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Is an archaeological contribution to the theory of social science possible? Archaeological data and concepts in the dispute between Jean-Claude Gardin and Jean-Claude Passeron

Sébastien PLUTNIAK

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Abstract

The issue of the definition and position of archaeology as a discipline is examined in relation to the dispute which took place from 1980 to 2009 between the archaeologist Jean-Claude Gardin and the sociologist Jean-Claude Passeron. This case study enables us to explore the actual conceptual relationships between archaeology and the other sciences (as opposed to those wished for or prescribed). The contrasts between the positions declared by the two researchers and the rooting of their arguments in their disciplines are examined: where the

sociologist makes use of his philosophical training, the archaeologist relies mainly on his work on semiology and informatics. Archaeology ultimately plays a minor role in the arguments proposed. This dispute therefore cannot be considered as evidence for the movement of concepts between archaeology and the social sciences. A blind spot in the debate, relating to the ontological specificities of archaeological objects, nevertheless presents itself as a possible way of implementing this movement.

Key words: Theoretical archaeology, epistemology of social science, scientific dispute, EHESS.

Since it was founded in 1975, the *École des hautes études en sciences sociales* (EHESS) has regularly organised “discussion days”, aimed at bringing together its various component disciplines around a single research question. Archaeology has featured among these disciplines since the creation, in 1960, of a *directeur d'études* (research director) chair at the *École pratique des hautes études* (EPHE; the EHESS was established in 1975, by splitting from the EPHE). This was created for Paul Courbin (1922–1994), a Hellenistic archaeologist who, in 1967, founded and ran a research department focused on archaeological methods, within the EPHE (Darcque 1996, p. 319). In 1987, the discussion days at the EHESS focused on the “Problems and objectives of social science research” and took place in Marseille (5 and 6 June) and Montrouge (12 and 13 June). Having read the programme of papers, Courbin decided to write to the president of the *École*, Marc Augé: he wished to bring to his attention that “1. Archaeology, a social science if ever there was one, did not feature at all. 2. It was nevertheless present everywhere” (Courbin 1987, p. 54). Having gone into some detail about the reasons for this apparent paradox, Courbin expressed his wish that “archaeology not be forgotten amongst all the prestigious and abundantly represented disciplines which are the pride of the *École*”. As a field archaeologist, and involved in an excavation, Courbin was unable to take part in these meetings, except through this letter. The situation was the same for one of the few other archaeologists in the *École*, Jean-Claude Gardin (1925–2013).

The above reference to the 1987 meetings illustrates the difficulties regarding the integration of archaeology into the social sciences, even in an institution which is specifically dedicated to them, such as the EHESS. This can be measured by both the limited numbers of archaeologists, and the equally tenuous position held by their specific knowledge and concepts. To state the existence of a particular kind of science, such as the social sciences, and to then claim the inclusion of archaeology among these is one thing; to examine the nature of the actual established (or, on the contrary, absent) relationships between these sciences is another. In this article, I hope to contribute to the second perspective. The reader may point out that the subject is not new, that today it is firmly accepted that archaeology is a social science (André Leroi-Gourhan is one of the authority figures regularly cited in this regard) and that much work has already been done on the connections between archaeology and ethnology, archaeology and history etc.: the work

of Alain Testart and others even more specific can be mentioned here ([Gallay 2011](#); [Latour and Lemonnier 1994](#)). Furthermore, this list could be expanded considerably by taking into account non-francophone literature. Can we be sure, however, that we are dealing with descriptions of these relationships between disciplines and not prescriptions of what they should be? This is far from certain, and the juxtaposition of disciplines regularly appears to be the most frequent relational operator.

So as not to resign ourselves too quickly to the idea that such juxtaposition is the only possible method, I will examine a dispute which presented the most favourable conditions for real conceptual movement. This argument saw two researchers from the EHESS oppose each other: the archaeologist Jean-Claude Gardin and the sociologist Jean-Claude Passeron (1930–). Two factors make their dispute a particularly favourable case. Firstly, both researchers brought together, in their respective disciplines, considerable experience in empirical research and reflexive skill. Secondly, their disagreement was specifically in relation to the nature and the expression of reasoning in the sciences which study humans, as well as to the positions held by these disciplines within science as a whole. While Passeron supported the idea of an epistemological specificity among sciences which he grouped into the category of “historical sciences” (the core of which is made up of sociology, history and anthropology), Gardin refuted this. It should be noted that their dispute fits into wider contemporary debates across the social sciences and the humanities, both between various fields, within particular disciplines.

