray traps. *Image and Logic* (hereafter *I&L*) starts not with the symmetries, explanations and predictions of high theory, nor even with the great puzzles and debates of experiment: it begins with the physicality of instruments. Out of such devices it is possible to piece together the changing, contested meanings of the categories of experiment and experimenter.

My aim, in an earlier book, *How Experiments End*, was to clear historical space from the dictates of theory. I wanted to begin a discussion about experiments outside the standard periods defined by the development of theory (quantum mechanics, special relativity, general relativity, and quantum field theory). When experimentalists argued about when and where an experiment had shown something, I registered these debates as historically central—as significant as the much-studied wars between wave and particle pictures of matter. Would statistical argumentation be accepted? Could a simulation count as a demonstration? How would subgroups aggregate their conclusions into a result the group as a whole could endorse?

I&L continues *How Experiments End* and critiques it. It shifts attention again—this time not from the problem complex of high theory to that of experiment, but rather from experimental issues to the instruments and techniques that transect experimental domains. *How Experiments End* asked: how did competing groups assemble results, handle competition, and consolidate an internal consensus that an effect was real? *I&L* follows material devices *across* problem areas—it traces people and machines as they moved from radar tracking of low-flying attack planes in the Pacific to computer memories and then to particle detectors. Not: How did cloud chamber groups compete with the counter groups in identifying the penetrating component of cosmic rays? Rather: How was the cloud chamber itself situated in the Victorian material cultures of photography, mimetic experimentation, and natural history? What made this kind of pictorial demonstration possible? *I&L* seeks to establish a place for a material culture of science, one not utterly dependent on theory or experiment.

By allowing concepts, practices, and the rhythm of change in the theoretical, experimental and instrumental subcultures of physics to be intercalated, much is gained. We can make sense of how physicists often experience coherence in physics as a whole, even while registering breaks in theory at some times and experiment or instrumentation at others. That felt continuity can issue from the multiplicity of partially overlapping strands, rather than a universal reduction basis to observation or theory. But if, as I argue, the differences between these various subcultures are powerful—if they use concepts in only partially overlapping ways, if they ascribe dissimilar weights to certain forms of demonstrations, if they attend different meetings and even publish in different places—then aren't we faced with incommensurability multiplied over subcultures as well as over time? The response of *I&L* is no, for complex reasons.

What interests me is that there are sites, both literal and metaphorical, where *partially* shared concepts and materials occur: "trading zones," as I have described them. For it is well known among anthropologists that in exchange it is possible—even to be expected—that the two sides differ substantially in the meanings they associate with the exchanged objects, even when accord is reached on *what* is to be traded. In addition, there is an enormous spectrum of linguistic and symbolic elaboration that may be involved in exchange languages—from the simplest of trading jargons through pidgins to full-fledged creoles. *I&L* addresses scientific exchange through the joint consideration of anthropological linguistics and material culture: interlanguages, inter-objects and rules of combination can be "traded" across subcultures.

Several features of this trading zone are important:

1. Localisation. Local, purposeful use of objects and language can perform even vital functions despite "global" differences in usage.
2. Diachronicity. The common usage of language and objects need not be static; it is precisely of interest to see, historically, how the trading zone is expanded, contracted, or stabilised.

3. Contextuality. There is no *a priori* structure to the way interlanguages evolve, and in particular the situatedness of each "parent" language matters. Context is needed to grasp what is put in exchange, how that exchange is viewed, and how the exchange evolves.
4. Hybridity. Just as there is no absolute division of "trade" and "pure" languages (English was once a trading language) there is no radical division between inter and originary disciplines.

Illustrative of a trading zone is MIT's wartime radar laboratory (*I&L*, Chapters 4, 9). Thrust together by the necessities of global conflict, the physicists and electrical engineers could barely communicate at first. Physicists aimed to solve radar problems with their familiar techniques of 'fundamental' electromagnetic theory, but their methods foundered on the shoals of complexity. Engineers sought to use their tools from telephony and radio design, but microwaves rendered their techniques useless. Slowly, painfully, the physicists combined their conceptual objects with the algebraic-manipulative strategies of the engineers for circuit design. If the "lexical" structure came from the more powerful physicists, the "grammar" (algebra of combination) came from the engineers. What emerged from the radar lab was neither Maxwellian electrodynamics nor radio engineering. By the time peace came, the Rad Lab pidgin was recognisable as a new field: "microwave physics". In the coordination and confrontation of subcultures, the flattening, homogenising category of "collaboration" is of little use by itself. Instead, *I&L* asks: What comes from where and why? *Which* techniques are taken up and which are discarded? *Which* rules for combination of material elements circulate in the common trading zones and which remain in the constituent subcultures? These historical specificities are crucial for grasping the ways in which material culture functions inside and outside the trading zone.

As I use it in *I&L*, the term "material culture" points initially to detectors and their use as nexus-objects of exchange. As usually understood, "material" points towards a materialist history in which the concrete means of knowledge production is lodged within social history. "Culture" vectors us towards locally, provisionally shared meanings, symbols and values—in short, towards cultural history. But I reject any opposition that sets a materialist social history on one side and an idealist cultural history on the other. Instead, it is precisely by keeping in play both physicality and meaning that material culture can serve a vital role within science studies. Such a conjunction is vividly illustrated (*I&L*, Chapter 6) when the 'logic' physicists rejected Alvarez's factory-style bubble chamber: they aimed to a restore 'control' over the experimental workplace in *both* a labour-historical (Marxist) sense *and* in the epistemic (Baconian) sense. To "be an experimenter" they demanded the integrity of their work by building, running, and analysing their experiments *and* they insisted on maintaining the manipulative power over the apparatus that would give them confidence in their results. Only when both conditions of control were fulfilled did they imagine being able to restore their sense of being experimenters.

How, then, might history use the "material culture of science"? One insightful group of anthropologists defined the term as the "objectification of social being," (Criado 1995, p. 196) and their formulation helps. Interpreting instruments can supplement the textual record of results; it can outline the process by which an argument is assembled in both a social and epistemological sense. Material culture permits an interpretive history of social and epistemic traces, dinosaur footprints in hardened Jurassic mud. But this direct use of material culture is only part of the story. In *I&L*, I am also after the reverse use: what notion of experiment, what self-concept of the experimenter is presupposed by the objects? A graduate student in a massive particle physics collaboration gets a Ph.D. for a Monte Carlo simulation of a detector component that may or may not be built. Her practice is hybrid: part theory, part experiment. She herself enters a hybrid category: *she* is part theorist and part experimentalist. Such restructuring of social and professional categories, such retoolings of standard techniques are important. They show us that our histories cannot assume static categories of experiment and theory or of disciplinary identities. "Experiment" and "experimenter" are always in flux; even the term "context" shifts when experiments change landscapes, require billions of dollars, and alter industrial practices. "Context" is not a fixed large world that determines the small world of science because physics saturates much of the modern world and cannot be understood outside it

Having spent years writing a piece of work that argues for the multiplicity of ways in which shared language is used, it would be foolish for me to defend one exclusive reading of *I&L*. But as I wrote it, I had in mind three linked readings. At a first, *historical* reading, *I&L* is the story of microphysical detectors. (I used the term "microphysical" to avoid ahistorically linking the post-World War II particle physics with prewar cosmic-ray, atomic, or radiochemical concerns.) Many of its chapters begin with specific devices, and I contend that these instruments fall into two competing traditions. Once turned into a track-following instrument, the cloud chamber became a model, technically, pedagogically and epistemically for subsequent *image* detectors such as the emulsion and bubble chamber. Similarly, other physicists trained in the non-visual ways of the electrical Geiger-Mueller counter, produced a series of *logic* detectors (flash tubes, spark chambers, wire chambers) grounded directly on the techniques and evidentiary products of the counter. In short: my clustering of instruments into "image" and "logic" is based on a handing down/taking up of *training* (via pedagogical descent such as Millikan-to-Anderson-to-Glaser), *shared techniques* (for example, optics, track analysis, and photography), and characteristic *modes of demonstration* (statistical-digital versus pictorial). During the final quarter of the twentieth century, these two traditions merged at all three levels.

Schematically:

IMAGE TRADITION	LOGIC TRADITION
cloud chamber	Geiger-Mueller Counter
nuclear emulsion	spark chamber
bubble chamber	wire chamber

Aspects of this image-logic analysis extend to other domains, such as optical and radio telescopy that, while rooted in very different scientific worlds, formed a trading zone around digitised pictures.

With the image/logic argument in mind, a second, *historiographical* reading becomes possible, a picture in which science is disunified, but where that disunity is patched together through a quilt-work of locally-shared practices. This picture respects the heterogeneity of scientific work, its different periodisations, values, and standards of demonstration within (sub)cultures; it allows dynamic, rarefied boundary regions between subcultures. This second reading is offered as a set of tools, not as a single theory for writing the histories of all scientific work. It aims above all to open up room to ask questions otherwise excised from histories mired either in pure homogeneity (rational reconstruction) or disjunctive heterogeneity (block relativism).

