Psychology; Psychiatry, History of; Psychological Treatment, Effectiveness of; Psychological Treatments, Empirically Supported; Schizophrenia; Syndromal Diagnosis versus Dimensional Assessment, Clinical Psychology of; Wundt, Wilhelm Maximilian (1832–1920)

Bibliography

Berrios G E, Hauser R 1988 The early development of Kraepelin's ideas on classification: a conceptual history. *Psychological Medicine* **18**: 813–21

Blashfield R K 1984 *The Classification of Psychopathology—Neo-Kraepelinian and Quantitative Approaches*. Plenum Press, New York

Engstrom E J 1991 Emil Kraepelin: psychiatry and public affairs in Wilhelmine Germany. *History of Psychiatry* **2**: 111–32

Engstrom E J 1995 *Kraepelin—Social Section*. In: Berrios G E, Porter R (eds.) *A History of Clinical Psychiatry. The Origin and History of Psychiatric Disorders*. Athlone, London, pp. 292–301

Hoff P 1994 *Emil Kraepelin und die Psychiatrie als klinische Wissenschaft. Ein Beitrag zum Selbstverständnis psychiatrischer Forschung*. Springer, Berlin

Hoff P 1995 *Kraepelin—Clinical Section*. In: Berrios G E, Porter R (eds.) *A History of Clinical Psychiatry. The Origin and History of Psychiatric Disorders*. Athlone, London, pp. 261–79

Hoff P 1998 Emil Kraepelin and forensic psychiatry. *International Journal of Law and Psychiatry* **21**: 343–53

Kraepelin E 1883–1927 *Psychiatrie*. Nine editions: 1883, Abel, Leipzig; 1887, Abel, Leipzig; 1889, Abel, Leipzig; 1893, Abel, Leipzig; 1896, Barth, Leipzig; 1899 (2 vols.), Barth, Leipzig; 1903/04 (2 vols.), Barth, Leipzig; 1909–15 (4 vols.), Barth, Leipzig; 1927 (2 vols.), Barth, Leipzig

Kraepelin E 1918 Ziele und Wege der psychiatrischen Forschung. *Zeitschrift für die gesamte Neurologie und Psychiatrie* **42**: 169–205

Kraepelin E 1919 Die Erforschung psychischer Krankheitsformen. *Zeitschrift für die gesamte Neurologie und Psychiatrie* **51**: 224–46

Kraepelin E 1920 Die Erscheinungsformen des Irreseins. *Zeitschrift für die gesamte Neurologie und Psychiatrie* **62**: 1–29

Kraepelin E 1983 *Lebenserinnerungen*. Hippius H, Peters G, Ploog D (eds.), with the collaboration of P Hoff and A Kreuter. Springer, Heidelberg Berlin New York. English translation (1987): *Memoirs*. Springer, Berlin

Van Praag H M, Kahn R S, Asnis G M, Wetzler S, Brown S L, Bleich A, Korn M L 1987 Denosologization of biological psychiatry or the specificity of 5-HT disturbances in psychiatric disorders. *Journal of Affective Disorders* **4**: 173–93

Verwey G 1985 *Psychiatry in an Anthropological and Biomedical Context—Philosophical Presuppositions and Implications of German Psychiatry 1820–1870*. Reidel, Dordrecht

Weber M M 1991 Ein Forschungsinstitut für Psychiatrie. Die Entwicklung der Deutschen Forschungsanstalt für Psychiatrie München 1917–1945. *Sudhoffs Archiv* **75**: 74–89

P. Hoff

Kuhn, Thomas S. (1922–96)