In archaeology, Gardin confronted the movement known as “post processual” (or “interpretive”) archaeology which developed in particular in Great Britain as archaeologists assimilated certain “post-modern” ideas and rejected the earlier propositions of the New (or processual) archaeology. Gardin explored some of the work of Ian Hodder (1948–), who was the leader of the movement ([Gardin 1987b](#)), and later commented on the development of these debates ([Gardin 1999](#), p. 124, [Gardin 2009b](#), pp. 179–178). During the 1990s, the challenging of the criteria of scientificity in archaeology led him to a more general critique of the idea of the epistemological “Third Way”. This supposes the existence of a means of producing knowledge – and, as a result, of a type of knowledge – which is simultaneously distinct from literature and from the so-called “hard” sciences. This denomination refers to the distinction popularised by Wolf Lepenies (1941–) between the “three cultures”, which correspond with the three types of knowledge mentioned above ([Lepenies 1988](#)). In the Gardin collection of the Strasbourg National University Library (currently being catalogued), the documents collected in the folder entitled “Third Way” attest to this interest. For Gardin, the positions defended by Passeron constitute a francophone example of this more general trend ([Gardin 1995](#), p. 23). Conversely, Passeron presents the work of his rival as an example of scientific positivism which defends the illusion of the unified nature of the sciences, a positivism exemplified by the theories of the philosopher Karl Popper. Passeron, by contrast, defends a middle ground based on a double criticism aimed, on the one hand, at ambitions

(promoted by Gardin, in particular) to formalise reasoning and, on the other hand (though this time in line with Gardin), at the most radical forms of linguistic reductionism. As such, Passeron clearly distances himself from work conveniently grouped under the label of the linguistic turn, such as that of Hayden White, for whom history could be assimilated to topology, the abstract science of symbols (Passeron, Moulin, et al. 1996, pp. 299–300).

The dispute, focused principally on the possibility of differentiating types of scientific reasoning, is thus a potential example of confrontation, and therefore exchange, between archaeology and the “human sciences”. I will examine it using a very simple question: what in this dispute was strictly “archaeological”? How is archaeology involved, whether by the introduction of knowledge or by its conceptual frameworks? Is this dialogue, between a sociologist and epistemologist of the “historical sciences”, and an archaeologist and proponent of a “practical epistemology”, an example of the actual inclusion of archaeology in the conceptual network of the social sciences? In short, will this dispute reveal a discrepancy in the accusation of reclusion which is often levelled against archaeology in France (Audouze and Leroi-Gourhan 1981; Guerreau 2001, pp. 142–143; Dufal 2010)?

Firstly, I will summarise the chronological and spatial dimensions of the dispute. Although the two authors were respectively a sociologist and an archaeologist, I will demonstrate secondly that the issue which brought them into conflict in fact stems from an issue related to the philosophy of science. I will then go on to show that the authors approached this issue first and foremost in its epistemological dimension, to the detriment of the ontological aspects. Finally, by exploring the authors’ use of empirical examples, archaeological concepts and the ontological properties of the data analysed in this science, I will highlight the ultimately tenuous role played by archaeology in this debate.

1 From the analysis of discourse to the epistemology of social science

1.1 Chronology of the dispute

During the 1960s, Gardin conducted several research projects relating to the automation of non-digital information. His first contribution to this field dates from 1958, with his paper “On the coding of geometrical shapes and other representations, with reference to archaeological documents”, presented at the International Conference on Scientific Information in Washington, a milestone in the history of the automation of documentation (Gardin 1959). Of the 232 publications of Gardin’s that I inventoried, 33 deal specifically with documentation issues, most of which were published in specialised volumes. I have not included the 75 publications about the application of documentation in a specific field, predominantly archaeology. From 1960 to 1971, Gardin directed the *Centre d’analyse documentaire pour l’archéologie* (CADA), based initially in Paris (1962–1964) and later on the

