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- GEORGES CANGUILHEM, *A Vital Rationalist: Selected Writings from Georges Canguilhem*, edited by François Delaporte and translated by Arthur Goldhammer. New York: Zone Books, 1994. Pp. 481. ISBN 0-942299-72-8. £24.25, \$36.25.
- Canguilhem has, across the century, carefully spied out how, in the history of science, ‘obsessional constraints’ take hold of ‘the curious yet docile mind’ (p. 72): yet he never argues that acknowledgement of such obstacles to understanding entails the levelling of all knowledge-claims, the restoration of myth in the face of modernity (pp. 367–9). This selection, covering his philosophy of biology and medicine is graced by another gorgeous Zone Books production and Paul Rabinow’s brief, substantial introduction, but Canguilhem himself doesn’t seem to have had a hand in its compilation. Goldhammer’s translation finds easily both a crisp historian’s style, for work on baroque physiology or Comte, and surprising literary power for psychoanalytic speculation by Canguilhem as cultural critic. Some, caught up by recent interest in Canguilhem, might wish for full translations of his work on the reflex concept and of *Etudes d’histoire et de philosophie des sciences*, enormously influential in France. Here, instead, selections from these books are amalgamated with chunks of those already translated, *The Normal and the Pathological* (NP) and *Ideology and Rationality*. But this format allows the juxtaposition of material from the books with translations of scattered papers from a fifty-year span. Those pained at the omission of notable untranslated writings, like early 1960s essays on Bachelard and Darwin, can trace them, thanks to Camille Limoges’ seventy-page critical bibliography, an outstanding resource on twentieth-century French intellectual life.
- Delaporte divides the material into five parts. They cover methodology, epistemology of biomedical sciences, the history of concepts (cell,

reflex, biological object), textual interpretations (Descartes, Comte, Bernard), and positive problems about life and normality. It is, admittedly, hard to produce what Rabinow calls 'a kind of coherent "book"' from Canguilhem's overlapping, distributed output of essays: but some editorial strategies are frustrating. Although a survey essay 'Vie' of 1973, from the French *Encyclopaedia universalis*, is translated in sequence with only brief omissions, Delaporte has fragmented and levelled other texts oddly to fit his conceptual categories. This creative editing lacks a sense of development in Canguilhem's methods or interpretations. Extracts are undated, and it is difficult and time-consuming to calibrate passages across bibliography and source list. Chapters include material from different periods of Canguilhem's career, with entries for encyclopedias and general history of science texts beside papers of quite different status written for specialist collections.

While the collection falls short of an easy access introduction, much of value here extends the work already available in English essays on the epistemology of medicine written after *NP*. Canguilhem develops a distinction between active interventionist medicine, aligned for him with mechanism, and a watchful patience which he applauds, since 'a dynamic body deserves an expectant medicine' (p. 129). Pursuing the thought from *NP* that all human physiology is applied physiology, Canguilhem argues that there is no loss of epistemological status in the conversion of theory into therapy (p. 153). Canguilhem also aligns information theory further with his stress on the contextual nature of adaptive body-environment relations or 'discussions'. Happy, like cognitive scientists, to apply the concept of information at sub-intentional levels of explanation, he views organisms as temporary pockets of biological stability, where improbable organization permits brief resistance to inevitable thermal equilibrium. Though noise might be productive (p. 88), Canguilhem defends the integrity of 'an organic order firm in its orientation if precarious in its incarnations'.

Without further context, it is hard to judge how Canguilhem's views, such as his battery of arguments against mechanism in the life sciences,

might have shifted in response to other historians of science, or to wider cultural changes. He construes mechanism narrowly, as incompatible with information theory, incompatible with a notion of biological function, and as applicable primarily to geometric and quantitative approaches to the discontinuous motions of neuromuscular systems. It is less plausible now to think (as Canguilhem did in 1937, comparing Aristotle on slaves) that the Cartesian beast-machine doctrine was linked, in intention or consequence, to a programme for justifying vivisection: and more plausible now, in a cyborg age which requires what Canguilhem called an 'organology' to explain machines, to find active matter within historical mechanisms.

The relation of mechanism and authoritarianism in Canguilhem poses sharper problems, and this is one area in which Limoges' hope for links between Canguilhem's *oeuvre* and the traces of his career (p. 386) is realized. Canguilhem's early topics included pacifism, Pirandello, colonialism and suicide as well as Leibniz, Kant and Bergson: the full dedication to medical studies which gave his mature philosophy such power occurred only when, in 1940 Toulouse, he refused to teach according to the Vichy regime's orders. Canguilhem's active engagement in the Auvergne Resistance was not long over when he pointed out interrelations, in cell theory, between totalitarian politics, with individuals sacrificed to higher organic society, and the vitalistic biology of German Romantic nature philosophers, in which organisms are continuous wholes conceptually prior to their components. It is not that alternative 'French' pictures of discontinuous organic molecules were any less ideological, being interdependent in turn with Enlightenment politics of atomic individuals in contractual association. The overdetermination of theoretical concepts is no bar to their efficacy or epistemological dignity. But the historian of science, not a scientist but a maker of judgements, in this context had clear values to apply. The emphasis here later shifted, for 'the analytic method in conjunction with the discontinuous imagination' (p. 167) came more strongly under fire through Canguilhem's attention to a biological specificity which he thought mechanists miss, since life and death are

not problems for physics and chemistry. But the role of national differences in the history of physiology continued to exercise him, conflict between French and German schools cropping up in various domains, from bacteriology to energy utilization in industrialized bodies.

Canguilhem remained confident that psychoanalysis of knowledge is compatible with scientific realism, where the latter opens space for the normative criticism and judgements of hierarchy essential to philosophy (p. 384). This reminds us that Foucault's celebrated alignment of himself with Canguilhem, against phenomenologists and existentialists, as investigators of knowledge, rationality and concepts rather than of meaning, experience and the subject, should not encourage neglect of their differences. As Foucault noted, Canguilhem's polemical realism required the retention of a true/false dichotomy as a judgemental, rather than simply descriptive or conversational, tool. Canguilhem often uses evidence from present sciences, refusing to collapse them into mere vehicles of normalization. Indeed, the category of the normal is not, for Canguilhem, necessarily linked to surveillance, that of the pathological not wholly disciplinary. Like Foucault, he stresses the priority of infraction over regularity, infringement over law, for there are no (normative) norms without something to regulate. But since, for Canguilhem, we cannot step back from current norms, we must inevitably be militant, intolerant, in their expression or defence (p. 364). This need not be the individual's complicity in an imposed regulatory apparatus, for biological (unlike social) norms are intrinsically resistant. There is no diatribe against the subject in Canguilhem, as in many influenced by him: indeed he insists on the phenomenological, qualitative nature of the health/disease opposition, tempted occasionally to posit something like 'no disease without awareness'. But Rabinow's suggestion (p. 18) of an idiosyncratic 'not-so-latent existentialism' underestimates the extent to which Canguilhem bypasses consciousness: awareness is corporeal, the wisdom of the body testing the adaptability of the internal environment to an inconstant external environment of 'leaks, holes, escapes and unexpected resistances' (p. 356). Pathology as negativity

does involve error, the risk of catastrophe being inevitable in maintaining normativity: but Canguilhem sees no existentialist responsibility here, only (as *NP* has it) traces of anguish.

Thinking of subjectivity in Canguilhem reveals the difficulty of extending his deeply biological view of norms to the cognitive domain. Leriche's picture of health as 'life lived in the silence of the organs' does not easily apply to psychology, an area Canguilhem has rarely approached directly, beyond the deconstructive history of his 1958 paper 'What is psychology?' (not included here despite the claim to the contrary on p. 411). There are hints, about psychology and politics, in three manuscripts on norms and normativity from which Delaporte includes selections (undated and arranged out of Canguilhem's order). After reworking themes from *NP*, they examine the problematic equation of earlier with inferior mentalities which followed Piaget and Levy-Bruhl. But while historicizing overoptimistic rationalism, Canguilhem defends the possibility of comparing and evaluating mentalities. We should see 'the modern mentality' not as definitively superior, but as normative in its ideals of openness to testing in new conditions. The disastrous, conservative impulse to revert, impossibly, to a wholesale tolerance of childhood fantasy and puerile myth (p. 362) in overreaction to positivism ignores the painful awareness of the desire/reality gulf for which adults and moderns have struggled. Resolution of reason's crises lies only in future invention and adaptation, not in deceptively reviving past norms as if on equal footing: 'try as one will, a plurality of norms is comprehensible only as a hierarchy' (p. 364). The tension here runs deep between democratic faith in a rational fallibilism, and Nietzschean joy in the risky aggression of expansionist normativity.

