One day hero. Jules Reboux at the crucible of prehistory in 1860s Paris

Héroe por un día. Jules Reboux en el crisol de la prehistoria en el Paris de 1860

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Abstract

The historiography of prehistoric archaeology, smoothly leading from the establishment of human antiquity by Boucher de Perthes in the late 1850s to the disciplinary and classificatory framework set up by Gabriel de Mortillet in the 1870s, still leaves considerable gaps in our understanding of the practical and conceptual developments of the field. The forgotten figure of the amateur Jules Reboux, who explored from the mid 1860s to the mid 1870s the quarry sites of Levallois, on the northern outskirts of Paris, proves in this respect salutary. Given the nature and scope of the controversies he raised, understanding Reboux's various claims should help us reach beyond the usual front-rankers and the scripted scenarios. With his terminological and graphic innovations, Reboux's contribution to nineteenth-century prehistoric archaeology was in fact quite fundamental, touching on such paradigmatic issues as the recognition of stratigraphic superposition, the study of stone implement manufacture and use, and the recourse to implement types for chronological classification.

Key words: *History of archaeology. Prehistoric archaeology. Archaeological practices. Terminology. Illustrations. Precursor. Stratigraphy. Technology. Typology. Jules Reboux. Gabriel de Mortillet.*

Resumen

La historia de la arqueología prehistórica, centrándose en el establecimiento de la prehistoria por parte de Boucher de Perthes a finales de los años 1850 y en las clasificaciones establecidas por Gabriel de Mortillet en los años 1870, ha dejado sin responder numerosas preguntas relativas a los diferentes desarrollos prácticos y conceptuales. La olvidada figura del amateur Jules Reboux, quien exploró los sitios de Levallois en los años 1870, es un ejemplo paradigmático de esta situación. Dada la naturaleza y la envergadura de las controversias que levantó, el estudio de las diversas propuestas de Reboux nos permitirá comprender los orígenes de la prehistoria mas allá de los protagonistas habituales y de las narrativas establecidas. Con sus innovaciones terminológicas y gráficas, la contribución de Reboux a la arqueología prehistórica de su tiempo se antoja fundamental para entender cuestiones claves como el reconocimiento de las superposiciones estratigráficas, el estudio de la producción y el uso de los útiles de piedra y el uso de dichos útiles para elaborar clasificaciones cronológicas.

Palabras clave: *Historia de la arqueología. Arqueología prehistórica. Practica arqueológica. Terminología. Ilustraciones. Precursor. Estratigrafía. Tecnología. Tipología. Jules Reboux. Gabriel de Mortillet.*

1. Introduction

Friday the 30th August 1867 should really have been Jules Reboux's day of glory. The eminent delegates to the *Congrès international d'archéologie et d'anthropologie préhistorique* (the CIAAP, then held in Paris in conjunction with the 1867 *Exposition universelle*) had all gathered that morning at the sand-quarries of Levallois, for Reboux to guide them through the quaternary sections on which he had been lecturing the previous week. The excursion itself went without incident, and several participants even took the initiative, probably for the first time ever in the history of research, to engage together in some comparative experimental flintknapping (Louis Lartet 1867:365). Unfortunately for Reboux, however, this on-site visit did little to dispel the misunderstandings raised by his lecture, and nor did it really enhance his scholarly reputation. To the contrary, his remained an uphill struggle throughout the following decade to secure allegiance for his claimed succession of "flaked", "knapped" and "polished" stone implements at Levallois. As we know, this north-western suburb of Paris has since acquired its everlasting archaeological notoriety as the eponymous site of a distinctive Palaeolithic flintknapping technique[1]. Jules Reboux, in contrast, has been rapidly and utterly forgotten by his contemporaries, and indeed in all subsequent historiographic accounts – despite the fact that his contribution to nineteenth-century prehistoric archaeology was in many respects fundamental, touching on such paradigmatic issues as the recognition of stratigraphic superposition, the study of stone implement manufacture and use, and the recourse to implement types for chronological classification.