CNRS campus in Marseille (1964–1971). Alongside this, from 1960 to 1966, he also directed the *Section d'automatique documentaire* (SAD) of the *Institut Blaise Pascal*, located in Paris, 23 rue du Maroc. From 1971, he decided to concentrate on his archaeological research in Afghanistan. In 1974, he nevertheless published a collection entitled *Les analyses de discours* (Gardin 1974). This book fed the debate which put him in opposition to other practitioners of this type of analysis, such as Michel Pêcheux (1938–1983), although he is not mentioned explicitly (on this subject, see Léon 2015, pp. 144–148).

A few years later, Passeron presented an initial wording of his general epistemology of the historical sciences in his thèse d'État (a work in which he brings together more than twenty years of research), *Les mots de la sociologie* (Passeron 1980). Here he discusses “illusory [methodological] solutions”, and dedicates a chapter to the “artificial paradise of formalism”. His criticisms are especially aimed at certain aspirations of automation in discourse analysis, a domain populated mainly by sociologists, psycho-sociologists and mathematicians. He mentions Gardin's work and takes up a number of criticisms presented by him (Passeron 1980, pp. 139–141). He adopts in particular the “relevance tests” prescribed by Gardin (Gardin 1970, p. 648), aimed at measuring the “differentiating capability of the categories used to ‘describe’ the texts” (Gardin 1974, p. 21) and which Gardin saw as safeguards against a complete removal of responsibility on the part of the analyst, in favour of the machine.

In 1991, Passeron published a reworked version of his thèse d'État under the title *Le raisonnement sociologique. L'espace non-poppérien du raisonnement naturel* (Passeron 1991; translator's note: as this book has never been translated into English, all quotations have been translated specifically for this paper). In the “*Propositions récapitulatives*” (summary propositions) which conclude this work, the author posits that “sociological reasoning” constitutes an alternative form of scientific rationality, common to all “historical” sciences. Gardin's “logician” works are explicitly challenged, not in relation to the analysis of discourse, but in terms of the general epistemology of the humanities. This is particularly true of the chapter “*Les contrôles illusoire*s” (illusory controls) (p. 158) and, especially, in the “*Propositions récapitulatives*” (p. 373). Here, Passeron targets a collective volume edited by Gardin, *Systèmes experts et sciences humaines. Le cas de l'archéologie* (Gardin et al. 1987). Gardin was subsequently invited by the editors of the *European Journal of Sociology* to write a review of *Le raisonnement sociologique*. This meticulously critical piece was initially published under the title “*Les embarras du naturel*” (“The discomforts of the natural”) (Gardin 1993) and then republished three years later by Ariane Miéville and Giovanni Busino, on the occasion of a special edition of the *Revue européenne des sciences sociales*, entitled “*Pavane pour Jean-Claude Passeron*” (“A Tribute to Jean-Claude Passeron”: Gardin 1996). In later articles, Passeron responded to his various critics (including Gardin, though he does not mention him specifically): “*Logique et schématisation dans l'argumentation des sciences sociales*” (Passeron 1997), “*Logique formelle, rhétorique et schématisation*” (Passeron 2002), and even clarifies his arguments against “logicism” in general, in

“*Le cas et la preuve. Raisonner à partir de singularités*” (Passeron and Revel 2005, pp. 31–40). Later, Gardin summarised and included this dispute in a synopsis of the evolution of his own work since the 1950s, while remarking in passing that Passeron and his various co-authors had successively defended varying positions on the modes of reasoning which belong to the social sciences (Gardin 2009b, pp. 174–175).

