The social context of research practice and the priority of history

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Zusammenfassung: Der Zur Zeit der Dominanz des Positivismus herrschende Glaube an die übergeschichtliche Gültigkeit der naturwissenschaftlichen Methodologie und ihrer Produkte ist durch neuere Arbeiten auf den Gebieten der Wissenschaftssoziologie, Wissenschaftsphilosophie und Wissenschaftsgeschichte ins Wanken gebracht worden. Die moderne Wissenschaftslehre geht davon aus, daß jede real existierende Wissenschaft von den historischen Bedingungen ihrer Existenz in ihrem Wesen geprägt ist. Folglich müssen die Objekte wissenschaftlicher Forschung - selbst die auf theoretischem Vorverständnis beruhenden und von methodologischen Konstruktionen mitbestimmten empirischen Daten - nicht als rein natürliche, sondern als historische Tatsachen angesehen werden. Das geschichtliche Verständnis einer Disziplin wie der Psychologie ist daher keine bloße Zutat, sondern betrifft das Wesentliche.

Abstract: During the period when positivism was dominant scientific methodology and its products were believed to rest on ahistorical principles. More recent work in the sociology, philosophy and history of science has undermined this belief. Modern science studies are based on the realization that the nature of any actually existing science will be profoundly determinded by the historical conditions of its existence. Hence the objects of scientific research must be regarded, not as purely natural, but as historical facts. This even applies to empirical data because they are heavily dependent on theoretical preconceptions and methodological constructions. The historical understanding of a science like Psychology is therefore no mere garnish but is converned with the essential nature of the discipline.

Half a century ago virtually everyone regarded the products of natural science, its discoveries and findings, as being different from other products of human activity in that they transcended the mundane circumstances of their production. Although scientific results were of course produced by people of flesh and blood, working under specific historical conditions, the results themselves were generally seen as independent of these origins once they were received into the canon of genuine science (see however Fleck, 1935).

Such beliefs were reflected in the philosophy of science which predominated in the Anglo-Saxon countries at that time. It had been a primary concern of the logical positivists to set down criteria by which one could distinguish science

from non-science. A standard philosophical rationale for the purity of science was based on an explication of the purity of its language. It was said that in the language of science every statement could be either derived by necessary deduction from first principles, and/or it could be demonstrated to be true by reference to direct empirical observations. By contrast, the mundane language of erveryday life was sloppy and ambiguous; most of the statements it contains were neither derivable by any process of strict logical deduction, nor were they unambiguously verifiable by empirical observation.

American psychologists were understandably fascinated by a philosophy that promised to end the persistent doubts about the scientific status of their field. By spelling out the criteria of scientificity in a very explicit way logical positivism gave psychology a clear set of conditions whose fulfillment should result in broad recognition of the psychologists´ claims to the status of scientists (Mandler and Kessen, 1959). This philosophy seemed to provide the discipline with a prescription for how to become a genuine science, and in the next few decades this prescription was quite thoroughly internalized by large sections of the discipline (Toulmin and Leary, 1985).

In the meantime the world has changed. Attitudes to science have become, on the whole, more ambivalent than they were half a century ago. (When I say „science", throughout this presentation, I am of course using the term in its usual Anglo-Saxon sense of „natural science"). This ambivalence is a consequence of the way in which certain negative aspects of the application of science intruded on people`s consciousness, whether in the form of weapons of mass destruction or in the form of environmental damage or threats to health as veryday facts of life. Inevitably, such developments have tarnished the once unblemished image of science in people´s minds.

As might have been expected, these developments have been accompanied by a change of fashions in the philosophy of science, a change that had already been placed on the agenda by the internal difficulties of logical positivism.

In the post-positivist phase that has prevailed during the last quarter century the differences between science and non-science have not seemed nearly so great and so clear cut as they seemed before, and the need to make this distinction has lost some of its urgency. A new field of social studies of science has flourished, and this field takes scientific activity as an object of sociological examination in exactly the same way as any other human activity (Knorr-Cetina, 1984, provides one well known example for such studies, but the English language literature in this field is by now enormous). Doing science is now seen as being as much a matter of social organization, competiton for scarce resources, social interests, thetorical persuasion and consensus building as many more mundane forms of human activity. Thus is science robbed of its moral exclusiveness. Perhaps we

should see this disenchantment of science as the last stage in the „disenchantment of the world" that Max Weber diagnosed as being characteristic of the modern period.

Within Anglo-Saxon philosophy of science the process of disenchantment manifested itself in a turning away from the relentless formalism that had been so characteristic of the period dominated by logical positivism. The old philosophy had tried to establish the purity of science on the basis of the purity of its language. This was largely based on a strict separation of an empirically grounded data language and a formal theoretical language. Unfortunately, this distinction could never be rigorously justified and soon became a casualty of powerful philosophical attacks (Achinstein, 1968; Hesse, 1970; Hanson, 1959; Quine, 1953). There is no need for me to cover this well known ground here. Suffice it to mention that the empirical statements of experimental science are never formulated in a language that simply refers to sensory impressions but in a theory loaded language. When they are talking as scientists, physicists do not talk about displacements of light points in their visual field, they talk about measuring the length of ligtht waves. But reporting on the length of light waves already presupposes a wave theory of light as well as some theoretical understanding of the measurement process and the instruments it relies on. Psychologists do not empirically report marks on paper but scores on an intelligence or personality test. That presupposes a massive framework of theoretical presuppositions that make possible the identification of certain pieces of printed paper as intelligence tests and other pieces as personality tests, not to speak of the battery of assumptions and decisions that lies behind the concept of a „score"(Danziger, 1990a).