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DAVID E. LEARY (ed.), **Metaphors in the History of Psychology**. Cambridge Studies in the History of Psychology. Cambridge: Cambridge University Press, 1994. Pp. xiv + 383. ISBN 0-521-42152-7. £12.95, \$17.95 (paperback edition).

Metaphors in the History of Psychology was first published in 1990. The publication of the paperback edition indicates the publisher's hope that the book will be used more widely as a textbook. There can be little doubt that the book deserves such status. The book consists of eleven chapters, nine of which study the metaphors used in various subfields and traditions of psychology. The remaining two chapters come from the editor's pen. They provide an extensive and useful summary of the literature on metaphors and their role in science (chapter 1) and suggestions for further research (chapter 11).

Chapter 2 is a study of metaphors and models in neuropsychology. Its author, Karl H. Pribram, provides a fascinating sketch of the successive impacts of telecommunications, control systems engineering, computer science and holography. James R. Averill details the history of five key metaphors of emotion: emotions as inner experiences ('He *felt* his anger rising'), as physiological responses ('Anger made his *blood boil*'), as the animal in human nature ('He responded *sheepishly*'), as diseases of the mind ('She fell *madly* in love'), and as driving forces ('He was *driven* by fear'). Averill concludes by introducing and defending a sixth metaphor, that is, emotions as social roles (chapter 3). Paul McReynolds suggests that there have been five 'root' metaphors of motivation in psychological discourse. Persons have been construed as 'pawns', 'agents', 'natural entities', 'organisms', or 'machines' (chapter 4). Chapter 5, on cognitive metaphors in experimental psychology, is co-authored by Robert R. Hoffman, Edward L. Cochran and James M. Nead. It is the outstanding piece of the collection. The authors first sample central cognitive 'metaphorical themes' as these occur in natural language (for example 'consciousness is up, the unconscious is down', 'minds are containers', 'understanding is seeing') and then show how these and similar metaphors have been, and still

are, at the centre of Western psychology. This is followed by six case studies of how specific metaphors have informed experimental practice. These case studies are about perception and psychophysics, pattern recognition, Gibson's ecological psychology, motor skills, attention research and representation. The authors conclude with general comments on the 'misuse' of metaphors and the call for a 'cognitive-experimental approach to the philosophy of science'. Jerome Bruner and Carol Fleisher Feldman provide an all too short overview of metaphors of consciousness and cognition. They divide these metaphors into those that present cognition as 'reproductive' (for example the mind as the wax tablet) and those that depict cognition as 'productive' (for example the mind as the searchlight) (chapter 6). Logical positivism and behaviourism are of course well known for their hostility towards metaphors in scientific theories. Laurence D. Smith shows in chapter 7 not only that the theories of both are themselves permeated by metaphors, but also that their very opposition to metaphors is typically expressed metaphorically. Social psychology and its guiding metaphors are the topic of Kenneth J. Gergen in chapter 8. Gergen gives examples of how the social group has been variously construed as a human organism writ large, as a physical structure, an animal laboratory, a machine, a homeostatic process, a symbolic interaction, a marketplace, a stage and a system. Theodore R. Sarbin's chapter 9 deals with metaphors of unwanted conduct. He displays the metaphorical character of the very term 'mental illness', explains the two root metaphors of psychopathology (the mind as an autonomous thing; the universal transmission of force), and studies 'metaphors of intervention' (for example 'moral management'). Finally, Kurt Danziger combines historiography and history of psychology. Danziger chastizes historians for artificially imposing continuity. One way to capture discontinuity is to study the change of root metaphors and their links to the wider culture. Danziger illustrates these claims with examples from the history of associationism. Particularly telling is his reminder that the idea of associations of separate and independent entities was used in the eighteenth

century to bind together social theory, cosmology and psychology (chapter 10).

This is a fine and important anthology, and should be of interest to any student of metaphors and their role in science. Nevertheless, a few critical comments seem to be called for. First, in their attempt to cover psychological theorizing from antiquity to the present, most of the studies use a very broad brush. Most of the papers are basically lists of metaphors used in a specific field or tradition. That method will disappoint historians of science who prefer more detailed and context-sensitive accounts. Secondly, there is too much overlap in the theoretical pronouncements of the nine studies. Although they all basically share the same view of metaphors, each one discusses it separately, often over several pages. Thirdly, the central thesis of the book is that metaphors are irreducible and central parts of science. In this day and age, to argue for this thesis amounts to belabouring the obvious. The challenge today seems to be rather to display in detail the social uses of specific metaphors, and to explain why some metaphors were successful where others failed. Fortunately, this point is recognized by the editor himself (p. 359), and thus it is easy to agree with him that 'this volume...begs for a sequel' (p. 361).

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ANNIBALE FANTOLI, *Galileo: For Copernicanism and for the Church*, translated by George V. Coyne, SJ. Studi Galileiani, 3. Rome: Vatican Observatory Publications, 1994 (distributed outside Italy by University of Notre Dame Press, Notre Dame, IN). First edition: pp. xix+540. ISBN 0-268-01029-3. Second edition, revised and corrected, 1996, pp. xx+567. ISBN 0-268-01032-3. \$21.95.

RIVKA FELDHAJ, *Galileo and the Church: Political Inquisition or Critical Dialogue?* Cambridge: Cambridge University Press, 1995. Pp. viii+303. ISBN 0-521-34468-8. £35.00, \$54.95.

Our thirst for the Galileo Affair is evidently unquenchable. To provide temporary relief, here

are two more books on the subject. Beyond their attempts to understand the circumstances that led to Galileo's abjuration and house arrest in 1633, they share a number of traits. Both are demanding and require slow reading; their nine hundred pages are correspondingly difficult to summarize adequately, let alone evaluate fully, in the space allotted here. Both books add depth to the Affair, take seriously the larger context(s) in which it unfolded, and both interact vigorously and very differently with earlier historiography on the subject. Finally, both books share a wholesome distrust of conspiracy theories.

In approach, execution and conclusions, however, the books differ markedly. Feldhay's takes an aggressive historiographical stance and develops an intricate, controversial argument about the pervasive role of Dominican-Jesuit polarities in the Galileo Affair. Among the chief merits of her case are her sharpening of the reader's awareness of cognitive and institutional tensions within Counter-Reformation Catholicism, and her reopening of the philosophical and theological worlds of the Dominicans and Jesuits.

Fantoli's thesis, as expressed in his title, is neither new nor particularly controversial. The great strengths of his book are his command of sources, his careful expositions and analyses of texts, and his critical evaluations of evidence and interpretations, which he has been distilling and blending for twenty-five years. For health reasons, his book must be savoured slowly and in small doses. It will have a long shelf-life, for it seeks understanding rather than attention. What it lacks in glamour and smooth prose is more than repaid in reliability and critical acumen.

By comparison, Feldhay's book at first looks more like a *vin nouveau* – a Château Foucault 1995. Her allusions to power knowledge structures, the gaze, conditions for possibility, and the transgression of boundaries are mostly wineskin-deep, however, for her general approach is vintage intellectual (and institutional) history. Thus, 'The different meanings assigned to one key term [= hypothesis] hold the clue for the alternative narrative structure suggested here' (p. 9). Freed from telling 'how it *really* happened' by the impossibility of doing so, but

bound by the ‘duty to understand the *kind of thing that could have happened*’ (p. vii, *her italics*), Feldhay’s frankly hypothetical arguments (for example pp. 54, 294) are spirited and heady, even though spoiled grapes seem to have slipped into the vat.

Key differences between the two books may be gauged by their understandings of the events of 1616 and 1633. Behind these, Feldhay sees the longstanding and complex struggles of the Dominican and Jesuit orders. The 1616 condemnation of Copernicanism had less to do with literalism or authoritarianism (for example p. 212) than with Dominican and Jesuit disagreements about hypotheses and education, which transpire in the two different accounts of the meeting between Galileo, Cardinal Bellarmine, SJ, and the Inquisitor Segizzi, OP, and in the 1620 censure of Copernicus’s *De revolutionibus*.

With respect to 1633, Feldhay sees Galileo’s refusal to admit that he held the ‘absolute truth’ of Copernican theory not as disingenuous, but as consistent with the view of hypotheses in Bellarmine’s signed affidavit (as opposed to the famous unsigned account of the meeting). Whereas the Dominican position remained stable between 1616 and 1633, the Jesuit ‘voice’ shifted toward the Dominican side (as gauged by differences in texts by Bellarmine in 1616 and Inchofer in 1633, two highly idiosyncratic figures that Feldhay nevertheless sees as representative of the Jesuit order at the time).