1.2 The contexts of the debate in Marseille and Paris

In addition to the publications, certain places also played a part in the dispute. Marseille and Paris are of particular interest in this regard. In the early 1970s, Gardin left Marseille and the direction of CADA, and returned to Paris to oversee the launch of the new *Centre de recherches archéologiques* (CRA) which he had largely helped to create. In 1982, Passeron was elected as a *directeur d'études* at the EHESS, with a chair entitled “sociology of arts and culture”. In keeping with the decentralisation taking place within the EHESS, he moved to Marseille and founded the “*Centre d'études et de recherches sur la culture, la communication, les modes de vie et la socialisation*” (CERCOM, located in La Vieille Charité), in addition to the journal *Enquête*. This laboratory contributed to the renewal of the dynamism in social science in Marseille during the 1980s and 1990s. In the 1990s, a meeting between Passeron and Gardin was organised there on the initiative of André Tchernia (1936–). A *directeur d'études* at the EHESS and a specialist in Classical and sub-aquatic archaeology, Tchernia was also interested in formal approaches in archaeology, which he had practised during collaborations with Gardin and researchers from CADA (École française de Rome, 1977). Furthermore, the debate between Gardin and Passeron also continued during seminars: those of the “*Raison et rationalités*” (reason and rationalities) group, run by Giovanni Busino (1929–) (Passeron 1997) and, in particular, one called “*Le modèle et le récit*” (the model and the narrative), which took place at the *Maison des Sciences de l'Homme* in Paris, between 1995 and 1999 (Gardin 2001; Passeron 2001). During each of these meetings, the debates focused on the general epistemology of scientific knowledge relating to humans.

2 A philosophy of science issue

2.1 An ambivalent distancing of philosophy

Since the debate was epistemological, both authors found themselves in a domain which was, in theory, neither that of the sociologist nor that of the archaeologist. How did they situate themselves in relation to what appears to be a transgression of their legitimate fields of activity? This point is of importance: it directly concerns the distribution of legitimacy regarding the metadiscourse relating to a science. This legitimacy is claimed by the historians and sociologists of science, who have a troubled relationship with philosophers and their older prerogatives in this domain (Shapin 1992).

In this regard, both authors claim the same detachment from philosophy. Passeron, who trained at the *École Normale Supérieure* on rue d'Ulm in Paris, like other sociologists or anthropologists of his generation, thus claims a detachment from the discipline which had been at the heart of his education. Nevertheless, as was already the case in the work he carried out in collaboration with Pierre Bourdieu (1930–2002), his practice of sociology contained an underlying philosophical element. This ambivalence has at times been highlighted as being part of Passeron's style. In *Le raisonnement sociologique*, it initially manifests itself by the explicit denial of any philosophical influence:

“[...] I explained [during an interview], though without any great hope of being believed, that my epistemological reflections were born directly of my perplexities as an investigative sociologist, and not of any nostalgia for the distant philosophical studies of my youth” (Passeron 2006, p. 20; see also aussi Passeron, Moulin, et al. 1996, pp. 275–279.)

In spite of this, Passeron's texts display a certain number of traits typical of philosophical writing: abundant references to philosophical authors and publications, as well as the use of *more geometrico* reasoning, through propositions and corollaries. This mode of presentation, of which Spinoza's *Ethics* is the paradigmatic example, is particularly obvious in the “*Propositions récapitulatives*” of *Le raisonnement sociologique*. In the book's index, one observes the names of Karl Popper, Ludwig Wittgenstein, Gaston Bachelard, Gottlob Frege, Saul Kripke (Passeron 2006, pp. 662–666). In short, philosophy, pushed out the door by a criticism of biographical determinism, comes back through the window, in the methods of writing and of presenting arguments.

As for Gardin's texts, references to works of philosophy of science are, if not absent, quite sporadic: in the article “*Les embarras du naturel*”, there is only one reference, to Bertrand Russell (Gardin 1993, p. 162). And while elsewhere he may have mentioned Nelson Goodman or Daniel Dennett, this was precisely to provide contrast to the objectives of the “practical epistemology” which he defends (Gardin 1987a, pp. 245–246). Gardin also differentiates his approach from those developed in logic and in psychology. If he mentions the logic of Gottlob Frege (Gardin 1980, p. 15) or the logicism of Charles Morris and of the Vienna Circle (Gardin 2003, p. 8, Gardin 2009b, p. 170), it is to emphasise the tenuous links which tie them to his own sense of logicism. More generally, the rare positive references to philosophical work can be summarised in the mention – recurrent and unchanging – of the “field-related logics” of Stephen Toulmin (Gardin 2003, p. 8, Gardin 2009b, p. 170: see Gardin 1987c, p. 195, Gardin 1997b), the “crude uniformities” of Bertrand Russell (Russell 1918, p. 118: see Gardin 1991, p. 96, Gardin 1993, p. 162, Gardin 1995, p. 20) or, potentially, reference to the works of Roy Howard or Karl Popper (Gardin 1987a, p. 255; Gardin 1999, p. 121). In fact, Gardin begins his 1993 critique by introducing himself exclusively as an archaeologist and takes care to deny any skill in sociology:

“What should be understood in the world of scientific research

when we say that reasoning is natural? This question has intrigued me for a long time. It is asked in my field, archaeology, as well as in sociology [...] because one could be justifiably surprised that my knowledge, restricted to the archaeology of central Asia, would enable me to give an opinion on works focusing on the ‘teaching system, cultural diffusion and the reception of works of art’ gathered in this anthology.” (Gardin 1993, p. 152).

A rejection of philosophy and an assertion of a rootedness in their field: despite this double gesture, common to both authors, the dispute ended up shifting to the terrain of philosophy of science and moves away from the practical concerns of the inquiry, be it sociological or archaeological.

2.2 Data and procedures of knowledge in analysing science

The issue at the heart of the dispute was in relation to the possibility of distinguishing between types of reasoning and, more specifically, the characterisation of the “natural” reasoning which was of particular importance to Passeron. In addition, the debate explored the conditions of possibility, in the humanities, of the “deindexation” of concepts in relation to their historical context. In other words, must we necessarily analyse these modes of scientific reasoning in a historical way, that is, by relating them to the contexts of their production and pronouncement (in science as a whole, and in the “human sciences”)? If the “deindexation” in question is possible, another issue arises, in knowing to what degree this may be applied. The solution to this problem determines the possibility of formalising utterances in the humanities: indeed, this deindexation is a prerequisite of any attempt at formalised abstraction of an utterance or its component concepts. Where Gardin argued for the possibility of formalising utterances to a certain degree, Passeron, in contrast, maintained that historical reasoning contains properties which prohibit this “deindexation”, and ultimately, therefore, any formalisation.

Gardin considered *reasoning* and the *degree of generality in the definition of objects* to be two separate things. The level of generality necessary for defining types – and the criteria which determine whether they can be associated with empirical data – varies according to the type of phenomena observed (material, social). Nevertheless, the modes of reasoning are the same in natural science and in historical science. With reference to the Big Bang, he adds that hapax legomena – the most extreme form of uniqueness of a phenomenon, and therefore the most difficult to classify – are not specific to human phenomena (Gardin 1993, p. 158; this argument also features in Gardin 2009b, p. 174). However, Gardin does not go into more detail about what *varies*, or about the categories into which fall archaeological data (material, mental, social?). Thus, both authors’ divergence on the (epistemological) issue of reasoning coincides with a common undervaluing of the ontological aspects, that is, the properties relating to the very existence of the realities being considered.

2.3 Undervaluing of the ontological aspects

The wavering, between an ontological and an epistemological approach, which is present in Passeron's *Le raisonnement sociologique*, remains, for the anthropologist Gérard Lenclud, an "unresolved issue". Concerned by this indecision, Lenclud nevertheless emphasises that, fundamentally, Passeron appears to construct his epistemology of the "historical" sciences on an ontological theory: the inherent uniqueness of human facts (Lenclud 2001, p. 453). He notes that on this basis, the author's theoretical construction remains nevertheless predominantly attached to the epistemological aspects, relating to reasoning. Elsewhere, in noting this shortcoming, Lenclud lamented that the distinction introduced by Passeron between "synoptic social sciences" (history, anthropology, sociology) and "specific social sciences" (linguistics, economics, demography) was not based on a joint analysis of their ontological and epistemological specificities (Lenclud 1991, p. 265).

In this regard, the priority given by Passeron to epistemology over ontology may help to explain why archaeology was not taken into consideration in *Le raisonnement sociologique*. Another reason for this omission can most likely be found in the general absence, in France, of archaeologists in interdisciplinary debates in social science, as illustrated by Courbin's letter, mentioned in the introduction. Under these circumstances, it is true that Gardin himself extended Passeron's theories to include archaeology, which the former included by default among the "historical disciplines". Nevertheless, as we will see, consideration of the ontological aspects, particularly in relation to archaeology, would not take place without further perturbing definitions of the various social sciences, their objects and their relations.