Finally, in writing *I&L* I had in mind a third, *emblematic* reading. By this I mean the material culture of physics understood not as a case study typical of scientific method, but rather a history of physics as part of the world. Physics as caught in the ever-altering dreams of ideal knowledge, in the productive tactics of industry, and in the explosive clash of war. Material cultures of physics enter simultaneously as symbol and as symbolised: the cloud chamber appears as philosophically idealised knowledge generator *and* as Victorian craft; the bubble chamber as model for postwar science and as centralised factory. The billion dollar late twentieth-century collaboration creates models of demonstration using the web and simulations, while drawing on the structure of the multi-national corporation. Examined with care, these instruments of physics tell us a great deal about the historical trajectory of knowledge, seen across the breadth of the modern era.

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IN THIS book Peter Galison addresses an important issue highlighted by recent history, philosophy and sociology of science. Asking "How do the arguments that scientists use to establish confidence in these things emerge?" he sharpens the focus on practices whereby scientists create and establish the stuff of science: methods, techniques, instruments, languages, observations, data, interpretations, theoretical beliefs. This comprehensive study shows in detail just how much science is itself an object of the forces for change that it generates. The science in question is elementary particle physics, but much of what Galison shows here is widely applicable to modern science. Galison's instrument-centred view highlights the ways that new problems call for new techniques and concepts; how these change methods, the interdisciplinary mix of projects, patterns of communication and control, and laboratory organisation (around large international collaborations and extensive networks of computer analysis). Science is inherently fluid, active, and changing, a Heraclitan flux whose techniques, vocabulary, organisation and rules of conduct are constantly reworked. Galison shows by example that the emergence of evidence-based argumentation cannot be captured by universal logics or underlying algorithms for knowledge production. He is also at pains to show that constantly changing order is far from chaotic.

The core of the book describes the development of the image and logic traditions of experimentation, concentrating on the detectors that mediate between the production of phenomena and the production of evidence. The mimetic or image tradition developed from Charles Wilson's turn of the century cloud chamber physics that mimic atmospheric processes in the laboratory, and which led to the particle chamber. This detector called for new techniques of recording and displaying the tracks of elementary particles, culminating in nuclear emulsions and the bubble chamber. The mimetic approach prized detailed, visual information about a very few particle events. The logic approach involved detecting, classifying and counting large numbers of particle events, developing devices to replace human counters, the spark and wire chambers, and producing statistical information about whole populations of events. For several decades there were two distinct and largely independent ways of establishing the existence and properties of elementary particles: quality images or quantities of data. By the 1960s the two traditions began to converge. The time projection chamber, with the use of pulse counting and computer analysis, could generate three dimensional visual images of large numbers of events, so combining the quality of images with quantity of data.

This book defies summary in so short a review, so I will focus on a few of the many issues that I found important and stimulating. One of these is how rules of conduct relate to our notion of practice. Galison challenges the tacit knowledge gambit which sociologists have used to underwrite a view of science as an array of distinct cultures, each having rules that define what counts as phenomena, evidence, problems, solutions, and knowledge. Individuals cannot always articulate the rules they are following. Sociologists use this fact to support the relativisation of knowledge to conceptual schemes and these, in turn, to practices. However, there are no hard facts that support relativism or any other epistemological position. Galison treats inarticulateness about rules as an invitation to attend to those pieces of practice that do not yet fit the public discourse of science and to ask, Why not? Where others have seen only the differences that define incommensurate experimental cultures, Galison provides both a careful delineation of each (based on continuities of pedagogical methods, technical skills and demonstrative styles) and plenty of evidence about communication and 'trading' between the different disciplines and specialisms within each of the two traditions. It is this sort of inter-field work that made the major innovation—the convergence of image and logic technologies—possible. Instead of appeals to the irreducibly social basis of the acceptance of linguistic and material innovations we are shown how new concepts, techniques, and technologies of making and communicating evidence develop together. Frontier work calls for making do, inventing new rules to deal with unexpected situations. The rules of the game are always emerging and are often unclear. Galison's study has much to say about how it is possible play a game in which the rules are not merely negotiable, but always changing.

The rise and merger of the image and logic traditions has significance far beyond the history of particle physics. The contrast reflects what I take to be two basic modes of human experience (sensory experience, counting) and of thought (visualisation, classification and symbol manipulation). Having dispensed with underlying logics of method and argumentation in favour of the diversity and specificity of practices Galison would, I suspect, resist the suggestion that his history can be interpreted in terms of universal cognitive propensities or structures. Since cognitive science has until recently approached mental processes as if they are algorithms run in brains working in splendid isolation from other brains, I would agree. Nevertheless, studies as rich as his offer a challenge to cognitive science. Instead of positing cognitive structures needed for a discredited algorithmic view of science, 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.

The history described in this book is part of a larger, profound change, which these scientific developments have helped to accelerate. Humans are naturally analogue devices, capable of interpreting experience without recourse to sorting and counting. The history of science can be described as a process of digitalisation, where digitalisation is a process of extending human counting capabilities through the creation of concepts, categories and the procedures that enable classification, manipulation and data analysis. It is inherently practical and technological, depending on devices as well as abstract concepts such as number and rigour. Galison shows how, in the logic tradition, digitalisation was repeated for each of the steps needed to display as published histograms the effects of particle interactions inside large complex machines. Scanning, measuring, reconstructing tracks, kinematic analysis of tracks and experiment analysis were digitised so as to be automated. Error reduction was also an objective, just as it had been for Charles Babbage's calculating and difference engines.

Digitisation is what most non-scientists have in mind when asked what distinguishes science from other human activities. It involves displacing human (analogue) interpretations and error-prone procedures by numerical description and exactly repeatable (digital) procedures, and a shift from relatively unmediated interaction with a world of natural, elemental forces (via the development of particle and bubble chamber science as means for re-creating nature in laboratory experiments), to a world in which 'natural' phenomena are completely replaced by representations. Computer simulations, whether of physical processes or experimental data, are representations in this sense—material to work on.

Human experience has been shaped by elemental forces very different from those investigated by microphysics. Nevertheless, microphysics has far more continuity with earlier and other sciences than Galison recognises. He is right to attack the monolithic notion of experiment as a durable method bequeathed to modern science by the new science of Boyle, Galileo and others. It is a symptom of the self-transforming character of science that the nature of experiment is constantly in flux. Yet I am not convinced that the central role of computer simulations is a profoundly novel departure. True, these are unlike most new scientific instruments in that they ceased to be substitutes for mechanical devices. Simulations are neither experimental machines nor theoretical apparatus, yet computer methods became central to the production of images, counts and the logic of images and counts underlying scientific knowledge about elementary particles.

To me, their significance is that they enabled the convergence of sensory and numerical modes of representing nature that physics had formerly treated as quite distinct. Nor was this confined to physics: the technologies and methods of visualising counts were readily applied in other fields, for example, to the analysis of numerical data about field strengths in basaltic ocean floors. These methods produced both images and plots. Some geophysicists rejected these at first because they did not resemble the originating data (just as some had denied that Wilson's cloud chamber phenomena could be treated as evidence about real particles), yet the images became 'crucial' when it was realised that they displayed new properties, in particular, symmetries for which only one explanation —sea floor spreading—seemed plausible.

Technologies have always been central to the production of knowledge; so have the construction and manipulation of representations. When we consider the power of thought experimentation, the shift from using real experimental data (whether in analogue or digital form) to simulated data seems less dramatic. Thought experiments are a form of mental simulation made possible by lived experience as well as by abstraction and the imposition of logical or mathematical discipline. This most accessible form of argumentation merges images and procedures in the idealised physical processes of an experiment. Simulations are not identical to thought experiments, but the similarities indicate how heavily represented the scientist's world had become already by the end of the seventeenth century.

Where microphysical practices have led, the availability of cheap computing power allows the rest of us to follow. Many people now live in a world in which description, communication and methods of manipulation increasingly depend on the computable processes developed to enable microphysical practices. I am thinking not just of DARPA and the internet or of CERN and the world wide web, but also of the acceptance of computer simulation methods in other sciences and their applications in communications, policy making and entertainment. Some of the technologies that are transforming our culture originate in the techniques that form a key strand of Galison's story.

Galison asks whether experimental physics can be conducted solely with simulated data. This is like asking whether theoretical science can be conducted solely with thought experiments. My answer is that it can, but only up to a point. Locating that point raises profound issues about real and represented worlds which *I&L* also addresses. Galison stresses the importance of understanding the significance of changes in the scale of experimentation and data analysis, of the industrialisation of research, of the shift from individual investigators leading teams to experiments controlled by those analysing the data. He argues that administrative control over working practices and manipulative control over natural processes are inextricably linked aspects of objectivity (p. 430). Underlying all this is the recognition that confidence in evidence is closely related to an evaluation of how directly a phenomena-producing method interacts with objects and processes in nature. During the past three decades, practitioners as diverse as Eugene Ferguson (an engineer) and Harry Kroto (chemist and nobel laureate) have insisted that science education should not be so dependent on computer simulations. They fear that scientists will lose the ability to apply commonsense knowledge about how the world is. Why is this necessary for science? After all, microphysical practices are not reducible to knowledge of ordinary commonsense world, long since banished since primary qualities displaced ordinary, sensory experience (not to mention the use of thought experiments to show how misleading common sense can be).

