

Simon Schaffer

**FROM PHYSICS TO  
ANTHROPOLOGY  
-- AND BACK AGAIN**

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**Simon Schaffer**

*Department of History and Philosophy of Science  
University of Cambridge, U.K.*

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## ABOUT THE AUTHOR

Simon Schaffer was exposed to prickly pears at a tender age in Australia. He is now Reader in the History and Philosophy of Science at Cambridge University, where he tries to bridge the gap between Britain and Germany, as well as between his own and neighbouring disciplines. He is working with a number of others on the emergence of laboratory science 100 years ago. Simon Schaffer likes museums, anthropologists and footnotes. One of his major publications (with Steven Shapin, *Leviathan and the Air Pump*, 1985) is concerned with the historical anthropology of the experimental life.

The present pamphlet is of obvious interest to anthropologists because of its focus on Rivers and Boas. But its appearance so early in the series is an indication of our editorial commitment at Prickly Pear Press to exchange across the boundary separating the academic disciplines of anthropology and the history of science. Intellectual renewal at this time depends on excavating the roots of twentieth century knowledge and extending enquiry beyond the limits of professional specialisation within the universities. Much of this agenda is contained in embryo in Simon Schaffer's strikingly original essay.

## FROM PHYSICS TO ANTHROPOLOGY AND BACK AGAIN

Technique is an action which is effective and traditional. There is no technique and no transmission in the absence of tradition"

(Marcel Mauss, 1934)

How can we recognize the shackles that tradition has laid upon us? For when we recognize them, we are also able to break them"

(Franz Boas, 1938)

### *A Physicist in the Field Cambridge, July 1896*

A scene of traditional field technique: on a July day in 1896 an English zoologist and a physicist from the South Seas set out together from Cambridge to the village of Barrington, five miles away. They take cameras, calipers and record cards. They interview, measure and photograph as many of the villagers as they can, they take pictures of children playing, and late in the afternoon go back to town. The zoologist is delighted, makes notes on string games and skull sizes; the physicist less happy, almost sneering. "You can't imagine how slow-moving, slow-thinking the English villager is. He is very different from anything one gets hold of in the colonies". Thus the quick-thinking New Zealander Ernest Rutherford, recently arrived at the Cavendish Laboratory, already established in one of the world's most famous laboratories as an expert on radio waves and the new techniques of X-ray photography. His companion was Alfred Haddon, who combined Dublin-based zoology work with the administration of an anthropometric laboratory there and lectures on physical anthropology in the Cambridge anatomy department. The Barrington trip was part of a major and eventually hopeless attempt to complete an ethnographic survey of the races of Britain

sponsored by the British Association for the Advancement of Science between 1893 and 1897. Cambridge students, like Rutherford, were drafted onto the project. Folklorists, anthropologists and museum collectors could not easily collaborate on this massive task. Haddon learnt his lesson. His famous 1898 expedition to the Torres Straits was designed as an exemplary and thoroughly professional exercise. In his classic study of *Evolution in Art*, a pathbreaking essay on Melanesian design published a year before his visit to Cambridgeshire villages, Haddon highlighted these troubles of specialism and expertise in moving from the laboratory to the field. He confessed that he was neither "an artist nor an art critic, but simply a biologist who has had his attention turned to the subject of decorative art".<sup>1</sup>

I am neither a fieldworker nor, as yet, a subject of fieldwork, but an historian who has had his attention turned to anthropology. A **turn** is a performance, a change of direction, a revolution, a temporary attack of giddiness. My turn involves a journey from my normal home, the history of the experimental sciences of the later nineteenth century, to more exotic climes, the myths and customs of early fieldwork in the strict sense of the term. When anthropologists reflect on their own discipline they use terms designed for the analysis of other cultures to define their own. Half a century ago Goldenweiser compared the culture hero Franz Boas to the "supernatural animals or birds who bestow culture upon man", beasts found in traditional mythologies. George Stocking has described the debate between Malinowski and Radcliffe-Brown as "a theoretical falling out between two lineage elders". Clifford Geertz reports reaction to the publication of Malinowski's diary as "a minor scandal which erupted in anthropology: one of its ancestral figures told the truth in a public place". James Clifford writes that museum objects should be returned to "their lost status as *our* fetishes".<sup>2</sup> Founders of the discipline are the ancestors; schools of training and method are represented as complex networks of kinship and tribal rivalry.

The stories of disciplinary origin, in this case tales of early fieldwork, are routinely displayed as heroic myths told by the elders to initiate to explain the meaning of otherwise incomprehensible customs. Making Rivers, not Malinowski, the founder of British anthropological fieldwork is an exercise in genealogical story-telling. Stocking once described "Malinowski's enactment of Rivers' program" as a "mythic transformation". In the inaugural *Prickly Pear* pamphlet Anna Grimshaw and Keith Hart have shown us why these stories matter, why these figures and their values should be re-counted. It's an old theme in this town. In a stout defence of Malinowski's originality against that of Haddon and Rivers, published in *Cambridge Anthropology* fifteen years ago, Paul Jorion pointed out that battles about such origin stories help define the very content of the discipline. Historians of the natural sciences, chroniclers of Rutherford's physics for example, are (to put it mildly) less self-conscious about the spontaneous myth-making involved in all tales of heroic founders. More's the pity. The impressive reflexivity with which anthropologists speak of themselves helps focus the status of the enterprise they profess.<sup>3</sup> My purpose in this pamphlet is to probe a few aspects of this enterprise and to offer some comments on the disciplinary history upon which it depends.

Historians of nineteenth-century laboratories will be fascinated that the prickly pair of field workers, Franz Boas and William Halse Rivers, were initially trained in a highly specific form of laboratory science. Before his first field trip to Baffinland in 1883, Boas completed a physics PhD at Kiel on the absorption of coloured light in distilled water. Then in 1882 he worked in Berlin on problems in psychophysics and physiology with the leading anthropologist and cell physiologist Rudolf Virchow. He was taught astronomical mapping at the Berlin Planetarium and learned how to estimate subjects' perceptions of colour. Similarly, before Rivers accompanied Haddon to the Torres Straits in 1898, he worked as an experimental neurologist and psychologist in

London with Hughlings Jackson. He studied laboratory physiology and psychology in Jena with Binswanger (where he decided that "I should go in for insanity when I return to England") and collaborated at Heidelberg with Kraepelin on fatigue measurement. By 1897 Rivers was in charge of both of England's experimental psychology courses: at University College London and in Cambridge. In the Torres Straits, he conducted celebrated trials on the islanders' skin sensitivity and their response to colours and visual illusions. Much has been written of the turn to fieldwork and away from speculation. In 1905 Haddon told the British Association of a simultaneous shift in zoology, psychology, sociology and ethnology away from the world of the "arm chair philosophers".<sup>4</sup> Some of the most eminent inquirers who began empirically to study indigenous peoples in the name of ethnographic science did not arrive in the field from their armchairs, nor from their verandahs, but from their laboratory benches.

The title of this pamphlet is shamelessly adapted from George Stocking's rich 1965 essay, "From Physics to Ethnology", a study of Boas's turn from experiments on optical perception and physiological geography to anthropology in the early 1880s. Stocking dramatically contested a salient feature of the myth: the representation of field work as a conversion experience, a moment of supreme revelation and vocation. Instead, he showed that Boas's turn from laboratory physics to field ethnography was gradual, continuous and explicable: "there was no sudden realization of the significance of culture....His viewpoint developed slowly...from his total life experience".<sup>5</sup> Stocking's holistic explanation of Boas's move to the field and to the relativist model of culture is compelling. Experience of laboratory science is treated by Stocking as a source of intellectual matter, of Boas's concerns with varying perceptual capacity between subjects, and of his interest in problems of causal explanation. Here I wish to complement this account with a consideration of laboratory life as culture. In other words, I wish to discuss the kind of *work*

which laboratory investigation involved and trace the implication of this work for its extension to the field.

In our own culture, the laboratory is treated as a privileged place where nature speaks in a remarkably immediate manner. There's a puzzle here: because labs are such peculiar places, organised so carefully and stocked with such dedicated techniques, the work done there tells us how things are everywhere, anywhere, always and doesn't depend on the peculiarities of the lab itself. It for this reason, perhaps, that the work done in labs has often been neglected or else taken for granted. This, I'll suggest, is my own discipline's form of reflexivity, learnt directly from anthropologists: the subjection of the laboratory to field study. In recent science studies, ethnographies of the laboratory, the museum and the observatory have become common. Scientists have become subjects. Attention to texts has been displaced by an attention to practices. The work of science is seen as a complex network of practical activities which engage with the natural world, rather than the formation of a set of theoretical propositions checked by the simple observation of that world. So the methods of science studies have changed, and often use the styles of fieldwork to enter the laboratories and field stations of the sciences as participant-observers, where analysts record and meditate upon their own encounter with scientists in their workplaces.<sup>6</sup> Metaphors of fieldwork are ubiquitous. For example, the work of replication, which is fundamental to the establishment of facts in science, is seen in terms of the links between spatially separated laboratories and the means scientists use to make a successful device work elsewhere. Maps of science and networks of skills and techniques have become the principal motifs of recent work. The aim is to show that the obviously universal grip of modern science is the result of a process which deliberately multiplies the places where scientific techniques can be made effective, rather than the result of an historically cumulative and inevitable advance of understanding.