3 Archaeology: mute, absent

3.1 The discomfort of archaeological facts

Although the archaeological object was not treated as such by Passeron, he nevertheless provided a number of elements which could be applied to it, when he defined the "empirical world" in his "*Propositions et scolies*" (propositions and corollaries):

"Set of observable occurrences: all that is observable, nothing that is not. It goes without saying that vestiges, in so far as they constitute directly observable occurrences, permit, through use of presumptive reasoning which sometimes attains certitude, the reconstruction of a larger field, that of indirect observation: in the restricted meaning of the word 'history', the procedures of this presumptive reasoning make up the 'historical method'" (Passeron 1991, pp. 398–399).

The vestige encompasses indifferently of all that which lasts through time and which enables, by direct observation of it, the indirect observation of

past states of the entity in question. No difference is made between a textual or non-textual vestige: history and archaeology are thus presented on a single plane. This is also the case in Gardin's work.

It would be a mistake to consider that Gardin had a "naive" relationship with the archaeological object, as evidenced by his particularly precocious critique of the application of multidimensional statistical analysis in archaeology, when these are employed in a way that is indifferent to the nature of the archaeological entities (Gardin 1965). He did not, however, develop his reflections on this subject much further. In his later works he certainly deals with ontology, but more in relation to the meaning of the term in informatics than in philosophy, whereby the distinction between an archaeological fact and a social or historical one would be studied (Gardin 2009b, pp. 182–183). Moreover, as his work on Asia demonstrates, for Gardin, archaeology's objective is to produce knowledge about the past states of collective human entities:

"[...] archaeology, as a form of history, based on material remains instead of or in addition to textual sources." (Gardin 1987a, p. 235).

Archaeology is therefore no different from history, and for this reason, he extended the range of Passeron's theories to include archaeology (Gardin 2002, p. 22)... even though Passeron had in fact avoided doing so (though for reasons which remain unclear). It is therefore not surprising that in this dispute the ontological aspects did not constitute an argumentative resource for Gardin.

After being surprised at the absence of detailed analyses of the status of language during the debates of the "*Le modèle et le récit*" seminar, Gardin asks: "Does my field play a different game, impracticable in sociology or in any other social science?" (Gardin 2001, p. 469). The equating of archaeology with history and sociology, taken as a given, despite their respective objects not being discussed or compared, has a tendency to – justifiably – cause surprise. If archaeology constitutes a counterexample to Passeron's theories, as Gardin would have it, to what extent is this due to the nature of the realities studied by this science and, in particular, to the central yet distinct status of language within it? Gardin does not provide a response to the question: archaeology's inclusion in the social sciences is a blind spot in his position. The focus is on the procedures which enable reasoning to be carried out, since the aims are taken as non-problematic. In sociology, language is always available to define objects (we can conduct an inquiry into suicide while ignoring the discourses, but it is always possible to repeat it and include them). On the contrary, in archaeology, the analytic procedures focus on the non-discursive properties of archaeological objects (if these have discursive properties, they are processed using methods for the criticism of historical, epigraphic or philological sources). It should also be noted that in informatics and the automation of documentation, language itself is the object of study: a language that is, however, heavily controlled and defined, even if just by writing. In this regard, it is noteworthy that *Les*

analyses de discours discusses arguments expressed solely in textual form (Gardin 1974).

Thus, if archaeology is a “human science”, then Gardin’s interest is more focused on the second term in the expression (science) than the first (the unique nature of the human as an object of knowledge). As a result, the lack of weight behind the archaeological examples in his discussion of Passeron’s theories should not be surprising.