The recognition of Reboux's contribution leads us outright to the 'precursor' quandary. Granted, attempts to rehabilitate hitherto unacknowledged forerunners are likely nowadays to raise eyebrows. Historians of science have long been weary of the hagiographic, retrospective and presentist overtones of the exercise, whose biographical emphasis is often accompanied by a damaging neglect of any social, institutional or material factors in the production of knowledge (for recent relevant discussions see Moro Abadía & Pelayo 2010 as well as Marc-Antoine Kaeser, this volume). That granted, the identification of precursors can also serve other purposes, notably those of exposing historical and conceptual lacunae in our understanding of the ways science functions and unfolds. In this vein, it is not simply in order to give his dues back to Cesar that Jules Reboux deserves here our sustained attention. Given the nature and scope of the controversies he raised, recovering Reboux should help us reach beyond the usual front-rankers, the fixed spotlights and the scripted scenarios of conventional disciplinary historiography – an historiography that still leads too smoothly from Jacques Boucher de Perthes to Gabriel de Mortillet and thence (in the French tradition at least), to Henri Breuil and to André Leroi-Gourhan … It was indeed during the "morning after" the *annus mirabilis* of 1859, so to speak, once the barriers of biblical chronology had been so dramatically 'shattered' by the tenacious custom official from Abbeville, that our unlikely innovator came to play his catalyst role (see Gamble & Kruszynski 2009, Hurel & Coye 2011 as well as Schlanger 2012 on the sometimes overplayed significance of '1859' in the history of research).

Jules Reboux was active and involved from the mid 1860s to the mid 1870s, just at a time when the Stone Age depths of human antiquity were gradually becoming a full-fledged scientific proposition to be systematically invested and investigated, an expertise-based, terminology-dependent, discipline-demanding, debate-driven research programme in its own right. Years, that is, when stratigraphic, technological and typological considerations struggled to fit together and fall into place so as to gain (retrospective) evidence under Mortillet's indisputable guidance. Indeed we will have occasion later in these pages to reconsider the work of Gabriel de Mortillet (1821-1898), the fiery railway engineer, geologist and politician who rejoined the *Musée des antiquités nationales* in the mid 1860s to exert his growing domination on Palaeolithic research worldwide (see notably Coye 1997, Richard 2008, Schlanger 2014). In order to get there, however, we will first pay throughout this paper sustained attention to the claims of little known actors and the intricacies of long-forgotten controversies, which constituted at the time key drivers for scientific research. This will enable us to grasp just how crucial and timely was Reboux's contribution to the *archaeological* foundation of prehistory. In the spirit of this volume as a whole, this will serve to underline the broader values and challenges inherent to the history of archaeology.

2. The man from Levallois

Besides his passion for collecting artefacts of all kinds and his penchant for terminological and visual innovations, next to nothing is known about Jules Reboux. He died in 1882, but very little is available on his life, in terms of biographical indications, prosopographic entries, necrologies

or portraits, let alone manuscripts or correspondence. Pending further biographical and archival research we will have to make do with information derived from published accounts. According to membership lists for the CIAAP and the *Société d'anthropologie de Paris*, as well as the lesser known *Société parisienne d'archéologie et d'histoire* and the *Société des sciences et arts de Vitry-le-François*, Reboux was a 'proprietor' or a 'geologist', residing at n° 3 rue de la Plaine in Les Ternes – a street renamed rue de Montenotte in the 1860s, when the whole quarter was integrated into Paris' 17th arrondissement. It was in this rapidly expanding neighbourhood that Reboux concentrated his scientific explorations, notably in the sand quarries and building sites around Neuilly, Clichy, Batignolles and of course Levallois. As he put it, implicitly recognising the links between urban renewal and rescue archaeology, "the development works of Paris [by Baron Haussmann] did not have as their sole result the embellishment of the capital; they also provided for science some unexpected discoveries" (Reboux 1868:222, and see on these urbanistic connections Pinon 2011, Van Damme 2012).