Nevertheless, it appears that shared experience is necessary for the confidence scientists come to have in the microphysical manipulations that are the subject of this book. This is why in his final chapter Galison defends an historicised neo-Kantian view of science in relation to its objects. I concur with his attempt to escape from the dualistic assumptions held, implicitly, by many science studies writers. This dualism leads us to misunderstand the centrality of practice to the construction of representations. The attempt to exchange information between different cultures of practice leads to new ways of manipulating and understanding the world. The possibility of comparing schemes presupposes considerable agreement, in terms of which differences may be described. Relativism lost sight of this commonality; constructivist studies have retrieved it through the rediscovery of culture—especially the material culture of science. As Donald Davidson argued, we need to lose the world invoked by conceptual schemes, yet the commonplace world of shared experience must not be lost. In following Davidson's appeal to the epistemic priority of "unmediated touch with the familiar objects whose antics make our sentences and opinions true or false" (p. 841) Galison invokes the argument of Hacking's *Representing and Intervening*. Everyday knowledge of a world, expressed through shared representations which work so well that we rarely need to challenge or change them, shapes the approach that scientists take to the invention and proving of both instruments and of investigative techniques. Hacking's argument for three types of grounds for believing in instruments (recognition of similarity, effects of intervention, and physical understanding) comes to rest on knowing the world in ways that predate primary qualities or Galilean thought experiments. The study of dispositions to believe that are based on other beliefs formed through shared experience is the province of cognitive, social and developmental psychology.

I contend that the last of the great dichotomies—the dualism of worlds and representations—may be dispensed with by paying more attention to the cognitive function of digitisation as well as to the more visible, social processes of communication, rather than by philosophical arguments about the nature and status of language. Philosophers generally take the descriptive and referring role of language to be its primary function in science. This position imports dualistic assumptions into the discussion (as Galison notes of Putnam), instead of focusing attention to how languages are actually used in the construction of new knowledge. Galison's study of pidgins and creoles has much to teach philosophers here. He shows that the descriptive function of language is in fact accomplished by scientific work, that is to say, description emerges from constructive activity. The capacity to describe implies some consensus about what is the case and is the outcome of a wide range of different activities. More important still, the referential function of language is derivative in that it is involved in and dependent on these other activities. So, the language of a scientific domain is not first created and then applied to what it describes. That language cannot be divorced from what it describes in the way that dualism prescribes because—as shown by Galison's examples of communication between groups—what really matters is what languages allow people to do.

As for the displacement of hands-on, bench-top manipulation by experience of virtual or simulated worlds, and of experimental data by Monte Carlo simulations of data, it is more likely that our concept of 'how the world is' will change. This has already happened in the case of thought experiments, where some philosophers argue that the convincingness of the mental enactment proves that reality is ultimately mathematical. This sort of argument fails to recognise the specificity of thought experimental practices or their dependence on the commonplace world of familiar, shared experience. Galison's book shows how observational technologies alter the character of human experience in ways that undermine just such attempts to argue from styles of argumentation associated with particular representational practices (thought experiments, computer simulations) to a universal epistemology of science. Here is another reason why this book should be well and widely read.

There is no elusive, stable set of timeless rules whereby we access a world beyond particular practices. The important message, elegantly expressed and extensively documented, is that though the sun has set on the timeless algorithmic view of the relations of instruments, theories and experiments, it is "a sign of vibrant life, not fragility, that the material culture of the laboratory is in flux through changing modes of collaboration, techniques, simulations, and disciplinary alliances" (p. 838). Galison's image of microphysics as a constant flux of changing practices, rules, alliances, boundaries and languages suggests that microphysical science is very like the subatomic world that it purports to be about.

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By William J. McKinney

Introduction

TO MAINTAIN that modern science employs technology is to make a claim so obvious that one might question the value of stating it at all. From Boyle's air pump to Leuwenhoek's microscope, and beyond to scanning tunnelling microscopes and Monte Carlo computer simulations, modern scientific knowledge has, in one way or another, been linked to laboratory technology. To say that science is, however, "technological" is another matter. In Peter Galison's latest contribution to the historical study of scientific experimentation, *I&L*, we find just such a claim in a book both sweeping in its scope and finely tuned in its attention to laboratory detail. As the follow-up to his *How Experiments End*, Galison's latest work is not only an important addition to the historical study of scientific experiments, but it is also so brimming with new philosophical insights that it will most certainly provide grist for the scholarly mills of historians, philosophers and sociologists of science well into the next century.

I maintain that Galison's central thesis, that there exists a "trading zone" in the laboratory where theory meets experimental practice in the production of data, offers us a striking alternative to the scepticism of contemporary cultural studies of science. Galison has achieved what no scholar heretofore has accomplished in any kind of detail. He has discovered a stable foothold upon the slippery slope which exists between the old positivist program, with its attention to, and almost unquestioning faith in, the authority of experimental data, and the almost Feyerabendian epistemic anarchy of what Galison calls the anti-positivist movement, with its focus on socio-cultural interests and theoretical and historical contexts. Galison, perhaps once and for all, shatters the distinction between the "evidence model" of the positivists and their intellectual heirs (see, for example, Franklin 1986, 1990, 1993) and the "interest model" of the strong programme and the cultural studies of science movement (see, for example, Pickering 1984a and 1984b).

> Lost in such dichotomies is a sense that what holds physics together is neither a single, unified, deductive or inductive apparatus, nor a Potemkin village of rationality hiding the raw exertion of competing interests. Physics is a complicated patchwork of highly structured pieces: instrument makers thoroughly versed in the manipulation of gases, liquids and circuitry; theorists concerned with the coherence, self-consistency, and calculability of the behavior of matter in their representation of matter most finely divided; and experimenters drawing together instruments into combinations in pursuit of novel effects, more precisely measured quantities, or even null results. (p. xx)

To be fair, Galison does not wish to universalise his results in order to make grand pronouncements upon science in general. Consider, for instance, his historical disclaimer regarding the construction of cloud chambers.

> This book is a brief for *mesoscopic history*, history claiming a scope intermediate between the macroscopic (universalizing) history that would make the cloud chamber illustrative of all instruments in all times and places and the microscopic (nominalistic) history that would make Wilson's cloud chamber no more than one instrument among the barnloads of objects that populated the Cavendish Laboratory during this century. (p. 61, italics in original)

Therein lies the primary virtue of this work. While recognising that the instruments studied in each chapter are "dense with meaning" (p. 2) and embody "strategies of demonstration" (p. 2), Galison refuses to argue that his historical data apply beyond their context, while also recognising the profound epistemic consequences of viewing scientific knowledge as embodied within such laboratory contexts. The next section of this essay will outline Galison's argument for the existence of laboratory "trading zones" between theory and experiment, a means by which Galison will argue that in spite of the disunity of science, science remains epistemically vital. Then the last section will discuss the broader consequences of *I&L* for the philosophy of science, arguing that, by taking technology seriously, we can, by taking Galison's lead, open new vistas upon our understanding of science and its claims of epistemic authority.

Galison's Thesis

I take it that at least one question which motivated Galison's ambitious study is, quite simply, "What makes science believable?". With ease and meticulous attention to historical and philosophical detail, Galison examines the positivist, post-positivist and anti-positivist reactions to this question, tracing our meta-scientific endeavour from the unified science project of the logical positivists to the contemporary anti-positivist movement. In the end, Galison maintains that science is indeed believable, but paradoxically maintains that it is in its disunity that we find reason for belief. Consider the following claim.

> This is a book about the machines of physics. Out of the experimental apparatus come the delicate track images that have launched, backed, and challenged the abstractions of unified field theories—pictures that, as symbols of science, have graced the covers of a hundred books. My goal is not to begin with the tracks and position them within arguments for great experimental discoveries, such as those of the psi, the omega minus or the positron. Nor is my goal to retell, once again, the long march of theories of matter from atoms to quarks. Instead, I want to expose the practice presupposed by these images, to peer into all that grubby, unplatonic equipment that lies such a long way from Lie algebras and state vectors. (p. xvii)

From such seeming disunity, Galison paints a robust alternative to models depicting science as theory-laden, interest-infected and culturally determined. Alternatively, Galison maintains that the admitted disunity of science is a virtue, for herein we find the epistemically crucial trading zone.

Consider but one of Galison's voluminous historical studies as an example. The introduction of bubble chambers into the physics laboratory in the 1950s and 1960s highlights a clear case of science's disunity. Science became, in Galison's words, "industrial grade." In the immediate post-World War II decades, physics changed from an enterprise dominated by solitary physicists or teams of physicists, to one populated by engineers, technicians, machinists, computers, detectors and, yes, physicists.

> Into this protected reserve of the inner laboratory—the relatively quiet microecological niche of the physicist—slammed the massive bubble chambers. There was no separate sphere for the individual worker.... By the 1960s, the total number of people involved in running experiments on this machine reached a hundred or so, divided into a wide assortment of semi-autonomous subgroups. Specialists devised software and hardware for data reduction; engineers handled aspects of safety, design and construction; lay scanners encoded raw data into Dalitz and effective mass plots.... Changes had occurred in almost every respect, from the kinds of physics questions being asked to the instruments and work structure that shaped routine tasks. Closely allied to these developments were fundamental debates over the nature of experimentation. (p. 316)

It is within such disunity, between the clashing cultures of scientists, engineers and technicians that we find, in Galison's terms, the "intercalated periodization" of scientific change. It is not the case that theory changes against the foundation of an unchanging empirical base (the positivist claims), nor is it the case that each change in theory brings in its own set of empirical claims (the anti-positivist claims). Rather, theory changes along its own time-line, experiment changes along its own time-line and instrumentation changes along its own time-line.

