

**From *Le savant et le politique* (Weber, int. Aron, 1959)  
to *Le savant et le populaire* (Grignon & Passeron, 1989)  
from an English perspective\***

**Abstract**

*Aron published as *Le savant et le politique* (1959) [the scientist and the politician] a long introduction to the first French translations of Weber's two lectures of 1918 – *Wissenschaft als Beruf* [Science as vocation] and *Politik als Beruf* [Politics as vocation]. This article comments on Aron's introduction. It then looks at the divergent responses to Aron's work of Jean-Claude Passeron and Pierre Bourdieu, both of whom were 'mentored' by Aron. Bourdieu and Passeron developed a sociology of education and culture in the 1960s. Passeron retained the Weberian distinction whereas Bourdieu saw social research as an instrument for political action. I comment on the political implications of the comparable development of Cultural Studies in England in the 1960s and 70s, and I come back to France to consider Passeron's introduction to the translation of Hoggart's *The Uses of Literacy* (1959) as *La culture du pauvre* (1970). I focus next on the situation in 1979 at the year of the publication of Bourdieu's *La distinction* and Lyotard's *La condition postmoderne*, before discussing *Le savant et le populaire* [the scientist and the ordinary person] (Grignon & Passeron, 1989) and trying to draw out some conclusions which relate sociology to popular culture and populist politics.*

KEYWORD: *Political, Weber, Aron, Bourdieu, Passeron, Hoggart.*

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**Introduction**

This article attempts to say a few things about Aron's introduction to Weber's two lectures of 1918, published in French for the first time in 1959. It then looks at the divergent responses to Aron's work of Jean-Claude Passeron and Pierre Bourdieu. I then make some comments about the contemporary situation in the 1960s and 70s in England, and I come back to France to consider Passeron's introduction to the translation of Hoggart. Next, I focus on the situation in England and France in 1979, before discussing *Le savant et le populaire* and trying to draw out some conclusions which relate social science to popular culture and populist politics.

**Aron**

In the first paragraph of his introduction, Aron could have been writing about himself:

Max Weber a été un homme de science, il n'a été ni un homme politique ni un homme d'Etat, occasionnellement journaliste politique. Mais il a été, toute sa vie, passionément soucieux de la chose publique, il n'a cessé d'éprouver une sorte de nostalgie de la politique, comme si la fin ultime de sa pensée aurait dû être la participation à l'action. (Weber, int. Aron, 1959, 7)

[Max Weber was a scientist. He was neither a politician nor a statesman, occasionally a political journalist. But, all his life, he was passionately concerned with public affairs and never ceased to exude a sort of political nostalgia, as if the ultimate goal of all his thought should have been involvement in action.]

Aron's introduction to Weber's lectures is self-regarding but it also offers some critique. Aron explains that Weber insisted that it is not possible 'en même temps' [at the same time] to be a scientist and a politician but that, equally, Weber asserted that 'on peut prendre des positions politiques en dehors de l'université' [one can adopt political positions outside the university]. In other words, the activities had to be kept separate but they had to impinge on each other reciprocally. There are logical grounds for this reciprocity because the pursuit of causal explanation in science relates to purposive action. As Aron summarises Weber's view:

Une science qui analyse les rapports de cause à effet ... est donc celle même qui répond aux besoins de l'homme d'action. (Weber, int. Aron, 1959, 8)

[A science which analyses the relations between cause and effect ... is therefore one which itself responds to the needs of the man of action.]

However, there is no necessary causal connection between science and action. Aron says:

La compréhension de l'action menée par les autres dans le passé ne conduit pas nécessairement à la volonté d'agir dans le présent. (Weber, int. Aron, 1959, 9)

[The understanding of actions taken by others in the past does not necessarily lead to the will to act in the present.]

In other words, Aron is tacitly making it clear that his view of the function of history is not at all historicist. He preserves human freedom by insisting that our historical perceptions of the past do not determine future events. Importantly for our purposes, Aron tries to insist on the separation of the man of science from man in his everyday humanity. He continues:

Il n'y en a pas moins, philosophiquement, et, pour employer le jargon à la mode, existentiellement, un lien entre la connaissance de soi et celle des autres, entre la résurrection des luttes que se sont livrées les hommes disparus et la prise actuelle de position. (Weber, int. Aron, 1959, 10)

[Even less is there, philosophically, and, to use the popular jargon, existentially, a link between knowledge of self and of others, between the resurrection of the struggles which concerned dead men and position-taking in the present.]

and, a little further on, he elaborates:

La réciprocité entre la rencontre avec l'autre et la découverte de soi est donnée dans l'activité même de l'historien. La réciprocité entre la connaissance et l'action est immanente à l'existence non de l'historien, mais de l'homme historique. (Weber, int. Aron, 1959, 10)

[Reciprocity between the encounter with the other and the discovery of the self is a given in the very activity of the historian. Reciprocity between knowledge and action is intrinsic to the existence not of the historian but of historical man.]

In other words, Aron is arguing that the knowledge of a man acting as a scientist does not dictate his behaviour as a man. People have multiple identities and there is no necessary integration of knowledge acquired in following the rules of autonomous intellectual discourses with the behaviour inducing personality of the scientist. It is significant that Aron has a swipe at existentialism in this context. The opposite view to the one Aron is upholding would argue that, as individuals, we are involved in a process of self-totalising self-construction and that the acceptance of multiple identities manifest in fragmented and discrete roles is evidence of a lack of authenticity and of bad faith. Aron is tacitly advancing the argument against Sartre which he was to make in full in *D'une Sainte Famille à l'autre*, published in 1969.