Reboux, who already owned substantial collections of coins, gems and ambers, initially went to these quarry sites in search for crystallised and oxidized metals (see Reboux 1868 as well as Mortillet 1872a). In 1859 however, he unexpectedly chanced upon an elephant molar at a depth of 9 metres in the Hénain quarry, on the Route de la Révolte in Levallois. It soon came to his attention that in Picardie, in broadly comparable quaternary deposits, such fossil remains had been found in association with worked stone implements of very high antiquity. He thence resolved to find similar implements on his own turf: "dedicated research has for long been undertaken in distant lands, when we have under our own feet, in Paris itself, the most certain proofs of the ancient stone industry" (Reboux 1866:130). By the end of the 1860s, his weekly rounds through some fifty distinct quarry-sites had resulted, in addition to countless fossil bones, in an impressive collection of over five thousand flint instruments.

So far, the figure cut by Reboux rather resembles that of a "local amateur", not far removed from that outlined by Andrew Christenson (this volume) with regards to boundary formation in early American archaeology. A member of several learned societies, Reboux regularly attended meetings and ensured that his papers were read (even if in absentia) and, as importantly, published. Notwithstanding this participation in the instances of science, Reboux was primarily perceived as 'a man on the ground' – a ground which happened to lie at the heart of the metropolis, rather than in some far flung provinces or colonies. Often described somewhat condescendingly as an "indefatigable", "perseverant" and "zealous" explorer, his essentially parochial expertise – or at least his continuous presence in the field – helped him secure numerous and valuable specimens for professional men of science to identify and interpret. This was by and large the pattern with the fossil bones, whose specific determination and paleo-environmental interpretations Reboux readily entrusted to such leading palaeontologists as Edouard Lartet, Ernest-Théodore Hamy and especially Albert Gaudry of the *Muséum national d'histoire naturelle* (cf. Gaudry 1866, Reboux 1866).

With the stone implements, however, matters were substantially different. To be sure, Reboux was generous also with these, eager to show and to share his finds around. Over the years, he must have donated or loaned hundreds of specimens – to the *Musée des antiquités nationales* in Saint-Germain-en-Laye, to the *Gallerie de l'histoire du travail* at the 1867 *Exposition universelle*, to the *Muséum national d'histoire naturelle*, and, as a substantial bequeath upon his death, to the *Musée Carnavalet de l'histoire de Paris*[2]. But whether he saw it as his duty or his due, it was also Reboux himself who from the onset undertook to identify and to name these stone implements, to describe their forms and the process of their manufacture, to ascribe them to archaeological epochs, and overall to place them within a broader classificatory scheme of his own making.

3. The tripartite scheme

Already at the 1867 CIAAP meeting, just before the excursion to Levallois, Reboux could confidently state that the hundreds of worked flints he has handled over the years

> (...) have enabled me to recognise three very different modes of working: these are the simply flaked flints (*éclatés*) (Fig. 1a), the knapped flints (*taillés*) (Fig. 1b) and the polished flints (*polis*). These worked flints are not found mixed together in a disorderly manner, but rather regularly distributed in different levels, always in broadly the same order. The flaked flints at the base, the knapped ones in the intermediary layers, and the polished one right on the surface. I have never found knapped flints with those flaked, nor polished flints with knapped ones (Reboux 1867:104).