If Feldhay is right, the Dominicans’ position was decisive for the affair, both intellectually and politically. Their rigid Thomism defined the terms of the official censures of Copernicanism in 1616 and 1620 as well as the 1633 trial of Galileo; and their inquisitorial stance and power struggles with the Jesuits forced the latter defensively to distance themselves from Galileo, with whom they (especially Christopher Scheiner) initially had much in common. If the Jesuits erred, it was because the Dominicans made them do it. Urban VIII appears only as the mouthpiece for what Feldhay takes to be – wrongly, in my view – a Dominican scepticism about knowing the natural world.

For Fantoli, by contrast, the events of 1616 have much to do with authoritarianism and

literalism, and little to do with differences about hypotheses or educational philosophy. In 1616 – an age of absolutism – the theologians were asked to evaluate not the cogency or limits of arguments for Copernicanism, but the status of two isolated propositions wrenched from all argumentative context. The verdict declared heliocentrism formally heretical, explicitly because it contradicted ‘the sense of Scripture, according to the literal meaning of the words’ (Fantoli, pp. 216, 451). As for 1633, Fantoli finds that even the Jesuits as a group had (*pace* Galileo) little to do with the events leading up to the trial. Galileo did have enemies, but they were a motley crew with little more than their enmity in common, and their power to influence events has been exaggerated. Unlike Feldhay, Fantoli believes that Galileo was disingenuous in denying his belief in the truth of Copernicanism, and that he aggravated his case in so doing. Urban VIII looms large behind the final condemnation: without his strong personal conviction that Galileo had betrayed him, the trial would, according to Fantoli, never have taken place (p. 456), inimical attempts at mischief notwithstanding.

In his preface Fantoli denies that his work makes an original contribution to the history of science, or presents a full biography of Galileo. His professed goal is, rather, to illustrate the ‘complex dialectical interplay’ among a variety of factors that impinged on Galileo’s life and his ‘eventual conflict with the Church’ (p. xv). His modesty notwithstanding, he has produced an impressive piece of scholarship with a daunting level of documentation, some of it new. Fantoli has in one case even checked the seventeenth-century transcription of an edict against the original, in spite of the fact that the former was published several times (p. 218). His nearly two hundred pages of notes are full of alternative interpretations of texts and events, and of (usually judicious) trenchant criticisms of the literature.

Two small examples illustrate the extent of Fantoli’s achievement. First, Feldhay calls Santillana’s account of the 1633 trial ‘still the most authoritative’ (p. 65), a statement that, thanks to Fantoli, no longer holds. Secondly, from Fantoli’s winnowing, the scholar who

emerges as the most frequent purveyor of vulnerable interpretations is Stillman Drake. Given Drake's stature and ubiquity in Galileo scholarship, everyone must now come to terms with Fantoli's criticisms, whether or not they agree with him.

Fantoli's interpretations often aim to dissolve apparent paradoxes and eliminate conspiracies and forgeries, to which he prefers the normalcy of bureaucratic procedure and mundane explanations. The revised edition of his work makes this point even more forcefully (pp. 446ff) than the first. Given the 1616 condemnation of heliocentrism as heretical, the *Dialogo* made Galileo 'vehemently suspect of heresy'. This being so, the abjuration of the suspect was the expected outcome of normal inquisitorial procedure involving suspicion of heresy, and should not be seen anachronistically as an abuse of judicial power. For Fantoli, the abuse lay in the doctrinal absolutism that led to the 1616 condemnation of Copernicanism.

The world that Fantoli depicts is an even-tempered and moderate one, in which Galileo and his opponents make mistakes, of which Fantoli is very understanding. Thus he dismisses the rumours about a concerted Jesuit antagonism to Galileo as unproven (as well as unnecessary and insufficient to explain Galileo's troubles); they should be read as the latter's attempt to make sense of Urban VIII's change of affect towards him. Conversely, with respect to the phases of Venus, Fantoli relies upon an improbable chronological argument to salvage Galileo's originality from Westfall's less charitable reading of the episode. Fantoli sees Galileo's claustration of his two young daughters as nothing more than 'egoistic' – even though he had to try twice to circumvent Church rules that prevented parents from putting away their underaged children.

The patient reader of Fantoli's book will find his unhurried approach conjoined with a satisfying chronological even-handedness, including attention to the oft-neglected decade before the trial. But the great strength of his book is the way in which Fantoli at almost every step shows the reader the evidentiary basis for his analyses as well as his historiographical judgements.

Whereas Fantoli takes on his predecessors'

specific arguments piecemeal, Feldhay paints the literature on the Galileo Affair in the broadest of strokes: the historiography that precedes her work presupposes a grand Enlightenment myth, according to which the reason of Galileo's science clashes with the unreason of the monolithic, authoritarian Church. To this, Feldhay opposes the salutary conviction that the problem of Galileo and the Church requires delving deeply into church history, which she proceeds to do boldly. Her corrective proposes to substitute a nuanced reading involving philosophical, theological and political tensions among the 'cultures' of the established Dominicans (medieval, dogmatic, inquisitorial, oriented to the *vita contemplativa*) and of the Jesuit *arrivistes* (modern, pragmatic, oriented to the *vita activa*, compromisers and transgressors of boundaries).

But Feldhay has cleft one monolith, only to produce bipolar Dominican and Jesuit megaliths. This totalizing strategy not only overlooks the considerable diversity within the orders, but also typically constructs the culture of each from a single representative (pp. 55, 57, 63, etc.), a hazardous move indeed. It is Molina, SJ, versus Bañez, OP; Bellarmine, SJ, versus Lemos, OP; Inchofer, SJ, versus Riccardi, OP. She forgets that a Dominican censor approved and praised Molina's book (also the case with Galileo's *Dialogo*), and that the Dominican Campanella wrote an *Apologia* for Galileo, which used 'hypotheses' in a sense very different from that of Feldhay's 'Dominican culture'. Nor is it clear when or how the Dominicans became contemplatives – except by way of contrast with the Jesuits. In the end, anthropological or sociological conclusions derived from a database of one to several individuals will not convince sceptics.

Whereas some historians see the contradictions between the two documents about the 1616 admonition of Galileo as symptoms of a conspiracy, Feldhay reads them as expressions of genuine theological tensions between the Dominican and Jesuit cultures.

These culture wars developed in a late-sixteenth-century quarrel over grace, predestination, and God's knowledge of what Feldhay calls 'contingent futures' (read: future con-

tingents). This acrimonious controversy ('De auxiliis') involved almost twenty years of Dominican attacks on the work of the Jesuit Molina. Feldhay argues that 'De auxiliis' shaped not only the political alignments of the Galileo Affair, but also the issues at stake in it, notably the relation of God's omniscience and omnipotence (divine power/knowledge, as it were) to the status of 'hypothetical knowledge', and eventually astronomical hypotheses. Feldhay contends that the Jesuits considered 'hypothetical objects' to be legitimate objects of knowledge whereas the Dominicans did not. The order (that is, Tolosani, OP) saw Copernicanism first as unproven, but eventually as unprovable (as illustrated by the non-Dominican Cardinal Maffeo Barberini, later Urban VIII, who allegedly reflects the Dominican view).

Feldhay argues that the Dominicans' "doctrinarian" Thomism, paradoxically imbued with sceptical overtones, could have led to the theologians' rejection of Copernicanism in 1616, and to the pope's arguments against its defence later on, up to the trial of 1633' (p. 294). The Jesuits, by contrast, are open to hypotheses, have much in common with Galileo, and enter into dialogue with him (as illustrated by the controversy over sunspots). But the publication of the *Dialogo* forced them to disown their shared ideas – notably Christopher Scheiner's work on sunspots – in response to Dominican pressures toward orthodox Thomism, thus leaving Galileo twisting slowly in the breeze.

Feldhay's presentation of her book as bucking the entire historiography of the Galileo Affair rests on a selective reading, however. Whereas her favourite whipping boy is de Santillana's elderly *The Crime of Galileo* (1955), which she still admires, she does not acknowledge the existence of Blackwell's *Galileo, Bellarmine, and the Bible* (1991). This omission is striking, for Blackwell's approach to the Church is not monolithic and addresses several key Jesuit issues that appear in Feldhay's own account, as well as some that perhaps should have (such as the decisive importance of obedience in Jesuit culture, and of Bellarmine's helpful compromise in the papal resolution of the 'De auxiliis' controversy).