3.2 The use of archaeological examples

Unlike Karl Popper, who limited his use of examples to a method of refuting statements, Passeron favourably envisaged their usage as the principal probative method in historical science (Passeron 1991, pp. 289–290). Gardin, though he did not specifically take a position on this point, made several uses of the example of archaeology to back up his propositions. Let us examine the position of these examples in his argument. I wish to make clear that I have deliberately omitted mentions of archaeology when in relation to the analysis of texts and not of archaeological facts (for example in Gardin 1999). In a text published in 2002, Gardin recalled the reservations he had expressed in his 1993 article. These had, he said, “their source in the counterexamples that the long history of archaeological research provides in abundance” (Gardin 2002, p. 22). Interestingly, it turns out that the text in question contains a very limited number of archaeological examples: there are only two, and their development is limited. These examples are revisited, in somewhat greater depth, in his contribution to the *Le modèle et le récit* seminar (Gardin 2001, p. 467). Gardin believed he had found in archaeology two counterexamples to Passeron’s principal theories: the one stating the absence of knowledge cumulability in historical science (proposition 2.1, Passeron 1991, p. 364) and the one supporting the impossibility of conducting experimental and predictive reasoning.

Against the theory of an impossible cumulability, Gardin offers the example of history and prehistory. The observation of new data regularly invalidates theories. New theories will, in turn, be invalidated, but “[...] this widening of the empirical bases of our constructions is nonetheless a cumulative process.” (Gardin 1993, p. 155). For history, he gives the example of the discovery of the Dead Sea Scrolls, knowledge of which was added to what was already known of the origins of Christianity. For archaeology, he refers to the evolution of theories on anthropogenesis (Gardin 1993, pp. 158–159) and palaeobotany, which has increased our knowledge of the origins of agriculture (Gardin 1993, p. 155). In a later text, he underlines the temporal specificity of the archaeological mode of inquiry:

“On the long time scale which is inherent to archaeology, it appears that ‘reality’, in the relative and temporary sense of the term defined above, eventually wins out over the cultural biases we are subject to [...]” (Gardin 2009a, p. 27.)

As such, archaeology illustrates, according to Gardin, that it is possible to both produce stabilised knowledge about human phenomena, and to revise

it, to improve it, by taking into account new empirical data (see also Gardin 2001, p. 467). As we can see, the archaeological and historical examples stand alongside each other, ultimately with no differences being envisaged.

A second of Passeron's arguments rejects the possibility of an experimental approach in the humanities: the uniqueness and variability of the phenomena prohibits the making of "all other things being equal" comparisons. As a result, it is impossible to evaluate the "relative validity of concurrent theories". Gardin objected that one can read the clause "all other things being equal" in two ways: in theory, he agreed with Passeron that nothing allows us to state that we "reason in an 'unchanging context'"; nevertheless, in practice, and "until proven otherwise" this is possible (Gardin 1993, p. 156). Indeed, how does one explain the predictive effectiveness of certain archaeological theories – no specific example is given in the 1993 text – if it is impossible to experiment (that is, isolate variables and identify the stable and regular relationships between them, all things being equal)? Gardin thus admitted the possibility of distinctions and local generalisations, stabilised in "field-related logics" (as developed by Toulmin). He later recognised, and added to this, two archaeological procedures which he identified as experimental approaches: the "directed observation", which we practise during field survey or when digging test trenches based on the presumption that they will (in)validate a proposition, and the making of "useful fakes", such as flint knapped by archaeologists or reference examples of pottery typologies, which provide a standard that enables differences to be measured (Gardin 2001, p. 468 and 470 respectively).

This means of weakening, by virtue of pragmatic criteria, the logical requirements which weigh upon reasoning was a frequent motive for the positions defended by Gardin. Thus, the simulation of reasoning requires that one proceeds "as if" the discursive practices counted as a local expression of more general rules of reasoning (Gardin 1997a, p. 52; Gardin 1999, p. 121). Gardin put forward that the logicist approach:

"[...] consists of 'doing as if' the majority of the theories about a given subject was an unresolved problem, an intellectual challenge to be overcome, rather than an unavoidable phenomenon, or even desirable, not something to be concerned about, in any case." (Gardin 2001, p. 472.)

Passeron aimed his criticism directly at this use of the hypothetical, even though simulation is used indirectly here, to simulate, not the phenomena themselves, but the conditions of possibility for simulations formed by the theories (Gardin 1996b: 196). Passeron's determined rejection of what could be seen as a limited and modest ambition appears to betray his attachment to another "*contrôle illusoire*" ("illusory control"), which is missing from the inventory that he had established: an over-assessment of the probative nature of historical indexation on the value of propositions in social science.