This approach has immediate relevance to the task this pamphlet sets itself. For a very long time, historians and philosophers of science took physics as the exemplary form of knowledge and they took the work of the physics laboratory to be the model for the means scientists use to find out about the world. If anthropological approaches to the natural sciences were used at all, they were based on functionalist models in which some highly abstract cosmologies were related to extremely crude ideologies. There wasn't much attempt to look at local techniques or cultures of the workplace. Rather weirdly, fieldwork was seen as an imperfect form of life which aspired to the status of physics. Cambridge, as usual, had its own protagonists of this view, notably William Whewell, inventor of the word "scientist" and (amongst many other splendid achievements) mid-Victorian founder of the Natural Sciences Tripos, who saw all sciences save mechanics and optics as in a state of advance towards the ideal these "pattern sciences" set<sup>7</sup>. One of the most interesting recent changes in science studies has been a dramatic inversion of the hierarchy. Fieldwork is now taken as a pattern for analysing laboratories, both as a method of study and a good description of what happens there. Historians of science have not, so far, done much to explore the roots of this shift in their own work. This is why the links between laboratory sciences and early anthropological fieldwork look so fascinating. Existing studies already promise a great deal. Stocking paid very careful attention to the practical philosophy which Boas learnt in the laboratory, in particular, the lesson that routine measurements seemed to depend on the experimenter's subjective judgment. Commentators on Rivers such as Slobodin, Langham and Kuklick have documented his laboratory work on experimental psychology in great detail. Their work provides a great deal of evidence that lab techniques can be directly connected with the strategies of fieldwork<sup>8</sup>.

These suggestions deserve exploration. In what follows, I shall concentrate on the historically specific laboratory culture of the late nineteenth century. I should emphasise at once the peculiarity

of this laboratory system: in the nineteenth century sciences, especially those committed to exploration of varying fields and analysis of their populations, the museum, hospital, clinic and observatory were at least as important.<sup>9</sup> Towards the end of this pamphlet, the museum and the hospital will be given some attention. But of course, the laboratory has already, and too often, been used as a tribunal against which to judge fieldworkers' alleged failings. In Marvin Harris' well-known critique of Boas and his works, we were told that Boas's interest in psychophysics "was nothing more than an attempt to provide a laboratory basis for Kant's ideas" in the wake of the Germans' "great advances in the laboratory sciences". Harris argued *contra* Stocking that the Baffinland trip *was* a drastic conversion whose effects were delayed only because of Boas' search for a geography job in Berlin. Above all, he reckoned Boas the fieldworker was "good about the wrong things" because of his absurdly exaggerated inductivism. We all now know that no laboratory scientist is a naive fact accumulator. "Under modern laboratory conditions...there is less opportunity to confuse mere induction with science", Harris observed. Each contact during "every minor field trip" has the same status as "hundreds of hours of time with the Brookhaven cyclotron or the 200-inch Palomar telescope". How does an encounter with a Kwakiutl story-teller turn into a million-dollar physics experiment? Harris used a commonsensical story about laboratory work to damn those anthropologists who seem to behave differently in the field. Others use the same story to make the field less important. Take Ian Jarvie, whose 1964 book, *The Revolution in Anthropology*, involved a philosophical enquiry into functionalist field methods. Jarvie presented himself as a heretic. He argued that fieldwork, the initiation ritual of his tribe, should no longer be compulsory. Jarvie's own work was "my apology for not yet having left the verandah." He frankly and and embarrassingly charted the reasons proffered in fieldwork's favour: a rite of passage for the profession, a means of studying vanishing tribes, an opportunity for wealthy foundations to engage in visible charity, a challenge to the apparently self-evident

conventions of one's native culture. Jarvie dismissed each of these, principally by appealing to Popperian method. He claimed that in the sciences theories were not constructed by the simple-minded accumulation of data and the induction of some notable result. Instead, inquiry must proceed by the formulation of bold, testable conjectures and their subsequent ruthless criticism. Such criticism might involve empirical inquiry: it need not require such inquiry by every single scientist. Hence some, but by no means all, anthropologists should engage in fieldwork, "the observational factual basis of scientific social anthropology". I am not here concerned with the virtues of Jarvie's proposal. I am fascinated by his brief and unexamined claim, formally identical to that of Harris, that "fieldwork has the same functions as laboratory experiments in science".<sup>10</sup>

Neither Harris nor Jarvie interrogated the course of laboratory experiments. They assumed that such experiments worked just as the ideal of bold conjecture and ruthless criticism suggested. But what happens if we concede the relation between experiment and fieldwork and then take seriously more recent, sociologically informed, studies of laboratory life? This is to follow up a remark of Clifford Geertz, who backs his summary of the problems affecting empiricism and realism in fieldwork with recent analyses of experimental science by T.S. Kuhn and Mary Hesse, in which both inductivist and Popperian models are fiercely challenged.<sup>11</sup> I'm not going to use a self-evident model of laboratory life. Instead, by turning to Heidelberg and Cambridge, Vancouver Island and Murray Island, I'll try to evoke some of the most important features of that strange new world of the fin-de-siècle. I want to use these travels to comment on reflexivity. Nowadays fieldwork is supposed to reveal at least as much about the fieldworker as it does about the cultures the fieldworker studies. This turns into a general claim about western science. Thus Martin Bernal movingly displays the racist academic polity which sustained the construction of a science of the ancient world around 1800; Edward Said stresses the "exteriority" of Orientalism. It "has less to do with the Orient

than it does with 'our' world".<sup>12</sup> There's something liberating about this urgent self-obsession. But there's also something very troubling here. A commonplace of late nineteenth century laboratory life was self-experiment. Boas and Rivers, for example, ruthlessly tested their own responses in order to tell scientific stories about culture and subjectivity. They did not stop doing this outside the laboratory. A challenging feature of these early anthropologists was their careful exploration of the role of the investigator's self in the making of knowledge. They often highlighted the culture and the techniques of the inquirer. But this crucial enterprise was systematically suppressed and then denied in the immodest and ferocious realism of subsequent fieldwork. Perversely, both the realists and their critics, the recent apostles of exteriority, have something in common. They share a modernist precept. They all assume that reference to the inquirer's self helps subvert the progressive potential and the intriguing authority of empirical studies. It's not so, and Boas and Rivers help us see why.<sup>13</sup>

### *The Shackles of Tradition Heidelberg, April 1877*

On 23 April 1877 the teenager Franz Boas attended his very first lecture from the Heidelberg chemist Robert Bunsen. "It is still a very beautiful feeling to have sat at the feet of such a master and hear his words": characteristic hero-worship from a young student in the early years of the *Kaiserreich*. Bunsen was a sixty-five year old representative of the pre-eminent German generation of scientific triumph and political compromise. For these frock-coated bosses of the bright, overcrowded institutes of late nineteenth century science, many radical ambitions had been destroyed in the wake of the failure of the revolutions of 1848 and the establishment of Bismarckian hegemony in 1871. Natural scientists forged an ideology of material interests, marrying their ambition for expanded research and teaching provision within the universities to the aims of industrial development and rationalised



imperial power. Boas, in complete contrast, would recall his upbringing in a "German home in which the ideals of the revolution of 1848 were a living force"; he would recall, too, the dreadful and "unforgettable moment" when a theology student, one of his university friends, told him that no-one had "the right to doubt what the past had transmitted to us". Heidelberg had among its students one of the highest proportions of aristocrats, the conventional repository of imperial tradition. Kiel, where Boas followed his professor Theobald Fischer from Bonn in 1880, had one of the lowest. Boas temporarily conformed to the values of the violent student world, bore the scars of its duels. But as a Jew, recognisably an "inappropriate" member of this ghastly culture, he paid the price of academic antisemitism. These scars stayed with him. In 1891 a Massachusetts paper warned its readers against allowing their children to be stripped bare and measured by this "alleged anthropologist" with his "visage seamed and scarred from numerous rapier slashes". The rich seams of resistance to militarism and imperialism helped sustain Boas' fierce antiracism, his eventual endorsement of democratic socialism, his protests on behalf of academic freedom.<sup>14</sup> In Germany compromise had dominated his university career, or so he reckoned. He wrote of a compromise between the inevitable materialism of the laboratory physicist and the fascination of the subjective life; intellectually and politically, a very fragile balance worked out through years of field work and museology between the claims of material interest, scientific racism, and the free spirit<sup>15</sup>.

In early 1877, as Boas reached university, Nietzsche composed his handbook for free spirits, *Human, all too Human*. The essay was prompted by conversations with another German-Jewish psychologist, Paul Rée. Recuperating from syphilis and on leave from his prestigious university chair, Nietzsche responded with energy to the claims of laboratory science: "mankind cannot be spared the horrible sight of the psychological operating table, with its knives and forceps". The laboratory, he predicted, would replace the philosopher's study as the source of knowledge about human

nature. "Now that science rules which asks after the origin and history of moral feelings and which tries as it progresses to pose and solve the complicated sociological problems".<sup>16</sup> This was the heroic, potentially disastrous, predicament of young scientists in the 1870s. The laboratory milieu of physics, physiology and psychology were unprecedented social formations. New scripts, new roles. Before the second quarter of the nineteenth century, there were no organized experimental institutions of this kind. There were no laboratories where experimenters were deliberately and painstakingly trained in technique. There were no large arrays of students and technicians, assistants, support staff and masters, who collaborated in rather complex hierarchies to produce knowledge of natural phenomena and human behaviour. There was no bulk production of the host of instruments, devices and machines designed to measure and invigilate this range of phenomena. Changes in training and practice accompanied these new scientific disciplines, and prompted new tasks for inquirers and subjects, psychological observers and performers, physiological experimenters and victims among them.