More serious are the flaws in Feldhay's

discussions of Dominican and Jesuit philosophy and theology, which are central to her thesis about the importance of the 'De auxiliis' controversy for the Galileo Affair. Her expression 'contingent futures' is not a translation problem, but the most obvious symptom of her unfamiliarity with the basic logical and philosophical vocabulary of the texts she cites, which leads to serious misunderstandings of the issues she represents. Among other problems, for example, she misconstrues the meanings of *necessitas consequentiae* versus *necessitas consequentis*, which refer straightforwardly to the necessity of the inference versus of the conclusion (not to 'human acts' or 'conditional necessity' versus 'the object under discussion' or 'absolute or objective necessity', respectively, as Feldhay variously contends, pp. 178f). Her inferences and interpretive leaps from divine to human knowledge also present serious difficulties for her thesis. In discussing the Jesuit Molina, she claims that his concept of God's 'middle science' – his solution to the problem of God's foreknowledge of future contingent propositions – makes possible a 'science of hypothetical objects' that is inconceivable on Aristotelian or Thomist or Dominican terms. From this, Feldhay concludes that 'the boundary between the true and probable knowledge was blurred in their [= the Jesuits'] discourse' (p. 181). Feldhay then links this divine 'middle science' with the (human) mixed or middle sciences that use mathematics to treat physical phenomena, and connects Thomas of Lemos, OP's critique of Molina's 'middle science' with scepticism about natural knowledge (pp. 185ff). This is how the 'De auxiliis' controversy bears on astronomical hypotheses.

But this blurring of categories across the boundary of divine versus human knowledge is not tenable. What are the grounds for believing that Molina's 'middle science' – one aspect of the divine omniscience intermediate between two other aspects of omniscience – bears a relation to the human 'mixed sciences'? Omniscience would admittedly contribute much to natural philosophy or cosmology, but this tool was accessible neither to mere seventeenth-century mortals, nor to Jesuits. Even if Feldhay is right about Jesuit theology blurring the bound-

ary between the true and the probable when the knower was God, the claim that divine 'middle science' helped the non-omniscient blur true and probable knowledge of the physical world seems highly improbable. At the very least, it requires some evidence.

Feldhay has overlooked one Molinist argument that does parallel the heliocentrist arguments formally. Molina, SJ, defends his controversial 'middle science' on grounds of instrumental coherence: if 'middle knowledge' were false, it could never reconcile with free will the four doctrines of predestination, foreknowledge, providence and reprobation, as it does. Had Bellarmine, SJ, conceded such a non-demonstrative argument, Galileo would have been very pleased indeed.

Finally it is not clear why Maffeo Barberini's use of divine omnipotence to undermine the necessity of the Copernican hypothesis (and every other natural philosophical hypothesis as well) is Dominican in inspiration (p. 210). Barberini's argument certainly points out the fallacy of affirming the consequent. To the extent that it also makes a sceptical thrust, the future pope's argument resembles more strongly the Pyrrhonic strains of the Counter-Reformation Jesuit fideists in Richard Popkin's *History of Scepticism* than any Dominican view that Feldhay cites.

The differences between these books are many, including profound differences of self-representation, but they also share some surprising similarities. Fantoli has produced the kind of 'grand narrative' that Feldhay claims to be questioning. Yet behind her hypothetical language, Feldhay's narrative is equally grand – even Sisyphean, since she believes that 'the riddle... may be insoluble' (p. vii). Her heuristic gives freer rein to the imagination in suggesting new possibilities; his gives precedence to critical parsimony by eliminating non-actualities. Despite these differences in approach, Fantoli and Feldhay agree that history is contingent, and that the Galileo Affair could have turned out differently. Indeed historians themselves seem condemned to make sure that it will continue to do so.

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WILLIAM R. NEWMAN, *Gehennical Fire: The Lives of George Starkey, an American Alchemist in the Scientific Revolution*. Cambridge, MA: Harvard University Press, 1994. Pp. xvi + 348, illus. ISBN 0-674-34171-6. £39.95.

The alchemist George Starkey (1628–65) was one of the shadier figures associated with natural philosophy in the seventeenth century. Born in Bermuda, and educated at Harvard, Starkey sailed for England in 1650, and spent most of the rest of his life in London, dying there during the plague outbreak of 1665. Starkey's patrons included Samuel Hartlib and Robert Boyle, both of whom were excited by his skill as a chemist, and interpreted his arrival from beyond the seas as a sign of the gathering together of knowledge which would anticipate the millennium, for which they hoped. Despite such friendships, Starkey's career never really prospered in England, and, like many contemporary chemical practitioners, he hovered on the edge of debt, striving to make a living from the sale of remedies for ill-health. Such material uncertainty and obscurity contrasted bleakly with the self-confidence of Starkey's literary and alchemical persona, Eirenaeus Philalethes. Under the cover of this pseudonym, Starkey composed a number of works, drawing heavily on the ideas of Joan Baptista van Helmont, and engaged in violent controversy with rival alchemists and the chemical traditions which they espoused. His difficult, metaphorical writings inspired future seekers after truth, most notably Isaac Newton, in their own alchemical activities. However, their style, as well as their subject matter, has helped to ensure that Starkey has remained on the fringes of modern discussions of the Scientific Revolution. William Newman's study attempts to bring Starkey out of the shadows. In doing this, Newman also hopes to throw light on the broader question of the role of alchemy in seventeenth-century natural philosophy, claiming that alchemical ideas, in particular those derived from the writings of pseudo-Geber, revolutionized conceptions of the nature of matter.

Newman's previous writings on seventeenth-century alchemy inspired high hopes for this book. Unfortunately, *Gehennical Fire* did not prove to be up to standard. Although only a

short book, it betrays many signs of hasty composition. This is apparent in such things as Newman's failure to check the spelling of names found in seventeenth-century manuscript sources (thus Starkey is repeatedly said to have lived for a time in 'Rotherhith', rather than Rotherhithe), or in the almost unreadable prose of the chapter devoted to the alchemical background for Starkey's writings. In introducing a discussion of the alchemy of Bernard of Trier, Newman writes: 'here too we shall see the fusion of Geberian corpuscular theory with an increasingly hylozoic language, which again we must interpret as a vitalist tendency rather than a full-blown panpsychism'. He concludes the chapter, and his assessment of the influence of medieval Persian and Arabic alchemy on van Helmont's ideas, with a sentence of similarly rich verbiage: 'The nameless Ismaili acolytes of Jābir had planted a seed that would grow in unimaginable wise.'

The self-indulgence and opacity of Newman's prose would matter less if his argument did not also engage frequently in special pleading. This charge has particular substance for Newman's treatment of the influence of pseudo-Geber on seventeenth-century alchemy. The supposed debts of van Helmont, and thence of Starkey and Newton, to pseudo-Geber seem to me, at best, tenuous or remote. More generally, Newman's discussion of the Paracelsian and Helmontian traditions in chemistry is too selective to prove his case for the pervasive influence of pseudo-Geber. Newman has identified an important tradition in medieval alchemy, through his earlier studies of pseudo-Geber; unfortunately, his work on the seventeenth century seems at times to suggest that this was the only significant source for early modern alchemists, and that these alchemists had little original to say for themselves. Newman is tempted to concentrate on the writings of pseudo-Geber in part because of the corpuscularianism which he identifies there. Sensibly rejecting the approach of Carl Gustav Jung, Newman decodes the elaborate imagery of Starkey's chemical writings to uncover a corpuscular theory of matter, shared in part with van Helmont and Newton. Newman successfully identifies some of Starkey's chemical procedures, and usefully stresses their practical aspects. However, it is again unfortunate that

this represents only a partial treatment of the alchemical writings to which Newman refers. One does not have to endorse Jung's ideas to recognize that the imagery to be found in these works has religious and mystical, as well as practical, significance. Sadly, Newman appears to be largely oblivious to this, although he is alert to some of the social consequences of the use of secret languages and coded images in alchemical texts.

Newman relies heavily on the scholarship of Harold Jantz, R. S. Wilkinson and, above all, George Lyman Kittredge for his treatment of Starkey's early biography, and of his American education. This part of the book is relatively comprehensive, and works quite well. The same cannot be said for Newman's discussion of Starkey's career in England, which is confusing and incomplete. For instance, Newman argues that Starkey's relations with Hartlib deteriorated after the marriage of Hartlib's daughter to Frederick Clodius. Yet documents which Newman himself cites demonstrate Hartlib's continuing interest in and affection for Starkey after that date. Newman's description of Starkey's medical practice is unnecessarily brief, and does not explore the implications of some of the available sources, which detail, for example, several of the drugs used by Starkey, and the prices which he charged for certain treatments. The reader is given little idea of the scope of Starkey's practice, and, more seriously, little is made of the medical context for the chemical activity discussed by Newman elsewhere in the book. In describing alchemical matter theory, Newman pays scant attention to Paracelsian and Helmontian theories of the body, and their implications for both the imagery and the practice of alchemy. Although Newman does discuss Starkey's involvement in a number of medical and alchemical controversies, he does not always contextualize these fully. This problem recurs in his treatment of Starkey's influence on Newton's alchemy, where Newman is reluctant to consider the fact that Newton's theories of matter (and hence his debt to Starkey) changed over time. In general, Newman appears to be far less familiar with seventeenth-century England than he is with the medieval alchemical tradition.

