
HPS and STS: The Links

Aristides Baltas

Abstract

The paper presents the large contours of the history of two academic fields, namely History and Philosophy of Science (HPS) and Science and Technology Studies (STS), from their inception until their present state, and within the strange relations, or lack thereof, they have been entertaining with each other. The narrative tries to bring into focus both the conceptual and the institutional lines of development of those fields as well as to locate their progress in the various countries and traditions implicated. Emphasis is given to the currents of thought that had emerged in the intellectual scene of Paris in the 1960s because these continue to be largely misunderstood, despite their present day importance

Introduction

What I propose to examine in the present paper, and this in the extremely cursory manner allowed, not only by my all too evident shortcomings but also by the space and time at my disposal, are the developments undergone by the two academic fields uniting all of us here today, namely that of History and Philosophy of Science (HPS for short) and that of Science and Technology Studies (STS for short). These developments will be traced back to the beginnings of each field and will be followed up to the state each finds itself in at the present time. It is my hope that, my barbarously schematic treatment notwithstanding, we will have the occasion for ascertaining—and hence the possibility of discussing—the fact that these fields have followed separate paths and that the two remain distinct, despite the all too obvious links that, as a matter of philosophical principle at least, hold, or rather should hold, between them.

It is evident that the view I am going to present could only have been based on my readings, such as they are, and on the experience I have gained while working on these matters in Greece. I mention Greece

explicitly, for even the very short time I have been in Graz has made me realize that, at least in matters intellectual but perhaps not only intellectual, Greece and Austria bear striking similarities: they both can exhibit a glorious past, an uncertain present and a worrisome future. If the glory of the past and the uncertainty of the present are too obvious to bear elaboration, the worrisome character of the future has to do with the fact that Austria and Greece, both having the size they do, lie on the periphery of those parts of the world, basically the US but also the bigger countries of the EU, which play the leading tunes in respect to the production, the propagation and the reception of new ideas and of new fashions within either HPS or STS, as well as in respect to the foreseeable future of the related academic institutions and research organisations. In the case of Greece in particular, the upcoming Olympic games promise to tear down everything that has managed to remain standing despite all kinds of valiant efforts to the contrary.

Regarding what is here at issue, the main difference between Austria and Greece lies in the fact that the German language relates Austria directly to its glorious past while the same cannot be said of Greece. Although important traits of continuity tie Modern to Ancient Greek, philosophy and the entire spectrum of humanities disciplines have been practically non-existent throughout the four centuries of Ottoman rule and for many years thereafter. Regarding the formulation of new developments within those disciplines, Ancient Greek has given its place to Latin and Latin subsequently to French, Italian, German and now English. The modern Greek language has thus been severed from its past and has become almost totally isolated: except for native speakers, and it does not constitute a vehicle of thought and of expression as regards issues within either HPS or STS. Even if the situation is not so dramatic for the German language, and hence for Austria, it is an indisputable fact that, here as in most other places, we are obliged to communicate in English.

On the other hand, Greece is, in some sense, much more like the US than Austria. I mean that younger people having been obliged to study abroad in various countries, and hence bearing all kinds of philosophical and intellectual traditions, have started to return to Greece, in

order to find jobs and to leave their mark on the fields we are discussing. From this point of view, Greece constitutes a kind of melting pot. But a melting pot where nothing melts! Although each of us has at times either the opportunity or the obligation to talk to colleagues on matters of supposedly common interest, the effective work that each one of us does still remains very much isolated from the work done by the others. Whatever we manage to come up with, we do it alone. It is for this additional reason that what I am about to say is my responsibility and mine alone.

The emergence of philosophy of science

The mother tongue of philosophy of science, at least as we conceive this discipline today, is undoubtedly German, while equally undoubtedly, its birthplace is Vienna. A good starting point for ascertaining what this both means and implies is the opening phrase of J. Alberto Coffa's by now justly famous book: *For better and worse, every philosophical development of significance since 1800 has been a response to Kant*.¹ From the point of view that concerns us here, I take this phrase as summarising succinctly both the sweeping scientific developments that cried for a new philosophical framework, radically different from that of Kant and in direct response to it, and the philosophical developments themselves, leading to the formulation of the Vienna Circle agenda, an agenda practically synonymous with the entire field of philosophy of science, at least until the 1960s.

The story starts with Kant because it was Kant who had managed to give seemingly unshakable foundations to both the physics and mathematics of his time, while encompassing these in a masterful philosophical system of truly impressive unity and convincing power. However, from the middle of the 19th century onwards, the developments first in mathematics and then in physics started exhibiting recalcitrant characteristics: the Kantian framework experienced more and more difficulties in trying to accommodate them philosophically. Non-Euclidean geometries, Cantor's theory of sets, the arithmetisation and the concomitant rigourisation of the calculus by Bolzano, Weierstrass, Dedekind and others,

the fundamental crisis in mathematics, Hilbert's work on geometry and on meta-mathematics, the new logic of Frege and Russell on the one hand, together with the appearance and confirmation of the special and then the general theory of relativity, as added to the then incomprehensible developments of quantum mechanics on the other hand, were showing more and more clearly that the Kantian pure intuitions and a priori categories as well as the Kantian synthetic a priori itself were not up to the task of accounting for them philosophically. A radically new philosophical framework was called for.