All these resources and practices were developed, initially in the German lands, during the mid-nineteenth century. The first teaching laboratories in physics were established in cities such as Berlin and Heidelberg. The first physics lab in Britain was set up in a Glasgow cellar in the 1840s; Cambridge founded the Cavendish in the early 1870s. New laboratory regimes had to be painstakingly constructed. After 1848 Heidelberg became emblematic of this development. There Bunsen was provided with a new chemistry laboratory; Kirchhoff, his most famous colleague, helped set up the Friedrichsbau, a superb research and teaching institute which by juxtaposing labs for physiology, physics and other sciences soon came to symbolise the unification of the sciences. Bunsen and Kirchhoff made their name and that of their institution with spectroscopy research, bringing laboratory trials on optics and chemistry to the very centre of modern natural science. Every summer, the laboratory was used by students to

design their own experiments. Just before Boas arrived, Kirchhoff left for a more prestigious post in Berlin. So did Hermann Helmholtz, hired at Heidelberg in 1858 and physiology professor there for fourteen years. It was during these years that Helmholtz helped invent laboratory training in physiological psychology. He was the dominant figure in the group of natural scientists who set out to make the failure of the 1848 revolution into a triumph for laboratory technique, exact measurement and politico-economic power. He worked with his students in the Friedrichsbau on problems in the psychology and physiology of sense perception, devising new instruments for measuring perception and its defects, and lecturing on the philosophical, practical and political connexions between physics and aesthetics. The theme was the extension of secular power. He told his Heidelberg students these links would "give intellect power over the world".<sup>17</sup>

Whose intellect? Which world? This power was hard to establish. Kiel University, which Boas left in 1881, was a rather different symbol of the alliance of power and the scientific intellect. Kiel was annexed by Bismarck for Prussia in 1867 and the city's university became a testing-ground for university reform. In 1870, the Prussians backed a Commission to manage scientific study of "the German seas", run by an alliance of businessmen and scientists, including Boas's professors Karl Möbius and Gustav Karsten. The Kiel Commission became a very significant base for chains of experimental field-stations, scientific voyages and economically useful laboratory work. There were continuing contrasts between lab provision for different sciences, physical, physiological, oceanographic. Distinguished physicists did spend time in Kiel: in 1882 Heinrich Hertz was hired as theoretical physicist and he was succeeded by Max Planck in 1885. But Hertz had to build a laboratory at his own expense in his own house. Before this, the sole experimental physicist there was Karsten, trained in physics at Berlin in the 1840s, alongside the future leaders of physiology and experimental physics such as Helmholtz and Virchow. In Kiel Karsten set up a small laboratory which

held very few students -- it was barely more than a small room set apart for the use of precision measuring equipment. Less than a dozen students ever attended his experimental lectures.<sup>18</sup> Laboratory training was a new ritual which could never completely specify the way its performers behaved. Initiates were often left to their own devices and to those on the laboratory bench. Support staff were crucial, but were never granted the status of scientists. They were only visible when things went wrong. In 1896 the tyro Rutherford made a typically revealing remark about the Cavendish Laboratory's brilliant technician Edward Everett: "the best glassblower in England....but of course he doesn't understand the theory very much". Gentlemen learnt how to value, and ignore, their servants. Training stressed that the physicist's own senses, an ability to discriminate colours or sounds, for example, were a crucial source of precision measurement. Investigation of human sensory capacity was ineluctably linked with the attempt to make a quantitative laboratory science. This explicitly connected the work of the laboratory physicists with that of the new experimental physiologists and psychologists.<sup>19</sup>

Physiology laboratories were equally recent. Physiology institutes were initially set up by Ludwig at Leipzig and Donders at Utrecht. Both men were also sponsors of early psychological work. The physiology institute at Kiel was in direct competition with that of Ludwig. The first Cambridge physiology laboratory was established by Michael Foster during the 1870s, where Haddon was one of the first students and where, in 1897, Rivers was hired to teach physiological psychology after spending time working at Heidelberg. The earliest psychology laboratory was that run by Wilhelm Wundt at Leipzig from 1879, where electromagnetic hardware was deployed quantitatively to estimate reaction times and sensory responses of human subjects. Most later psychology laboratories were explicitly modelled on this Wundtian prototype. In Kiel, Boas worked on problems in the threshold of sensory perception using these techniques. The new labs in engineering, physics, physiology and psychology became breeding grounds for

new kinds of professional. An example: between 1893 and 1918 the chief of state-funded precision measurement in the world of German engineering was a Heidelberg graduate, Ernst Hagen. Like Boas, he sat at the feet of Bunsen in the 1870s. He became Helmholtz's assistant in Berlin in 1877, he travelled to North America (but to study Edison's electric light bulbs, not the culture of the Inuit or the Kwakiutl), he worked at Kiel as a naval physicist, he possessed "amazing business skills". Or take Karl Brandt, trained at Berlin under the physiologist Emil du Bois Reymond in the 1870s. Emulating the celebrated scientific *Challenger* expedition of 1873-6, which included the first ethnographic contact with Torres Strait islanders, Brandt became an expert marine biologist who was hired as zoology professor at Kiel in 1888 and from then on ran much of German marine science, including supervising the research associated with the building of the militarily decisive Kiel Canal. Brandt turned his institute into a clearing house and laboratory tribunal for world-wide oceanographic expeditions up to and beyond the War.<sup>20</sup> These were the roads Boas did not take. Workers such as Boas and Rivers could not and did not take their own laboratory life for granted. They were themselves involved in making the conventions of laboratory science at least as actively as they were helping develop the practices of field work. And this means we cannot and should not explain the methods of fieldworkers by appeal to a self-evident model of the way laboratory sciences proceed.

### *A Nervous Confidence Cambridge, April 1903*

Every Sunday morning, between April 1903 and December 1907, two men would sit down at an elegant table in a room in Second Court, St John's College. They had a regular routine. On the table were ice cubes, jugs of hot and cold water, compasses, knives. One of the men, in his forties, bearded, would travel up by train from a London hospital the previous night. He would turn his head away, his left forearm carefully bound and exposed to his

companion's probing with ice, the hot water, and the needles. On Sunday evenings, when the first part of the ordeal was over, the two men used to discuss the results. They tried varying the experiments. The subject would read some of his favourite poetry, the hairs on his wounded forearm stiffening thrillingly under the gaze of his companion: "the pilomotor reflex", they decided. They performed "control experiments on normal parts", including the victim's penis. What was the point of all this? The splendidly willing victim was Henry Head, Cambridge-trained physiologist, eminent London medic, veteran of the world-famous physiology programme run by Ewald Hering at Prague. His companion, owner of the room, the compass needles and the water-jug, was Rivers. On 25 April 1903 Head had two nerves in his forearm severed. Rivers then spent the next four-and-a-half years testing the gradual recovery of feeling in the affected area, developing his account of the distinction between "protopathic" and "epicritic" forms of sensory response, the former cruder, evolutionarily prior, exhibited in the penis, the latter finer, more developed, detectable in the colon. Look out behind you: according to Head, "the day of the *a priori* psychologist is over, as far as sensation is concerned".<sup>21</sup>

Laboratory scientists often liked turning themselves into subjects. Before the early nineteenth century, natural philosophers routinely tried experiments upon their own bodies, electrocuting or gassing themselves into states of insensibility to determine the behaviour of nerves and muscles in altered states. Under the old regime, it was argued that only very special humans could give a reliable account of their own reactions in such amazing trials. Women, servants, children, the sick and the mad were all supposed to be excluded from the community of trustworthy subjects. They could be subjected to experiments, but not perform them. Social and technical changes within the period of Boas' and Rivers' training transformed these conventions. Experimenters developed new techniques of self-registering instruments, chart recorders and inscription devices which recorded inputs directly and which were used throughout the new laboratories. At the same time, the

production of large numbers of experimenters allowed a formal control on those whose reports of their own bodily condition could be trusted. The collaboration of Rivers and Head was one of the most famous of these heroic self-experiments. "Most of the facts of nerve distribution...can be elicited from a study of hospital patients by means of simple tests", the two men conceded. "But such patients can tell little or nothing of the nature of their sensations...introspection could be made fruitful by the personal experiences of a trained observer only". Training and technology were designed, therefore, to be self effacing and self revealing. "Since Head was at the same time collaborator and patient, we took unusual precautions to avoid the possibility of suggestions". It was argued that instrument complexity, very specialized technique and professional formation did not damage, but instead guaranteed, the representativeness and naturalness of these new experimental settings. The material culture of the labs was developed in order to be transparent to nature's messages. And all sorts of other sites, Cambridge college rooms, French cattle farms, Melanesian islands, could be turned into laboratories by trying to rebuild this material culture elsewhere.<sup>22</sup>

This laboratory culture defined new social relations and new objects and subjects for study. The culture was designed so that a few basic mental functions of allegedly normal human beings could be isolated and measured. In order to make these measures a careful social and material technology had to be designed. The social technology organised a homogenous group of laboratory workers who could alternate as observers and subjects of experiment. The division of labour within the group was provided by the social structure of the German university career structure. The material culture integrated the person of the experimental subject in a complex of instrumentation. When James Ward, a student of Ludwig, tried to get a psychology lab set up in Cambridge in 1877, he was accused of trying to "insult religion by putting the human soul in a pair of scales". Wundt's prize student James Cattell set up a short-lived psychology lab inside the Cavendish in 1887 using

apparatus for psychophysical and psychometric testing brought directly from Leipzig. In the 1890s Ward was more successful in backing Rivers for the new Cambridge psychology post.<sup>23</sup>

Rivers worked very closely with the boss of the new Cambridge Scientific Instrument Company, Horace Darwin (Charles' son), to design a new machine for administering visual illusions to experimental subjects. In Leipzig, Wundt's students made new machines, such as chronoscopes and kymographs, to measure the times their subjects took to respond to such visual stimuli and record these subjects' performances graphically. An industrial affiliate of this laboratory marketed such devices worldwide. Boas used them at the Chicago Exhibition in 1893, where he organised the ethnology exhibits and also administered widespread mental tests to visitors to the show. At the psychology department at Clark University, where he worked between 1889 and 1892, and at Columbia where he was hired by Cattell in 1897, Boas had the full range of experimental psychological machines available to him. These were the same machines which Rivers took to the Torres Straits in 1898. The Torres Strait expedition took light tests and clocks, colour and eye tests, dynamographs and electromagnetic machines. The kymograph, devised by Carl Ludwig as a barrel recorder designed to give a graphical trace of such variables as plant growth, heartbeat or reaction times, was distributed in most physiological and psychological laboratories by the 1890s. It was equally crucial for the human physiology which was led by Foster and pursued by Haddon and Rivers in Cambridge, and for the psychological work initiated by Wundt and then taken over by his students. The tachistoscope, a device for the very rapid presentation of a series of images to a psychological subject, was developed alongside the tachyscope, a machine which allowed a large audience to see an apparently moving image. The laboratory, the exhibition and the cinema were closely connected. Communities of experts were defined by their mastery of such devices and their willingness to participate in experiments using them. As one of Wundt's most eminent