Gehennical Fire is a disappointing book,

which is much less conclusive in its treatment of Starkey and of his ideas than it ought to have been. It is marred by a narrowness of conception, and by hastiness in execution. Yet, despite this, the richness of Starkey's career, and the interest of his writings, do become clearer as a result of Newman's efforts. As an author, an alchemist and a chemical physician, Starkey deserves attention, and his work demonstrates the breadth and ambition of the new philosophy in the seventeenth century. His life was also a telling example of how slight the rewards of new practices and new learning might be. It would be a great pity if the shortcomings of Newman's book were to condemn George Starkey to spend more dark days in the penumbra of historiography.

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RENE TATON and CURTIS WILSON (eds.), *The General History of Astronomy, Volume 2. Planetary Astronomy from the Renaissance to the Rise of Astrophysics. Part B: The Eighteenth and Nineteenth Centuries*. Cambridge: Cambridge University Press, 1995. Pp. xiii + 281. ISBN 0-521-35168-5. £45.00, \$69.95.

This part of the *General History of Astronomy* takes the interacting stories of observational astronomy and celestial mechanics from the publication of Newton's *Principia* to roughly the end of the nineteenth century. While it is not necessary to have read Part A to appreciate the second instalment, clearly the two are intended to go together, and some knowledge and understanding of the earlier period must be assumed. The wide-ranging subject matter in this book is organized under five headings, of differing breadth and depth, with some being tidier than others: the early reception of Newton; eighteenth-century celestial mechanics; observational astronomy and the application of theory in the late eighteenth and early nineteenth centuries; nineteenth-century theory; and the application of celestial mechanics mainly in the nineteenth century. Within these headings, different experts have tackled either developments in particular problems, for example the shape of

the earth or measuring the solar parallax, or the contributions of leading astronomers. Inevitably there is overlap both of people and of themes, but the editors have achieved a good level of consistency and cohesion between the essays.

Treatment of the early reception of Newton is problem-specific and concentrates on vortex theory, the shape of the earth, the motion of the lunar apse, precession, solar and lunar tables, and the return of Halley's comet. One of the sadly striking features of this part of the contents page is the number of authors who are no longer with us. However, in the essays they have left we have excellent reminders of their scholarly achievements. Eric Aiton's account of the competing claims of Cartesian vortices and Newtonian celestial dynamics is characteristically based on close textual analysis, including manuscript as well as printed sources. He traces the reception of Newton's ideas through reviews of the *Principia* and subsequent publications of leading proponents and opponents, particularly, as might be expected, Leibnitz, identifying the void and attraction as the chief obstacles to Cartesian acceptance of the Newtonian system. However, we learn that the recognition by the ablest Cartesians of the explanatory power of the Newtonian system led them to combine Newton's mathematical theory with physical vortices, before finally abandoning vortices in favour of universal gravitation (their consciences being calmed by such even-handed statements as that by Maupertuis on the equal unintelligibility of impulsion and attraction!). This leads into Seymour Chapin's account of debates over the shape of the earth, through which several notions pertinent to the development of planetary astronomy in the eighteenth century flow. It touches on physical measurement and increasing precision as well as the derivation of theoretical solutions from assumptions about the forces involved, nicely illustrating the main themes of the book as a whole.

The following two chapters, Craig Waff's essay on Clairaut and the motion of the lunar apse, and Curtis Wilson's on the precession of the equinoxes, move the story on both in time and in space. They also – partly through their structure – bring out the towering individuals of eighteenth-century astronomy: Clairaut, Euler, d'Alembert, Daniel Bernoulli and Bradley, among

others, take their place in the canon. The text also starts to become increasingly mathematical; perhaps this is inevitable, but it slightly calls into question the claim by the publishers that this volume should be accessible to the ‘interested layman’. In addition to the early signs of the mathematics to come, the section on the reception of Newton also includes contributions on the essential material for astronomical activity, and on one of the topics which pre-occupied all astronomers around the mid-century. The former, Eric Forbes’ and Curtis Wilson’s account of Lacaille’s solar tables and Mayer’s lunar tables, reminds the reader of the need for skills in observation and data reduction in addition to expertise in mathematical theory. The accuracy of astronomical tables in predicting the positions of astronomical objects was ever the essential test of their usefulness to astronomers, and Forbes and Wilson demonstrate effectively why the names of Lacaille and Mayer must be added to any roll-call of eighteenth-century astronomy: their tables represented a huge increase in accuracy over previous ones and, particularly in the case of Tobias Mayer, were based on a previously unmatched skill in manipulating data. Finally, and fittingly, in this section comes Craig Waff’s account of what Lalande referred to as ‘the most satisfying phenomenon that Astronomy has ever offered us’: the return of Halley’s comet. Waff takes us through Halley’s own prediction, subsequent constructions of comet ephemerides, and the final search which both established the comet as a permanent member of the solar system and provided beautiful confirmation of the Newtonian system.

After the excitement of the first section, the second, on eighteenth-century celestial mechanics, is perhaps more consolidatory in nature. Focusing on the efforts of five mathematical astronomers – Euler, Clairaut, d’Alembert, Lagrange and Laplace – Curtis Wilson and Bruno Morando give an account of the innovations and refinements introduced by these men to the three-body problem, to the point that the theory was so honed it was possible to predict planetary positions to within a few arc-seconds. Wilson’s chapters cover the contributions of the first four astronomers to perturbation theory,

with supporting roles played by Daniel Bernoulli and Boskovic. They are complemented by Morando’s detailed analysis of Laplace’s pivotal position, as the one who both synthesized previous achievements, thereby allowing Whewell to describe astronomy as the queen of the sciences, and defined the problems with which nineteenth-century celestial mechanics would largely be concerned.

Laplace’s legacy is revisited in later chapters. Before that several other major eighteenth-century achievements are analysed under the general title of observational astronomy and the application of theory. Included are measurement of the solar parallax and the discoveries of Uranus and the first asteroids. This period is the one in which the fundamental astronomical measurement, the astronomical unit (AU) – the mean distance between the sun and the earth – was tackled in some way or other by almost all astronomers. In a particularly well argued essay, Albert van Helden demonstrates how significant solar system events were used to provide crucial data from which the AU could be calculated. Especially important were the transits of Venus of 1761 and 1769, which were recognized as sufficiently important to persuade Britain and France between them to pay for six expeditions to observe them. As shown by van Helden these observations were made at considerable expense and personal sacrifice, and set a precedent for international scientific co-operation which has since become routine. The most celebrated event in eighteenth-century solar system history is then described by Michael Hoskin, who interestingly sets his account in the context of attempts to understand the number and spacing of the planets. Not, as is made clear, that William Herschel was preoccupied with such matters. But the chapter is structured around discussion of the Titius–Bode law of planetary distances, and it was certainly the search for the ‘missing’ planet between Mars and Jupiter which led to the identification of Ceres, the first asteroid, even if, as Hoskin shows, Giuseppe Piazzi like Herschel was at the time engaged in an entirely different enterprise.

The remainder of the third section turns to methodological issues, concentrating on orbit determination, the introduction of statistical

reasoning into astronomy, and the theory of errors. Here the story moves on again into the nineteenth century and a new leading cast of players is introduced, including Carl Gauss and Friedrich Wilhelm Bessel. The treatment of early statistical reasoning, by Oscar Sheynin, is rather cursory, although this is presumably a topic which will be considered in greater detail in other volumes of the *General History of Astronomy*. But taken as a whole these three chapters demonstrate most effectively how developments in the reduction of observational data were assuming increasing importance in the practice of astronomy, and how skills in the relevant methods were set to become the hallmark of leading astronomers during the nineteenth century. As F. Schmeidler says in concluding his discussion of the theory of errors, 'the great achievements of celestial mechanics in the nineteenth century...were all to involve arduous and painstaking application of the method of least squares' (p. 207).

The final two sections in the book are briefer and more synoptic than the earlier ones. In two of the three chapters constituting the two sections, Bruno Morando takes the story of Laplace's achievements on into the mid- and late nineteenth century. In the first, he describes the 'golden age of celestial mechanics', moving swiftly through the works of Hamilton and Jacobi, the discovery of Neptune, theories of the movement of the moon, planetary satellites, and finally the contributions of Henri Poincaré, whose death we are told marks the end of the 'triumphal epoch of classical celestial mechanics'. The last section provides an account of three centuries of lunar and planetary ephemerides and tables, and details of satellite ephemerides to 1900. In some ways these two sections are unsatisfactory, in that they are too brief and the book as a whole seems to peter out rather than finish. This is presumably partly attributable to its being just one part of one volume of a general history, and any verdict must take this into account.