1. Life and Works

Thomas S. Kuhn was born July 18, 1922 in Cincinnati, Ohio. He earned a Bachelor's degree (1943) in physics at Harvard University. During his graduate work in theoretical solid state physics with John H. van Vleck who later won a Nobel prize, he was introduced to the history of science by James Conant, then President of Harvard. In 1947, while preparing a course on the development of mechanics before Galileo and puzzling over passages of Aristotle's physics, he discovered something that set the agenda of his further work. When he read Aristotle with the set of contemporary physical concepts in mind, much of Aristotle's physics seemed either awfully wrong or even outright meaningless. But by changing the ascribed meaning of some of the Aristotelian key concepts (in some cases only slightly and in others more deeply), a whole new world of physics became accessible, namely that of Aristotle and his contemporaries. After completing his Ph.D. in theoretical physics in 1949, Kuhn switched entirely to the history of science where he began by applying the insights he gained through his experience of Aristotelian physics. From 1948 to 1951, he was a Junior Fellow of the Harvard Society of Fellows, and from 1951 to 1956 Assistant Professor for General Education and History of Science. From 1956 to 1964, he was a member of the faculty of the University of California at Berkeley, mainly in the History of Science Department. In 1957, he published *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*, which became a very successful textbook. Kuhn spent the year 1958–9 as a Fellow at the Center for Advanced Study of the Behavioral Sciences in Stanford where he became aware of the different patterns typical of the interactions among natural scientists on the one hand, and among social scientists on the other. The difference typically concerns a broad consensus of natural scientists with respect to the foundations of their discipline, in contrast to social scientists who typically dispute them.

This observation became very important when Kuhn wrote *The Structure of Scientific Revolutions* (*SSR*) which was published in 1962. This is the most widely read book ever published in the history and philosophy of science, and it also had a tremendous impact in the social and behavioral sciences, most of all in the sociology of science. Its key term, paradigm, is now a household word. Some of its other central concepts like normal science, paradigm shift, or incommensurability are now commonly used in a large number of diverse disciplines. However, many of the interpretations the book (or its parts) has been given were vigorously rejected by Kuhn as deep misunderstanding of what he had intended to say. By now, more than one million copies of the book have been

sold in English, and it has been translated into at least 26 languages. In 1964, Kuhn joined Princeton University as a Professor of History of Science; he was also a member of the Institute of Advanced Study from 1972 to 1979. Kuhn's historical work after *SSR* mainly concerned the genesis of quantum mechanics. *Sources for the History of Quantum Physics* (Kuhn et al. 1967) contains an inventory of relevant material including interviews with many of the contributors to quantum mechanics. *Black Body Theory and the Quantum Discontinuity, 1894–1912,* (Kuhn 1978) is a controversial narrative of the introduction of the quantum hypothesis. Some of Kuhn's papers both on the history and the philosophy of science are contained in his *The Essential Tension*, published in 1977. In 1979, he became Professor for Philosophy and History of Science at the Massachusetts Institute of Technology until he retired in 1991. From the 1980s on, Kuhn worked on a book whose final working title was *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*. In this book, he tried to elucidate the foundations of scientific change by a new theory of meaning of empirical concepts, especially of taxonomic kind terms. Furthermore, he elaborated on the evolutionary metaphor for scientific development with which *SSR*'s last chapter ended. Kuhn could not finish the book due to his death on June 17, 1996, in Cambridge, MA.

2. *Kuhn's Theory of Scientific Development*

Kuhn's main subject in the philosophy of science was a developmental scheme, or a schematic description, of scientific development which primarily concerned the basic (i.e., curiosity-driven) natural sciences. The intended description is schematic in the sense that it presents scientific development as a succession of different phases whose main features are independent of the specific subject matter of the respective science. For each one of these phases a specific mode of scientific practice is characteristic. This theory was first presented in *SSR* but was later refined in various respects.

The developmental scheme that Kuhn suggests should hold with only minor variations for all basic scientific disciplines. It consists of the following. Before reaching maturity, nascent scientific fields typically are characterized by controversies between competing schools. There is no consensus among the practitioners of the emerging field. Each one of these schools has a particular view of the respective research domain, and typically these views derive from extrascientific sources. Kuhn has called this phase of scientific development 'preparadigmatic.' The competition among the schools eventually may end when one group produces an exemplary solution to a preeminent research problem with two characteristics: it is sufficiently unprecedented to attract the members of the other schools, and it is sufficiently open-ended to leave enough interesting problems for further scientific work. These model solutions are called paradigms. They serve to guide research implicitly in the succeeding period called normal science. When a science has reached its first normal science phase it is said to have matured.