I don't want to discuss whether Aron accurately represents Weber here, but he immediately raises some theoretical objections to Weber's position, the first of which I want to consider. Aron continued with these three sentences:

On s'est demandé dans quelle mesure la pensée propre de Max Weber s'exprime adéquatement dans le vocabulaire et les catégories du néo-kantisme de Rickert. La phénoménologie de Husserl, qu'il a connue mais peu utilisée, lui aurait, me semble-t-il, fourni l'outil philosophique et logique qu'il cherchait. Elle lui aurait évité, dans ses études sur la compréhension, l'oscillation entre le 'psychologisme' de Jaspers (à l'époque où celui-ci écrivait sa psycho-pathologie) et la voie détournée du néo-kantisme qui n'arrive à la signification qu'en passant par les valeurs. (Weber, int. Aron, 1959, 10-11)

[It has been asked to what extent Max Weber's own thought is adequately expressed in the language and categories of Rickert's neo-Kantianism. Husserl's phenomenology, which he knew but used little, it seems to me, would have supplied him with the philosophical and logical tool that he was looking for. It would have enabled him to avoid, in his studies on the understanding, oscillation between the 'psychologism' of Jaspers (at the time when he was writing his psycho-pathology) and the indirect route of the neo-Kantianism which only reaches meaning by passing through values.]

Aron seems to be implying that the problem of Weber's adherence to Rickert's neo-Kantian epistemology is that, in imitation of Kant's Critique of Pure Reason, the boundaries of historical understanding are situated categorally within a logically a priori 'historical reason', whereas if Weber had lived to know both Husserl's Cartesian Meditations and his *The Crisis of European Sciences*, he would have had the possibility of recognising that categories of thought derive pre-predicatively from the Life-world. Aron seems to be implying that he has the advantage over Weber in this respect and that he has followed Weber's thought but has replaced the transcendental idealism which he derived from Rickert with a transcendental phenomenology derived from Husserl. This is what Aron seems to be saying, but I want to suggest that it was Bourdieu who was to deploy phenomenology descriptively, deprived of its transcendentalism, whereas Aron's thinking continued to rely on a neo-Kantian framework. This latter point is apparent in Aron's discussion of what he calls the continuation of Weber's notion of the disenchantment of the world by science in which he considers two kinds of threat posed by contemporary science. The first is that scientists, particularly natural scientists, have become intimidated by the consequences of the exploitation of their science. The second is that totalitarian political states insist on the nation-state allegiance of their scientists and seek to control the pursuit of objective truth. Aron argues that the fallacy inherent in this second menace is that it ignores, as he puts it, that there is a "République internationale des esprits qui est la communauté, naturelle et nécessaire, des savants." [international republic of minds which is the natural and necessary community of scientists] (Weber, int. Aron, 1959, 15).

This community operates according to its own rules and “Les problèmes à résoudre leur sont fournis par l’état d’avancement des sciences ...” [the problems to be solved are provided by the state of advancement of the sciences] and not by any political state. Aron then takes the example of his friend Jean Cavailles to illustrate both this point and the point that we all have multiple identities or live plurally in a range of contexts. As a French soldier, Cavailles fought against the occupying Germans, but, as a man of science or logician, he remained a disciple of international mentors – Cantor, Hilbert, and Husserl. Aron concludes that when a state tries to dictate to science what should be its objects or its rules of activity, what we have is the “intervention illégitime d’une collectivité politique dans l’activité d’une collectivité spirituelle, il s’agit, en d’autres termes, du totalitarisme, saisi à sa racine même” [illegitimate intervention of a political collectivity in the activity of a spiritual collectivity, a question, in other words, of the very essence of totalitarianism] (Weber, int. Aron, 1959, 16). The important point to note here is that Aron assumes that these two kinds of collectivities are categorally different. Whilst he makes no comment whether the political collectivity is socially constructed, he uses the word ‘spirituelle’ [spiritual] to show that a scientific collectivity has transcendental status, that its social existence reflects a logical necessity. Aron proceeds to commend Simmel for having described brilliantly “La pluralité des cercles sociaux auxquels chacun de nous appartient, et il voyait dans cette pluralité la condition de la libération progressive des individus” [the plurality of the social circles to which each one of us belongs, and he saw in this plurality the condition for the progressive liberation of individuals] (Weber, int. Aron, 1959, 16-7) and he contrasts this celebration of plurality with the fundamental totalitarian impulse:

Ce souvenir nous permet de juger les tentatives de totalitarisme pour ce qu’elles sont: des efforts proprement réactionnaires pour ramener les sociétés au stade primitif où les disciplines sociales tendaient à embrasser tous les individus et les individus tout entiers. (Weber, int. Aron, 1959, 17)

[This recollection allows us to judge the efforts of totalitarianism for what they are: intrinsically reactionary attempts to lead societies back to the primitive state where social disciplines tended to encompass all individuals and individuals in their entirety.]

It is clear from Aron’s other writing at the time that these words are a thinly veiled attack on Durkheimian social science and, associated with this, an attack on the ideology of the 3rd Republic which could be said to have deployed Durkheimian social science to legitimate a socialist, totalitarian state.