3.3 The epistemological deficiency of archaeological concepts

A third and final element of the potential integration of archaeology in the dispute concerns not the data processed in archaeology, but the concepts which are developed and used within it. From this viewpoint, Gardin and Passeron were unable to make use of the resources produced by their respective disciplines in the same way.

Regarding sociology, it is quite possible (and debatable) to connect some arguments which are sociological in nature, and others which are epistemological in nature. It is interesting to note that in *Le raisonnement sociologique*, Passeron distanced himself from one of his earlier publications, *Le métier de sociologue*, co-written in 1967 with Pierre Bourdieu and Jean-Claude Chamboredon. In this work the authors took on the challenge of building an epistemology of sociology based on a reflexive sociology of this discipline (Passeron, Moulin, et al. 1996, p. 322). Gardin disagreed with Passeron's use of sociological explanations. In relation to the function of "special languages" (i.e. those specific to a scientific field), Gardin accused him of having reduced their function to one of differentiation and social justification of expertise:

"The position of specialist that is given to us would, it is true, be in danger of becoming clouded if we did not have the possibility of using special languages [...]; but the principal function of these is – one hopes – not simply to differentiate us. A theoretical descriptive language's *raison d'être* is, after all, to express or to establish a theory." (Gardin 1993, p. 163¹.)

The combination of epistemological and archaeological analyses appears much less obvious than it actually is, in the case of sociology. Firstly, this is because, while specifically archaeological concepts may exist (such as negative contexts or certain taphonomic processes) – and this remains a subject of debate – it is difficult to imagine the way in which these could inform an epistemological analysis. Indeed, an "archaeology" of knowledge supposes that these concepts may be used to analyse the creation and spread of knowledge as is the case with concepts defined by sociologists or psychologists (metaphoric uses of archaeology, including the one popularised by Michel Foucault, are irrelevant here, since they do not make use of concepts belonging to archaeology as it is conducted by archaeologists). Secondly, if such archaeological concepts exist, they are relative to the description of the data. The operations which render these events intelligible in archaeological reasoning are themselves carried out with the help of concepts borrowed from ethnology, sociology and even primatology and biology. Thus, in the absence of (real or relevant) archaeological concepts, Gardin was forced to

¹Gardin later revisited this argument, but this time conceding both the epistemological and sociological reasons for demarcating scientific languages (Gardin 1997a, p. 53). Elsewhere, he distinguished between the sociological and psychological factors supporting the choice of an epistemological option (Gardin 2001, p. 466 and 474 respectively). While he admitted that non-epistemological factors could be relevant to our ability to understand short sequences in the history of science, he denied their pertinence to its long term evolution (Gardin 1999, p. 122; Gardin 2009a, p. 27).

look elsewhere for an analytical repertoire capable of informing the epistemological analysis: specifically, to linguistics, in the broad sense of the term (semiology, discourse analysis, document automation and formal linguistics).

Conclusion

The dispute between Passeron and Gardin is among the most in-depth francophone intellectual debates in which an archaeologist sought to represent archaeology. Archaeology has indeed been represented in this debate, but its own resources were in fact scarcely used, as I hope to have demonstrated. Thus, neither the concepts, nor the empirical facts considered by archaeologists, have significantly contributed to this debate. A blind spot – the inclusion of the ontological specificities of archaeological data – is nevertheless an area which deserves to be researched in greater depth. In the case of the dispute between Passeron and Gardin, such an in-depth study would be possible based on their respective ideas of the linguistic properties of the data analysed, as well as the status of language in the operations of knowledge: this aspect appears to be crucial for the definition of sociological and archaeological data. Regarding the latter, a number of recent publications have in fact given increased attention to their ontological properties (Olsen et al. 2012, Lucas 2012, Boissinot 2015, Niccolucci et al. 2015). This may be the way in which francophone archaeology could integrate itself into the conceptual kula of the humanities: or, to borrow Blaise Dufal’s elegant phrase, to stop being “*enfermée dehors*” (“locked out”) (Dufal 2010).

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