The construction of such a framework, however, was neither easy nor straightforward. The Kantian views were so deeply entrenched during all this period that, to give but a single example, all three major schools attempting to found mathematics on a secure basis (Hilbert's formalism, Frege's logicism and Brouwer's intuitionism) were obliged to appeal to Kantian ideas, albeit to different ones, in order to formulate and to implement their programs.

Nevertheless work continued unceasingly, work conducted mostly in German. This is to say that all the protagonists of the mathematical and physical developments in question, with the exception of Russell and a few others, were speaking German while also their main interlocutor, the one proving incapable of accommodating such developments philosophically, was speaking German too. It was thus only natural that the outcome of such work, the new philosophical framework required, would itself be formulated in German. The logical positivism and the subsequent logical empiricism of the Vienna Circle finally emerged to fill the bill.

The agenda of logical positivism and of logical empiricism can be conceived as part and parcel of the program making up the radically new approach to philosophy named philosophical analysis. This was an approach that cut philosophical questions down to size and analysed the language they were cast in by means of the new logic of Frege and Russell, so as to finally solve or dissolve them once and for all. The program was particularly exciting, filling its protagonists with enthusiasm, for it appeared as itself scientific and thereby as capable of laying finally to rest all the long-standing philosophical puzzles. The corresponding final solution was in sight and hence the true scientific character of the philo-

sophical endeavour itself could at last be established. The component of the movement devoting itself to the philosophical analysis of science for its part neatly separated the 'context of discovery' from the 'context of justification' and concentrated on the latter in order to come up with a satisfactory analysis of the structure and of the logical characteristics of mature science. It based itself on the new logic for the purpose, and resorted to some of the main ideas of either positivism or classical British empiricism while the concomitant ideas of empirical content, theory reduction and the like made the developments in physics appear as capable of being finally accounted for philosophically. In the process, the philosophy of mathematics became a fully separate discipline and was left to its own devices.

The rise of Nazism and the upcoming war dismantled the philosophical community in Austria and in Germany, obliging most of its members to emigrate to the free English-speaking countries and particularly the US. The philosophical soil there was relatively virgin and therefore particularly hospitable to new views while the philosophical schools already at work in that country, for example American pragmatism, tended to be congenial to the ideas brought in by the immigrants. Accordingly, within a relatively short period of time, these new ideas managed to dominate the American philosophical scene. By the end of the war, scientific philosophy and philosophy of science had established themselves firmly in American academic and research institutions while, in this sense, Carnap, Reichenbach, Feigl, Hempel and others had been effectively transformed into major American philosophers. By the end of the war, philosophy of science was already speaking English

From philosophy of science to HPS

The story has now to switch to the still independent discipline of history of science for, by the end of the war, new and exciting developments had already started to happen within it.

It should be noted that long before that time, history of science was already a legitimate academic discipline. This, although it represented

only a relatively minor part of the programs of history departments, focusing either on the aspects related to science of particular historical periods, like the ancient world, or concentrating upon the description of major scientific discoveries and the biographies of major scientists. It was the publication, among other works, of Herbert Butterfield's *The Origins of Modern Science* in 1949, of *The Mechanization of the World Picture* by E.J. Dijksterhuis in 1950 and of various essays by Alexandre Koyré, written between the 1930s and 1950s, and collected in different volumes, among which the *Galilean Studies* and the *Newtonian Studies*, which changed the picture radically. From that point onward, a new domain of study had been established, a domain possessing its own object, its own conceptual means, and its own particular methods. This is the domain that centred on the Scientific Revolution and which subsequently developed so as to encompass most of the later developments of science. This is the domain that concentrated on the history of scientific concepts and the establishment of scientific laws and theories called 'internal history of science' ever since.

These developments affected philosophy of science almost directly. For it started to become obvious not only that the Scientific Revolution could and should form a major area of study in its own right but also that such study would be capable of shedding new light on the conceptual structure, on the methodology and on the various other internal relations and characteristics of contemporary science. Contemporary science itself was not devoid of history and the careful study of this history should be capable of providing important lessons for its own fuller understanding. In this way, all the ingredients for the establishment of HPS were firmly set into place and the institution of the corresponding departments or programs started to become almost an obligation for many universities.

By contrast, the program of logical positivism, despite the liberalisation that turned it into logical empiricism, did not enjoy perfect health during the same period. Among other things, the seemingly irresolvable problems of induction, the issues around basic statements, protocol sentences and the like, the insurmountable obstacles encountered by the efforts to reduce systematically theoretical to observation terms, the

problems induced by the Duhem-Quine thesis, Hanson's arguments regarding the impossibility to cleanse observation from theory, were perceived as so many blows to the ambition driving the supporters of logical empiricism. In this climate, the importation of history of science within the discipline of philosophy of science and the concomitant creation of HPS appeared to promise a new way out.