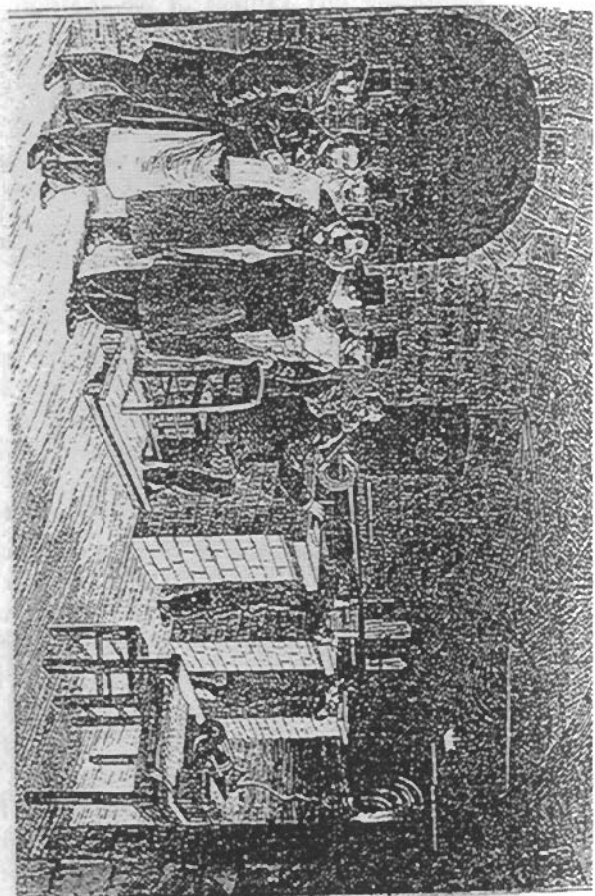
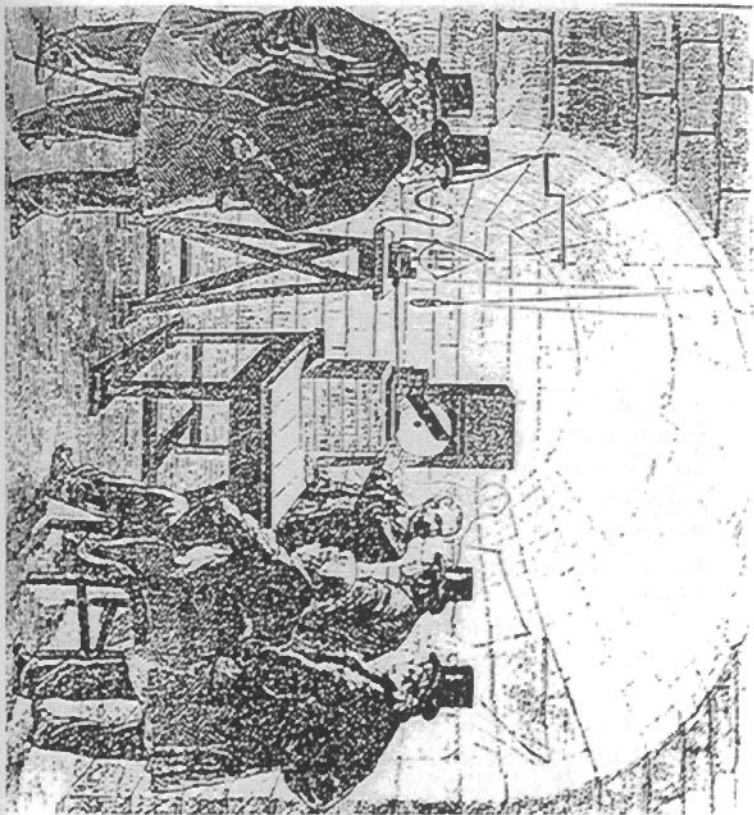
American followers, E.B. Titchener, put it: "the experimenter of the early [eighteen] nineties trusted, first of all, in his instruments; the chronoscope and kymograph and tachistoscope were - it is hardly an exaggeration to say - of more importance than the observer". And in debate with Rivers in 1916, Titchener argued that "the first necessity of experimental psychology, as a science, was the standardizing of instruments and procedures".<sup>24</sup>

These laboratory techniques of complex instrumentation, lengthy training and the division of labour of experimenters and subjects all helped make a new, controversial and unstable model of the person. Laboratory subjects were wired up. Topics which had traditionally belonged to the fields of theology and metaphysics, such as the relation between mind and body, were now to be managed in laboratories by techniques which resembled telegraphy offices and astronomical observatories. Before the world war, Rivers lacked such a formal laboratory. In 1897 Michael Foster gave him a small room in the physiology laboratory and in 1901 he was assigned a small cottage on Mill Lane, a place so rat-infested that it was remarkable he did not become a behaviourist.<sup>25</sup> Long before behaviourism, human beings looked inside themselves and found cables, galvanometers, switches. Techniques of telegraphy and astronomy were explicitly designed to measure the performance of human beings whose structure and function were understood by analogy with electromagnetic networks. Electromagnetic chronometry was simultaneously the means for estimating subjects' external behaviour and the model of subjects' internal structure. To justify the claim that each set of nerves transmitted a specific signal, Helmholtz argued in 1863 that "nerves have often and not unsuitably compared to telegraph wires...According to the different kinds of apparatus with which we provide its terminations, we can send telegraphic dispatches, ring bells, explode mines, decompose water, move magnets, magnetise iron, develop light and so on. So with the nerves". The human sensory system was supposed to emulate the worldwide communications network which British and German physicists

and engineers helped build between the 1860s and 1880s.<sup>26</sup>

In constructing this subject of psychological enquiry, experimenters built laboratories which closely matched astronomical observatories. German and British astronomers confronted a major problem in establishing accurate and comparable values for celestial observations. The observatory managers found that different observers recorded different times for the transit of stars' images across their telescopes. They called these differences the "personalities" of the observers. To construct a world astronomy network it was necessary to measure and control these differences. So by the mid-nineteenth century workers in observatories were increasingly surrounded by networks of clocks, chart recorders and scrutineers. Observatories became subject to factory regimes. This was the culture to which Boas was subjected when he worked with Helmholtz's close ally the astronomer Wilhelm Foerster at the Berlin Observatory just before his journey to the Arctic. Wundt and his allies in "mental chronometry" transferred this new astronomical technology into their labs. Select subjects, their own students, were made to watch lights moving across measured scales while listening to the beat of accurate pendulum clocks - just the setting of the normal astronomical observatory. So the practical culture of the psychology laboratory, where Boas and Rivers first worked, represented typical human behaviour as the special behaviour of the astronomical observer. Mental function in all humans was to be investigated as if subjectivity were especially evident in the act of quantitative judgments of the size and type of measurable stimuli. Furthermore, the labour process of these trials relied on swapping observers and subjects, the *Reagent* and the *Versuchsperson*. The claim was that results of trials performed in these specially managed settings by specially trained humans could easily be generalised to all settings and all subjects whatsoever.<sup>27</sup>

Rivers' most important projects were at the boundaries of physics, pathology and psychology: colour blindness and optical illusions.



Testing observers' personalities in preparation for observation of the transit of Venus, 1876.

Source: *L'illustration*, Vol. 68.

Helmholtz had portentously urged that both colour vision and depth perception could be explained in terms of psychophysics, in terms of the specific nerve signals from different types of receptors in the retina and from the accommodation and displacements of the eye. This was to make physics triumphant at physiology's expense. His theory was carefully embodied in a widely distributed machine, the ophthalmoscope, which allowed the experimenter directly to inspect the retina itself. It was also defended through the received analogy with astronomy. Just as astronomers "inferred" the position of stars from data gathered at the telescope, so in all humans the "ordinary acts of seeing" involved "unconscious inference" accounted for the management of illusions. Helmholtzian humans were represented as rational denizens of a universal observatory.<sup>28</sup>

Against this account, Head's former supervisor Ewald Hering claimed that both colour vision and depth perception could be explicated physiologically, in terms of reactions within the eye's system. And to contest Helmholtzian authority Hering designed his own battery of optical equipment: ranges of coloured papers and wools, colour-mixers and colour blindness testers to mechanize the presentation of coloured images to subjects. In the Torres Straits, Rivers reckoned the Leipzig "papers are no so largely used by workers on colour vision that they may be regarded as standard colours".<sup>29</sup> In a series of enquiries launched in 1896, he used these techniques first to demonstrate that size illusions did not require real motions of the eyeball, then to explore the speed with which such illusions could be produced. Helmholtzian psychologists argued that subjects tended to exaggerate the size of verticals because of the increased effort required for the eye to move up and down. First Rivers dropped a series of paralyzing and exciting drugs into his eye to test the effects on depth vision, then he tried the drugs on others, mainly colleagues at St John's ("I need hardly say that in all cases I have avoided leading questions"). He switched to a more reliable machine, a tachistoscope to present such lines in rapid succession. Since it seemed that his subjects

still exaggerated vertical size under these circumstances, and since they did so more unhesitatingly than after long consideration of the image, he reckoned that such illusions could indeed be assigned to physiological, not psychophysical, factors. These instrumental discriminations relied on the culture which he tried to shift to the field.<sup>30</sup>