This is not the book to consult for any broad historical interpretation. Apart from the occasional reference, for example, to the Seven Years War or to the significance of the Revolution and Terror to Laplace's career as a

scientist, the focus is entirely on the astronomy. Taken on its own terms, however, the book has much to offer. Although it would be a challenging read for the interested layman, students of both astronomy and history of science will find a wealth of useful information, presented on the whole in clearly argued, scholarly essays. As the book has been some time in production, the bibliographies are not always entirely up to date, but the suggestions for further reading allow those who wish to, to pursue particular topics. It is well indexed (once it is appreciated that the index is to both parts of the volume), and overall provides a good introduction to the subject of planetary astronomy in the eighteenth and nineteenth centuries.

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GORDON L. HERRIES DAVIES, *North from the Hook: 150 Years of the Geological Survey of Ireland*. Dublin: Geological Survey of Ireland, 1995. Pp. xi + 342, illus. ISBN 1-899702-00-8. £34.00, \$57.00.

Gordon Herries Davies (HD) has done it again! One might have supposed that his history of the Geological Survey of Ireland for its initial fieldwork and map publication from 1845 to 1890 – his *Sheets of Many Colours* (1983) – was the definitive work. But this is proved *not* to be the case by the present sumptuous production, with its considerable number of colour plates and profuse black-and-white illustrations.

Well, to some extent the author does go over ground that he has traversed previously, but the earlier book is devoted more particularly to maps and actual mapping, whereas the present volume tells the reader more about the internal workings of the Survey, its activities in economic geology and educational matters, and more particularly the personal relationships that obtained over the years between members of staff – the internal politics of the organization. And, of course, *North from the Hook* (so titled because the Survey's mapping began near Hook Head in County Wexford in the south-east of Ireland) carries the story well beyond the

confines of the nineteenth century and almost to the present – though HD is somewhat reticent about the work and personalities of those still alive, whereas he makes every effort to ‘tell all’ about the goings on of the earlier years.

To my shame, I have spent but twenty-four hours in Northern Ireland, and have never been to the Republic. So in one sense I am singularly unqualified to review *North from the Hook*. But perhaps not, for HD clearly has that mythic creature the ‘general reader’ in mind, and I think he would say that what he really wants to do is tell the world, even to the antipodes, about the history of Irish geology in an intelligible fashion. And surely this task has been accomplished in impressive – nay dazzling – fashion. So HD’s is not a book that will appeal to specialists alone, though the author’s erudition shines through, despite the fact that he has deliberately chosen not to weigh down the text with scholarly apparatus.

I rather envy HD his task. Ireland is a place of modest size. Its geology (so it seems from afar) is not inordinately complex. Go to London, Tokyo, New York, China. You will *never* really come to grips with such places. Or study the geology of Australia. You will never be able to get a secure handle on it, visiting all the interesting sites. But after a lifetime of study in geomorphology and the histories of geography and geology, with special reference to Ireland, I believe that HD really knows as much about his subject as perhaps anyone will ever know (unless we bring in geophysics and the complexities of petrology, geomorphology being his forte). How splendid to be in total command of a subject, instead of just skating over it! HD has, I believe, examined *all* the relevant archives, read *all* the relevant publications (most of which he seems to own), and walked over and closely observed *all* the relevant sites. That is something!

For many readers a great deal of what HD has to say must inevitably be new, for the Irish Survey was severed from its British parent in 1905, and thus the previous Survey histories of Flett, Bailey and Wilson leave us in the dark about the Irish Survey in the twentieth century, even for the limited periods that they cover. On the other hand, *North from the Hook* inevitably covers some of the same ground as *Sheets of*

Many Colours for the nineteenth century, as for example the notable work of Beete Jukes on the rivers of south Ireland, or the mistaken views of Edward Hull on the rocks of south-west Ireland, arising from the transfer of some of the Old Red Sandstone to Silurian. What is now offered is a much more detailed and intimate view of the work of the surveyors in their day-to-day duties, and the difficult conditions under which they often had to work, both in the field and at the headquarters, which was not even equipped with electric light until the Second World War. HD points out that the life expectancy of an Irish surveyor was less than it should have been, with the rough conditions and indifferent pay. But a rare photographic glimpse into a homely field residence of one of the officers (William Wright) in 1914 does not suggest circumstances that were too awful.

Regarding personalities, I read again with some astonishment of the behaviour of George Kinahan, particularly with respect to his Director, Hull. From HD’s account, it would appear that Kinahan was repeatedly insolent and uncooperative, would not conform to the official rock nomenclature, and was wont to criticize Hull’s ideas in print. It is well known that Kinahan was a thorn in the Survey’s flesh, but knowing more now I find it even more mysterious as to how he got away with it. I doubt that he would have done so if he had been working directly for Murchison or Geikie, but I suppose he was able to browbeat the less eminent and able Hull, who, incidentally, was quite willing to reciprocate Kinahan’s criticisms in print. It was all rather unedifying.

But leaving aside such questions, the remarkable part of the story, only told now in detail for the first time so far as I am aware, is the virtual collapse of the Survey after Irish Home Rule was eventually achieved in 1922. It was reduced to a mere rump, and achieved little in the 1920s and 1930s, with morale at a desperately low ebb and the organization’s pulse feeble. (HD frequently uses the simile of a pulse – strong or weak – to represent the activity and success of the Survey.) It was of course a difficult time for Ireland, and for the economy world-wide. But the lack of government support for the Survey was so striking that one might suppose that there was

some deeper reason of a political or ideological nature. One might have expected that with political independence achieved Ireland might have been keen to develop its geological knowledge, if only for nationalistic reasons. But the Survey may, as HD suggests, have been 'tainted'. During its preliminary nineteenth-century mapping it had not achieved much in the way of mineral discoveries, and the hoped-for coal deposits had not materialized (for although much of Ireland's base rock is Carboniferous the coals have apparently been stripped away by glaciation, leaving limestone behind). The Survey in the nineteenth century had ultimately been under English control, and there was a suspicion that it had not been as diligent as it might have been in hunting for economically important materials, as a kind of 'plot' to keep Ireland economically backward. There was, it seems, no truth in this, but it may have seemed to be so to the new, scientifically uninformed, Irish paymasters.

This raises the question of what one should do once a survey has been completed – and Ireland was, I believe, one of the first countries in the world to complete its initial survey. What should happen next? Should the Survey be wound up or wound down, or somewhat enlarged? If the last, some new role would be needed. The practice that has often been followed has been to turn the organization into a more general research institute, investigating problems in economic geology, and doing consulting work at home or abroad. Also, an ongoing educational role is important. Under the leadership of the diminutive but distinguished Grenville Cole, the Irish Survey began to do something like this after the completion of the preliminary survey, giving much attention to a major soil survey (published by James Kilroe); and water supplies were also examined extensively as piped water began to enter Irish homes.

Even so, after the death of Cole in 1924 it seems that the Survey languished desperately. HD represents this state of affairs as being due in part to the contingencies of Cole's demise and the regrettable resignation of the able Wright. But it was perhaps part of a wider decline of Irish science, which extended back as far as the 1870s, as the 'Anglo-Irish ascendancy' declined. HD adverts to this but holds back from analysing it

in detail. Was it really part of some much larger processes of change whereby scientific research in Catholic countries was declining at that time? I suspect so. And I suspect that HD suspects so. But he does not choose to pursue the matter in this book quite as much as the 'general reader' might wish.

Eventually, after a rather unsuccessful and little remarked directorship of Douglas Bishopp in the 1940s, things began to look up significantly; with the appointment of Murrough Vere O'Brien, and with the triumph of the discovery in 1956 of a major deposit of ores of zinc, lead, copper and silver at Tymagh in County Galway the old idea that Ireland was barren so far as economic geology was concerned was shown to be mistaken. (There is, by the way, no suggestion that the Anglo-Irish had deliberately kept quiet about Tymagh, though indications of the ore body had been found in the nineteenth century.) Subsequently, since the directorship of the South African Cyril Williams from 1967 to 1987, the Survey has gone from strength to strength, being properly funded and housed and entering once again the mainstream of geological research, with a huge increase in recruitment.

HD writes of all this, and much more, with exuberance, panache, verve, style and some wit. He makes pleasing little digressions, and introduces himself into the story on more than one occasion, telling the reader how he came by an important early book, or a rare early map; or what it feels like to be at a site where some important geological discovery was made. He is a confirmed Collingwoodian, placing himself (metaphorically) in the shoes of the people about whom he writes as far as and as often as possible (indeed I should not be surprised to be told that he has climbed into one or two surviving pairs of surveying boots, just to get the proper feel of them). This makes for the great success of this book. For so close has been the engagement of the author with his subject that he is able to tell his story almost as if he had been one of the members of the Survey himself, going about his business. The joy of the enterprise comes through. And this is why I, as a historian, envy HD for the task that he has undertaken and so successfully accomplished. I wish that I had had the opportunity to live the life that he has lived.