However, Aron is not able to hold this line entirely. He immediately concedes that science can be seen to be ‘partially’ determined by social, historical and racial factors. However, he insists that there is a fundamental difference between accepting that the character of science is shaped by its social milieu and accepting that its agenda can be determined by political authorities. As Aron comments:

Dans le premier cas, la communauté de la science continue d’obéir, pour l’essentiel, à ses lois spécifiques. Dans l’autre, elle abdiquerait son autonomie et mettrait en péril, du même coup, sa vocation et ses progrès ultérieurs. (Weber int. Aron, 1959, 17)

[In the first case, the scientific community essentially continues to obey its own specific laws. In the other case, it would abdicate its autonomy and, at one stroke, place its vocation and future progress in jeopardy.]

This argument led Aron to conclude that it would be fatal to deduce from the fact that social science is in part dependent on its social context:

... la conclusion que les sciences sociales ne sont que des idéologies de classes ou de races et que l’orthodoxie imposée par un Etat totalitaire ne diffère pas en nature de la libre recherche des sociétés

pluralistes. Il existe, quoi qu'on en dise, une communauté des sciences sociales, moins autonome que la communauté des sciences naturelles mais malgré tout réelle. (Weber, int. Aron, 1959, 19)

[the conclusion that the social sciences are only the ideologies of classes or races and that the orthodoxy imposed by a totalitarian state is not different in kind from the free research of pluralist societies. Whatever one says, there exists a social scientific community, less autonomous than the community of the natural sciences but, in spite of everything, real.]

Aron proceeded to outline the constitutive rules of this community of the social sciences; first the absence of restriction on research and the establishment of the facts themselves; secondly, the absence of restriction of any discussion and criticism of findings or methodologies; and thirdly, the absence of any restriction of the right to disenchant reality. For Aron, the community of the social sciences has to retain the right to question what he called the 'mythologies' which dominate our behaviour whether these are imposed by communist or democratic states. He insisted:

Par crainte d'être accusés d'antidémocratie, ne nous arrêtons pas devant l'analyse des institutions parlementaires telles qu'elles fonctionnent à l'heure présente en Europe. (Weber, int. Aron, 1959, 22)

[For fear of being accused of being anti-democratic, let us not flinch from analysis of parliamentary institutions such as they function currently in Europe.]

Indeed, by allowing free criticism of itself, democracy demonstrates its superiority.

I want to take two main points from Aron's introduction to Weber. The first point is that although Aron had been appointed professor of sociology at the Sorbonne in 1955, his philosophy of social science was derived from his philosophy of history. His view of the participant historian related to Weber's view of the roles of the scientist and the politician in as much as Aron's view of participation was primarily that of the political scientist in politics. He tended to regard historical reality as essentially political reality. Social scientific explanation clarifies a subordinate domain of political reality and Aron's hostility to the Durkheimian tradition was that it sought to subordinate politics to social relations and to see sociology as the instrument for actualising individual and collective relations and of establishing a coherent, totalised social solidarity which renders the political sphere moribund. Aron's subordination of social scientific explanation was mirrored by his wish to subordinate social and cultural movements to changes brought about by 'legitimate', constitutional, political means. Hence his hostility to the events of May, 1968 and his use of his own terminology in describing the student revolt as a 'mythology.' The second point is related to this: the community of social science within which it is practiced is an intrinsically autonomous community. To regard it as socially constructed would be to insert social scientific explanation within a totalising Durkheimian social scientific worldview.

### **Bourdieu and Passeron**

My view is that initially Bourdieu and Passeron were united in carrying out a research agenda which followed from Aron's views. In the early 1960s they undertook research which, as Aron recommended, questioned whether aspects of French democracy in the 5th Republic were true to the vision of an inclusive, socialist republic of the 3rd Republic. In particular, was the education system perpetuating the values and the privilege of the dominant classes and denying the cultures of the dominated? Were new technologies of the time, such as photography, allowing for the expression of indigenous culture or was there an increasingly homogenised culture imposed by the mass media? Were museums and art galleries perpetuating social exclusion or were they the disguised instruments of state control? Were Malraux's motives in establishing the Maisons de la Culture essentially political in sustaining the subordination of popular culture

to a state approved high culture? Bourdieu and Passeron came to the study of education and culture not with the intention of contributing to the analysis of culture or education per se but with the intention of considering how educational and cultural systems functioned politically. Hence their use of Aron's terminology in their attack on mass culture in "Sociologues des mythologies et mythologies de sociologues" (Bourdieu & Passeron, 1963) and hence their interest in comparing educational systems within different political systems as evidenced by their contributions to *Education, développement et démocratie* (Castel & Passeron, 1967). As normaliens, both Bourdieu and Passeron had, of course, been trained philosophically. The incipient differences between their positions became apparent in the joint production of *Le métier de sociologue* (Bourdieu, Chamboredon, & Passeron, 1968) in which they tried to set out the epistemological preliminaries for sociological enquiry. In a sub-section of their introduction – 'Epistémologie des sciences de l'homme et épistémologie des sciences de la nature' [epistemology of the social sciences and epistemology of the natural sciences] – they appeared to be in agreement that the legacy of the competing philosophies of social science of the 19th Century offered a false dichotomy between positivism and hermeneutics and that the solution should be the establishment of an epistemology which would be particular to the social sciences. The proposal was:

Pour dépasser ces débats académiques et les manières académiques de les dépasser, il faut soumettre la pratique scientifique à une réflexion qui, à la différence de la philosophie classique de la connaissance, s'applique non pas à la science faite, science vraie dont il faudrait établir les conditions de possibilité et de cohérence ou les titres de légitimité, mais à la science se faisant. (Bourdieu, Chamboredon, & Passeron, 1968, 27)

The way to move beyond these academic debates, and beyond the academic way of moving beyond them, is to subject scientific practice to a reflection which, unlike the classical philosophy of knowledge, is applied not to science that has been done – true science, for which one has to establish the conditions of possibility and coherence or the claims to legitimacy – but to science in progress. (Bourdieu, Chamboredon, & Passeron, 1991, 8)

As Jean-Claude Passeron has said to me, the production of the text of *Le Métier de Sociologue* was like the preparation of agreed doctrinal statements at the Councils of Nicea or Trent in the early church. The process showed him the disjunction between shared language and shared thought. The idea of submitting practical scientific research to systematic reflexion united Bourdieu and Passeron in as much as both were opposed to theoretical theory or speculative theorising, but their conceptions of reflexion were very different. Just as Passeron, in, for instance, "La photographie parmi le personnel des usines Renault" (Passeron, 1962), had sought to analyse the emergent discourse of photographic criticism as manifest in everyday language, whereas, by contrast, Bourdieu was concerned with analysing the institutionalisation of an aestheticism of photography in the growth of photographic clubs as social phenomena, so Passeron was concerned to reflect on the deployment of linguistic categories in social scientific research whereas Bourdieu emphasized the need for social scientists to establish themselves socially as an epistemic community. It is significant that the introduction ended with a passage which was deferential towards Durkheim, quoting from *The Rules of Sociological Method*. Bourdieu, Chamboredon, and Passeron concluded the introduction with the comment that: "In short, the scientific community has to provide itself with specific forms of social interchange. . . . (Bourdieu, Chamboredon, & Passeron, 1991, 77)

In other words, the uneasy compromise of *Le Métier de Sociologue* was that Passeron's inclination to subject social scientific discourse per se to rigorous scrutiny was absorbed within a Durkheimian conception of the need socially to construct the community within which such scrutiny could occur. A fundamentally Weberian interest in rationality was absorbed within a conception of a socially constructed community which would have been anathema to Aron's understanding of a social scientific community. My view is that

Passeron's work retained this linguistic/logical orientation, as shown in *Les mots de sociologie* (Passeron, 1980) and in *Le Raisonnement sociologique* (Passeron, 1991), whereas Bourdieu's work took a turn towards philosophical anthropology, mixed with phenomenology and ontology, as indicated by the sub-title of *Réponses: Pour une anthropologie réflexive* (Bourdieu & Wacquant, 1992). Passeron retained his interest in language and reasoning, related to Husserl's *Logical Investigations* and to Wittgenstein's *Tractatus Logico-Philosophicus*, as introduced by Bertrand Russell, interested, in other words, in the logistic rejection of psychologism and in the early developments of logical positivism, whereas Bourdieu was, perhaps, more influenced by the late Husserl, whose position had been modified by contact with Heidegger, and by the late Wittgenstein of the *Philosophical Investigations*.

My main point is that, trained in a philosophical context which was primarily concerned with epistemology, Bourdieu and Passeron both became practising social scientists who found themselves analysing education and culture as a consequence of the political science and political orientations of their mentor, Raymond Aron.

### **Historically contemporary Britain**

I want now to digress a little to compare this with the situation in England at the same time. Richard Hoggart's *The Uses of Literacy* was first published in 1957. He had studied English Literature as an undergraduate at the University of Leeds, which was his local, hometown university in the North of England. Hoggart had no training in philosophy or sociology. *The Uses of Literacy* emerged out of his experience of the disparity between two cultures. He described the culture from which he had emerged. He did not reflect epistemologically on the status of his observations in relation to his social position as a transfuge, nor did he attempt to analyse sociologically either the origins of the cultural canon which he had imbibed or of his background.

At the time Hoggart was writing the *Uses of Literacy*, sociological study in the UK was mainly confined to the London School of Economics. A recent history of British Sociology – A.H. Halsey: *A History of Sociology in Britain* (Halsey, 2004) – gives an account of the development of the subject in that one institution. It contains one chapter, called “British Post-war Sociologists,” which was first published as a journal article in 1982, in which Halsey analyses the careers of the cohort of sociology students at the LSE in the early 1950s (of which he was one). He names about 12 people who graduated from the LSE in about 1952 and who proceeded to become professors of sociology in new sociology departments in British universities outside Oxford and Cambridge. He argues that most of these were ‘grammar school’ students, the equivalent to lycée students, of lower middle class or working class origins. Most were politically left of centre and Halsey quotes a comment made to him by Raymond Aron in about 1967 to the effect that “The trouble is that British sociology is essentially an attempt to make intellectual sense of the political problems of the Labour Party” (Halsey, 2004, 70). Although we can see that this was a typical remark of Aron, it is mainly true that sociological research in England in the 1960s was the work of a generation of reforming socialists. Their work can be seen to have been connected with a social movement which reinforced the social policies of the Labour Government under Harold Wilson from 1964 to 1970. Some were specifically interested in social mobility and education, but most importantly for our purposes there were none interested in the sociology of culture and there was little interest in the epistemology of the social sciences.