And consider the consequences of such a claim. To remove science from its empirical foundation has long been thought to be the first step toward an unsavoury scientific relativism where knowledge claims are so massively theory-laden that experimental evidence is seen as merely epiphenomenal. There would exist no theory-independent observations for, as Pickering argues in his important *Constructing Quarks* "Each phenomenological world was, then, part of a self-contained, selfreferential package of theoretical and experimental practice" (Pickering 1984b, p. 411). This is a succinct statement of Pickering's "theory-experiment symbiosis," the claim that theory requires cooperating experiment to survive, and *vice versa*. This is an extension of Kuhnian incommensurability wherein competing theories cannot be judged outside of their contexts, and hence science does not possess a theory-independent arbiter for theoretical disputes. Yet, Galison goes beyond Pickering, complicates the relationship between theory and experiment by adding a third independent variable—instrumentation—and argues that it is in the intercalation of these three semi-autonomous cultures that science is epistemically robust. There is no independent stream of unchanging experimental data upon which changing theory rides, that is to be sure (see Galison's illuminating Section 9.2). But, borrowing from Hacking, Galison's historical studies show us that experiment does indeed have a life of its own, and that experimental techniques are often robust in the light of theory change. However, Galison also shows us how theory can be robust in the light of changes in experimental technique, how instrumentation can remain robust in the light of experimental or theoretical changes and, in short, how the production of microphysical knowledge depends upon the interactions of these three semi-autonomous cultures.

Kuhnian incommensurability objections notwithstanding, Galison borrows from the cultural anthropologist and maintains that just as any two dissimilar cultures often interact through trade, so too do the dissimilar cultures of microphysics.

> The anthropological picture is relevant here. For in focusing on local coordination, rather than global meaning, one can understand the way engineers, experimenters, and theorists interact. At last I come to the connection between place, exchange, and knowledge production. But instead of looking at laboratories simply as the places at which experimental information and strategies are generated, my concern is with the site—partly symbolic and partly spatial—at which the local coordination between belief and actions takes place. It is a domain I call the trading zone. (pp. 783–784)

Thus, Galison finds within the material culture of microphysics the evidence of the trading zone. His discussion of laboratory buildings in Chapter 9 is most illuminating: "I suspect, therefore, that in the floor plans we are seeing far more than pragmatically situated air ducts; we are witnessing a physicalised architecture of knowledge" (p. 785). Galison posits the science buildings at Brandeis University and the University of Virginia (both *ca.* 1961) as examples, illustrating the material embodiment of old positivist beliefs in strict disciplinary separation and the distinction between theory and experiment. What follows is a detailed discussion of the construction of various physics laboratories from the Fermilab facility to the Stanford Linear Accelerator to the MIT Radiation Laboratory.

Yet, within these structures, Galison maintains, material and intellectual trading zones exist. "I intend the term 'trading zone' to be taken seriously, as a social, material, and intellectual mortar binding together the disunified traditions of experimenting, theorising and instrument building" (p. 803). From the "joint Experimental-Theoretical Seminars" planned in the initial days at Fermilab (p. 829) to the material and intellectual "people interactive zone" of the aborted SSC project (pp. 830-831), Galison shows how the disunity of science becomes a strength.

> It is the *disorder* of the scientific community—the laminated, finite, partially independent strata supporting one another; it is the *dis*unification of science—the intercalation of *different* patterns of argument—that is responsible for its strength and coherence. It is an intercalation that extends even further down—even within the stratum of instruments we have seen mimetic and analytic traditions as separate and then combining, image and logic competing and then merging. So too could we see divisions within theory, for example—as one incompletely overlapped the other. (p. 844)

Quite simply, Galison shows us that scientific knowledge is constructed. His evidence includes the vestiges of that construction as embodied in the material of microphysics—its buildings, work spaces, instruments, computer simulations and the like. To be sure, the trading zone is a social place. Galison's detailed justification for his anthropological approach (Section 1.6) acknowledges the profound social character of the trading zone. Yet, physics emerges as more than just the product of social context. Physical knowledge emerges as a technological construction in *I&L*, and it is toward this crucial insight that I turn now.

Concluding Discussion

Philosophers of science have typically ignored the material dimension of scientific knowledge production, and in spite of the last two decades' experimental turn (in addition to works by Franklin and Pickering, see also Hacking 1983 and Ackermann 1985), few if any have chosen to view instrumentation as a material phenomenon which embodies theoretical presuppositions and experimental practice. *I&L* is a notable exception, and offers philosophers of science a challenge. The challenge is to recognise the fact that the material culture of science is a map of theoretical, experimental and instrumental commitments which, in Galison's trading zone, combine to construct scientific knowledge in a technological sense.

Galison illustrates that philosophers of science have been content to focus on the linguistic side of science, almost exclusively.

> The positivist and antipositivist periodizations have a grandeur to them: they both sought and found a single narrative line that would sustain the whole of science—in observation for the positivists and in theory for the antipositivists. Both agreed that language was the linchpin of science—though the positivists looked for a language of experience, and the antipositivists located the key in terms of theory. (p. 793)

Such a focus would blind us to the existence of the trading zone, particularly its material dimensions. Thus, as Galison counsels, the time has come to focus upon scientific practice, not simply scientific discourse. I maintain, then, that the challenge for philosophers of science is to reconceptualise science in the light of works such as Galison's and an examination of the philosophy of technology.

That Anglo-American philosophy of science and continental philosophy of technology have little to nothing in common with regard to journals, professional societies and philosophical style is testament to the fact that, at least philosophically, science is thought to *use* technology, but it is certainly not *technological.* Technology may be scientific, but the converse is generally not regarded as true. Galison's historical studies paint a different picture of science, one where science is embedded in technology and where science is, indeed, technological in its very essence.

Consider the need to sort through the voluminous data of early bubble chamber experiments. On one side stood Luis Alvarez and his contention that human beings, with their keen capacity to recognise patterns, must remain central to the data gathering process. On the other stood Lew Kowarski, an early proponent of automated data processing. In discussing these two "reading regimens," Galison notes that, "Embodied in the technology of reading is a social order of the workplace, an epistemological stance toward discovery, and a vision of the relationship between physics and the engineering arts" (pp. 371–372). Ultimately, of course, such data analysis was automated, since by the mid-1960s track measuring reading machines staffed by human observers would require outrageous labour commitments in order to analyse the millions of data points generated annually. Nonetheless, the recognition is clear—instrumentation embodies certain commitments in the theoretical and experimental scientific subcultures. A more robust philosophy of experiment must take seriously science's technological nature.

Ihde presents a clear and succinct account of the relationship between science and technology which paves the way for a new philosophy of experiment: "[h]ere the focus will be upon the interface of the philosophy of science and of technology by way of looking at the *embodiment of science in technology*" (Ihde 1991, p. 67). Philosophers of technology and of experiment agree that technology focuses, and indeed limits, the scientist's attention to whatever the particular instrument was designed and built to reveal. Consider the two quotations below.

> To experiment is to create, produce, refine and stabilize phenomena.... That is why I spoke of creating and not merely discovering phenomena. (Hacking 1983, p. 230)

> Modern physics is not experimental physics because it applies apparatus to the questioning of nature. Rather, the reverse is true. Because physics, indeed already as pure theory, sets nature up to exhibit itself as a coherence of forces calculable in advance, it therefore orders its experiments precisely for the purpose of asking whether and how nature reports itself when set up in this way. (Heidegger 1977, p. 21)

Note the similarities between the above quotations and, for example, Pickering's (1984a) arguments that weak neutral currents were constructed, rather than discovered, to further the interests of the high energy physics community. By recognising, however, the technological, rather than the social, construction of scientific knowledge, the philosophers can gain a more rich understanding of experimentation than offered by either the positivists and their intellectual heirs or the strong programme (see McKinney 1995) for a detailed articulation of this thesis).

Galison's *I&L* offers us a detailed lesson in understanding just how deeply technological science really is. Technology embodies the various levels of theory, whether they be pre-theoretic assumptions or more fully developed hypotheses, and *constructs* phenomena. Science, then, emerges as technological, and its theories and data emerge as technological products. Yet, rather than skidding down the slippery slope to epistemic relativism, Galison maintains that it is precisely because of science's disunity, its social negotiations and its striking discontinuities, that science is robust. This is not to say that Galison is a naive metaphysical realist. His neo-Kantian realism (see Section 9.9) offers us a pragmatic realist's justification for a science whose results are technologically and socially constructed. This is a most valuable study in the history, philosophy and sociology of science, and for that, Peter Galison deserves our collective thanks.