Thus, the so-called empirical statements that occur in scientific reports and texts depend as much on particular theoretical frameworks as they do on the sensory experience of individual scientists. But it is not only that some kind of theoretical framework is necessary for formulating an empirical result, it is also the case that the apearance of the empirical result depends on prior methodological decisions. Experimental facts are not usually discovered just lying around the laboratory or blown in through the window by the wind. They have to be painstakingly constructed with the help of complex instruments and carefully thought out procedures. But the use of one set of instruments and procedures rather than another depends on decisions and assumptions that are certainly underdetermined by any list of scientific facts known at the time. In a very real sense, empirical data are not the starting point of science, rather they are the yield that science ends up with as a result of an elabotrate process of production (Bhaskar, 1978).

It is clear that if we want to understand this process of production we will have to pay a great deal of attention to underlying theoretical assumptions and the methodological choices that play such an important role in the constitution of empirical data. No longer is it permissible to relegate questions about the genesis of theoretical assumptions to some limbo of irrationality, called the „context of discovery", that was created with the express purpose of preserving the purity of scientific rationality in the form of a „context of justification". Real science does not conform to this hopelessly idealized image. The rational and the irrational are thoroughly entangled, or, to put it another way, the rational side is not nearly so rational and the irrational side not nearly so irrational as the older view would have had us believe.

What then is the origin of the implicit and explicit theoretical assumptions and methodological choices that constitute the empirical yield of science? Part of the appeal of the idealized view of science was due to its heroic image of the individual scientist. Scientific activity was the arena of a tournament in which the ingenious man of science (generally it was always a man) pitted his wits against nature (feminine, of course) who was reluctant to yield up her secrets. The products of science therefore depended on three sources: Nature herself, specific psychological qualities of the individual scientist, and certain principles of scientific rationality and morality that regulated the scientist's practice. These last were conceived ahistorically - they were basic rules of ethics, like the rule of honesty, for example, that had not changed since the time of the Ancient Greeks (Ben-David, 1971).

Such an account could be regarded as useful only as long as its function was regarded as prescriptive rather than descriptive. Of course people knew that the actual practice of science included features not covered by this account, but those features were relegated to the history of science, a field of messy detail that was quite separate from the lofty world of principle inhabited by the philosophy of science. However, this separation has not lasted. As the interest in science shifted from a prescriptive interest in an idealized science to an interest in the actual practice of science as a human activity so the study of the history of science has become crucial for an understanding of the nature of science.

Once we see science, not as an idealized abstraction, not as a set of disembodied propositions, but as an activity engaged in by real flesh and blood people, then the heroic image of the individual scientist confronting nature collapses. The timeless canons of scientific method turn out to be not timeless at all but subject to massive historical change as well as profound variation that depend on local conditions and traditions (Danziger, 1990b; van Strien, 1990a). The epistemic acces to nature that science provides is always a collective access,

and the arena within which the ingenuity of the individual scientist is allowed to operate is an arena constituted by social groups whose life and whose struggles are subject to the same mundane constraints as are those of other social groups.

Before the raw givens of nature can become data for science, can become the kinds of things that science can actually work with, they have to be transformed by the collective activity of human investigators. This transformation is not only material - chemical substances have to be purified, for example - but also conceptual. I have to think of the white powder in front of me as a chemical compound with a certain molecular structure before I can set to work on it as a chemist. As P. van Strien has reminded us: „Facts are always interpreted facts. The development of science depends on the success of competing traditions in constructing a plausible account in which these „facts" are interpreted in the light of theory" (van Strien, 1990a, p. 39).

In other words, insofar as objects are objects *for* science they exist within a certain conceptual framework. But the conceptual frameworks of science, as we know very well, have undergone much change in the course of history. They are historical products. It follows that the objects of science which only exist within these frameworks (and take their meaning from them) are also historical objects that change in the course of human history (Danziger, 1993).

To quote the German philosopher of science, Kurt Hübner:

„Meist haben scheinbar gleiche Gegenstände, mit denen es die Wissenschaft im Laufe ihrer Geschichte zu tun hatte, nur eine gewisse Familienähnlichkeit. Ob es der Weltraum ist, die Weltzeit, der bestirnte Himmel, die bewegenden Kräfte der Körper usf., man würde vergeblich etwas in all diesen Gegenständen streng Gemeinsames suchen, das alle wissenschaftlichen Theorien, die ihnen gewidmet sind, wie eine Art roter Faden durchzieht, ein Gemeinsames, das sich langsam erweitert und nicht auf dem kontinuierlich aufgebaut wird. Es fiel uns schwer, allmählich zu begreifen, daß nicht in allen Punkten der Welt die gleiche Zeit abläuft. Es mag uns noch schwerer fallen einzusehen, daß wir keineswegs immer von demselben reden, wenn wir dieselben wissenschaftlichen Gegenstände einst und heute zu erforschen meinen, weil esa keine durchgängigen Identitäten gibt, die sich hier in Strenge durchhalten ließen." (Hübner, 1979, p. 218).