Related to this a-theoretical dimension of English sociology in the period was the exclusivity of British analytic philosophy. This was well documented by Ernest Gellner in his book of 1959 entitled *Words and Things*. He accused British analytic philosophy, and particularly the Wittgenstein of the *Philosophical Investigations*, of having lost interest in reality. English philosophy was dominated by Oxford and

Cambridge faculties which meant that there was very little engagement between philosophy and sociology. Even at the London School of Economics where Karl Popper taught from 1946 to 1969, there was a separate department of the History and Philosophy of Science where the thinking impinged very little on the practical research carried out by sociologists or anthropologists.

What I am trying to show very quickly is that in England the emergent study of culture was developed by literary critics who had no sociological training; sociological research was mainly involved in supporting the social reforms of the Labour government without any interest in culture or in epistemology; and philosophy was a self-contained discipline which had few relations with either literary criticism or sociology. At first the study of culture and philosophy were a-political whilst sociology was linked politically with the work of the socialist government.

Stuart Hall steered the emergent study of culture away from its association with the value-orientation of Leavisite literary criticism. He had come to England from Jamaica in 1951 and he studied English literature at Oxford. In 1964 he was appointed Research Fellow in the Centre for Contemporary Cultural Studies which Hoggart had founded at the University of Birmingham in 1962. Hall became Director of the Centre at the end of the 1960s when Hoggart left. Hall was associated with Raymond Williams and others in the production of the *May Day Manifesto*, first issued in 1967 and then more widely distributed when enlarged and published as a Penguin paperback in 1968. As Rojek puts it: “after Hall became de facto Director, the political complexion of the Centre became more obviously harnessed to a public critique of capitalism” (Rojek, 2003, 76). Hall’s view was that the student movement of 1968 was the decisive factor in radicalising and politicising the Centre.

The 1970s was the most productive period for the Centre for Contemporary Cultural Studies at Birmingham, offering cultural criticism as a form of political opposition to the emergent domination of the political ideology of Mrs Thatcher who became Prime Minister in 1979. In a collection of ‘Working Papers in Cultural Studies, 1972-79,’ published as *Culture, Media, Language* in 1980, Hall published a retrospective piece entitled: “Cultural Studies and the Centre: some problematics and problems.” Under his leadership, the Centre had attempted to go beyond the work of Hoggart. He wrote of *The Uses of Literacy* that “it did attempt to deploy literary criticism to ‘read’ the emblems ... of working class life, as particular kinds of ‘text’, as a privileged sort of cultural evidence. In this sense, it continued ‘a tradition’ while seeking, in practice, to transform it” (CCCS, 1980, 18). Hall equally recollected that the opening of the Centre ‘triggered off a blistering attack specifically from sociology’ (CCCS, 1980, 21). He characterised the sociologists as saying: “if Cultural Studies overstepped its proper limits and took on the study of contemporary society (not just its texts), without ‘proper’ scientific (that is quasi-scientific) controls, it would provoke reprisals for illegitimately crossing the territorial boundary” (CCCS, 1980, 21). In other words, Hall was aware that the work of the Centre had consciously rejected the ‘literary’ tradition of cultural analysis and had also challenged the authority of the social scientific study of contemporary society. However, this awareness was not accompanied by any philosophical reflection on the boundaries between science, culture and politics. As Rojek has commented, citing Tudor: *Decoding Culture*, London, Sage, 1999,: “some critics hold that Cultural Studies has avoided the question of epistemology altogether” (Tudor, 1999, 191, cited in Rojek, 2003, 14). Hall did not seek to derive his authority from any epistemological reflection but, rather, from ideological commitment to a stance articulated by reference to Gramsci, whose *Selections from Prison Notebooks* had been published in English in 1971. Hall embraced the idea of the ‘organic intellectual’ such that he contended that the Centre had to work at a theoretical level but that its researchers could also not absolve themselves “from the responsibility of transmitting those ideas, that knowledge, through the intellectual function, to those who do not belong, professionally, in the



intellectual class” (Morley & Chen, 1996, 268). To return to Aron’s terminology, Hall had no philosophical understanding of ‘science’ but, like Aron, he believed in the social and political function of a class of intellectuals who possess some kind of undefined autonomy.

## **France**

Passeron’s translation of *The Uses of Literacy* was published in 1970, that is to say after Hoggart had left the Centre for Contemporary Cultural Studies and, therefore, precisely when the emergent English field of Cultural Studies was trying to shake off the legacy of the literary and textual tradition. By contrast, Passeron celebrates the way in which the *Uses of Literacy* practises the kind of sociology which is appropriate for the study of the working classes or the *classes populaires*. Precisely because Hoggart’s book is essentially literary or particularly strong in registering working class language, Passeron tried to use Hoggart’s work to support his contention that sociological analysis involves documenting the ways in which people articulate their own experiences linguistically, to support a kind of linguistic ethnomethodology, or, to relate this to a phrase used by Bourdieu, to support a linguistic analysis of texts of ‘spontaneous sociology,’ like those supplied as appendices in *Travail et travailleurs en algérie*. I just want to make one point about Passeron’s ‘présentation’ of the translation of *The Uses of Literacy* as *La culture du pauvre*. Passeron comments that one of the most original aspects of Hoggart’s book is his capacity to question the image of the working classes and their values held by other classes. He notes:

Sans doute, le passé de l’auteur, né et élevé dans une famille ouvrière, devenu boursier, puis universitaire et chercheur, le place-t-il dans une position particulièrement favorable pour apercevoir la signification de classe de ces jugements sur les classes populaires qui ont, dans les classes cultivées, toute l’opacité des ‘évidences naturelles. (Hoggart, trans. Passeron, 1970, 17)

[Undoubtedly the author’s background, born and brought up in a working-class family, becoming a scholarship boy and then an academic and researcher, places him in a particularly favourable position to appreciate the class significance of those judgements of the popular classes which, amongst the cultivated classes, have all the opacity of ‘natural evidence.’]