The processes through which the local, particular performance of experiments, instruments and technologies is compared with other locales are called "calibration". Without calibration no laboratory technique can be got to work anywhere else. But calibration succeeds only when the proper community agrees that two different phenomena are in all relevant respects identical. A thermometer must be calibrated: it is exposed to a known heat source to check the value of temperature it displays. Calibrators reckon that all other sources to which the thermometer will be exposed will be identical to this surrogate source. So agreements about calibrations also help define the contents of the world. The response of Henry Head's private parts to Rivers' ice cubes helped define what "protopathic" meant. Sometimes calibrations and self-experiments are institutionalised. In the 1880s European and American ethnologists formed "mutual autopsy" groups to guarantee a good supply of big brains. They knew where to look. John Wesley Powell, head of the Ethnology Bureau and Boas' enemy at the Smithsonian, was one of those whose brain was measured *post mortem* to back a correlation between brain size and psychic ability. These morbid self-help groups had already agreed that high-class subjects like Powell, Virchow or Cambridge wranglers had high psychic ability. The consensus helped them decide what brain size meant when they took their calipers elsewhere.<sup>31</sup>

The techniques devised in European laboratories and observatories in the late nineteenth century explicitly relied on such calibrations. Reliable machines made humans reliable delegates. Before they were sent out on carefully managed imperial expeditions sponsored by such metropolitan institutions as the British Association for

the Advancement of Science to observe the transit of Venus across the Sun, astronomical observers were tested for their timings of the transit of surrogate stars across bright light sources. Their personalities measured, they became reliable subjects, trustworthy instruments of European scientific culture. Calibration simultaneously disciplined roving observers and, in principle, the subjects they observed. The same techniques were adopted by field-workers everywhere. Rivers and his colleague William McDougall tried giving up tea, coffee and drink to test the effects of these drugs on muscle fatigue by dosing themselves alternately with a caffeine-alcohol mixture and a neutral placebo. Francis Galton, elitist guru of psychometric testing and social statistics, even pulled off the trick of making his sense of smell into an efficient computer: "I taught myself to associate two whiffs of peppermint with one whiff of camphor, three of peppermint with one of carbolic acid, and so on....I convinced myself of the possibility of doing sums in simple addition with considerable speed and accuracy by means of imaginary scents". Rivers and his colleagues liked this trick: they quoted it with approval in their reports from the Torres Straits as they tried poking glass tubes, baptised rather grandly "olfactometers", into their own and the islanders' noses to test their sense of smell. The Englishmen put up with this, but the islanders didn't. "The attention of the subjects immediately flagged as soon as they disliked or lost interest in the experiment". Self discipline made European scientists into self conscious instruments sent out to measure, observe, detect. They were *subjects* in every sense: topics of disciplined enquiry, subordinates of regimes of power, and bearers of consciousness.<sup>32</sup>

### *Just Following Orders* *Vancouver Island, June 1888*

Self-experiment was one way in which laboratory techniques spotlighted scientists' own culture. It made the significance of their body techniques completely inescapable. Attempts to shift these techniques elsewhere provided other ways of focussing on

the conduct of the investigators themselves. These scientists had one well-known way of ensuring that their delegates would behave properly when far from home: they gave them rule books, they handed out instructions. The rule books had perverse effects. Sometimes, anthropometric manuals turned their readers into graverobbers. At the start of June 1888 Boas and Hastings, the one a delegate of the British Association "as well known here as a mongrel dog", the other a local photographer, set out across the stormy harbour of Victoria, the capital of British Columbia. They were searching for human bones. Over the next few days, the two men uncovered three headless skeletons, took pictures of them, measured their dimensions. "It is most unpleasant work to steal bones from a grave, but what is the use, someone has to do it", Boas wrote. Over the next few nights, he had nightmares about skulls and bones. Later, he tried to sell the bones to American museums. A decade later, during the Jesup expeditions on Vancouver Island, Boas's collaborators George Hunt and Harlan Smith kept up the enterprise. While Hunt got local permission to take the bones, Smith told Boas that "I thought what the Indians did not know about would not hurt them". If these men had to do this, it was because they were following the orders of the anthropometric programme. As Horatio Hale, head of the British Association survey, told Boas in early 1888, "we are acting for the Section of Anthropology" so "facts of physiology should have special attention".<sup>33</sup>

We are now very thoroughly informed indeed about the way in which manuals like the British Association's *Notes and Queries* helped govern these changing practices of fieldwork after the 1870s.<sup>34</sup> For example, Charles Darwin tried using standardised questionnaires in his work on artificial selection by animal and plant breeders and in his study of human expression. As Jim Secord has pointed out, the reach of this Victorian squire-naturalist "extended around the world". He relied on his own celebrated voyages. But more important were the complexes of travel narratives, such as those of Philip Gosse on Jamaica, Hooker on



Sikkim or Livingstone on South Africa, or the reports of local experts such as missionaries, medics and administrators. "The difficulty is to know what to trust", Darwin told Huxley. In the cases of breeding and facial expression, he could help calibrate responses from informants by issuing standardised sheets of questions. The "Questions on the Breeding of Animals" were issued in early 1839.<sup>35</sup> The very first set of questions commissioned by the British Association was released in the same year. Charles Darwin's questions on human expression, issued in 1867, were included in the first edition of *Notes and Queries*: "general remarks on expression are of comparatively little value, and memory is so deceptive that it ought not to be trusted". Included, too, were means to calibrate judgments of skin and eye colour. The Bristol physician and ethnologist John Beddoe, who designed these sections in the 1870s, reproduced the colour charts initially designed by the scientific racist Paul Broca. "Even educated men", Beddoe explained, "differ very widely as to the appreciation of colours and their nomenclature". Throughout the handbook the behaviour of English men was used as the surrogate for all other races, genders and classes.<sup>36</sup> Detailed instructions were provided for the position, attitude and illumination in which such hues were to be observed. In the Torres Straits, Rivers worked hard to standardise the space in which he administered his tests. The lighting of the verandah on Murray Island was hard to manage. He tried to calibrate Melanesian brightness against an English summer's day: "I soon found that some of these days were too bright to give satisfactory results with Europeans".<sup>37</sup> As if to render this project emblematic, the celebrated fourth edition of *Notes and Queries*, composed principally by Haddon, Rivers and their colleagues in the wake of the Torres Straits expedition, carried a scale of inches and centimetres embossed in gold on its front cover.

James Urry reminds us of the increasingly powerful implication that the handbook be designed for trained fieldworkers, not ill-disciplined travellers. Standards were set during the *Challenger* voyage in the mid-1870s, when such scientists as Harry Moseley,

trained at Leipzig by Ludwig, moved between his shipboard laboratory and a succession of Melanesian islands, conducting with equal vigour trials on marine fauna and on islanders' counting techniques.<sup>38</sup> A decade later the British Association sponsored a new questionnaire for workers in the Northwest Pacific Coast: by 1887 they had commissioned Boas as their agent there. Stocking observes that "his employment marks the beginning of the collection of data by academically trained natural scientists defining themselves as anthropologists". George Dawson, trained by Huxley in London in the 1870s and then boss of the Canadian survey, reckoned that Boas took "an unreasonably discouraging view of the difficulties in conducting measurements and investigations of this kind. No doubt they would be better done if they were all done by a trained observer, still better if they could all be done by one trained observer like Dr Boas himself, but we must do the best we can under existing circumstances". Boas' fieldwork of 1888-97, later repudiated, bears the hallmarks of these rules.<sup>39</sup> But the instructions were not themselves stable. Thus Shruballs' opening section of 1912 on physical anthropology stressed that "investigations into functions can only be usefully carried out by those who have had a long and complete laboratory training". Rivers contributed his own version of such investigations, including an outline of the "terminology of social organization" explicitly designed to standardize the way workers represented their field experience. In his 1910 paper on the genealogical method, Rivers chose a bright analogy which harked back to earlier contacts with Galton's statistics: he reckoned that memorized pedigrees were "preserved in the minds of the people and by [their] means we are able to study the laws regulating marriage just as in a civilized community one can make use of the records of a marriage registry office....we can express statistically the frequency of the different kinds". The implication of this strategy for the sociology of the *anthropological* community was spelt out. "Up till recently ethnology has been an amateur science. The facts on which the science has been based have been collected by people who usually had no scientific training...By means of

the genealogical method it is possible to demonstrate the facts of social organization so that they carry conviction to the reader with as much definiteness as is possible in any biological science". The idea was to use the routines of training to make field experience count back home.<sup>40</sup>

But there's an obvious puzzle with all this talk of reliable delegates and professional fieldworkers, of training and rule-following. It makes too much of exteriority, and places too much reliance on home-grown experience. No rule-book carries with it sufficient instructions for its own application, as the Wittgensteinian slogan has it. The systems of self-experiment, self-discipline and self-instruction summarised here only work as precedents, skilled techniques applied from moment to moment elsewhere. In a famous lecture on body techniques and training systems markedly indebted to Rivers and Head, Marcel Mauss pithily made the point about the interaction of tradition with craft skill: "this is the place for the notion of dexterity, so important in psychology as well as sociology".<sup>41</sup> And this is an observation about the cultures the fieldworker represents, that in which the worker is trained and that which is studied. Fieldworkers who carried *Notes and Queries* in their breast-pockets did not, and could not, mechanically enact its instructions. Neither the Kwakiutl of Vancouver Island nor the Meriam of the Torres Straits engage in the robotic performance of the dictates of a cultural system. Anna Grimshaw and Keith Hart have reminded us that functionalist pictures of a rule-governed society gained their plausibility in the powerfully bureaucratic world of the interwar state system. The same world produced equally powerful arguments *against* bureaucratic domination, from liberation movements and from critical intellectuals alike. Grimshaw and Hart also point out the elements of justice in anthropologists' claims to democratic and egalitarian virtue.<sup>42</sup> I want to strike a similar balance. Boas and Rivers made much of the specialist training required for effective fieldwork. They laboured to calibrate and standardise the results of their experience in order to produce reliable accounts of what

they found and make them count back home. But *therefore* these experiences became occasions where they tried out their own formation, the techniques and traditions to which they themselves belonged. Trials surprise. Victorian Cambridge, Bismarckian Kiel and Gilded Age Chicago all placed the highest value on hard facts and formal rules. Craft skills and handy techniques were put at the bottom of the ladder of learning. I conjecture that the experience of fieldwork in the 1890s and after challenged these hierarchies. It put these values in question in the most material and practical way. Perhaps this is why, for Rivers, "all civilized society was a sort of South Sea Island"<sup>43</sup>