And this is exactly what happened. Tom Kuhn's *The Structure of Scientific Revolutions*, published in 1962, with its amazing worldwide success, came as the thunder blow that changed the picture drastically. New concepts, new puzzles, new areas of focus, new major approaches took over HPS with remarkable rapidity. Philosophy of science had taken its historicist turn by marshalling history of science to serve its own purposes, while by the same token, logical empiricism was relegated to the dustbin of history. Or so it seemed at the time.

Theory change, theory comparison and theory choice, incommensurability and scientific progress, rationality and relativism, were the new issues brought forth by the historicist turn which, in order to be tackled, required the devising and the development of new conceptual means and new general approaches. Kuhn's paradigms and scientific revolutions, Lakatos's hard cores and research programs, Feyerabend's devastating criticisms and methodological anarchism, Laudan's research traditions, with all their close followers and bitter opponents, came thus to dominate the scene of HPS for almost two decades.

Subsequently however, a kind of disillusionment started to settle in. The problems these approaches faced multiplied with no definite solution in view and no consensus forthcoming. The consequence was that all general approaches to science, of either the logical empiricist or the historicist variety, started to be perceived as congenitally incapable of coming up with a viable overall picture of science. HPS then began to split into many sub-branches each of which followed its own path, almost without regarding what was happening in the others. The most important shift was a new in-depth concentration on the particular disciplines. Philosophy of physics proper, focusing on the particulars of quantum mechanics and on those of theories of space and time or on the peculiarities of statistical mechanics and of quantum field theory; philosophy

of biology, focusing on the theory of evolution, on the neo-Darwinian synthesis or on issues regarding the autonomy of biology; philosophy of medicine at both the cognitive and the ethical levels; the workings of psychology and of psychoanalysis; philosophical readings of the achievements of neuroscience or concentration on the philosophical dimensions included in cognitive science and related to various issues in the philosophy of mind, started to gain many adherents and to form the main facet of the philosophy dimension of HPS. In addition, the more technical aspects of philosophy of science, as centred on probability theory and on Bayes's theorem on the one hand, and on the methods brought in from the developments in Artificial Intelligence on the other, attracted other adherents, for these approaches promised to yield definitive results concerning at least the confirmation features of science. Finally, a return to more straightforwardly philosophical issues, such as the debate between scientific realism and antirealism, as manifested in the works, among others, of Ian Hacking, Bas Van Fraassen and Nancy Cartwright, added yet another element in the breakup of the previously fully integrated HPS. In sum, the philosophy dimension of HPS became irremediably split into various distinct and fundamentally unrelated parts and has remained in that state ever since.

We should note that Ian Hacking's *Representing and Intervening*, published in 1983, had important effects on the subsequent fate of this breakup itself. For the work in question, while offering a concise, highly articulate, summary of the development of HPS up to the period it was written, and while arguing for the author's specific brand of realism (the realism of entities), it also opened up the issue of the relative autonomy of experimental traditions in respect to theory construction. This aspect of the work led the way to in-depth studies of specific experimental procedures and on how experiments are actually being conducted, bringing into the picture the conventional and, more generally, the social aspects of experimental practice. A good example here would be Peter Gallison's *How Experiments End*, published in 1987. It thus started to appear that internal history of science, focusing exclusively, as it did, on concepts, laws and theories, could do no real justice to the complexity of the scientific enterprise. This is to say that the history dimension of HPS

had started to have troubles of its own, troubles that would become accentuated by ideas coming from a quite unexpected direction.

From social constructivism to science studies

A philosophical approach claiming to be a direct descendant of Kuhn's was the 'Strong Program', the fundamentals of which were formulated by Barry Barnes's *Scientific Knowledge and Sociological Theory*, published in 1974, and by David Bloor's *Knowledge and Social Imagery*, published in 1976. Bloor and Barnes, two sociologists of science working at the University of Edinburgh, claimed that the cognitive aspects of science are in effect socially constructed and thus fully dependent on factors such as interests, conventions, traditions, power relations and prestige. Accordingly, scientific achievements should be gauged in terms of social success rather than truth. The program in question styled itself 'strong' precisely because it set as its task the establishment of this highly contentious reduction of the whole phenomenon of science, as it was treated up to then by both philosophers and historians of science, to sociology of science, as conceived by these two authors.

The issue of relativism, brought forth by Kuhn's work and endowed with additional vividness through Feyerabend's verve, offered grounds for an initial hearing of the claims the Strong Program was urging: the views of Barnes and of Bloor were given the floor of HPS, gaining thereby almost instant notoriety. But philosophers very soon turned their backs on them. Very few were convinced while most considered the Strong Program as too extravagant a hypothesis even to start to take seriously. Paradoxically enough however, the program in question had decisive corrosive effects on the history dimension of HPS as it was conceived and practiced up to then.