Normal science is characterized by a broad consensus of the practitioners of the field about fundamental questions, and consequently, by a particular mode of research. This mode of research can be described by a five-dimensional analogy to puzzle-solving where exemplars of puzzles include chess problems and crossword puzzles. The analogy concerns:

(a) The existence of regulations constraining acceptable approaches to and solutions of problems. In the case of science, these regulations are given only implicitly, namely as part and parcel of the paradigmatic solutions.

(b) The expectation of the solubility of appropriately chosen problems. In the case of science, this expectation is fed by the conviction that the appropriateness of the paradigmatic solutions extends beyond the particular problems they are solutions of.

(c) Missing intentions of fundamental innovation of the guiding regulations. In the case of science, normal science intends to exploit the cognitive potential intrinsic to the paradigmatic solutions, and does not intend to overturn them.

(d) Absence of test or confirmation of the guiding regulations. In the case of science, this is due to the fact that in normal science, failure to solve a research problem is seen as a failure of the researcher and not of those regulations that guide research. Either the research problem was not chosen well, or the researcher was unable to solve it despite its solubility.

(e) Individual motivations: to prove oneself an expert puzzle-solver. In the case of science, this is evidenced by the extremely elaborate system by which scientific achievements are rewarded (e.g. scientific prizes including the Nobel prize, honorary degrees, invited lectures, nominations, etc.).

Normal science is always confronted with anomalies, i.e., with phenomena or problems that behave contrary to the expectations supplied by the paradigm. Anomalies do not usually call the validity of the guiding regulations of normal research into question. But under special circumstances they may, and then they become 'significant anomalies.' In such cases, the practice of science changes into 'extraordinary science' or 'science in crisis.' It aims at amending or even overthrowing the yet binding regulations. Its research focuses on the significant anomalies and their context. Extraordinary science resembles prenormal science in that it tends to develop competing schools. However, it is more focused than prenormal science as all these different schools have to deal with the same set of

significant anomalies and they all want to retain as much as possible from the earlier period of normal science. If this research leads to a new theory that is accepted by the scientific community because it can lead to a new phase of normal science, a scientific revolution has occurred. Scientific revolutions in Kuhn's sense are thus 'the tradition-shattering complements to the tradition-bound activity of normal science' (Kuhn 1962). The rejection of the older theory is accompanied by a change of the problem-field and its related standards of solution, and by a corresponding change in basic scientific concepts. Some old concepts are discarded, some new ones are introduced, and some change meaning, sometimes in a subtle way. Kuhn even describes revolutions as transformations of the world in which scientific work is done, although it is not easy to make explicit and plausible what is exactly meant by this locution. Kuhn compresses these features of revolutions into the concept of 'incommensurability': a relation that holds between successive traditions of normal science. In *SSR*, the concept of incommensurability had not become entirely clear. It was, therefore, the subject of much criticism as well as misunderstanding. Most of Kuhn's philosophical work after *SSR* aims at a clarification and further explication of the concept of incommensurability.

Because of incommensurability, we must, according to Kuhn, rethink the concept of scientific progress in the natural sciences. First, scientific progress is not cumulative, due to conceptual changes during revolutions. Cumulativity implies that something that is a part of science at some point in time will, neglecting small corrections, remain a part of science forever. But the conceptual changes that occur during revolutions are much more than small corrections of the existing body of knowledge. They amount to a thorough conceptual reorganization, and typically to a changed ontological perspective. This holds in spite of the fact that very often, much of the older knowledge is retained, especially for practical applications, and that, in the case of quantitative theories, limiting relations exist between the older and the newer theory. Furthermore, Kuhn denies that scientific progress is an approach to truth. Instead of conceiving of scientific progress as a teleological process, i.e., one that is goal-directed, we should think of scientific progress in an analogous way as Darwinian evolutionary theory conceives of evolution. Darwinian evolutionary theory states that there is no goal of evolution towards which it is directed. In a similar way, in scientific development there is no 'set goal' which would be 'a permanent fixed scientific truth' that science approaches (Kuhn 1962). However, Kuhn stresses that this does not imply that there is no progress in the sciences. There is progress in the sciences, though not in the form of an increase of verisimilitude. Rather, there is progress in the form of 'an increase in articulation and specialization' of scientific knowledge (Kuhn 1962). Therefore, the widespread characterization of Kuhn's theory as entirely relativist is simply false.