### *Body Techniques*

#### *Mer, Torres Straits, May 1898*

Three weeks after reaching Mer in May 1898, Rivers began handing out sheets of paper to the islanders who visited him. He persuaded them to draw papaws, crocodiles, frigate birds, any subject they liked. When they'd finished, he would annotate the pictures with the time the drawing had taken and details of its execution. In England, he'd already discussed obtaining such drawings and the information they might contain. He got Haddon's children to provide him with comparable pictures. At exactly the same time, Haddon also persuaded the Meriam to produce some drawings - especially of tortoise shell masks and their use in dance. Were these drawings experiments? If so, what evidence did they provide? A particular trial conducted in the laboratory or the field may, in principle, be made relevant to a very broad range of issues. Trials which might be evidence of islanders' senses might also be evidence of the lighting in the psychologist's room or of Melanesian design conventions. The Meriam were confronted with visitors who counselled them on diseases, encouraged them to draw, to gaze at charts of numbers and letters, who filmed them, excavated their masks, recorded their songs, and stuck glass tubes up their noses.<sup>44</sup> The field trials which Rivers and his colleagues conducted in the Torres Straits have become as mythic for the

					Torres		Ua-Mu-ma-lu				
Batoh	6.6	5.5	Pipai	5	2	Aijon	50m	Brya	10	Camet/Si	15
Poi	5.6	4.7	Tu-tua	4.4	3	Seligan	40	Dol-lud	15	Bibh	25
Uo	5.5	4.6	Oba	4	3	Rivers	50	Pan	10	Ara	30
Lodi	9.5	8.4	Jai-Dan	3.7	2.2	LiD	35	Uli	20	Lodi	40
Dubai	5	4	Bahy	2.4	2.5	Wih	90	Lyi	50	Ab	40
Wama	8	6	Jicb	4	3	Sambo	20	Chaki	50	Year	4
Pansa	5	5	Daney	4.8	4.8	Bony	50	St. Pasa	20	Poi	20
Kreba	4.5	4.6	Dich	3.2	3	Zamb	4	Jai-Dy	20	Dalai	30
			Lamover	5	4	Tapa	2-	Chy	15	Pansa	25
			Kam	2	3	Pansa	50	Ab-Jan	25	Wama	30
			Wika	3.2	3.1	Dich/pt	20-	Kedut	10		
			Della	3.5	2.8	Coachis	15	Dalyi	10		
			Lama	3	3	Dawra	20-	Kama	15		
			Sapipi	3	2.8	Kaiya	30-	Dudo	15		
			Poi-Poi	4.5	4.5	Jan	30	Kudo	25		
			Tu-hufobi	4	4	Della	7-	Lan	25		
			Uora	7	4.5	Kom	15	Dudra	15		
			Jamas	3.8	4	Laha	4-	Josid	20		
						Jai-Ris	10	Yano	25		
						Ril-Lamin	15	Pipipi	25		
						Luk	2-	Uli	15		
						Pasurji	25	Pipipi	25		

Results of tests on two-point discrimination on Mer, Torres Straits, August 1898: The first five names are those of the expedition members, the others those of Meriam.

Source: Haddon Papers, University Library, Cambridge



Drawings by Ellia made for Rivers on Mabuig, Torres Straits, September 1898.

Source: Haddon Papers, University Library, Cambridge

history of psychology as their other projects have been within anthropological memory. The "psychology laboratory" on Murray Island was in fact the space where islanders presented themselves to Rivers and his friends for medical treatment. The English scientists worked very hard indeed to turn verandahs and beaches into laboratories.

Rivers and Haddon summarised the means they used to get the islanders' collaboration: "in nearly all the observations there was no doubt that they were doing their best; in fact, I am doubtful whether, when collecting comparative data in some more or less European community, it will be possible to excite the same amount of interest and to be certain that the observations are being made with zest and conscientiousness equal to that of the Torres Straits Islanders". Rivers found that the islanders were very susceptible to exaggerate the size of vertical lines. He already reckoned this was a physiological performance, so unlikely to vary cross-culturally. And he found that they were peculiarly immune to the Mueller-Lyer illusion, the apparent shortening of line length when the line is bracketed by convex arrows. This too confirmed his Cambridge results, since he reckoned this illusion was psychological in origin. His team's work on colour perception was equally dramatic. The islanders were capable of exceptionally fine discrimination between different samples of coloured wools, even though this capacity was not matched by the crude colour names which they employed. He derived the same lesson from a repetition of a classical Wundtian trial, subjects' capacity to discriminate between the impression of one or two points of "a small pair of carpenter's dividers with blunt metal points" applied to their skin. Rivers reported that the distance between these points at which the islanders could tell the difference, their "limen", was much lower than that of the Englishmen. English subjects didn't only include his co-workers on the islands - they were calibrated with trials on his Cambridge students and a dozen Girton schoolchildren. Such fine capacity, he suggested, was bought at the price of "higher mental development". But this defect provided

no warrant, the team reported, for any claim that the islanders were at a lower stage of evolutionary development. Environment and habit explained difference in capacity. On this economic model, "if too much energy is expended on the sensory foundation, it is natural that the intellectual superstructure should suffer". Rivers reported that this was testimony to the islanders' strongly developed attention to visual cues, much stronger than in urbanised Britain. Rivers and his colleagues reckoned that in Europe psychological experiments would be more reliable if conducted on professional, self-concerned observers just like themselves. But on Murray Island the opposite was the case. Melanesians would give more reliable results than English students, because the islanders would never "speculate about what they are being asked to do". So tests on Torres Straits islanders were reliable just because of the innocence of the subjects; tests on Cambridge students were reliable because of the care with which professionals judged their own responses. These lessons about gesture reveal the practical work required to allow the shift of fragile facts from the field. They also show how tests in the Torres Straits could begin to foreground the techniques of European culture.<sup>45</sup>

Such results were very widely debated and have been ever since. C.S. Myers, Rivers' eulogist and collaborator on the expedition, judged that these trials "will ever stand as models of precise, methodical observations in the field of ethnological psychology". Another of Rivers's collaborators, C.G. Seligman, doubted their anthropological relevance but conceded that the "psychological results of the expedition were...important in themselves in settling matters in doubt or dispute", by showing that differences in capacity were personal. Their most penetrating historian, Henrika Kuklick, argues that "by the standards employed in the Torres Straits experiments the research done in European laboratories was inadequately controlled...they demonstrated the unreliability of laboratory research conducted in ignorance of subjects' social situations".<sup>46</sup> However, such "demonstrations" were certainly not universally compelling nor were disputes effortlessly settled. To

give such trials authority it was necessary to secure the calibrations and conventions upon which these inferences relied. And to secure these conventions, it was necessary that the culture of Rivers' fieldwork be transferred elsewhere. This was hard work, not easily to be achieved simply by the publication of the expedition's results. For example, in a powerful critique Wundt's leading disciple, the Oxford graduate Edward Titchener, revealed that he was by no means convinced that his Cornell University lab culture needed revision. Indeed, he argued the opposite. The Torres Straits experiments were the defective ones.

In each case, Titchener appealed to his own lab culture as the tribunal of the island experiments. He knew that in bright light the level at which blue colours could be discriminated dropped. Rivers said the islanders found it hard to make this discrimination. Titchener guessed that this was because they were tested in poor light. He also reckoned that the islanders' limited capacity to name different colours was a consequence of their restricted vocabulary. At Cornell, he tried this test on his own students, including the future chronicler of psychology Edwin Boring. When the students were told to limit their vocabulary in naming colours, they behaved just like islanders.<sup>47</sup> In each case, an inference which Rivers judged demonstrative could be challenged by invoking features of the field which he had not mentioned. This is why calibration matters. It restricts critics' freedom of manoeuvre. Without means of standardising *in advance* the illumination of the space where subjects were tested or the vocabulary used to describe colours, Rivers could argue that the islanders were bad at discriminating blue and Titchener that they were simply badly lit or uninterested.

The puzzle of turning from lab to lab, and between lab and field, therefore, is that the number of differences which might count is certainly too long completely to specify in advance. Titchener's critique hinged on the difference between field and laboratory trials. "A field test should set the subject a task which is both simple and definite; it should be capable of performance in a

relatively short time and with apparatus that is strong, portable and relatively cheap; it should be laid out so simply that its conduct is easily mastered and so definitely that there can be no variation in its procedure; and it should yield results that are directly relevant to the object of the test, are expressible in numbers and are thus intercomparable". Titchener urged the differences between the protocols of such experiments and those to be demanded from the laboratory worker: "a set of instructions, carefully formulated and intelligently grasped; an instrument of precision; a large number of observations, made in accordance with a prescribed method and sufficient for mathematical treatment; a variation of conditions to throw this or that aspect of the subject matter into high relief; experiments distributed regularly over months or years". Titchener saw no way of transferring these techniques. But he did see the need to police the fieldworkers from the laboratory. So he insisted that the Torres Strait results "remind us, time and again, that human nature is much the same the world over". The alleged similarity between islanders and psychologists warranted both destructive and constructive inferences from the field to the laboratory. These inferences are cultural moves.