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By Harry M. Marks

THE history of modern physics is, for Peter Galison, a history of devices—the instruments physicists use to produce, 'observe,' measure and interpret experimental phenomena. Physicists, he argues, are attached to particular instrumental traditions: attachments formed by training, by sensibility and ideology, and by the more mundane materialities of funding and institutional identities. Galison's is unmistakably a material history, in the Marxian sense, which reveals a hidden history of production: the production of electrons, protons and charmed particles, but also the production of social relations, among physicists, between physicists and data analysts, physicists and the engineers who design their instruments. The spirit is Marxian, rather than Marxist. Marx's analyses of capital's self-expansion play no role here, and the rigid determinism of the late Marx is also lacking. Rather, Galison's approach resembles that of the economist Joseph Schumpeter insofar as he offers a dynamic history in which the technical capacities of new experimental technologies can turn the current generation's customary practices—their ways of doing physics—into obsolete relics, machines and men consigned to the dustbins of history (I exaggerate only slightly).

To be more specific, Galison tells the story of two instrumental traditions. First is the 'image' tradition, initiated by the cloud chamber of British physicist C.T.R. Wilson, in which atomic and sub-atomic events leave a visual trace in the detector. Galison traces the history of the image tradition, from Wilson's *fin de siècle* instrument, through the post World War II invention of the bubble chamber, to Louis Alvarez's transformations of the bubble chamber at the Lawrence Berkeley Laboratory (LBL) in the 1960s. This is a history joined by personal connections. The American Robert Millikan taught himself Wilson's cloud chamber technique and passed it on to Carl Anderson, who "then instructed

Donald Glaser, the inventor of the bubble chamber"; it is Glaser's students who end up staffing Alvarez's labs. Along with know-how, physicists pass along an aesthetic, a preference for visualising 'the golden event'—a particular trace which marks the appearance of a particular sub-nuclear event. But no matter how powerful the elective affinities between a given group of physicists and an instrumental tradition, the social lives of instrumental communities are constantly being transformed by technological innovation. Thus, the cloud chambers of cosmic ray physicists, in which events are directly observed, gives way to an instrumental tradition in which events are recorded on film, film in turn then scanned by dozens of non-physicist labourers. The social relations of this 'emulsion' tradition in turn are replicated in Alvarez's bubble chambers of the 1960s and 1970s, with literally hundreds of workers involved in a complex division of (alienated) labour, in which machine design, machine operation, the recording and analysis of experimental data and the interpretation of experimental results are increasingly the specialised tasks of distinct groups, of varying skill levels and training.

The second instrumental tradition Galison describes is the logic tradition, best captured for non-physicists like myself by the Geiger counter. Like the image tradition, the logic tradition has a history of socialised practices and transforming innovation, albeit one with a few more twists and turns than the image tradition. The final quarter of the book deals with the joining of these two traditions in the Time Projection Chambers (TPC) of the 1980s, and concludes with the tradition represented by an experimental-less physics, the physics of computer-simulated experiments.

It is faint praise to say that Galison provides the most fully realised picture we have of any experimental science, a rich analysis of how the experimental communities of modern physics operate both epistemically and institutionally. One of the book's points is that the intellectual life of an experimental community cannot be understood apart from its instrumental traditions and the social relations they give rise to. Galison has absorbed the lessons offered by historians of technology Edwin Layton and Walter Vincenti about the role of technical traditions in shaping new technologies. He surpasses them (and other historians of scientific instruments) in the level of detail and the subtlety of argument he provides. There are lessons, both empirical and methodological, on virtually every page, for which my rough plot summary above does not do justice. As Galison notes, a crude notion like Big Science serves the historian of postwar physics about as well as the notion "big building serves the architectural historian" (p. 553). Alvarez's lab in the 1960s was not the same as the LBL two decades later. The scale of operations and the extent of the division of labour differed, with implications as to how experiments were designed and performed, results recorded, data analysed and interpreted, and articles written.

There can be no single plot for such an intricate and lengthy narrative, but there is surely one governing theme: the progressive alienation of physicists from the work of observation and interpretation, an alienation produced by a social division of labour which increases over time. Cloud chamber work was Robinson Crusoe-ish, a solitary physicist, perhaps with a mate or two, doing the observing. Physicists at post World War II emulsion chambers hired women technicians to scan photographs of subnuclear events. But the human eye was unable to keep up with the increasing data output from Alvarez's bubble-chamber machines; physicists had little choice but to turn to computers to aid their quest for the golden event. The ultimate stage of alienation was reached by the 1970s, when an increasing scale of production made it impossible to tell who had conceived the experiments, or who was responsible for designing the experimental apparatus. Faced with a division of labour operating on an international scale, traditional notions of authorship or of the investigator disintegrate. Modern physics is ineluctably a corporate enterprise, and physicists are workers on an equal social footing with the engineers who once worked for them.

'History' figures in two ways in Galison's account. The first is history in the ontological sense, of change over time and of difference in time. Galison gives as good an account as we are likely to get of the ways in which this history shapes physics. Like a good Marxian, Galison's history pays due account to both structure and contingency. Structure is produced by the experimental/instrumental cultures, and the institutional commitments they produce. Change occurs through the technological agency of new instruments which will produce new classes of phenomena or (in some cases) simply produce more phenomena more quickly. Nor are all outcomes determined: contingency plays an important role here, in the horrific explosion of liquid hydrogen at the Cambridge Electron Accelerator, which transformed both accelerator design and design management in its aftermath, and in the poignant tale of Maria Blau, the Viennese refugee physicist whose technical and intellectual accomplishments were never translated into material security, much less success (unlike many of her male collaborators).

Andrew Pickering has suggested, in the *Mangle of Practice* (pp. 205–208), that historical accounts such as Galison's suffer from a retrospective fallacy, being able to distinguish structure from contingency only after the fact. As a card-carrying historian, I'm not an impartial judge. The flaws of some structural accounts are well-known: determinist, globalising, teleological. Nonetheless, the value of these distinctions remains. Some historical processes, for example, family size and birth order, change more slowly than others, such as "who is president". And it is a long way from "nationalist tensions and imperial rivalries brought about armed conflict on a world scale in the early twentieth century" (structure) to "a Serbian nationalist assassinated Archduke Franz Ferdinand of Austria on June 28, 1914" (contingency). Until microsociology can hand us a method for handling these diverse phenomena, the first test of such distinctions should be empirical, rather than methodological.

Here 'history' enters into Galison's narrative in a second, more conventional sense: the stuff of political events, of the Second World War, the Cold War, and the militarisation of the science: "Without the fast timing devices, cryogenics, pulse-forming circuitry, and computer systems developed for the military industry, neither the bubble chamber nor the more advanced spark chambers would have amounted in the 1950s to more than curious prototypes for accelerator physics" (p. 551). Galison can hardly be accused of neglecting the world outside the laboratory walls. Yet I was struck by two things about the way the world 'outside' enters in. First is that, although we hear about the priorities of physicists, computer designers, etc. in addressing their military patrons, we don't really see how and why the military patrons viewed their scientific clients. In contrast to Harvey Sapolsky's work on the Office of Naval Research, Galison has little interest in the inter-service and intra-service rivalries which produced such great rewards for post-war science. Galison's accounts of military patrons and scientist clients are not symmetrical, and the result is that while I always understand the physicists' desires, I don't always understand why they were fulfilled. This is especially true for the discussion of the TPC in the 1970s: just how and why anyone in government agreed to finance this venture, which rivals Teller's later Star Wars venture in its technological conceits and Rashomon in its ever-changing incarnations, remains unclear. (It is easier to fill in the military-government side for the earlier period thanks to the work of folks like Robert Seidel and Stuart Leslie).

My second reservation has to do with the way Galison allows history (namely, the outside world of politics and production) to enter into his narrative. That is, we hear much more of politics (military funding) and dependence on corporate technology (Kodak films) in the post-World War II period than we do in the discussions of Wilson's late nineteenth century cloud chambers. Yet I suspect that the meteorological tradition Galison's describes was as dependent on mid-nineteenth century innovations in printing technology as were his troubled film detectors of the mid-twentieth century, a technology made obsolescent, Galison argues, in part because physicists no longer controlled its production. Yet to bring the outside world into the laboratory in the nineteenth century would be to disrupt the story of progressively alienated labour which forms one of the *leitmotifs* of Galison's narrative. As Daniel Kevles has observed in his review, Galison is not a romantic. Yet, as with the older Marx, there may be expressions of a younger romantic lurking somewhere beneath the surface of this tale.

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By Jeff Hughes

IN THE summer and autumn of 1932, radiologists in hospitals all over the world suddenly found themselves bombarded with requests for old radium therapy tubes. Their petitioners were nuclear physicists from laboratories in Europe and the United States who wanted to extract polonium, a radioactive decay product of radon, from the tubes for use in nuclear experiments. The previous few years had seen a fundamental change in the armamentarium of nuclear physics, involving a shift away from the optical scintillation method of counting sub-atomic fragments towards the use of electronic techniques such as Geiger counters and amplifiers. It was using these new techniques to detect fragments produced in the disintegration of beryllium nuclei by alpha-particles from polonium that James Chadwick of Cambridge's Cavendish Laboratory produced evidence for the existence of a new nuclear constituent, the neutron, in February 1932. Publication of the claim led to a frenzy of work as experimental physicists sought to replicate Chadwick's work and to learn more about the putative new particle, while theoreticians sought to include it in their mathematical models of the nucleus and nuclear processes.