There are several breaks between Kuhn's theory and the previous tradition in the philosophy of science, some of which explain the extraordinary resonance his theory found in the social and behavioral sciences. Six of them will be discussed briefly. The first is Kuhn's altered view of scientific progress that has just been mentioned. Second, this view implies the untenability of those forms of realism which assert that science at least approximately describes what is really 'out there,' independently of any observer. Instead, theories describe the world in terms of concepts that are historically contingent and which may change in the future. Third, because of this change of basic scientific concepts through revolutions, the classical conception of reductionism is also hardly tenable. In this classical conception, the redefinition of the concepts of the reduced theory by means of the concepts of the reducing theory played a key role. But if incommensurability prevails, some of the required redefinitions are impossible because of meaning shifts. Fourth, many of *SSR*'s assertions stood in marked opposition to Popper's critical rationalism. For instance, Kuhn's normal science, as seen from Popper's perspective, is bad science because it is not continuously directed towards critically testing the guiding regulations. Instead, some anomalies are not even pursued, but are ignored or even pushed aside. Alternatives to the paradigmatic framework are also typically ignored. Yet in Kuhn's view, critical evaluation of fundamental theories is restricted to the periods of extraordinary science, and even then, scientific practice is not simply an attempt to falsify theories by confronting them with basic statements about nature.

Rather, theory evaluation is a comparative procedure in which at least two theories are assessed with respect to their cognitive abilities. Thus according to Kuhn, theory falsification as described by Popper is a stereotype that does not occur in the actual history of science. A fifth consequence of Kuhn's theory is the abolishment of the idea that science is guided by the scientific method, as construed as a set of rigorous rules. This idea has dominated the understanding of modern science from its very beginning. But according to Kuhn, exemplary problem solutions guide scientific research in its normal phase. Their cognitive potential for research is not exploited by explicit (or fully explicable) rules, but rather by implicit analogies. New problems are identified in the light of solved ones, and new solutions are judged as legitimate in a like manner.

Finally, and most importantly in our context, for Kuhn the principal agents of science are communities, not individuals. In the philosophy of science tradition before Kuhn, no one questioned the identity of the principle agent of science. But armed with the distinction between individuals and communities, it is obvious that in the older tradition, the individual

scientist was taken to be the principal actor. The possibility of two scientists rationally disagreeing was not permitted, nor were there discussions about the gradual formation of scientific consensus out of disagreement, or of the development of disagreement from a previously established consensus. In other words, scientific communities were not present in the discussion at all. Science was seen as a one-person game, but for Kuhn it is, at its heart, a social enterprise. It is communities who are the ultimate evaluators of knowledge claims. It is communities who ultimately decide between competing theories and their associated modes of research, and it is communities who are seen by themselves and by others as responsible for some domain of scientific knowledge. Kuhn, therefore, repeatedly refers to the 'sociological base of my position' (for references, see Hoyningen-Huene 1993), but at the same time, he insists that this fact does not invalidate the essential epistemological component of his theory, i.e., its normative element. How is this to be understood?

Kuhn characterizes scientific communities by the values they hold. Typically, these values comprise accuracy, scope, consistency, fruitfulness, and explanatory power, among others. These values are instrumental in the evaluation of the application of theories and, partly derivatively, in the evaluation of the theories themselves. However, these communal values are not defined so sharply that each member of the community reaches exactly the same evaluation in a concrete case of application. Individuals may differ in the relative weight they attribute to these values as well as in the specific articulation of any one of them. Yet in normal science, these differences typically do not become apparent because there is a consensus about the paradigms. However, with the advent of significant anomalies, this latent difference between scientists belonging to the same community may become manifest due to different evaluations of the anomalies, resulting in dissent. The dissent may continue with respect to the different candidates proposed to resolve the crisis. A new consensus about some theory and its ability to solve outstanding problems can only emerge if, and when, (almost) all members of the community prefer it to its competitors, although their individual differences with respect to the values have not disappeared. Thus, during crisis resolution, the individual differences of the members of the community become irrelevant. It is the communal core of the values that prompts the choice of the new paradigmatic theory. Thus, the choice of the new theory is essentially an event that has to be described in sociological terms, as opposed to psychological ones. On the other hand, the normative dimension of theory choice has not disappeared. The reasons for the choice may still be normatively evaluated. Scientific values are an essential part of the reasons for choosing some theory. They are good reasons if they promote the aims of science. This is typically the case if the values in question are those values mentioned above (i.e., accuracy, fruitfulness, consistency, and the like).