To become at least viable, the main claims of the Strong Program had to pass the test of serious study of particular episodes in the history of science, viewed now as a field where social forces and social relations had always been the exclusive actors. The notoriety already acquired by the guiding ideas of the Program in question and the concomitant fact

that sociologists of science were playing now at centre stage armed many of them with strong enough motivations to undertake painstaking work in that direction. As a result, all kinds of studies of science were soon flooding the market, while quite a few historians of science of older persuasions were converted to the new kind of approach. Andrew Pickering's *Constructing Quarks*, published in 1984, Stephen Shapin's and Simon Schaffer's *Leviathan and the Air Pump*, published in 1985, and Mario Biagioli's *Galileo Courtier*, published in 1993, are some notable examples of studies carried in this direction. Works such as these, in conjunction with the heated debates among their authors and the corresponding supporters or opponents created a relatively large, closely knit community wherein historians of science that had remained internalist did not have too big a role to play. To put it almost brutally, the internal history of science became all but dead and buried while the ineradicable historical dimension of science became almost fully absorbed within the new imperium of this kind of sociology of science.

In conjunction to these developments, instruments of work and forms of approach peculiar to other disciplines which, up to then, had nothing to do with the study of science proper were brought into the picture to underwrite, from yet another angle, the exclusively social character of science. For example, the conceptual means of social anthropology and of ethnology were marshalled into the picture by Bruno Latour's and Steve Woolgar's *Laboratory Life*, published in 1986, whereby a new and quite surprising view of the scientific enterprise in its entirety saw the light of day. Quite a few others, needless to say, followed their example.

To cover approaches as disparate as these, approaches whose sole unifying trait was the eminence accorded to the social character of science, a new name, carrying all the corresponding latitude, was required. In addition, and if at all possible, this name was also to take into account the fact that this wholly new enterprise was launched and carried through its initial stages by *bona fide* sociologists of science. Happily, the two requirements could be simultaneously satisfied in the sense that a single name, already existing, could cover them both. This is the name 'science studies' which, by then, had a short history of its own. This is precisely the history of the beginnings of STS.

STS and its development

Sociology of science had existed well before these developments. Without touching the internal characteristics of science, it focused on issues related to the general social and political context within which the scientific enterprise evolves, on the social relations at work in scientific laboratories, on the various factors influencing the careers of scientists or the reception of their work, and other issues of the kind. Obviously enough, sociology of science worked in tandem with the externalist history of science which brought in the time dimension characterising the social aspects of science by studying these in different historical contexts. To the extent that sociology of science and externalist history of science concerned themselves with the products of scientific activity, with their social functioning and their technological applications, they shared grounds with the still underdeveloped field of history and philosophy of technology, often giving rise to the corresponding academic alliances.

On the basis formed by such alliances, some universities, most of which included a strong engineering component, proceeded to the establishment of specific academic units that would give structure to the forces at work in those areas by assigning to them well-defined aims. In this way, programs concentrating on the relations between science and technology, science and society, and technology and society started to emerge. The name STS, standing either for Science, Technology and Society or for Science and Technology Studies, was attributed at that time to the new academic units in question and has remained with us ever since.

The fact that STS programs were set up mainly in relation to the engineering schools of the universities involved endowed at least some of them with the corresponding practical orientation. The relations linking science, technology and society were not only to be studied theoretically or in the abstract. Issues of science policy and science management, the effective evaluation of the impact of technological products and innovations on specific social groups or on society at large, questions and methods related to risk assessment, the relevant ethical and political dimensions or parameters, were supposed to form important aspects of the corresponding curricula. In some cases, the work done in those units should

also be conceived of literally as fieldwork carried on outside the university bounds, in direct relation to the production units or the social groups implicated each time in the relevant issues.

It was within STS already structured in these ways that the Strong Program was formulated initially and had its subsequent explosive effects, driving most STS programs to modify their aims and to concentrate their forces on studies of science of the kind we mentioned. But the effects of this explosion did not remain merely an internal affair of STS. In the process, the new ways of conceiving history and sociology of science acquired enough academic clout to boost the prestige of STS and turn it into a serious contender for hegemony in respect to the overall study of science, in direct competition with HPS. The battle may not yet have reached its definitive outcome, but the net result is that, for all practical purposes, the study of science can no longer be conceived as falling under the traditional disciplines of philosophy, internal and external history, and sociology of science. By now, the study of science is carried out within two distinct academic units, concentrating on very different aspects of science and entertaining minimal relations with one another, if not constituting irreconcilable enemies, units whose names have remained the highly conventional and by now inaccurate HPS and STS.

As we said, although the Strong Program started as a fundamentally philosophical perspective on science, the successes it scored had much more to do with history of science. But a work in history of science cannot be successful, and cannot be recognised as that, if it follows inflexibly a dogmatic philosophical agenda, an agenda that itself sets as its task to 'prove'. As everybody had known for a very long time, the history of science, of whichever variety, needs to be relatively independent from specific philosophical positions; it should both be and appear to be relatively independent from very specific philosophical commitments. This is to say that the protagonists of the approach we are discussing found themselves obliged to attenuate substantially the stronger philosophical views they initially held and which had motivated them in the first place and thus become much more 'reasonable'. Success could be bought only at this price.