For the experimentalists, reproducing Chadwick's work in the summer of 1932 demanded the use of significant quantities of hard-to-obtain (and literally priceless) polonium, as well as particular kinds of electronic valves and ancillary counting equipment. Aspiring neutroneer Merle Tuve of the Carnegie Institution in Washington desperately sought supplies of polonium, telling one of his correspondents that "if I want to equal Chadwick's source I need to dun all of my friends for radon tubes". Similarly, when his team tried to build a linear amplifier of the kind constructed for Chadwick at the Cavendish by C.E. Wynn-Williams (who readily provided circuit diagrams and technical information), they found themselves stymied by their inability to obtain the particular kind of valve needed for the output stage of the amplifier and the lack of any suitable American equivalent. In the end, they were forced to rely on "the good graces of radio amateurs, especially those residing in seaports" in order to obtain the material bases to produce and study neutrons in the laboratory. Making neutrons and becoming a player in the exciting new field of nuclear physics demanded the precise reproduction of a particular material culture and particular ways of manipulating it.

The story could be repeated for almost every laboratory which sought to enter the field of nuclear research in the early 1930s. In its demonstration of the intimate connections between apparatus and materials, the organisation of laboratory work, the role of inter-laboratory communication and competition, and the creation and circulation of credible new facts about the natural world, this brief episode is also typical of much of the rest of microphysics before and since 1932. Not least because physicists themselves have always been acutely aware of the importance of materials and practices, the significance of material culture to our understanding of the processes and products of twentieth century physics (and indeed of natural science in general) can hardly be overestimated. It is therefore a great pleasure to welcome Peter Galison's long-awaited analysis of the material culture of twentieth century physics. Both as an overview of the development of microphysics and as an analysis of its material and evidential foundations, *I&L* is a *tour de force*. Against the theory-centred account of twentieth century physics which has characterised most histories of the subject to date, Galison has given us a richer, much more complex experiment-centred story in which glassware, photographic plates, valves, machines, laboratory organisation and human interactions take centre stage. The book will undoubtedly have a significant impact on the history, philosophy and sociology of physics and of twentieth century science more broadly because it stakes out a new philosophical and historiographical position in an increasingly contentious field. It is an impressive new benchmark.

Galison wants to understand "how pictures and counts got to be the bottom line data of physics". He achieves this by exploring

> the blown glass of the early cloud chambers and the oozing noodles of wet nuclear emulsion; the insistent hiss of venting nitrogen gas from the liquefiers of a bubble chamber; the resounding crack of a high-voltage spark arcing across a high-tension chamber and leaving the lab stinking of ozone; the

> silent, darkened room, with row after row of scanners sliding trackballs across projected bubble chamber images; the late-night computer screens flashing with the skeined complexity of rotating and disappearing tracks; the one remaining iridescent purple line cutting across the background of a terminal (p. xvii).

In so doing, the book also traverses the changing work structure of twentieth century physics, from the solitary experimentalist of the early years to the huge collaborative ventures of the recent past. Across the century, microphysics changed from an individual pursuit to a cottage industry to a corporate manufacturing concern to a multinational enterprise. With increasing industrialisation and specialisation, the notions of experiment, experimenter and scientific authorship underwent considerable change, occasioning considerable anxiety as traditional patterns of craft work were increasingly displaced by organisational models derived from industry and the military. And as the machines, teams and costs got bigger, the particles got smaller. The path from the cloud chamber and the Cavendish Laboratory to the Time Projection Chamber and SLAC is also the path from the electron and ion physics to the W/Z and the Standard Model. In almost every sense, microphysics at the end of the century was a radically different enterprise than it had been at the beginning.

Whence, then, its continuity? Rejecting both the positivist (Vienna Circle) and anti-positivist (Kuhn, early Pickering) block-periodisations of scientific development, Galison lays some philosophical ghosts firmly to rest and gives the theory-driven historiography of past physicist-historians a decent burial into the bargain. In their place he elaborates a much more multi-dimensional and fine-grained account which seeks to explain how microphysics has developed and retained its disciplinary stability through the interaction of relatively discrete instrumental, experimental and theoretical traditions or subcultures, each with its own particular skills, techniques, practical epistemologies and forms of organisation. At the points where these local cultures intersect, 'trading zones' can emerge in which linguistic or material pidgins (or full-fledged creoles) may develop, representing new, hybrid and sometimes stable forms of practice. Focusing on particle detectors and dividing the experimental tradition into image (mimetic) and logic (statistical) subcultures, Galison sketches out the development of the two regimes since the war and the competition between them. He weaves a seductive narrative of the ways in which their interaction has constituted a (sometimes) coherent way of seeing the world in which local, rather than global concerns reign supreme. Out of this interaction comes the cultural production and reproduction of microphysics 'in-the-large.'

There is much to admire and applaud in this account. The disaggregation of microphysics into historically situated subcultures each claiming epistemic superiority allows us a finely nuanced understanding of the local production of knowledge, for example, and explodes once and for all the notion of microphysics as a homogeneous and self-evident domain of inquiry. Similarly, the trading zone affords a very useful tool for exploring the ways in which scientific cultures and subcultures engage with each other to produce coherent consensual knowledge, and opens up new ways of thinking about how one might write a history of physics by decentring the traditional narrative of theory and attending to local practices and the ways they engage with each other to become 'global' representations of the physical world. Though Galison's use of the metaphor of pidgins and creoles is overstretched and ultimately fails to compel, the idea of cultural crossovers will undoubtedly generate a great deal of new research on traditions of instrumentation and theory both in the history and sociology of physics (including microphysics, where the understandably U.S.-centred nature of the book has left many gaps to be filled) and elsewhere in the sciences, where comparative studies with other disciplinary cultures would be fascinating.

For the 'broken narrative' of a prewar Golden Age of physics and a postwar explosion of Big Science, Galison substitutes a more subtle, practice-based narrative of continuity and incremental change within local traditions and subcultures. The significance of industry and of World War II in transforming the various cultures of physics, for example, are unpacked in beautiful detail. It is routine to claim that the war changed physics, but such claims too often go unjustified. Here (and in recent work by Paul Forman) we have a concrete account of the impact of the war on physicists' ways of thinking and of the effects of wartime developments on the social, material and epistemic culture of postwar microphysics. Likewise the emphasis on changing regimes of organisation in physics, the adoption of teamwork, the shift to purpose-built laboratories with industrial/managerial models of practice with all the anxiety and alienation flowing from it, changing (and conflicting) notions of authorship and epistemologies of experiment, the significance of engineers at various levels in the development of high energy physics all point to the embededness of microphysics in wider social and economic currents and to the cultural specificity of its various sites. And the highly nuanced analysis of the local negotiation of meaning between distinct subcultures, in which "pieces of devices, fragments of theories, and bits of language connect disparate groups of practitioners even when these practitioners disagree about their global significance" (p. 54) gives us a highly persuasive account of the ways in which the local becomes universal, for "the work that goes into creating, contesting, and sustaining local coordination is ... at the core of how local knowledge becomes widely accepted" (p. 47).

Already vaunted as the foundation of a new historiography, Galison's notion of mesoscopic history:

> history claiming a scope intermediate between the macroscopic (universalizing) history that would make the cloud chamber illustrative of all instruments in all times and places and the microscopic (nominalistic) history that would make Wilson's cloud chamber no more than one instrument among the barnloads of objects that populated the Cavendish Laboratory during this century (p. 61)

seems to offer the best of all possible worlds in its focus on experimental and conceptual practices and the complex interactions between them. And in its sheer mass of detail, *I&L* offers a persuasive argument for the new historical focus. At the same time, however, the approach as exemplified here is deeply problematic from an historical point of view, for to focus on the cloud chamber and the other instruments which form the backbone of the narrative (emulsions, bubble chamber, TPC) is to focus on objects we retrospectively know made 'significant' contributions to microphysics. Among the 'barnload' of objects in the Cavendish Laboratory in 1919, though, it was by no means clear that the cloud chamber would turn out to be as productive as it later did (or, indeed, what 'productive' would mean in such a context). The cloud chamber was just one of several instruments in a developing research programme in the 1920s whose evidential capacities were played off against each other in an opportunistic and open-ended way. The mass-spectrograph, scintillation counters, ionisation chambers, Geiger-Mueller counters, photographic plates and other techniques were all potential sources of evidence, and to understand how and why any one of them came to acquire its epistemological warrant is to understand the dynamics of all of them together in the context of a wider programme of research. Citing Rutherford's view of the cloud chamber as "the most original and wonderful instrument in scientific history" (p. 73) for example, Galison neglects to point out that the wispy tracks of the cloud chamber had come to have any significance at all precisely because they offered plausible evidence for Rutherford's own programme of atomic and nuclear physics. Patrick Blackett's famous 1924 'demonstration' of the artificial disintegration of nitrogen nuclei by alpha particles, glossed here as a self-evident triumph of the new technique (p. 119), was in fact heavily publicised by Rutherford and his supporters (among them his student and protege Edward Andrade, also uncritically cited) despite the fact that only eight tracks out of four hundred thousand could be interpreted as yielding evidence of disintegration, and despite the fact that experiments elsewhere—experiments ignored or marginalised by Rutherford and his students—gave less unambiguous, or even directly contradictory evidence concerning the processes of nuclear disintegration.