3. *Kuhn's Impact on the Behavioral and Social Sciences*

Kuhn's work has been extremely widely received in the social and behavioral sciences, more than any other philosopher's work in the twentieth century. Before Kuhn, mainstream social science had adopted a picture of the natural sciences that was based mainly on logical empiricism, and many social scientists attempted to assimilate their disciplines to that picture. However, success was limited. Neither the predictive, nor the explanatory power of a natural science like physics could be achieved. In addition, methodological controversy did not cease to exist even if in some field, a particular school became dominant for some period. In this situation, the alternative view of the natural sciences that Kuhn offered also seemed to suggest, as a corollary, a different view of the social sciences. More specifically, as Kuhn had incorporated elements in his description of the natural sciences which had been barred by the earlier tradition in the name of the objectivity of science, such as group processes and the like, the difference between the natural and the social sciences seemed to have decreased. In this way, the already comparatively high interest that social scientists have in the philosophy of science, given the epistemologically insecure status of their disciplines, became even more intense in the case of Kuhn's philosophy. At least to some authors, Kuhn seemed to suggest a kind of fresh start for the social sciences.

Three general facts about the reception of Kuhn's work in the social and behavioral sciences are striking. First, the reception was by and large uncritical, and in many cases very superficial. For example, there are literally hundreds of articles in which the reference to Kuhn appears to be a purely rhetorical device, making no substantial point. Second, Kuhn's developmental model was intended to apply only to the basic disciplines in the natural sciences. Kuhn says very little about the social sciences, basically only that they are typically in the pre-paradigmatic state (Kuhn 1962, 1977). This did not prevent many social and behavioral scientists from applying parts of Kuhn's theory to different aspects of their disciplines. Third, Kuhn's ideas have been used for the most diverse purposes. On the basis of his work, even outright contradictory assertions have been made. This is partly due to the fact that Kuhn's writings, especially *SSR*, have admitted of very different interpretations, which in turn derives from the inner tensions, ambiguities, and metaphors that they contain. In particular, Kuhn's evaluation of the rationality of science appeared as highly ambivalent, and his use of the paradigm concept

seemed at times to cover just about everything with which scientists deal. Thus, Kuhn's texts appear to have a high degree of plasticity, a fact that has been subsequently deplored by their author. Only a very close and patient reading of the texts can reduce this plasticity. But on top of the plasticity of Kuhn's texts, social scientists, due to their own agendas, have been seduced to read many things into Kuhn's work. Thus, many of the claims in the literature about what Kuhn does or does not support are not really contained in Kuhn's texts.

The predominant use of Kuhn's work in the social and behavioral sciences has concerned the current status of some discipline or its history. Is the discipline still in its preparadigmatic state or does it already have a paradigm? Or put another way, is it a 'mature' science which has reached the state of normal science? Has it had paradigms in the past? Are the changes that the discipline experienced in the past Kuhnian revolutions? Is the discipline currently in a Kuhnian crisis? Or are the social sciences fundamentally different from the natural sciences in being 'multiple paradigm sciences'? As mentioned above, these questions received the most diverse answers by different authors. Elements from Kuhn's theory were used by some to defend mainstream social science, and by others to develop a critical attitude towards it, or to defend or attack alternatives. Some authors tried to show that on Kuhnian terms, a given social science is very nearly like natural science, whereas others tried to demonstrate the opposite, and so on. No consensus has been reached in any of the social or behavioral sciences about these and related questions.