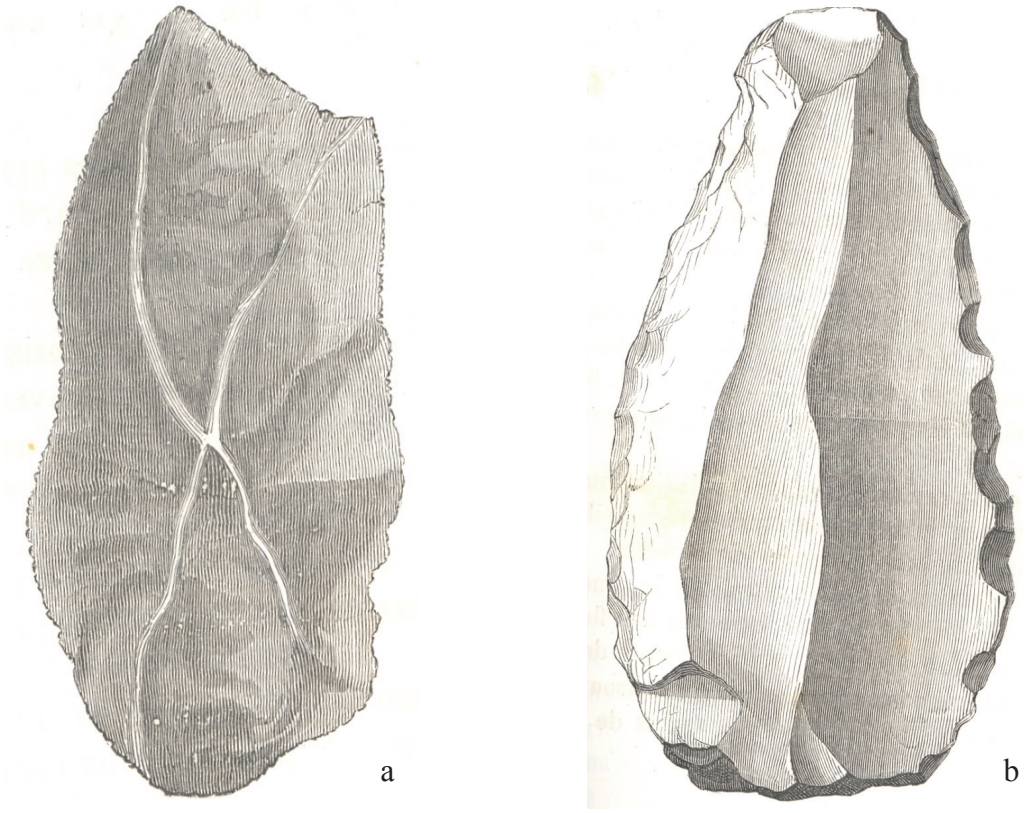

Figure 1. a. Flaked flint (*silex éclaté*) from the Levallois sand-quarries, given by Jules Reboux to the Musée de Saint-Germain. **1. b.** Knapped flint (*silex taillé*) from the Levallois quarries (Reboux 1867:104, 105).

As requested by members of the audience (Carl Vogt and Jens Jacob Worsaae), Reboux indicated that each kind of flint was characterised by its own fauna: horse and elephant occurred with the flaked flints in the lower levels, above them ox and deer were associated with the knapped flints, and lastly, near the surface, reindeer appeared with polished flints (ibid.:106-7). In a follow up presentation to the *Société d'anthropologie de Paris* in December 1869, Reboux further provided each epoch in his tripartite scheme with their specific name: he respectively designated the flaked stone epoch and the polished stone epoch "paléolithique" and "néolithique", following the latest terminology proposed by John Lubbock in his 1865 *Prehistoric times* and immediately thereafter endorsed also in French. For the intermediary knapped stone epoch, Reboux matter-of-factly invented the term "mésolithique" (Reboux 1869b:688 ff.). To be sure, this term has hardly anything to do with subsequent research on the Palaeolithic / Neolithic 'hiatus' or on post glacial microlith-rich industries. His conception is much closer to that independently advanced by the London based Irish archaeologist Hodder M. Westropp (e.g. 1866, 1872), who also made a (far less consistent or empirically minded) use of the term "Mesolithic" to argue for the universality of the Stone Age and the uniformity of its stages[3]. Reboux's "mésolithique" had as such little posterity, excepting brief mentions by the likes of Eduard Piette or Louis Capitan (see Capitan 1901: 359, and Coye 1997:219 ff). It does not however follow that his tripartite flintworking scheme as a whole was ignored or dismissed as irrelevant – to the contrary, his pioneering subdivision of the Stone Age created quite a stir, and most emphatically left its mark.