This retrospective selection of what we later know to have been 'key' developments and of individuals (Wilson, Powell, Glaser, Alvarez, Nygren) whom we retrospectively know made significant contributions to the subject has important ramifications both in the structure of the book and in the role it allots to the products of microphysics. The central analytical device of the book—the division of experimental microphysics into the two traditions of image and logic—seems at times radically overdrawn. While Galison is undoubtedly correct in pointing to the competition between 'pure' logic and image traditions in the 1950s and 1960s, there is a sense in which the two traditions are set up in the earlier period in precisely such a way as to create a 'tension' whose resolution is found in the 'postmodern' Time Projection Chamber of the 1980s. While such a portrayal may be pleasing to the sensibilities of physicists, what results is often a false opposition. For instance, the first linking of the two emerging subcultures of image and logic in Blackett-Occhialini counter-controlled cloud chamber of 1932–3 is glossed over in a short paragraph (pp. 119–120). While in this instance the electronic elements of the apparatus may not have been used directly for making counts, the ensemble nevertheless represents the hybridisation of two hitherto distinct regimes of practice—and was explicitly recognised as such by contemporaries: C.T.R. Wilson, father of the cloud chamber, fretted to a correspondent in 1933 about the 'luxuries' enjoyed by Blackett and Occhialini in being able to put a cloud chamber, a counter and a magnetic field together to produce "no end of queer things." The counter controlled cloud chamber became an important part of the material culture of microphysics in the 1930s and 1940s, and it is telling that Galison similarly downplays Donald Glaser's attempts to link the first bubble chambers to electronic counters in his attempt to 'save' cosmic rays physics (p. 327).

In this respect, it is puzzling, too, that the book contains relatively little on the interwar years, for it was then that the two traditions of image and logic really came into being in microphysics, both acquiring their significance in the wake of the Cambridge-Vienna controversy (treated briefly and in a different context on pp. 147–149) which rendered the optical scintillation-counting method untrustworthy. During and in the wake of the controversy, which cast doubt on the integrity of this key technique which had underpinned the experiments leading to the development of the nuclear model of the atom and five years' subsequent work, physicists at the Cavendish Laboratory and elsewhere sought to find new ways of producing reliable evidence of subatomic processes while harming as little existing work as possible. Blackett's use of the cloud chamber in 1924 (the words "golden event" deeply problematic here) must be seen in this context of conflict and uncertainty, as must the development of electrical methods of counting (here, incidentally, a great deal more could have been said about wireless skills and their role in mediating the deployment of electronic methods in physics laboratories, also about the development of counting circuitry which was a crucial unifying/globalising element in early 1930s nuclear physics). Early counters were themselves often as unreliable as the scintillation technique they ostensibly replaced, and, at least until the early 1930s tended to be complementary to other kinds of instruments rather than in competition with them.

The period 1928–1932 is also crucial to any account of the cultures of microphysics for it was then (perhaps again in response to the crisis of certitude precipitated in nuclear physics by the Cambridge-Vienna controversy) that quantum mechanics and its practitioners became accepted by experimentalists like Rutherford, who had previously had their own theoretical practices. It was this same period which saw the establishment of accelerators as legitimate tools in nuclear physics. As in the case of wireless for electronic detectors, experimentalists drew on skills from the wider culture—in this case electrical engineering—to develop resources to sustain the reductionist programme. Mathematical physics offered resources (crucially the notion of tunnelling) which seemed to point to a way out of the impasse which experimentalists had reached. In contrast to the exemplary treatment of postwar theoretical cultures, one misses a sense both of the historicisation of theoretical practices *vis-à-vis* experiment and of the varieties of theoretical practice in prewar microphysics and the way the balance between them changed over time: important because mathematical theory could be as much a source of legitimation for experimentalists as a photograph or a plot. In short, I would suggest that the interwar period is much more important in establishing 'how pictures and counts got to be the bottom-line data of physics' than Galison's analysis implies.

Running in parallel with the issue of retrospection, there is a deeper philosophical issue here, too. Clearly, Galison's is not overtly the sequential history of great discoveries from electrons to the quarks and prospectively beyond which characterise most accounts of microphysics. Instead he gives a detailed account of the contingencies by which various elements of the material culture of microphysics found their place in, and became constitutive of, the discipline. From Marietta Blau's use of dental

X-ray films in cosmic ray research to the cryogenic apparatus produced for the H-bomb programme which subsequently supported the large hydrogen bubble chambers at Berkeley, to take but two examples, microphysics was (and is) shot through with the products of industry and the military. Indeed, microphysics seems to have operated through a process of spin-in rather than spin-off, as physicists have constantly found new ways of going on by appropriating and incorporating heterogeneous resources into physics and constantly expanding its boundaries. Alongside all this contingency, Galison demonstrates the contestedness of almost every new development in microphysics. The interpretation of scintillation counts or Geiger counter clicks, the introduction of new data analysis practices, the design of new detectors, judgements about how image and logic practices and the cultures of theory should relate to each other, even proper organisation of work for an activity to count as physics, all seem to have engendered controversy among practitioners.

Given this emphasis on the contingency and contestedness of microphysics, then, Galison puzzlingly portrays its products—the subatomic particles and forces which are its ostensible objects of inquiry—as self-evident entities apparently unaffected by the exigencies of laboratory and library life. Partly perhaps a consequence of backgrounding arising from his principal concern with instruments and social organisation, this form of accounting nevertheless has the unfortunate effect of making particles appear to spring almost autonomously into being as soon as the relevant instruments and work practices are in place. There seems at times to be an epistemological chasm between the contingent world of laboratory machines and practices and the solidity of the 'facts' which eventually emerge from them, and there are many instances in the text where a machine or technique whose proper role we have just seen hotly debated is then, nevertheless, the source of some unproblematic 'discovery': for example, pp. 117–119 on discoveries in Cambridge and elsewhere in the early 1930s; p. 319 on the unproblematic 'successes' of the bubble chamber; p. 411 on the "discovery of a wealth of new unstable particles in the late 1950s and early 1960s". In the cloud chamber chapter, similarly, Galison moves much too quickly from Wilson's 1911 experiments involving the photography of tracks produced by radioactive sources inserted inside the chamber to Blackett's 1924 'demonstration' of nuclear transmutation and the string of 'successes' associated with the chamber later in the 1920s and 1930s. "After the cloud chamber," he asserts, "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" (p. 140). Partly true. But as I have already suggested, the heavily mediated cloud chamber photographs which graced the pages of the Proceedings of the Royal Society and other journals in the 1920s and 1930s were not self-evident representations of nature but carefully selected and presented representations which took their place in broader programmes of work from which they cannot be separated.

Galison announces that his goal "is not to begin with [photographs of particle] tracks and position them within arguments for great experimental discoveries" (p. xvii). But that is what, by default, he ends up doing. This residual essentialism—akin to what Andy Pickering long ago called "putting the phenomena first"—requires clarification, for it is hard to see how the products of a thoroughly contingent material culture could themselves be any less contingent. Indeed, in places, it is the apparently self-evident products of microphysics which seem to drive the story and to give the material culture meaning, rather than physicists' interpretative practices. In this respect, it is a pity that Galison chose not to engage seriously here with the last decade-and-a-half's literature in the sociology of scientific knowledge and science studies (Harry Collins, Bruno Latour, the emergent, post-mangle Pickering and others), for its emphasis on these themes would have made it much more difficult for him to sustain the distinction between 'natural' facts and the material culture which produced them. As the neutroneers of the early 1930s acknowledged, materials and techniques were entirely constitutive of new phenomena; and though phenomena might eventually become reified and naturalised, the historian should surely aim to capture the entirety of this constructive process, not simply its outcome.

The issue is, I think, an important one for historians of physics, and it perhaps reflects *I&L*'s positioning with respect to physics itself. Coming in the wake of the cancellation of the Superconducting Supercollider (SSC) and the post-Cold War crisis of legitimation in physics which partly underlies the sterile arguments of the 'science wars,' *I&L* traverses what has become politically fraught territory. SSC promoter Weinberg recently commented that though he is at home in the world of physics, he is only an interested tourist in the land of history. Galison is equally at home in both. For that very reason, perhaps, the book sometimes has the feel of what Stuart Leslie has (in another context) called the view "from the inside looking in". There is a sense in which the kind of essentialist talk-about-particles here mirrors that routinely used by physicists themselves in order to legitimate microphysics: the particle zoo really is out there waiting to be discovered: all we have to do is get the right tools and put the correct organisation in place, and *voila*! Whether this furthers us in our historical and critical understanding of the processes of physics—past and present—is open to question. Thus while at one level the book radically changes our view of microphysics by linking it firmly to industrial and organisational culture, at the level of the cognitive it leaves it fundamentally untouched. In the end, despite its emphasis on material culture and practice, *I&L* stays close to the canonical account of the history of microphysics. Ultimately, then, the historian is left with a slightly disappointed feeling that the book merely puts a material-culturalist gloss on the canonical list of heroic discoveries of microphysics, from the electron to the W/Z. By assuming the phenomena and epistemological pre-eminence of microphysics, it does not fundamentally change our view of how this particular domain of inquiry came to dominate twentieth century technoscience—a pity, for it will undoubtedly be through detailed studies of the generation and interaction of material cultures by the barnload that we will learn what—if anything—is special about microphysics.