Finally, that part of the reception of Kuhn's work to which it was immediately pertinent, namely the history and the sociology of science, should be discussed. Perhaps surprisingly, in the history of science proper, the reception of Kuhn's metahistorical work does not show up heavily in the literature. This may be partly due to the same reasons that Kuhn did not apply his characteristic concepts (like paradigm, normal science, etc.) in his own historical work. Historians very often try to avoid heavily theory-laden concepts when doing historical work because of the imminent distortions of what may be gleaned from the sources themselves. In addition, historians in the second half of the twentieth century have typically been quite critical about any generalizations about the course of history (such as a developmental scheme).

But in the sociology of science, Kuhn's impact has been tremendous. Pre-Kuhnian sociology of science either discussed the social structure of science or the impact of science upon society. But in contrast to other areas of sociology, the scientific content seemed to lie entirely outside the scope of sociology because of the purported objectivity of science, which precluded any direct social contingency. The most relevant sociological question asked with respect to scientific knowledge was 'what are the social conditions of a scientific community that are conducive to the production of such knowledge?' Answers to this question typically consisted in the description of a purportedly universal value system holding in scientific communities that virtually guaranteed the desired outcome of the activities of the community's members. However, with the advent of Kuhn's work, this view was challenged in various respects. The production of scientific knowledge now seemed not at all determined by time-independent, universal methodological rules. Instead of binding rules, research is guided by values which only influence, but do not determine scientific behavior. Such values are influenced by group-specific and even idiosyncratic factors. In other words, scientific knowledge now indeed seemed socially contingent, which licenses and even appears to demand, sociological analysis. Various schools developed analytical tools for this purpose, and a great many case studies of contemporary and past science were performed. Typically, the content of science was now seen as the result of negotiations among scientists in which human interests of a personal or political nature, or power relations, played an important, and sometimes even decisive, role. Due to an intense debate within sociology and interventions mainly from philosophers, these positions showed rapid development that continues today. It should be noted, however, that despite all of the changes in these positions, Kuhn himself remained extremely skeptical towards them. In his view, the epistemological dimension of scientific knowledge, i.e., the possibility of its normative evaluation, must be an essential part of an integrated image of science, but it had been entirely dismissed by the sociology of science (Kuhn 1991, 1992).

See also: History of Science; History of Science: Constructivist Perspectives; Mathematical Models in Philosophy of Science; Physical Sciences: History and Sociology; Scientific Controversies; Scientific Culture; Scientific Disciplines, History of; Scientific Instrumentation, History and Sociology of

Bibliography

Barnes B 1982 *T. S. Kuhn and Social Science*. Macmillan, London

Bernstein R J 1976 *The Restructuring of Social and Political Theory*. Blackwell, Oxford, UK

Gutting G (ed.) 1980 *Paradigms and Revolutions. Applications and Appraisals of Thomas Kuhn's Philosophy of Science*. University of Notre Dame Press, Notre Dame, IN

Horwich P (ed.) 1993 *World Changes. Thomas Kuhn and the Nature of Science*. MIT Press, Cambridge, MA

Hoyningen-Huene P 1993 *Reconstructing Scientific Revolutions. Thomas S. Kuhn's Philosophy of Science* (trans. Levine A T). University of Chicago Press, Chicago