The tests on two-point discrimination are a fine example. These relied on techniques which Rivers and Head also used at St John's five years later. In work on subjects' capacity to discriminate between stimulation of their skin with one or two points, lab experimenters found that paradoxical judgments, *Vexirfehler*, were common, especially because subjects became "practised" as the series of trials continued. The French psychologist Alfred Binet, working with his own daughter, concluded that there was no scientific way of determining the threshold for two-point determination. Judgments of twoness were irredeemably interpretative, because an entire range of sensations was experienced between the security of a single and an irredeemably double feeling. Kurt Danziger summarises Binet's conclusion about "the rather elastic meaning of the concept 'two' that had to be employed in this situation". Binet published his results in 1902.

Titchener then guessed that this elasticity had been at work in the Torres Straits. Educated Englishmen would only announce that they were experiencing a double impression when they did so "in a stricter sense", as their culture of precision would suggest. The islanders would report a double impression whenever anything different from simple singularity was perceived. Different subjectivities, different liaisons between experimenter and subject, would prompt these different reports. Islanders were not more sensitive, simply bolder. Rivers and Myers recognized that "many individuals", including themselves, could tell that they were being touched by two points even though they were not experiencing "two separate sensations. Among savage and half-civilized peoples, it is impossible to discover on what sensory basis a judgment depends". A reviewer in the *Athenaeum* commented that only "highly trained and naturally gifted psychological observers would be unlike Murray islanders in the respect deplored". Titchener performed these trials on himself, successfully reproducing islander responses. He concluded that in the absence of pre-established calibration "it is fatally easy for the fieldworker and the laboratory worker to misunderstand each other. I do not want the refinements of the home laboratory carried into the field...we cannot argue directly from laboratory trial to field test". While Titchener might argue that "it is useless to make tests in the field and to repeat them later on civilized subjects, until we know whether the test procedures are themselves methodically reliable", the sole means of ascertaining such reliability was to make these comparisons between fieldwork and the laboratory. Such arguments did not give demonstrative authority to Rivers' fieldwork - instead, they revealed the effort required to make this work count. The appearance of Cambridge scientists, coloured wool and pointed dividers on Murray Island changed the habits of the Islands' residents. Thus the evidential context of the experiments might not be the islanders' psychology but the status of fieldwork as such.<sup>48</sup>

Franz Boas, using exactly the same resources, made the same

shift towards the status of fieldwork in his paper on the transcription and phonology of indigenous language written in New York in autumn 1888. As Stocking acutely observes, this paper linked Boas's laboratory work at Kiel with his subsequent fieldwork in the Arctic and on Vancouver Island. Contemporary fieldworkers among Plains Indians had detected what were called "alternating sounds", differing vocalisations of the same syllables, and had argued that such vagaries revealed the evolutionarily primitive character of indigenous languages. Boas shifted the evidential context of field transcriptions from the native subject to the *transcriber*. He argued that in transcriptions "the nationality of even well-trained observers may be recognized" and that "a new sensation is apperceived by means of similar sensations that form part of *our* knowledge".<sup>49</sup>

The term "apperception" was a newly developed part of the theoretical technology of laboratory psychology. In the same year as Boas' attack on the reality of alternating sounds, Wundt's student Ludwig Lange worked on a series of canonical trials on reaction times, the standardised laboratory emulation of measures of astronomical personality. Lange and Wundt reckoned they could distinguish between rapid, mechanical perception, when the subject attended solely to the action to be performed after the stimulus, and slower, deliberate apperception, when the subject attended to the character of the stimulus itself. The relevant resource for Boas was a set of trials at Leipzig in which it was shown that subjects were able to apperceive familiar *words* just as fast as the same number of *letters* when both sets were presented in a random order.<sup>50</sup> The laboratory psychologists urged that subjects apperceived words holistically once they had become familiar with them. Boas tried this test on himself, by training himself to recognize lines of a specific length, then presenting himself with lines of widely varying lengths. Boas concluded that in length and colour trials, the capacity to distinguish between really different stimuli depended on training, attention and predisposition. The application to field transcription was obvious

to him: fieldworkers would use their own familiar tongue to interpret an alien one; they would as often hear as identical sounds which natives could distinguish; and this principle would be symmetrical. Tlingit speakers would pronounce the English "I" in a range of ways. Thus in attacking evolutionist phonetics Boas showed that the laboratory culture of self experimentation could be deployed to transform the evidential context of an experiment on Tlingit phonetics into an experiment on fieldworkers' psychology.

### *Get Real New York, May 1905*

On 17 May 1905 Boas, by then anthropology curator at the American Museum of Natural History for a decade, had a violent interview with the Museum's president, Morris Jesup, and its director, Hermon Bumpus. At issue was the function, layout and message of anthropology displays. Bumpus reckoned that the museum should aim at the widest possible popular audience; Jesup urged that an evolutionist layout of "primitive" cultures would teach New York's migrant population the virtues of self-improvement; Boas insisted on the pluralist functions of displays in education and in research, and the supreme value of integrating material culture into its full social setting. Within a week, he resigned.<sup>51</sup> The episode is now always taken to mark a portentous turn within cultural anthropology away from the museum towards the university. It teaches other lessons too. The nineteenth century laboratory was but one of a range of systems for the production of knowledge and the training of subjects. The museum (and, with Foucault, we must add the clinic) was at least as important for making, distributing and organising these subjects. So when the attention of fieldworkers was ineluctably turned back to the conditions of their own training and their own culture, they found the most important clues in museum layout or clinical treatment. We can think of these institutions as places for shifting evidence towards critical self-analysis.<sup>52</sup>

This characteristic switch dominated Boas's well-known debates in summer 1887 with the ethnology curators at the Smithsonian Institution, and his subsequent work at the American Museum of Natural History after 1896.<sup>53</sup> At stake in both projects was Boas' utopian aim to transfer the whole cultural milieu of indigenous society from the field to the museum. In New York, Boas struggled with the fact that it was "impossible to compel" visitors "to see the collections in such a manner than they will have the greatest possible benefit from a short visit". Just as Rivers' utopia was the use of psychological tests and genealogical systems to transfer social structure and indigenous capacity between Melanesia and Mill Lane, so Boas' "ideal of an ethnological museum" demanded "a collection representing the life of one tribe", a shift between Vancouver Island and Washington. Boas attacked the Washington curators Otis Mason and John Wesley Powell because of their commitment to typological arrangements of objects from widely different cultures arranged in evolutionist series. As in his critique of field phonetics, Boas shifted attention from the field towards the culture of the museum itself. Mason defended the aims of the museum as a provider of "charming food for thought" for its public. Powell discussed in some detail the practices which curators must use to classify and display complex and mobile tribal units. Boas wrote of alternative museological systems. Museums were at least as important sites of scientific work in the nineteenth century as were laboratories. In Berlin or Cambridge, the two were indistinguishable. So the cultural practices of museum life counted. As Ira Jacknis has rightly stressed, while the protagonists appealed to rival epistemologies, and while Boas urged that similar effects could well be produced by very different local causes, the culture in question was that of museum practice.<sup>54</sup>

In his debate with Powell and Mason, Boas explicitly evoked the possibility of a "psychological museum", proposed in the 1880s by Paolo Mantegazza, as a complement to his ethnographic layout: "a museum of ethnological objects arranged according to the ideas to which they belong". The resources of laboratory psychology

mattered here. In post at the Chicago Exhibition and then in New York, Boas used psychophysical techniques to organise and display life groups of startling appeal, carefully lit and staged, bolstered with enormous arrays of cultural material. "Visitors are compelled to see the collections in their natural sequence". The evidential context was to be secured in conditions which were based on the psychology of vision. His museum aim was precisely to construct an idealized setting, a laboratory for the holistic performance of culture, where subjects were forced to make the right inferences. This was a setting never realizable in the field and never realized to his own satisfaction in any museum. In November 1896 he reported that the function of life groups was to "transport the visitor into foreign surroundings...But all attempts at such an undertaking that I have seen have failed, because the surroundings of a Museum are not favorable to an impression of this sort...the contrast between the attempted realism of the group and the inappropriate surroundings spoils the whole effect". As Michael Ames has pointed out in his own discussion of Boas' programme of artificial contextualisation within the museum, it is wrong to identify the equally laudable aims of understanding other societies and of illuminating anthropological concepts. The culture of the anthropologists need not be confused with the cultures they represent, just as recent programmes for a "public understanding of science" barely distinguish between public knowledge of nature and public understanding of how the sciences work.<sup>55</sup>

In the end, these problems of realism and artifice were fundamental for all late nineteenth century moves between the laboratory and the field. The massive technology and material required to make a setting realistic, precisely what was required in a field trial or a life group, had to be invisible. In the 1912 edition of *Notes and Queries*, the Santa Fe archaeologist J P Harrington contributed a new memorandum on the best means of recording an unfamiliar language. In the light of Boas' 1888 critique, Harrington attempted to evoke the ideal field setting. Interpreters should have qualities which, if taken literally, would have made them indistinguishable

from perfect scientific instruments and ideal psychological subjects: "an honest and patient man, intellectually bright, who conceals nothing, ....who can translate and make intelligible to the investigator all that he hears and all that he knows". Were such an interpreter not to be found, Harrington recommended a veritable battery of "mechanical devices of the greatest service in correcting or corroborating the impressions which speech makes on the ears". These included a kymographic chart recorder, which "enables us to examine speech with the eye and apply the measuring rod to it", mouth measurers to estimate tongue and jaw position, artificial palates to fit into the roof of the mouth, and gramophones. When using the kymograph, one of its devotees pointed out, "the subject must be urged to considerable loudness in his singing in order to secure waves of proper amplitude". Surrounded by such an array of the investigators' own material culture, and only thus, it was claimed that the realistic and natural speech pattern could be recovered.<sup>56</sup>