On the other hand, no scholarly enterprise can move in a total theoretical vacuum and do without philosophical views altogether. To be undertaken in the first place and in order to reach its assigned goals, every such enterprise requires at least the modicum of theory that fixes its orientation, that articulates its methods, and that assures the effective functioning of the necessary normative constraints. While being attenuated, the initial views of the Strong Program on what science is and on how it works were still there to assure precisely these roles. However, it is a fact that these views did not remain alone for long. Very soon, an altogether novel theoretical configuration started to dominate STS, a configuration that came effectively to occupy the place that philosophy used to occupy within 'classical' HPS, a configuration for which no better name than that of 'theory' has been coined yet. To see what 'theory' amounts to and how it functions within STS we have to pass through what we may call the 'French connection'.

The French connection

In ways we will be examining shortly, the French connection brought into STS an array of names, such as Claude Lévi-Strauss, Michel Foucault, Louis Althusser, Jacques Lacan, Jacques Derrida, among others, names that had become famous in France, and not only in France, since the 1960s. These names were not only attached to the guiding ideas of the corresponding authors, as these had been received at US universities since then. They were also attached, in multiple and complex ways, to a wholly new set of areas of study, all of which had something or other to do with the composite identity of people, that is with the various layers or strata that such an identity then began to be perceived as being made up from. The issues of gender, of race, of sexual orientation, of ideology and of popular culture, of power relations at the everyday level, of the subaltern status of people excluded from decision centres and from high culture, of the post-colonialist situation in various parts of the world, started to be considered as essential to the study of human activity in general and of the products of such activity as well as for the

overall role and function of these products themselves. Hence also for the study of science and technology and of their products.

Both the importation of these French names and the formation of the new areas of study were not particular to STS. Practically all the humanities and social science departments of most US universities were correspondingly affected, the most notable exception being perhaps those of philosophy and of HPS. The net result was a set of very important, though controversial, changes in the curricula, in the internal organisation and in the corresponding interdepartmental relations, changes that had important repercussions in the overall structure of at least the relevant faculties of arts and sciences. To see how these changes occurred and became stabilised, we need to go through a detour whose main station is precisely Paris of the 1960s.

Paris in the 1960s

In the 1960s, Paris witnessed a radically novel movement of ideas, which many people at the time perceived as a kind of philosophical renaissance. This is to say that a new philosophical agenda was put forth, an agenda containing many and extremely varied side issues, but appearing nevertheless as possessing a single unifying thread. At the time, commentators named this thread 'structuralism' and more or less stuck to it despite the fact that practically all the authors concerned denied vehemently that such an appellation had anything to do with what they were actually doing or trying to achieve. To keep the measure of such denials, commentators sometimes named the movement in question 'post-structuralism', without, however, clarifying much either what exactly this 'post' was supposed to have modified or replaced or what is the exact content of this 'post' itself. To my mind at least, although the concept of 'structure' did play various roles within this movement, the philosophical agenda in question had aims much more ambitious than the elaboration and application of a concept. These aims amounted to no less than the elevation of the social and human disciplines to the dignity of proper sciences.

The paradigm was the structural linguistics of Ferdinand de Saussure whose *Cours de Linguistique Générale* was published posthumously by his students in 1916. Most of the protagonists of the movement in question considered Saussure as having endowed linguistics with its proper scientific status by proceeding to a particular set of substantial and methodological moves. Speaking extremely schematically, this means that this work was considered as having for the first time carved out the proper object of study of linguistics with the help of a particular, systematically knit together, set of concepts established for the purpose, among which that of 'structure' played a preponderant role. To put it very roughly, the object in question is made up from the structure of *langue*, as distinguished from the workings of *parole* that covers all the various acts of actual or possible enunciation. *Langue* itself, split into the levels of phonology, morphology and syntax, can then be studied by the system of concepts, which establish in the first place these distinctions themselves as well as their consequences. This co-constitutive relation between the object of linguistics and the system of concepts offering its knowledge was taken to be the paradigmatic move that all disciplines should go through, if they were to be accorded the status of proper sciences. For reasons we will broach in a moment, it was taken for granted that, in becoming what they are, the physical sciences had already passed through such procedures.

Lévi-Strauss was the first to take the lead and present his structural anthropology as founding scientifically the discipline of social anthropology. More or less at the same time, the *Annales* historians claimed that their way of practicing history was scientific while Althusser presented Marx's *The Capital* as founding the 'scientific continent' of history, that is as forming the basis for the scientific study of everything social and everything historical. Lacan, on his part, claimed that, through his reading, Freudian psychoanalysis had acquired the status of the science of subjectivity while we can see under a similar light what Foucault tried to do in respect to the history of ideas and institutions or what Derrida was after with his 'science' of the letter or 'grammatology'. Roland Barthes came up with a theory of literature and of general semiology also claiming scientific status while theoreticians of the cinema

tried to achieve something analogous in respect to their own object of study. We see then that the protagonists of the movement in question may have worked on different existing disciplines or on none in particular (in that respect, Derrida as well as Gilles Deleuze are cases in point), they may not have appeared as agreeing much with one another, they, or at least some of them, would not perhaps accept the placing of their work under such a banner, but there is enough textual and inter-textual evidence that can buttress our thesis: if Spinoza was imbued with the idea of God, the protagonists of the Parisian philosophical renaissance of the 1960s were imbued with the idea of science.