Kuhn T S 1957 *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*. Harvard University Press, Cambridge, MA
Kuhn T S 1962/1970 *The Structure of Scientific Revolutions*, 2nd edn. University of Chicago Press, Chicago
Kuhn T S 1977 *The Essential Tension. Selected Studies in Scientific Tradition and Change*. University of Chicago Press, Chicago
Kuhn T S 1978 *Black Body Theory and the Quantum Discontinuity, 1894–1912*. Clarendon Press, Oxford, UK
Kuhn T S 1991 The road since structure. In: Fine A, Forbes M, Wessels L (eds.) *PSA 1990. Proceedings of the 1990 Biennial Meeting of the Philosophy of Science Association*, Vol. 2. Philosophy of Science Association, East Lansing, MI, pp. 3–13
Kuhn T S 1992 *The Trouble with the Historical Philosophy of Science. Robert and Maurine Rothschild Distinguished Lecture, November 19, 1991, An Occasional Publication of the Department of the History of Science*. Harvard University Press, Cambridge, MA
Kuhn T S 1993 Afterwords. In: Horwich P (ed.) *World Changes. Thomas Kuhn and the Nature of Science*. MIT Press, Cambridge, MA, pp. 311–41
Kuhn T S 2000 *The Road Since Structure. Philosphical Essays, 1970–1993, with an Autobiographical Interview*. Conant J, Hangeland J (eds.) University of Chicago Press, Chicago
Kuhn T S, Heilbron J L, Forman P, Allen L 1967 *Sources for the History of Quantum Physics. An Inventory and Report*. American Philosophical Society, Philadelphia, PA
Lakatos I, Musgrave A (eds.) 1970 *Criticism and the Growth of Knowledge*. Cambridge University Press, London

P. Hoyningen-Huene

Kula Ring, Anthropology of

First set out in detail in Malinowski's classic *Argonauts of the Western Pacific* (1922), the 'kula ring'—a translocal, long distance exchange network connecting islands off the southeast coast of Papua New Guinea (Fig. 1)—has become a classic ethnographic reference point for the study of non-Western exchange practices. Yet apart from more limited work by Fortune (1932), Roheim (1950) and Belshaw (1955), field research yielding significant publications on kula was not carried out again after Malinowski until the 1970s (see Leach and Leach 1983). This article explains key features of kula, updating Malinowski's account where necessary on the basis of the more recent work, and modifying some common over-simplifications; in conclusion, it comments briefly on anthropological approaches to kula.

1. Some Defining Features of Present-day Kula

Modern kula centers in the exchange of white (*Conus*) armshells and red (*Spondylus* or *Chama*) shell necklaces in opposite directions around the islands (Fig. 1). Only middle and upper ranked shells have personal names, but all shells are qualitatively graded. There are other kinds of kula media (see Sect. 2), but only these shells currently circle the region establishing its basic socio-geography. Kula transactors, however, should exchange directly in a limited sector with partners or potential partners in participant communities on either side of them (although illicit bypassing occurs). Working a strategic politics of influence in the inter-island world, they attempt to make names for themselves that also impact on their standing in their own community.

Transactors obtain shells of one category on visits to partners in one direction and later transact them to their own visitors from the opposite kula direction. Typically these exchange practices engage long term debt involving a year or much more between a gift and its equivalent repayment. A major transaction with multiple, additional shell exchanges pinned to its passage may go on for years. Nevertheless, less prestigous exchanges may occasionally be completed with a visitor who brings the appropriate type of shell for immediate exchange, thus avoiding the risks of default inherent in time and distance.

Although kula participants are primarily men, recent research points to the contemporary importance of women traders in the Bwanabwana region (Macintyre 1983a) and, less prominently, in some other areas such as northern Mwadau and Dobu (J. Leach in Leach and Leach 1983, p. 17); nevertheless, women participants do not usually sail on kula. This research has also revealed a crucial type of shell ownership not noted by Malinowski (but probably operative in his time) called 'kitomu' (or cognates), which is fundamental to the *modus operandi* of kula. A shell's first owner is its maker. Owners may do as they will with their kitomu shells, keeping or else alienating them to obtain high value items like pigs, magic and cash or to pay off a kula debt; the shell then becomes another person's kitomu. Alternatively one may circulate one's kitomu in kula; an equivalent return becomes, on receipt, one's kitomu, the prior shell becoming the kitomu of the equivalent's donor. Thus particular kitomu are alienated, but, apart from default, one still owns a shell as kitomu (Damon in Leach and Leach 1983, Munn 1977, 1986, Weiner 1988, 1992 cf. also Godelier 1996, p. 90–1). Kitomu shells, empty of prior obligations, provide an element of freedom in transaction that variously aids owners in the incrementation of their kula.

2. Adjustment of Some Common Oversimplifications of Kula

Following Malinowski's emphasis, a common assumption is that modern kula entails a ritual

International Encyclopedia of the Social & Behavioral Sciences ISBN: 0-08-043076-7