It was all too easy for others to draw a different inference, to be "distracted" by this array of technique. Thus in his brilliant entries on method in *Notes and Queries*, Rivers warned of the dangers of the unwitting transport of European culture into the field. "It often happens that you ask for information in a way which seems to you to be perfectly simple and straightforward and your informant may be quite unable to respond....probably the formal question, framed on some category of European thought, put the matter in an unaccustomed light....it would have been necessary for your informant to see the matter in a light different from that natural to the people". The debate with Titchener was precisely focussed on the extent to which Cambridge techniques had been imposed on Melanesian cultures. Similarly, Boas pointed out fieldworkers' technique as an obstacle to the right recovery of genuine phonetics. He reckoned that indigenous peoples had been judged inferior because they'd been asked questions which did not interest them. In the museum, he tried to handle his own technique in order to make visitors recover the right cultural lesson. In order to produce



realistic effects on their audiences, these workers had to scrutinise the material culture and techniques of their own world.<sup>57</sup>

### *Back Again*

#### *Cambridge, January 1994*

The title of this pamphlet hints at the possibility of return. I mean this to be taken in several senses. I've suggested fieldworkers such as Boas and Rivers returned attention to their own culture. The return from the field to the metropolis revealed the fundamental political structures of their tradition in characteristic institutions of modern know-how: the museum, the hospital, the academy and the state. Boas' break with the American Museum of Natural History was part of a ferocious reaction against hierarchies of condescending populism and racist evolutionism.<sup>58</sup> Wartime carnage, as Michael Adas reminds us, was the crudest possible demonstration of the instability and violence of European mastery of machines, of scientific technique: the battlefield was "a huge, sleeping machine", the soldiers "primitives, bushmen", the field hospitals "the last word in science [following] the armies with motors, steam-engines, microscopes, laboratories, the first great repair shop the wounded man encounters". These new realities destabilised the easy relationship between western technological self confidence and the alleged barbarisms of other cultures. In January 1916, before his adopted country had entered the War, Boas appealed to "the fundamental idea that nations have distinctive individualities which are expressed in their modes of life, thought and feeling" to preach tolerance and pacifism.<sup>59</sup> In the same year, towards the end of the Battle of the Somme in which almost half a million British soldiers were casualties, Rivers joined the clinic at Craiglockhart Hospital where he began his work treating shellshock victims. This story has been well told elsewhere, by Rivers' patients such as Sassoon and Graves, and by historians such as Martin Stone and Henrika Kuklick. According to Rivers' colleague C. S. Myers, senior army officers reckoned that victims of shellshock were either mad, and to be

locked up in asylums, or sane, and to be shot for cowardice. Rivers' intervention, in collaboration with fieldwork veterans like Grafton Elliot Smith and Myers, used versions of Freudian analysis and his own work on protopathic responses in indigenous psychology to persuade his patients to relive buried memories. Rivers was not content to use analytic concepts of repression to explain how shellshock happened. He also used it to reveal and attack the bureaucratic structures of the British Army: military traditions which relied on stern exhortations from officers or on the culture of the stiff upper lip were among the causes, not the cure, of catastrophic mental collapse.<sup>60</sup> After the War, both Boas and Rivers turned to socialism to promote their agenda of freedom and progress. Boas voted for the Socialist Party and Rivers stood as a Labour candidate in London: "the times are so ominous, the outlook both for our country and the world so black, that if others think I can be of service in political life I cannot refuse".<sup>61</sup>

There's much about these projects we might now refuse: symbolic and real violence, intrusive and dogmatic experiments, the adherence to stern discipline. It's all too easy to see how these investigators imposed their own categories on the worlds they studied. Consider their construction of stories of primitive life which lack much historical sense, whether in Rivers' notorious diffusionism or Boas' supposition of an immemorial potlatch system which may in fact have barely predated the mid-nineteenth century.<sup>62</sup> There's much, too, to accept. Recall, for example, the centrality of technique in the training which Boas and Rivers advocated, their attack on professional restriction and their insistence on the widest possible deployment of new methods in the field, the museum, the classroom.<sup>63</sup> In this pamphlet, I have paid more attention to the culture in which these fieldworkers were trained than to the cultures which they *studied*. I have argued that much of the *content* and the *form* of their results can be explicated by this attention to their home rather than their destination. But I have also argued that this sensibility was present in the work of Boas and of Rivers too. This is my second sense of

return: the use of anthropology as an inspiration for science studies. Ethnographies of science need not lapse into a simplistic reduction which would treat scientists' accounts as nothing more than projections of their own culture. Two features of this claim need emphasis as we come home. First, the absence of other voices from these stories of field and laboratory work is indefensible. The field has changed. The Kwakwaka'wakw have invited Boas' descendants to potlatches, and have helped stage displays at the American Museum of Natural History. Torres Straits Islanders have expressed their demands for elected organisations and the defence of land rights in statements displayed in the Australian parliament.<sup>64</sup> In the episodes retold in this pamphlet, we see inquirers using, challenging and revising the social customs and conventions by which they live. They actively change traditional techniques in interaction with and, often, advocacy of, their interlocutors. And we should also notice that these exchanges with other agents: the technicians who built and ran the equipment, the subjects of psychological trials, museum visitors, perceptive islanders, native speakers. In encounters with these others, evidential contexts shift and trials test the institutions of such experiences. So, secondly, in the projects of Rivers and of Boas, it is crucial to understand the power of the assumptions, resources and materials upon which they drew. Their work helps show us that the return of field techniques to our own institutions can be revelatory and constructive. The recognition of our own traditions and techniques doesn't make encounters with other worlds worthless or impossible. These strategies can reveal the interaction between what Boas once famously labelled "the logics of science" and "the logics of life".

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"Streams of property, armor of wealth", in Jonaitis, ed., *Chiefly Feasts*, pp. 110-17.

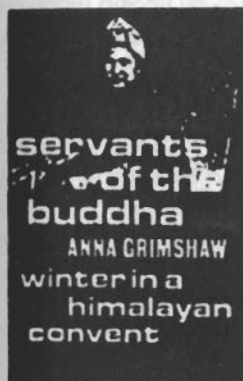
63- For eloquent accounts of these needs see Boas, 1919 in Stocking, ed., *Shaping of American Anthropology*, p.340; and the remarkable comments on the establishment of anthropology and Haddon's career in Cambridge in Q.D. Leavis, "Academic Case History" (1943) in F R Leavis, ed., *A Selection from Scrutiny* (1968), 2 vols. 1: 1-7.

64- For the Kwakwaka'wakw see *Chiefly Feasts*, pp. 22-69; for the Islanders see J Beckett, *Torres Strait Islanders: Custom and Colonialism* (1987).

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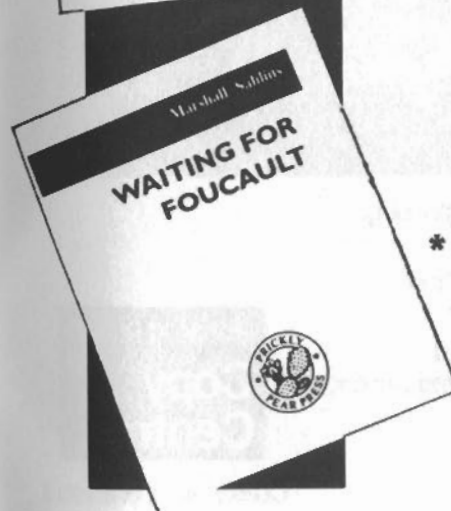
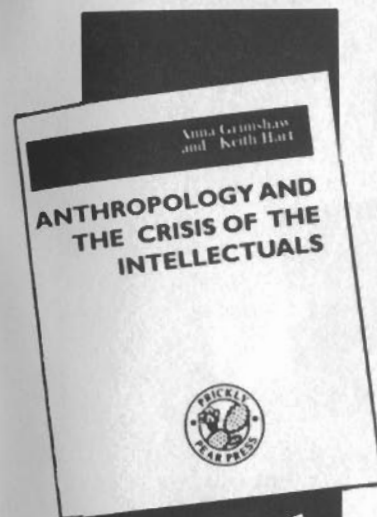
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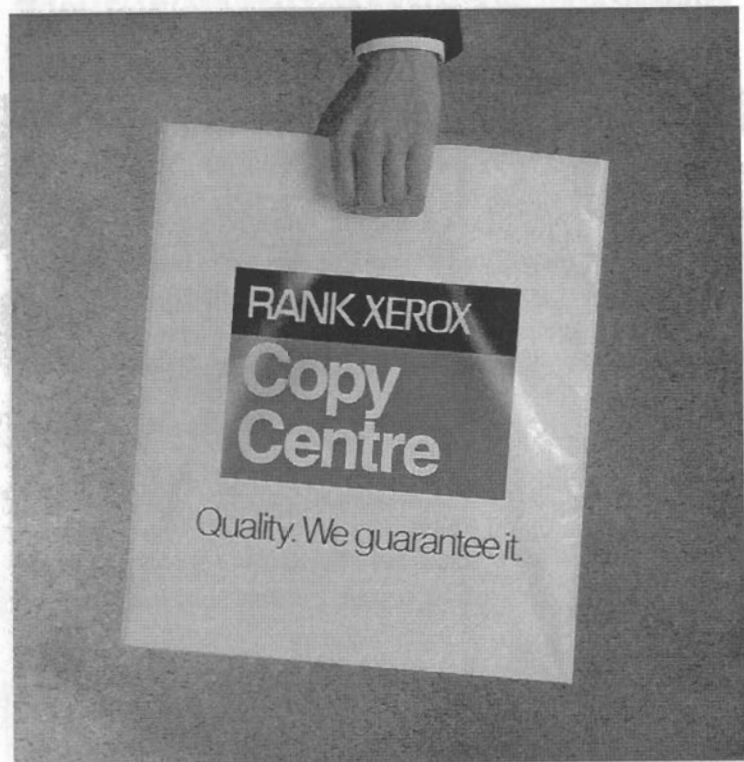
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