Passeron recognises that Hoggart was, as I have already described him, a transfuge. In this respect, Passeron recognized an affinity between himself and Hoggart and, at the same time, Bourdieu. The crucial difference, however, is that Passeron’s solution is completely unlike that attempted by Bourdieu in his “*Célibat et condition paysanne*” (Bourdieu, 1962). Bourdieu tried to engineer a conceptual encounter between the primary, unreflecting experience of Béarn peasants and the perspective on that experience which he had acquired as a social scientist who had attended Lévi-Strauss’s seminars. Ten years later, Bourdieu was to articulate this encounter as a methodology when he adopted Bachelard’s epistemological break to describe the three stages of theoretical knowledge, elaborated in *Esquisse d’une théorie de la pratique* (Bourdieu, 1972). By contrast, Passeron continues in his *présentation*:

“Mais, s’il est vrai que toute personnalité intellectuelle est socialement conditionnée et si aucune expérience de classe n’est capable d’engendrer par sa seule vertu l’attitude proprement scientifique (nulle grâce de naissance ne prédestinant jamais à l’objectivité de la perception sociologique, pas plus dans les classes privilégiées que dans les classes défavorisées, ou même dans les couches intellectuelles, n’en déplaise à Mannheim). . . . (Hoggart, trans. Passeron, 1970, 17)

[But, if it is true that every intellectual personality is socially conditioned and if no class experience is of itself capable of generating a properly scientific attitude (nothing ever causing a predisposition to the objectivity of sociological perception thanks to birth, no more amongst the privileged classes than amongst the underprivileged, nor even, pace Mannheim, amongst intellectuals). . . .]

In other words, scientific objectivity is not the preserve of any one class and is not socially constructed. All classes articulate their own self-understandings linguistically and these articulations have to be analysed intrinsically as 'science' rather than relatively as the products of different social groups.

This clear distinction between the position taken by Passeron and Bourdieu at the end of the 1960s and early 1970s relates as well to the difference between them in relation to the interpretation of *La Reproduction* (Bourdieu & Passeron, 1970). It was the paper given by Bourdieu in England in 1970 as "Reproduction culturelle et reproduction sociale" (Bourdieu, 1971) which consolidated the view that *La Reproduction* was arguing that cultural reproduction is an instrument in social reproduction whereas Passeron's position had consistently been that there are autonomous logics in operation in both the cultural and the social spheres and that there is no universally formulatable causal connection between the two. The position which Bourdieu was developing became clear in the argument of *Esquisse* (Bourdieu, 1972), as further developed in the English translation as *Outline of a Theory of Practice* (Bourdieu, 1977), but, in relation to class cultures, it was, of course, most apparent in *La Distinction* (Bourdieu, 1979). I want now to jump quickly to look at 1979 cross-culturally.

### **1979 in France and England**

*La reproduction* was published in English in 1977 and *Les héritiers* in 1979, both translated by Richard Nice who was at the time a member of staff in the Centre for Contemporary Cultural Studies. Nice also translated *La Distinction* which was first published in English in the USA in 1984 and in England in 1986, but there had been pre-publication releases of two chapters of the book in 1980 in the second volume of a new journal called *Media, Culture and Society*, in a number which also contained an article by Raymond Williams and Nick Garnham called: "Pierre Bourdieu and the Sociology of Culture" (Williams & Garnham, 1980). The English response to *Distinction* was ambivalent. On the one hand, the new field of Cultural Studies badly wanted to give itself some theoretical credibility and, to some extent, admission of French social theory provided some legitimation in opposition to English sociology. On the other hand, the publication of *Distinction* provoked what I would call a 'working-class' critique. In 1986, for instance, Richard Jenkins, by training a Cambridge social anthropologist and then working in the department of Sociology at the University of Swansea, and later to be appointed Professor of Sociology at the University of Sheffield after the publication of his introductory book on Bourdieu, wrote in the journal *Sociology*:

The superficiality of Bourdieu's discussion of the working class is matched only by its arrogance and condescension. In this ... he betrays the influence of his membership of French bourgeois cultural networks. Despite his good intentions, this perspective taints the entire analysis with a sense of the author's own distinction, and that of his intended audience. (Jenkins, 1986)