The same thesis can be corroborated from yet another angle. At the time all these authors started to work, the dominant intellectual figure in France was undoubtedly Jean-Paul Sartre. Practically all of our authors acknowledge his preponderant influence at the time they were studying while practically all admit that their own work constituted something like a revolt against his ideas. To be effectively carried out, such a revolt had to rely on external help and the best source for such help would be the ideas promoted by the germane teachers. Given the tightly knit structure and the various other unique characteristics of the elite institutions of higher education in France, it is no surprise that practically all the authors in question had the same teachers while it is a fact very seldom noticed that these teachers were *philosophers and historians of the physical and mathematical sciences* who, at least in that guise, had very little to do with Sartre.

Four such teachers can be singled out. In addition to Alexandre Koyré who, as we said, was one of the founders of internal history of science, there is, first and foremost, Gaston Bachelard, whose work on physics and chemistry analyses the autonomous character of scientific development, a development which, by its very nature, goes against the grain of allegedly inescapable empirical or philosophical requirements. The work of Bachelard ties indissolubly together both the philosophical and historical aspects of the study of science, coming up with his particular brand of historicist philosophy of science, which, at least in France, bears the name 'historical epistemology'. Next, there is Georges Canguilhem, the successor of Bachelard at the University of Paris, who developed

Bachelard's ideas in respect to the biological and medical sciences. And last, there is Jean Cavaillès, a philosopher of mathematics executed by the Nazis and close friend of Canguilhem, whose work can be covered by the motto '*Autrement dit, il y a une objectivité, fondée mathématiquement, du devenir mathématique*'.² Cavaillès's last work, *Sur la Logique et la Théorie de la Science*, written in prison, was published posthumously by Canguilhem with a preface by Bachelard. Although Cavaillès was not the direct teacher of the authors we are discussing, the strong endorsement of his views by both Bachelard and Canguilhem, to say nothing of his heroic death, made his few surviving works very important to all our authors.

We can see now in what sense the guiding ideas of those four philosophers *cum* historians of the physical and mathematical sciences were transposed into the guiding thread of the philosophical movement of the 1960s. To put it extremely schematically, the autonomy of scientific development that Bachelard was claiming for the case of physics and chemistry, Canguilhem for the case of the biological and medical sciences and Cavaillès for mathematics, could be based only on the co-constitutive relation between the object and the conceptual system of these disciplines. Once Saussure was perceived as having repeated this kind of move for establishing the scientific status of linguistics, the way was open for trying to do the same for all the human and social disciplines. The revolt against Sartre is therefore synonymous with the effort of our authors to emulate their teachers and do for the social and human disciplines what the teachers themselves had already achieved for the physical and mathematical ones. Given the Parisian intellectual atmosphere where everybody is supposed to know everything, there was no need for long explications as to what they were after. They could take for granted that everybody understood that their approach to the human and social disciplines was analogous to what they considered as established for the case of the physical and mathematical sciences. Their vehement denial of 'structuralism' can thus be easily explained.

However, this mis-appellation had important effects, if not on the self-awareness of our authors themselves, certainly on the overall reception of their ideas in France and abroad, and all particularly in the US. It is to this that we must now turn.

The passage to America

Americans have always considered Paris a particularly exciting city. Following time-honoured traditions, many American students, therefore, have always deemed it a must to spend time in Paris in order to study, what else, the French language and literature, while delving into the intricacies of Parisian culture and exploring what is new in respect to literary criticism.

For the American students visiting Paris in the 1960s, the city was even more exciting than expected for, among many other things, a wholly new movement of ideas was then in full swing, a movement which everybody was calling 'structuralism'. To them, 'structuralism' appeared as a movement that concentrated on texts and on the 'elementary' operations of reading texts, of writing texts and of speaking about texts. In one word, for the students in question this was a movement about literature.

Given the importance accorded at the time to Saussure's linguistics as well as the common knowledge that Lévi-Strauss, the initiator of 'structuralism', owed a lot to the poetics of his friend Roman Jakobson, this view of what was then happening in Paris is hardly surprising. In addition, if we take into account the fact that, at the same period, Althusser was urging everybody to read Marx, that Lacan was expressly presenting his own work as a 'simple' re-reading of Freudian psychoanalysis, a re-reading based, moreover, on Saussurean linguistics, that Foucault was talking a lot about texts, of what they allow to be formulated and of what they exclude, that Derrida was sharply distinguishing writing from speech, reversing, or rather deconstructing, all the relevant hierarchies, that Barthes was promoting a new 'scientific' way to approach literature and all systems of signs, that cinema theorists were talking about reading films as texts, it was only natural that the American students, witnessing all this activity for the first time, would perceive all the major figures of the philosophical renaissance of the 1960s as literary theorists and literary critics, not as philosophers.

This mistake is understandable, for the education of students coming from the language and literature departments of US universities could not arm them with the means for understanding what philosophy

amounted to and how it was practiced in Paris. For such students, philosophy was strictly identical to analytic philosophy, as based on the logic of Frege and Russell and as occupying itself with its various pet puzzles; it had nothing to do either with literature or with grand ideas that could inspire people and ask for life-long commitments. Accordingly, the major French intellectual figures who at the time were putting forward grand schemas and all-encompassing political ideals could not be philosophers. At best, they could be theorists of the text, where text was supposed to cover practically everything. Derrida was already taken to have said that much: *'il n'y a pas de hors-texte'*.