I had better get me to Ireland before I am a year older!

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DEEPAK KUMAR, *Science and the Raj, 1857–1905*. New Delhi: Oxford University Press, 1995. Pp. xv + 273. ISBN 0-19-563562-0. 425 Rupees.

Science and the Raj charts the development of Western science in British India from its origins under the East India Company through to the ‘high noon’ of imperial rule. Kumar shows how science was integral to the consolidation of British rule in India; both as a technical resource and as an ideology of Western domination. In the colonial context there were, according to Kumar, few opportunities for scientists to engage in original and independent research, since science was strictly subordinated to the needs of the colonial state – which were principally military and economic. For the same reasons, education in science and technology was conducted on a utilitarian basis, with Indians being educated to perform what were largely subordinate tasks. Nor did the few Indians who benefited from a first-class education in science have much opportunity to develop their careers. Even highly talented scientists, such as the chemist J. C. Bose, were given little encouragement in their research, and suffered the indignity of a professorial salary only half that of their European colleagues.

The book has five core chapters, the first being a survey of the early phase of colonial science, prior to the assumption of direct rule by the British government in 1858. Kumar then moves on to consider the organization of science in British India, scientific education, and scientific research. The book concludes with a valuable chapter on indigenous attitudes to science, which makes use of sources in the Bengali vernacular as well as English. Kumar argues – convincingly to my mind – that the diffusion of scientific and technical knowledge in British India was circumscribed more by the limitations of British educational policy than any deeply ingrained, cultural resistance to Western science. Indians of all religions were eager to learn about science,

and scientific societies and journals were established independently of Europeans.

Few scholars of science in British India would dispute Kumar’s claim that science was cultivated for material gain rather than the purposes of enlightenment, yet it is, perhaps, going too far to state that ‘Colonial pragmatism demanded the complete subjugation of personal view-points to economic interests’ (p. 231). There was more scope for independent research in India than Kumar suggests. Even in the field of medicine, which is held up as an example of intellectual stagnation in India, there were significant developments in the period 1857–1905. Apart from Ronald Ross’s work on malaria – which Kumar cites as the sole exception – important research was conducted independently of government, for example into leprosy (Henry van Dyke Carter), goitre and cretinism (Francis MacNamara) and deficiency diseases (Robert MacCarrison). Even before 1857, which Kumar sees as a time in which the medical sciences were particularly neglected, there was considerable activity in the field of medical topography (arguably one of the largest scientific endeavours of the period 1825–50), and medical and physical societies flourished in the major centres of European habitation. Zoology was also a far more lively field of inquiry than Kumar indicates. No mention is made of Joseph Fayrer’s contribution to the study of venomous snakes or his founding of the Calcutta Zoological Garden, for example. Much of this independent research was certainly utilitarian in nature, but the purview of colonial scientists often extended beyond the immediate concerns of imperial rule. It is important to remember that many of those actively engaged in scientific work in the colonies considered themselves to be part of an international community of scholars, and that they addressed current intellectual debates as well as the needs of the colonial state.

One also wonders about the appropriateness of Japan as a model for what could be achieved by Asian scientists untrammelled by colonial rule. Japan was invoked frequently by Indian nationalists and here, again, by Kumar, as an Asian nation which had cultivated science in the service of its people (for example p. 238). But, although Japanese scientists achieved a great

deal – particularly in the field of bacteriology – it is doubtful whether the Japanese population as a whole benefited from these advances any more than the indigenous inhabitants of colonial India. In Japan, support for science was just as utilitarian, and just as dependent on narrowly military and, ultimately, imperial, considerations as it was in British India.

The nature of science in India and other colonies is likely to be the subject of debate for some considerable time, and it is a measure of Kumar's contribution that *Science and the Raj* will provide a source of reference and inspiration for many years to come. The book is extensively researched and draws on a wide range of published and archival resources; one only hopes that future scholars will be as diligent. The substance of the book is marred only by some careless copy-editing, which has allowed more than the usual complement of typographical errors to slip through the net. It would be unfortunate if these were to detract from what is, by any standards, a major contribution to the history of science and empire.

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ALAN E. H. EMERY and MARCIA L. H. EMERY, *The History of a Genetic Disease: Duchenne Muscular Dystrophy or Meryon's Disease*. London: Royal Society of Medicine Press, 1995. Pp. xviii + 248, illus. ISBN 1-85315-249-8. £20.00, \$40.00.

Having spent their lives working on the genetics of Duchenne muscular dystrophy, it is easy for Alan and Marcia Emery to believe that the history of this disease is intrinsically interesting and deserving a lengthy account. Since no historian seems to believe the same, they have taken pen to paper to relate this 'wonderful story'. They have carefully identified all those who have written about this fatal condition since the late eighteenth century, drawing attention to the strikingly diverse backgrounds of these writers. They realise, of course, that,

medical history reflects changes in attitudes to disease as well as changes in knowledge. It

therefore inevitably involves considerations of social and cultural matters as well as the motivations of those involved. Apart from describing scientific developments in the history of Duchenne dystrophy, we have therefore included information about the individuals themselves who have made significant contributions to the subject [p. 5].

Unfortunately, since the individuals upon whom they have chosen to focus are those who have made 'significant contributions' to the authors' preordained linear history from the eighteenth century to the present, it is not surprising that 'no uniform picture' emerges from their effort to contextualize the history of this disease. Had they paid more than just lip service to the work of historians of medicine their search might have been more fruitful. In fact, *The History of a Genetic Disease* is bound to be quite a frustrating book for most readers of the *British Journal for the History of Science*, regardless of their specific interest in disease and heredity, or in science more generally. The following comments will seem somewhat intemperate, but they must be voiced because this book raises so many questions about the presently contested relationship between scientists and those who would critically analyse the latter's activities.

Notwithstanding their very interesting observations about the social networks within which their actors moved, Alan and Marcia Emery make no attempt to explain why these actors chose to investigate the fatal muscular degeneration that so interests them. They cannot fathom why the findings by the Englishman Edward Meryon (1807–80) that the condition was familial, findings which were first published in the *Transactions of the Royal Medical and Chirurgical Society of London*, received little attention and the Frenchman Guillaume Duchenne (1806–75) got the credit for the discovery. They refer to parallels with the now worn story of Gregor Mendel's 'pioneering' but 'ignored' genetic investigations, ironically citing Robert Olby who has done so much to explain why such 'unreproachable' work was dismissed by Mendel's contemporaries. They could have learned from Olby to consider both how Meryon's work on muscular dystrophy was related to his earlier, materialist work on the

Constitution of Man (1836), and what significance contemporary readers might have attached to the relationship. Perhaps it was more significant than Alan and Marcia Emery seem to admit that the *Transactions* was so 'scientific' that it excluded all 'phrenological observations' (p. 45). Perhaps they should have read Adrian Desmond's *The Politics of Evolution* before denying any link between society, culture and their actors' motivations.

One might reasonably argue that I demand too much by asking that contemporary medical scientists interested in the history of their area of research should show some interest in the work of historians of medicine or science. After all, as Lewis Wolpert has pointed out, we historians have such a funny habit of confusing politics and good science. Yet, as a historian of modern biomedical science, I am sufficiently modest to believe that I can learn quite a lot about my subject by listening to what scientists tell me (of course, I then reinterpret our discussions by deploying my own professional skills). Thus, I think I would have gained considerably more if Alan and Marcia Emery had tried equally modestly to provide the eyewitness rather than the historian's account, and had also written more widely about research on hereditary diseases. It does not seem implausible that they were aware of such research, especially since Alan Emery studied with Victor McCusick, at Johns Hopkins University, which had been for a long time a leading centre for research in medical genetics. Even more frustratingly, Alan Emery has been a leading figure in the development of facilities for genetic counselling, but he tells us virtually nothing about this. It is then very difficult for the ignorant historian to understand if and how the story of Duchenne's muscular dystrophy differed from that of other hereditary conditions such as phenylketonuria, conditions centrally involved in geneticists' apparent shift since the Second World War from eugenic to therapeutic goals. All that there is to be gained from *The History of a Genetic Disease* is then the numbing bibliography on what has been written about the genetic nature of the disease; and a properly critical history of medical genetics, including counselling, still remains unwritten. There are, of course, important

reasons why this history should be written which transcend this historian's or these medical scientists' partisan interests. We need this history if we want to understand whether or not the recent accomplishments of 'reverse genetics', like the tracing of the pathway from genes to the tragic symptoms of Duchenne muscular dystrophy, are bringing us to the threshold of a 'new eugenics'. Does the future hold in stock more screening and 'selective abortions', or kits to fix any 'defective' genes?