Meanwhile, in 1979 Stuart Hall left the Centre for Contemporary Cultural Studies at Birmingham to become Professor of Sociology at the Open University (an institution which had been established in the mid-1960s by Wilson's Labour government to promote adult education). There he immediately co-produced an inter-disciplinary undergraduate course called 'Popular Culture' which ran from 1982 to 1987 and was very influential. The course was co-produced with Tony Bennett who succeeded Hall as Professor of Sociology at the Open University when Hall retired. The Birmingham Centre struggled to survive after Hall's departure and has finally been incorporated into the English department of the university. Many of the staff of the Centre migrated elsewhere at this time, some of them to my university, which was then called North East London Polytechnic (one of the 30 polytechnics which were established, again by the Labour government, in 1970, and which, for some people, were intended to become 'people's universities', see Robinson, 1968). One of the first undergraduate degrees in Cultural Studies in the country was established

in 1980 at my university, and it was initially enormously successful. In part, therefore, the development of Cultural Studies was aligned with a populist movement in relation to the development of higher education institutions. However, there were other forces at work. 1979 was the year in which Mrs Thatcher became Prime Minister and was to remain so for the following decade. 1979 was also the year of the publication of Lyotard's *La condition postmoderne* which was published in English in 1985. Mitterrand was elected president early in 1981 and there was a Parti Socialiste majority in elections later that year. Bourdieu was appointed to the Chair at the Collège de France commencing in the autumn of 1981, arguably losing some contact with the research projects of the Centre de Sociologie Européenne, based at the Maison des Sciences de l'Homme. Mrs Thatcher tried to destroy the socialist orientation of the new polytechnics and, by 1989, they were removed from the control of local authorities and forced to become elements in the competitive market of institutions from 1992 as 'new universities.' In 1983, a new journal called *Theory, Culture and Society* was established. Finally, in 1984, at Cambridge, Anthony Giddens founded Polity Press which quickly established itself as the main publisher of international social theory particularly through its publication of translations of Bourdieu and Habermas.

My crude interpretation of this series of events is that, from 1980, Cultural Studies and the sociology of culture both became severed from the movement which had attempted to use cultural analysis as an instrument in a socialist political movement. They became commodified in a postmodern knowledge market. My argument is, partly, that Cultural Studies in England were susceptible to this development precisely because they had never been adequately grounded epistemologically. This leads me to some comments on *Le savant et le populaire*.

### **Le savant et le populaire**

The text of 1989 was the final version, barely altered, of seminars given in Marseilles in 1982, and published by GIDES (Groupe inter-universitaire de documentation et d'enquêtes sociologiques) in 1983 and by CERCOM (Centre de Recherches sur Culture et Communication) in 1985. I want to say a few things about the discussion between Jean-Claude Passeron and Claude Grignon in as much as I see their positions as different reactions to the work of Bourdieu and, in particular, to the publication of *La Distinction*.

Grignon's position seems to be similar to that of Richard Jenkins in England which I have quoted. In relation to the categories proposed by Passeron, Grignon accuses Bourdieu, particularly in "Anatomie du goût" leading to *La Distinction*, of being a cultural legitimist. In a way which closely mirrors Jenkins, Grignon comments:

L'ethnocentrisme de classe, dont la sociologie légitimiste des goûts est constamment menacée, ne montre peut-être jamais autant le bout de l'oreille que lorsque le sociologue prétend, avec une ostentation condescendante, concéder une forme d'autonomie et même d'excellence aux classes populaires, à condition que ce soit dans l'ordre dominé, ou plus exactement extra-culturel, des consommations matérielles et de la satisfaction des besoins 'primaires.' (Grignon & Passeron, 1989, 48-9)

[Class ethnocentricity, constantly threatened by the legitimist sociology of taste, perhaps never shows itself in its true colours as much as when the sociologist claims, with condescending ostentation, to concede a form of autonomy and even of excellence to the popular classes, on condition that this is in the dominated, or, more precisely, extra-cultural sphere of material consumption and the satisfaction of 'primary' needs.]

For Grignon, as for Jenkins, Bourdieu was unable to recognize the intrinsic character of popular culture. There are two reasons why this might have been true of Bourdieu's analyses. It could have been because Bourdieu was evaluating dominated culture by reference to criteria established in dominant culture,

or it could have been, and this may be a reflection of the first reason, because Bourdieu assumed that in reality dominated culture defined itself by reference to dominant culture. In the mid-1990s I wrote a book called *Bourdieu & Culture* which was published in 2000 (Robbins, 2000). I tried to defend Bourdieu against these charges. I suggested two possible defences. Firstly, I thought that this kind of criticism derived from a failure to understand that Bourdieu was not operating with a realist epistemology. I argued that anglo-saxon critics of Bourdieu failed to understand that he operated with a Leibnizian distinction between reasons of logic and reasons of fact. In other words, I suggested that the perception of the relationship between dominant and dominated cultures or classes was, for Bourdieu, a logically necessary a priori reason whose relation to actual conditions of domination was dubious. Although I referred to Bourdieu's diplôme d'études supérieures on Leibniz's critique of Descartes in support of this argument, I am less sure about it now than I was. It may be a tenable defense in abstract, but the truth is that Bourdieu constantly failed to differentiate between reasons of logic and reasons of fact and, indeed, as evidenced by an article such as "Décrire et prescrire" (Bourdieu, 1981), tended to see the two as in a dialectical relationship which I take to be the substance of Passeron's objection to Bourdieu in his "Hegel ou le passager clandestin" (Passeron, 1986). It may be that I didn't realise that this logical defence was, as it were, always more Passeron's defence than Bourdieu's because, in opposition to Grignon, Passeron insists on the disjuncture between reality and linguistically formulated sociological explanation:

... devant tout matériel d'observation et même devant les résultats d'un traitement de données qu'il a lui-même construit et contrôlé, le sociologue prend toujours un risque énonciatif, puisque le tableau croisé le plus simple ne dit finalement que ce qu'on est capable d'en dire en langue naturelle. L'interprétation qui passe par le choix des mots est constitutive de toute énonciation sociologique." (Grignon & Passeron, 1989, 50-1)

[... confronted by all observed material and even by the results of an analysis of data which he has himself constructed and controlled, the sociologist always takes a risk of expression since even the simplest table only in the end expresses what is capable of being said in natural language. Interpretation which expresses itself through the choice of words is constitutive of every sociological statement.]