What these students were actually blind to was the fact that the French educational system did not make a clear distinction between philosophy as such from what, in other parts of the world and especially the US, would fall under specific branches of the arts, the humanities or the social sciences. This is to say that the way philosophy and its history are taught in French high schools or in the *École Normale Supérieure*, the most prestigious of the elite institutions of French higher education, in addition to the kind of preparation required for passing the equally prestigious post-graduate national examination of the *agrégation*, makes philosophy the mother discipline *par excellence*, the knowledge of which allows the effortless movement of graduates from this to that area while taking for granted that they are thus actually practicing philosophy. This is to say that no barrier is erected, institutionally or otherwise, between philosophy and other disciplines of the arts, the humanities and the social sciences, with the result that practically all major intellectual figures, regardless of where they concentrate their work, are first and foremost philosophers almost by definition. It was merely a contingent fact that the philosophers in question tended to occupy themselves at the time mainly with texts; they themselves would laugh heartily if they were told that this made them literature specialists, in the way this qualification could be understood in the US.

The mistake may perhaps be understandable but its consequences were momentous. The passage to America of the French thought of the 1960s appeared as merely amounting to the importation of a set of exciting new theoretical approaches to literature, an importation that

could therefore be initially hosted only by language and literature departments and nowhere near philosophy. The event marking this importation was a big conference at Johns Hopkins University held in 1966, where many of the protagonists of the philosophical renaissance of the 1960s appeared in the US in person, and together for the first and last time. The proceedings were published in 1970 by Richard Macksey and Eugenio Donato as *The Structuralist Controversy: The Languages of Criticism and the Sciences of Man*.

Although the little phrase 'the sciences of man' should have warned people that what was at stake covered much more than mere literary criticism, the storm that the importation in question brought about wiped out this clause and erased it from memory; criticism became the only qualification that stood up to the test of time. In retrospect, the reasons appear as obvious. Language and literature departments could boast for the first time of a wholly new and particularly exciting set of theories of their own, a set of theories which, in addition, bore the in-built potential for influencing lastingly other departments, a set of theories allowing language and literature to compete seriously, therefore, for money and prestige, a set of theories that would enhance enormously their position in the university hierarchy. Language and literature would thus cease to be a mere cultural appendix to the study of the 'serious' scientific disciplines or of philosophy and could legitimately aspire to become the undisputed centre of the faculties of arts and sciences.

And this is more or less what has happened. Students started flocking in and money started to flow, grants were won, new positions were assigned, new areas of specialisation became instituted, new major figures emerged from American soil. In addition, as these new ideas were capable of legitimising theoretically all the areas of study related to identity that we mentioned, the curricula as well as the barriers separating departments started to undergo pressure to the point of explosion. The Sokal affair is but one instance of the kind of episode that such tensions would sooner or later bring about. The net result, however, is that a wholly new theoretical configuration started to dominate the humanities and thereby STS, a configuration which, as we have already said, continues to be called simply 'theory'.

'Theory' in this sense envelops an extremely large and extremely loosely connected set of ideas that may refer at one and the same time to all kinds of disparate disciplines, approaches, areas of study and singular names. Philosophy, the history of ideas and of institutions of all kinds, the arts, popular culture, most of the social and political disciplines fall under it, while structuralism of either the Saussurean or the Lévi-Straussian variety, semiology and semiotics, deconstruction, Marxism in the different receptions of the term and especially those of Althusser and of the Frankfurt School, psychoanalysis, feminism, gay studies, subaltern or post-colonialist studies, in conjunction with the work of some major philosophers mainly of the 19th century, brush shoulders with *bona fide* literary texts of all periods and of all countries, from the Ancient Greek authors to contemporary fiction writers of differing genres and of unequal value. The idea that we are all living in some kind of 'post-modern' era, where no 'master narratives' can be put forward and argued for, an idea that gained centre stage through a radical misreading, at least in my view, of a work by another major figure of the philosophical movement we are discussing, namely Jean-François Lyotard's *La condition post-moderne: rapport sur le savoir*, published in 1979, added its own twist to things while appearing as legitimising this smashing to smithereens with all the attendant haze of ambiguities and confusion.

To close our story, we can say that, in conjunction with the ideas left over from the evolution of the Strong Program, this is the theoretical configuration that presently dominates STS, playing, as we noted, the kind of normative role that philosophy used to play in 'classical' HPS. And we should underline once again that, ironically enough, although philosophy and HPS should be the natural hosts of the French philosophical renaissance of the 1960s, for the protagonists of that movement were, if anything, philosophers and philosophers of science at that, these have remained practically the sole academic units that continue to resist them. The fact that philosophy and HPS departments have been doggedly adhering, almost with no exception, to the analytic tradition in philosophy can perhaps explain for the most part this peculiarly acute manifestation of blindness.