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By Alan Chalmers

PETER Galison has written a fascinating, descriptively rich history of the instruments used to detect sub-atomic particles in twentieth-century physics, drawing on a wealth of previously untapped archival material. Two distinct traditions are identified, distinguished and described, that of the image-forming detectors displaying individual tracks in photographic emulsions or cloud and bubble chambers, and that of the counters, the Geiger Muller counters and spark and wire chambers in which electronic logic circuits process masses of data to produce statistical arguments for claims about micro-entities. These two traditions eventually fused in the 1980s when advances in electronics and computing made it possible to integrate the results of many events and synthesise images electronically. In my review I wish to pursue the epistemological consequences of Galison's many-layered story, taking its descriptive adequacy for granted. (One minor aside is called for here. As one trained, in the nineteen fifties, in the Physics Department in which Cecil Powell was a Professor, I, like the other students, was aware that one of the main assets of that department lay in the skills of John Burrows, a glassblower of rare talent. I thought that this aspect of the 'material culture' at the University of Bristol was worth a mention.)

The issues in the book that are of direct epistemological significance are as follows. Galison complements recent work on experiment in science, which captures an important sense in which experiment can have 'a life of its own' by insisting that instruments have a life of their own too. Theory, experiment and instrumentation are inter-related, but each develops according to its own dynamic and at its own pace, so that a major transformation in one of them need not, and typically does not, coincide with a transformation in the other two. This picture of the 'intercalation' of semi-autonomous practices stands in direct opposition to that which sees science progressing through 'revolutions' that involve a complete change of the whole complex of theory and data and the practices linking them. Since the sixties, it has become common to view scientific work as unified under the umbrella of a paradigm or research program or some such framework, with the relationship between successive frameworks posing problems for the rationalist and fuel for the relativist. From Galison's point of view, the network of intercalated practices that constitute a science represents a disunity rather than unity, but it is precisely this disunity wherein lies the strength of science, insofar as it permits a particular practice to be progressively transformed against the stable background of other practices that are not brought into question at the same time.

Modern particle detectors involve coordinated work which is on an unprecedented scale and involves a network of specialisations that draw on the skills of engineers, computer programmers, Monte Carlo simulators, theoretical physicists and experimentalists. This is so far removed from the production of scientific knowledge by a single, white-coated scientist at the laboratory bench that it is difficult to single out any individual as 'the experimentalist'. Indeed, there is no single person who is in a position to grasp the sum total of what is going on, so that decisions about what to publish, which lines to pursue and so on are taken by committees, with the various specialists having their input. There is no Cartesian knowing subject having control over the process. As far as communication and collaboration between the areas of specialisation is concerned, say between the theoreticians and experimenters, Galison introduces the notion of a "trading zone". Just as two radically different cultures can interact in the market place, in spite of the fact that they have different aims, attribute different meanings to the items traded and even develop pidgins and Creoles as languages in which to communicate, so separate specialties can meet in science and meaningfully collaborate in spite of the gulf between them. So, for instance, we see 'phenomenologists' forming a bridge between the theoreticians and experimentalists, and using a language to which they can both relate, but to which they attribute their separate and distinct meanings, interpreting common words such as "muon" and "electron" in subtly different ways.

In this review I wish to press two related questions which I do not think can be unambiguously answered by reference to Galison's text. The first is the extent to which the complexity and degree to which a range of specialist practices are intercalated in modern particle physics represents something qualitatively new in science and the second is the extent to which particle physics can be considered to be an identifiable enterprise that, on the whole, has progressed in the twentieth century.

It can be argued that a notion of science as the possession of a knowing subject who comprehends and believes various theories or experimental results never was appropriate. An individual scientist works on raw materials, in the form of existing theories and experimental findings, that are not of his or her own making, and does so using pre-existing methods, or modifications of those methods, constrained by communally sanctioned rules. Any contribution made by a particular scientist will be incorporated into the work of others in ways that may go far beyond or may differ radically from what that scientist had in mind. This feature of science was brought home to me in a forceful way when a philosopher raised a question about the history of chemistry. He had decided that there was an important sense in which water was necessarily H_2O and yet this necessary truth was something that had to be empirically established. He asked me when, and by whom, this fact was discovered. The question cannot be answered in that form. While the formula for water had certainly been established by the end of the nineteenth century, the process that led to that knowledge was a complicated and protracted one to which many scientists working in different traditions made their contributions. The atomic chemistry of Dalton and Berzelius made the question concerning the formula of water a meaningful one but was incapable of yielding an unambiguous answer.

The path to that answer included work on gases by Gay Lussac and Avogadro, the latter introducing the idea that equal volumes of gases contain equal numbers of molecules, electrolysis pioneered by Humphrey Davy, work by physicists on specific heats and vapour densities, crystallography, and, perhaps most significant of all, organic chemistry and the role played by formulae and molecular structure in it. Cannizzaro showed in 1858 how, by assuming Avogadro's hypothesis, a unique and consistent set of formulae can be deduced from measurements of equivalent weights and vapour densities, so some might be tempted to locate the proof of the formula of water there. However, there is a difference between deducing a consistent set of formulae and deducing the correct set of formulae. Cannizzaro's method assumed the truth of Avogadro's hypothesis and, as far as chemistry was concerned, the evidence for its truth was indirect. The correctness of his formulae could only be ascertained by way of their agreement with formulae arrived at by other means, especially those that had emerged in organic chemistry. Avogadro's hypothesis eventually received support from the kinetic theory of gases, of which it is a consequence. The various collaborations that eventually led to a situation where the formula for water could be said to have been unambiguously established involved communication between traditions that used the terms "atom" and "molecule" in different ways. I am sure that various trading zones that facilitated collaborative work could be identified.

This lengthy digression is intended to probe the question of the extent to which Galison's construal of particle physics as involving contributions from distinct specialties, with the consequence that no individual scientist is in a position to be in knowing control of the whole process, represents something qualitatively new. There is no doubt that the phenomenon appears in a more striking form and on a broader scale in particle physics than in any other science past or present. Galison's vivid and detailed description leaves us in no doubt of this. It should serve to put an end to the currently fashionable attempts by Bayesian philosophers to understand science by reference to the degrees of belief that scientists are alleged to have in theories. But that is probably wishful thinking.

The analogy with trading zones can be interpreted in a way that casts doubt on there being a clear sense in which science progresses. As Galison himself describes the situation, the very different cultures or communities that interact via the trading zones have little, or perhaps nothing, of significance in common. In particular, the purposes served by the interaction in a trading zone will typically be very different for the two communities doing the trading. There is no sense in which the communities are involved in a joint enterprise with a common aim. I doubt if Galison would wish to say the same of the various groups contributing to particle physics. He himself sees the intercalation of a range of practices, and the disunity of science that it implies, as a strength of science, insofar as it helps to counter the extreme relativism that is a natural consequence of the view that scientific revolutions involve everything changing at once. However, merely noting that a change in one practice, in instrumentation, say, can be construed as progressive against a stable background provided by other practices is quite compatible with the idea that there are as many modes of progress as there are changes in practice. It does not help us to capture a sense in which particle physics as a whole can be said to have progressed steadily since the beginning of the century.

In raising my question about the progress of particle physics I do not wish to be interpreted as hankering after a cumulative account of the progress of science as measured by some fixed criterion. There is at least one alternative approach, which I have favoured ever since I was a fan of Gaston Bachelard in the seventies. A science is progressive insofar as it can situate itself with respect to past science and identify the obstacles that stood in the way of past science and which it has overcome. A change characterised as progressive in this way may well involve a change in standards, such as replacing determinism by indeterminism or demanding Lorentz rather than Galilean invariance, so there is no criterion of progress that is both fixed and substantive. But the position is not relativist because of the asymmetry involved. Galilean invariance can be seen as an approximation to Lorentz invariance for velocities small compared with the velocity of light but not the reverse, and Boltzmann statistics can emerge as a consequence of and approximation to Fermi-Dirac statistics in appropriate circumstances, but not the reverse. It is possible to construe the progress of quantum mechanics and relativity as coming about by removing obstacles inherent in classical physics. It is not possible to do the reverse. The historical stories that are told to exemplify progress of this kind will be to some degree distortions of actual history, insofar as the relations pinpointed with hindsight would not and typically could not have been appreciated at the time. But that does not entail that the relations do not exist. What is more, the 'rationally reconstructed' histories will be an important part of a physicist's education, serving to bring out the character and strengths of the current approach.

I am sure that the scientists involved in twentieth century particle physics, be they theoreticians, instrument builders, computer programmers or whatever, see themselves contributing to a common enterprise that is progressive. My question is, what kind of story do they tell, and what kind of story should they be telling, about the character of that progress? In what way can they situate the current state of affairs as an improvement on what came before. On page 799 of the book there is a diagram showing successive changes in instrumentation, experiment and theory, with the changes in one area taking place out of phase with changes in the others. Underneath the sketch is the caption "time" with an arrow pointing to the right. My question is, in what sense, if any, can that arrow be taken as indicating the direction of progress?

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