Hübner was speaking of the objects of physics. But if the objects of physics must be regarded as embedded in human history, how much more obvious is this in the case of the objects of psychology. The memory that a contemporary student of the area investigates is not the same object as that which Ebbinghaus tried to study by means of nonsense syllables, and neither of them has more than a tenuous connection with memory as understood by Aristotle (Danziger, 1990c). The individual differences that an Eysenck, for example, believes to constitute

objective features of the world have nothing in common with the individual differences pondered by Carl Jung. The „behaviour" studied by the „behavioural science" of the recent past is a very different object from that which inspired John B. Watson or Lloyd Morgan.

Such historical changes are due to changes in the framework within which different generations of scholars and scientists have operated. But such changes of framework are embedded in a general historical situation that includes the values, the implicit assumptions and the social interests of groups of investigators as well as their placement in a broader sociocultural context whose influence they cannot escape. Thus, if the objects of science necessarily exist within some theoretical framework, and if such a framework is always part of a broader historical context, it follows that the objects of science are historical objects. But in order to arrive at an adequate understanding of historical objects we must engage in historical studies (Jüttemann, 1986). That is why history can legitimately lay claim to a certain priority when we try to understand what it means to study some topic scientifically.

The history of science has, as philosophers like Hübner have pointed out, a propadeutic function. Among other things, it provides us with „a standard against which to judge the scope, validity, and applicability of the methods, principles, postulates, etc., that have been worked out by scientific theoreticians". It also has an important critical function that counteracts the „degeneration" that often sets in when a particular scientific position becomes generally accepted. The position is soon considered „self-evident" and eventually becomes something that can no longer be seriously questioned (Hübner, 1979, p. 94). In psychology one can most readily detect evidence for such a degenerative process on the level of methodology (Gigerenzer and Murray, 1987).

Contemporary philosophers who stress the propadeutic function of history of science had some notable predecessors, for example, Dijksterhuis (1961) and Duhem (1954). The latter believed that „the legitimate, sure and fruitful method of preparing a student to receive a physical hypothesis is the historical method", because, as he put it, „to give the history of a physical principle is at the same time to make a logical analysis of it" (Duhem, 1954, p. 268-269). Duhem advocated the historical approach even though he believed that physical concepts became „perfected" in the course of history, in other words, that later versions were in some real sense better than earlier versions. This is a position that is easier to maintain in physics than in psychology. Duhem showed that even though the more recent position might be more satisfactory scientifically one could not really understand scientific development without introducing the historical dimension.

In psychology the superiority of more recent positions is often not as obvious as it is in physics. How much more reason then to turn to historical considerations when trying to assess the status of recent „advances" in psychological theory. Of course, this means running the risk of discovering for oneself the truth of the old dictum that those who ignore history are condemned to repeat it. But that seems preferable to a state in which one lacks the means to distinguish real progress from the illusion of progress.

But is it still possible to make such a distinction if one historicizes science to the extent that I have? Let me try to answer by invoking an analogy. In the traditional view the process of science was seen rather like the painting of a picture, a landscape perhaps, that was gradually being represented on canvas. Both, the landscape and its pictorial representation were vast and complicated, but over a long period of time more and more details would be filled in, until one day the picture would be finished, and we would be able to enjoy the perfection of the final product.

To-day we know that we have to make some changes in this analogy. The picture we are confronted with rather resembles one of those canvases that has been painted over many times by different painters. After the first one each of them has reacted both to the work of his predecessor and to the landscape as he saw it. Of course, they didn't all see it in the same way, and in painting it they pursued different purposes. They learned from the techniques of their predecessors, but they also reacted against their apparent inadequacies. Each of them thought of his predecessor's style as old-fashioned and of his own work as advanced. This process need never end. But from the fact that there may always be a fresh artistic style, it does not follow that there exists no landscape to be painted. It also does not follow that one painting is as good as another. Some are better. But that is a judgment which is only possible on the basis of certain criteria which change historically. Does *this* change constitute progress? The problem is one of definition. There is no such thing as absolute progress, only progress in this or that sense. And that means that progress is often ambiguous - which is exactly the conclusion many people have arrived at with regard to scientific progress, or economic progress for that matter.

In concluding my argument I cannot improve on the words of P. J. van Strien: „It cannot be denied", he writes, „that awareness of the history of current theories also leads to a sense of the relativity and temporality of all theories. Of course, this will only lead to resignation if we are still hoping for timeless Truth. Problem situations are always historically relative situations. Can we then expect the answers to be context free?" (van Strien, 1990b, p. 313).

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