This comment reflects, at the time, Passeron's recently completed thesis on *Les mots de sociologie* which was an analysis of the practical logic of sociological enquiry rather than an a priori Leibnizian logic. Passeron is critical of Bourdieu from a more sophisticated point of view than Grignon and it is not a point of view which has had any currency in anglo-saxon debate about Bourdieu's work. In the opening part of *Le savant et le populaire*, Passeron outlines the cultural relativism position and the cultural legitimacy position. He seems more inclined to expose Bourdieu as a cultural relativist and he suggests that the fallacy of Bourdieu's position was that he wrongly tried to adopt in France a cultural relativist position which had worked in Algeria. Cultural relativism applies in a context of 'pure alterity' but within one society there is, instead, a situation of 'altérité mêlée' [mixed alterity]. Passeron's main criticism of Bourdieu is the one to which I have already referred which he summarises again in the following way. Bourdieu acquiesced in a misreading of *La reproduction* which failed to acknowledge that

... la connaissance des rapports de force entre groupes et classes n'apporte pas sur un plateau la clé de leurs rapports symboliques et du contenu de leurs cultures ou de leurs idéologies. (Grignon & Passeron, 1989, 27)

[knowledge of the power relations between groups and classes does not deliver on a tray the key to their symbolic relations and to the content of their cultures or ideologies.]

In place of a crude, Marxist schéma to model the relations between culture and social class, Passeron proposes a second schéma which has the possibility of integrating cultural analysis with ideological analysis. This schéma represents diagrammatically the complex nature of socio-cultural analysis. As Passeron says:

Une sociologie de la culture qui veut intégrer à ses analyses les faits de domination a toujours affaire à un circuit complexe d'interactions symboliques et de constitutions de symbolismes. (Grignon & Passeron, 1989, 29)

[A sociology of culture which wants to integrate the facts of domination into its analyses always has to take into account a complex circuit of symbolic interactions and of constructions of symbolisms.]

Passeron knows that his second schéma is no more prescriptive or definitive than the first:

Le schéma suggère évidemment l'apparence trompeuse d'un réseau routier que le chercheur pourrait parcourir sans problèmes. Ce n'est là qu'optimisme graphique. (Grignon & Passeron, 1989, 30)

[Manifestly the schema suggests the false appearance of a network of pathways which the researcher could traverse unproblematically. That's just graphical optimism.]

In other words, Passeron is in sympathy with Aron's criticism of Weber that he imposed an overly simplistic model on complex social reality. To return to my starting point, my contention is that Passeron is equally in sympathy with Aron's more positive interpretation of Weber's position in balancing the commitments of science and politics. Passeron's second schéma purports to offer a continuously self-modifying model of relations between culture and ideology which itself is scientific and non-ideological. The autonomous status of science is not questioned and political convictions or commitments are of a different order.

By contrast, Bourdieu tried to develop a conceptual framework which sought to represent the complexity of reality itself. Just as Passeron counteracts the Marxist schéma logically, Bourdieu tried, by developing the concept of 'field,' to moderate the crudity of a Marxist position. Bourdieu's contention was that there are in society socially constructed or institutionalised 'fields' within which autonomous cultural analyses are exchanged whilst these fields are themselves socio-economically conditioned. My second defence of Bourdieu, therefore, against the 'working-class' critique of his work, is that people like Jenkins and Grignon criticised *Distinction* prematurely without understanding that Bourdieu was sociologically situating the explanation offered by that text, as became clear in *Homo Academicus*. Grignon and Jenkins accuse Bourdieu of evaluating dominated culture by the criteria of dominant culture, but the point of *Homo Academicus* is that Bourdieu deliberately situates the view of culture taken in *Distinction* as a function of his own position within the 'game of culture.' I think this answers Jenkins and Grignon, but it does not answer Passeron.

A possible answer to Passeron has to be sought elsewhere. The legacy of Bourdieu's work has seemed to condemn researchers to repeating the analytical stance which consists in studying cultural phenomena as functions of the social positions of the publics constituting them. This is labelled as sociological reductionism. My view is that Bourdieu is misrepresented if he is regarded in this way as a sociological reductionist. My response to Passeron which, perhaps, rescues Bourdieu's project on different grounds, is that we need to acknowledge the influence on Bourdieu's thought of his reading of Husserl. As sociology, Bourdieu's work may seem to have reductionist tendencies, but my view is that Bourdieu did not believe in the truth of sociological explanation. Sociological discourse was an instrument of phenomenological reduction. His reflexivity sought not to be a sociological methodology but, rather, a way of generating an ontologically oriented scepticism. To put this another way, Bourdieu attempted to resuscitate

Durkheimianism by bringing it into alliance with the legacy of Husserl and Heidegger, whilst Passeron attempted to resuscitate Weber by retaining his commitment to the epistemological separation of the roles of scientist and politician or populist and adapting his methodology of social science in response to the contributions of linguistic philosophy. The challenge for us now is to decide whether the conditions of mass democracy enable us to retain any institutionalised detached rationality or whether, instead, we must acknowledge that our analyses of the social behaviour of others are conducted in accordance with socially constructed rules which are constituted by everyone and are themselves aspects of socio-cultural encounter and competition.

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