The present landscape

One should stress, I believe, that this situation should not be considered as merely negative. The continuing existence and institutional stabilization of 'theory' manifests, at least in retrospect, that curricula as well as barriers between disciplines and departments had been drawn, if not artificially, at least by obeying a kind of 'logic' that was indebted overmuch to the positivist views prevailing at the time in philosophy and in the sciences. Today the friction resulting from bringing together such disparate areas of study, genres, and kinds of approach has already started to make discernible theoretical allegiances and solidarities that were invisible before; has already given rise to new theoretical kinships and alliances; has opened the way for new guiding ideas, promising perhaps a fuller understanding of all the intricacies of our present condition. For this to happen however, the requirements of philosophy should not be pushed under the table. Together with Kant, we can maintain that, to orient ourselves within the present maze, we need a scrupulous guide, which only the rigour of philosophy can provide. The fact that the openness of 'theory' has already started to undermine irreversibly the distinction between the analytic and the 'continental' tradition in philosophy, the fact that the study of Hegel, of Nietzsche, of Heidegger, and of practically all the philosophers having influenced the French authors of the 1960s is carried out in many philosophy departments without raising too many eyebrows, are, I believe, signs promising that the road has started to open for a deeper philosophical renaissance, one that would bring together what is best in both the analytic and the 'continental' traditions in philosophy. For example, I see no deep philosophical reason forbidding John McDowell, say, from talking directly to Jacques Derrida, for the deeper philosophical intuitions behind Derrida's '*il n'ya pas de hors-texte*' are not that different, I presume, from those of McDowell when he maintains that even our starkest experiences of the world are articulated through our conceptual capacities.

Regarding the study of science proper, the insights gained through work done in both HPS and STS should be brought together within a unified perspective, articulated with all the requirements of philosophical rigour. To be viable, this should be a perspective that poses and tries to

answer questions that have either not been properly raised before or that still remain in limbo without, in the process, falling into the traps dispersed into the paths of the logical empiricist, of the historicist, or of the social constructivism program. If we hold fast to the lesson drawn by the developments I tried to sketch, namely that the traditional distinctions among philosophy, internal and external history, and sociology of science cannot hold much water, there is, again, no deep philosophical reason forbidding us to pose directly and try to answer how all these undeniable facets of science, conceptually transformed by our own work if necessary, do effectively hang together and how they manage to do this in the case of each particular scientific discipline. To go one step further, we can ask whether such a hanging together is similar in all disciplines or whether each carries its own particular configuration and thence ask what are the conditions assuring the identity of each and of its way of development.

Logical empiricism had hidden issues such as these behind logic and method; the historicist approach had hidden the same behind overarching historical schemes; and the social constructivist program has hidden everything behind the interplay of social forces, undeniable *per se*. On the other hand, what the French philosophers we have been discussing propose, namely that the subject matter of the various scientific disciplines cannot amount to an empirical given, for the effective carving out of such subject matter is inseparable from the effective establishment of the concepts that provide its knowledge, i.e. that the object and the conceptual system of a scientific discipline are co-constituted by a unique process, has not benefited yet from a fair hearing within either HPS or STS.

To conclude, I am convinced that all these issues and all these questions are of the sort that can assure viable and productive links between HPS and STS at the expense of neither but for the benefit of both and for that of the rest of us.³

Acknowledgements

It must be evident that the preceding does not pretend to be a close study of the theoretical traditions and the institutional peculiarities of

the disciplines and countries mentioned. At best, its ambition is limited to drawing the bare contours, the large-scale outlines, of the story I have been trying to tell. Despite appearances, the story is told from the viewpoint of Greece, that is from a place where one is obliged to try to make some modicum of sense of wildly different traditions and points of view, coming from all over the place, each pretending to bring the truth, the whole truth and nothing but the truth to the backward natives. The story is centred on the US because, for better or for worse, the present dominant position of this country, even in matters such as the ones we have been discussing, cannot be denied. My first hand knowledge of what was happening in Paris in the late 1960s and early 1970s as well as what I have managed to understand through my frequent visits to the US after 1986 have undoubtedly helped me to clarify things while adding to the inevitable bias that an account such as the above cannot but harbour. To conclude, it is a real pleasure to thank Michalis Assimakopoulos, John Beverley, Stathis Gourgouris, Peter Machamer, Alexander Nehamas, Maria Rentetzi and Liana Theodoratou for numerous discussions that helped me significantly to shed some necessary light on much of the above.

Notes

- ¹ J. Alberto Coffa (1991), *The Semantic Tradition: From Kant to Carnap to the Vienna Station*, Cambridge University Press.
- ² 'In other words, there is an objectivity, mathematically founded of the development of mathematics.' From his *Philosophie Mathématique*, Hermann, 1962, p. 28.
- ³ Lest I should be perceived as merely waving my hands at what others should be doing, I presume to refer to my 'Physics as a Mode of Production', *Science in Context*, 6 (2), 1993, pp. 569–616, as well as to my still in progress 'Physics as Self Historiography *in Actu*: Identity Conditions for the Discipline', which try to walk some first steps in the direction indicated.