In sum, it is time that scientists and historians started to engage one another in a more constructive manner, reading, rather than ignoring each other's work, and the Royal Society of Medicine, who published *The History of a Genetic Disease*, could do much more to encourage such engagement.

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FREDERIK NEBEKER, *Calculating the Weather: Meteorology in the 20th Century*. San Diego: Academic Press, 1995. Pp. vii + 255, illus. ISBN 0-12-515175-6. \$64.95.

Before the twentieth century there were three, largely separate, meteorological traditions. People working in an empirical tradition observed and collected weather statistics, theoreticians 'made the law of physics their starting point and established, as a branch of science, dynamical meteorology', whereas forecasters, often based on little data or theory, attempted predictions. The argument of Nebeker's well-structured history of modern meteorology is easy to state: these three traditions were unified in the middle of our century, and the agent of change was the electronic digital stored-program computer.

Locating a pivotal moment in the use of the stored-program computer is actually surprisingly rare within the history of computation-based sciences: the role of electronic computers is often downplayed and pivots found in turn-of-the-century business information technologies. Indeed, Nebeker provides interesting discussions of the use of earlier information technologies within meteorology: punched cards, tables, nomographs, slide rules, and so on. There is also, at

times, a fine sense of the interplay between theory, observation and organization, for example Nebeker's account of the calculational teamwork imagined by Lewis Fry Richardson, a Quaker pacifist who resigned from the Meteorological Office when it was taken over by the Air Ministry. In this case, the development of equations connected to designs of practical Meteorological Office organization, new ways of gathering data, and, completing the circle, further theory development (on vertical air flow and turbulence).

However, I have two criticisms. First, apart from a few individuals (Richardson, Vilhelm Bjerknes and John von Neumann) historical actors are two dimensional. Very little biographical context is given, and actors, in general, are treated primarily as *authors*. The impression is occasionally given that the history of meteorology can be entirely captured through what its practitioners reported and wrote. An epiphenomenon of this authorial bias is that often first names appear only as initials (for example p. 51, where six meteorologists, including G. G. Coriolis, are introduced in this manner).

A longer text would doubtless have allowed this criticism to be corrected (indeed much interesting material appears in the footnotes). However, my second criticism is more fundamental. Nebeker gives technological change a very strong, almost determining, role in his account, no more so than with electronic computers: 'although other factors, especially increasing support for meteorology, were important, the timing and pervasiveness of the transformation were largely determined by the development of computing technology'. The uptake of computers does indeed need explanation: in 1946 a major meteorological conference agreed that no mechanization of forecasting was likely in the near future, yet daily forecasts were under way at the United States Weather Bureau by 1955. Nebeker traces this change through the convincing success of the von Neumann-inspired Meteorological Project to devise algorithms for use on early computers such as the ENIAC. Nebeker too easily concurs with the promoters of the new numerical style of meteorology (such as Meteorological Project director Jule Charney) that the change was due

to computers, and even ascribes the beginnings of the 'redirection' to the 'mere prospect of computers'. Now there is no doubt that a numerical approach *did* rapidly dominate meteorology and that this change involved the spreading use of digital stored-program computers, which could handle more data and calculate forecasts in real-time. However, I will point to two undeveloped avenues which might qualify this technological 'impact'. First, Nebeker remarks in passing that 'the similarity of...Soviet and...non-Soviet work provides additional evidence for the effect of computers on research'. However, this argument is based on a report from 1960 by American meteorologists, and it is not clear how reliable they would be in identifying and presenting differences (a clear example of the need to embed authors in context), nor is it convincing from the evidence given that Soviet meteorology was indeed independent. Secondly, Nebeker gives no account of opposition to the numerical transformation of the discipline. He notes that 'the new style of meteorology required skills different from those meteorologists traditionally possessed', and histories of other disciplines would suggest this would be enough for conflict. Indeed, Nebeker mentions elsewhere that the formerly influential Bergen School 'benefitted hardly at all from the new computational power'. Another possible site of conflict could have been with users of analogue computers (declared as quickly 'obsolete' by Nebeker as part of the abandonment of other computational aids in the face of the digital computer). We know, through the work of James Small, that analogue computers should not be seen as a wrong turning in technological evolution, doomed by the invention of the digital machine. Instead, they were promoted by a different social group (often engineers) who contested the claims for superiority put forward by the digital camp in the 1950s and 1960s. The analogue computer offered speed (cited by Nebeker as one reason why *digital* electronic computers were adopted) and hands-on interactivity (surely invaluable in modelling and simulating a complex system such as the weather).

However, these criticisms should not deter historians of twentieth-century science and tech-

nology from reading *Calculating the Weather*. Nebeker is correct in stating that a history of meteorology has a general importance, not least because of the associated development of numerical methods and the computational techniques of modelling and simulation.

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DANIEL CREVIER, **AI: The Tumultuous History of the Search for Artificial Intelligence**. London and New York: Basic Books, 1993. Pp. xiv + 386. ISBN 0-465-02997-3. £17.99, \$27.50.

This book is a very good description of the history of the field of Artificial Intelligence (AI) and is based on extensive personal interviews with a number of prominent members of the field. Edward Feigenbaum, Marvin Minsky, Hans Moravec, Roger Schank and David Waltz all give the book strong compliments.

Crevier leaves little out in his history of the field and focuses on important players such as Leibnitz, Boole, Shannon, Turing, McCarthy and Schank, and significant events such as the 1956 Dartmouth (USA) Conference where John McCarthy first named this field. I like the way Crevier takes us through the development of AI from Boole right through all the thoughts of philosophers such as Frege and Russell up to the present day. It turns out that many of the big name philosophers and early computer scientists were motivated in their work by the possibility of an artificial intelligence. Crevier's gradual development of background history makes the book. The book discusses expert systems, robotics, 'smart' technologies, chess-playing, language processing, learning and connectionism. We have twelve chapters and an introduction which cover topics such as early computer architecture, the first AI programs, the golden years of AI, programming micro-worlds, knowledge representation, chess-playing, cognitive science and prospects for the future. I tried to think of something which Crevier has left out and could not. He is easy to read and he himself has a Ph.D. from MIT and teaches electrical engineering at ETS, the University of Quebec's School of Engineering.

There are a few things I could take exception to in the book. In general Crevier explains many of the concepts which are introduced so that the reader does not need much background knowledge. However, on (p. 4) in the introduction the 'mind/body problem' is mentioned without any explanation of what it is. Also, I was surprised to hear that Roger Schank is 'Tall, impressive, and bearded. Schank wears an expression that often suggests both the severity of a Jewish prophet and the peaceful sagacity of an Indian guru.' (p. 165). I am not sure what Crevier means by 'tall' here but I certainly would not describe Schank as tall in height, although, certainly, he is in terms of academic achievement.

While talking about clouds on the horizon of AI Crevier says that 'Few psychologists know how to program a computer, and the world chess champion is still a human being' (p. 109) in response to Herbert Simon's prediction 'That within ten years most theories in psychology will take the form of computer programs, or of qualitative statements about the characteristics of computer programs' (p. 108), but then he says of psychologists that 'The first one [new instrument] is a tool for expressing many of their theories in unambiguous terms: the computer program' (p. 247) and he stresses the use of computer programs in general as tools for psychologists. Hence, we have a contradiction here and it is due to Crevier's confusion (and one which is common in the computing world) between psychologists 'being' programmers and psychologists 'using' programs.

I am always worried about descriptions and that they are made clear in texts, and have been perusing over 'Anyone who has had a hand at programming computers knows about program loops. They occur when a weakness in programming logic causes the computer to repeat the same steps endlessly' (p. 251). However, what Crevier means to say here is that 'bad', 'unwanted' loops are the ones which are usually a weakness and good ones are fine!

As with any book there are typing errors. We have a typo with 'then' in the following sentence: 'Knowledge could also consist of procedures for applying the rules to the data structures, or for figuring out which rule to apply when more than one fit the situation'

(p. 79). 'Another parsing' seems weird in 'Checking with the figure, semantics would have revealed no blue pyramid on a blue block, and instructed syntax to try another parsing' (p. 100) and should probably be 'another parse'. Also, we have 'Early AI work was deliberately performed in toy task that did not require much special knowledge' (p. 308). We have 'Far from ossifying society, the codification of knowledge into expert systems, and their participation in routine decision making, may smoothe social intercourse' (p. 337). Here, the word 'smoothe' is certainly onomatopoeic but it is not clear that

it is meant to be so! However, all the good sides of Crevier's book far outweigh these minor glitches and I will certainly recommend the book for any introductory AI course.

Finally, AI has moved on since Crevier's book was written, and now we have major moves towards convergence and integration of important subfields such as language processing and vision processing. Such work is important for the construction of the future of SuperInformationHighways.

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