Before appraising the tenor and implications of these challenges, let us grant that Reboux did try to take these objections on board. Without diverting him from the thrust of his argument – fortunately, as it happens – this may explain his rather frantic reliance on arcane and short-lived expressions throughout his subsequent publications (whose contents as such remained fairly repetitive – see Table 1). Barely had the "paléolithique", "méso-

Flintworking	Epochs	fauna
Pierre éclatée (1867)	Paléolithique (1869b) Eolithique (1869c) Oolithique (1870)	Cheval, éléphant (1867) Ours des cavernes (1868) Mammouth, Rhinocéros (1873)
Pierre taillée (1867)	Grand Préssigny, Saint-Acheul (1868) Mésolithique (1869b) Néoolithique (1870)	Bœuf, Cerf (1867) Renne (1869b) Hippopotame (1873)
Pierre polie (1867)	Âge des dolmens (1868) Néolithique (1869b, 1870) Celtique (1869c)	Renne (1867) Cerf (1869b) Bos primigenus (1873)

Table 1. Jules Reboux's tripartite flintworking scheme at Levallois: main propositions and some variations (dates in brackets refer to publications in which the terms occur).

lithique" and "néolithique" trilogy been advanced that Reboux saw fit to rename them "oolithique", "néoolithique" and "néolithique" respectively (1870a). As for the term "paléolithique", it was occasionally replaced with "archéolithique", a synonym already proposed by Lubbock, but also with "éolithique" (1869c) – a term Reboux had apparently coined *en passant*, never to use again, and which Mortillet would a decade later famously reinvent (or reinvest) for the stone industries of tertiary man (Mortillet 1883: 18). Reboux's rather ungrammatical and redundant description of his investigations as "paléo-archéologiques" (1869a) or "archéo-paléontologiques" (1869b) further confirms his indecision. The same goes with the fauna, notably the reindeer (and the ox), ascribed variously to the "mésolithique" or to the "néolithique" epochs, all the while implying affinities with Edouard Lartet's "reindeer age". Together with other displays of ineptitude (for example, forgetting the depth at which an a priori noteworthy human cranium was found, cf. 1869a), Reboux clearly faced some difficulties in conveying with sufficient intelligibility the thrust of his Levallois claims.

However, as if to compensate for this relative opacity, Reboux also undertook to *illustrate* his tripartite scheme, developing in the process a particularly bold vision that deserves our recognition. Initially, Reboux had been content with a straightforward geological section of a quarry at Levallois (1866:131). In his 1867 CIAAP presentation, he chose to include some finely etched examples of the worked flints he had found there (see figure 1a, b here). By 1869, he went on to list, along the margins of a Levallois section, the modes of flintworking and the fauna associated with each epoch (figure 2). Finally, in 1873, Reboux added a further dimension with a truly unprecedented synthetic plate, in which a geological section is flanked by depictions of three fossil teeth to the right and three differently worked flints to the left (figure 3). In order to appreciate just how important was this remarkable image, at once destabilising and liberating, we need first consider the various objections Reboux had to contend with.

Whether or not they can be upheld in the light of contemporary (or subsequent) scholarship, what matters of course about these objections is that they can reveal some the taken-for-granted archaeological commonsense of the time, the 'normal science' if you will. Starting with the 1867 CIAAP, the discussion involved a veritable "who's who" of the nascent discipline: alongside the aforementioned C. Vogt and J. J. Worsaae, also palaeontologists E. Lartet, A. Gaudry and E.-T. Hamy joined the fray, as did archaeologists Emile Martin, Louis Leguay, Anatole Roujou, and, inevitably, Gabriel de Mortillet. The two latter scholars in particular pursued the debate through the next decade, jointed by further protagonists such as Eugène Dally, Eugène Belgrand, Clémence Royer, Ernest d'Acy and Stanislas Meunier. Of the plethora of protestations raised by this *beau monde*, some concerned matters of relative detail such as the correct identification of this or that fossil species, or the permissible usage of the terms 'dolmen age' or 'reindeer age'. Other objections however sought more broadly to dispel Reboux's alleged misunderstandings, and in so doing they highlighted, *a contrario* as it were, some fairly basic tenets of prehistoric archaeology. These debates concerned equally matters of stratigraphy and matters of technology and typology, although the distinctions between them – and this is in fact a crucial point – proved quite difficult to ascertain.

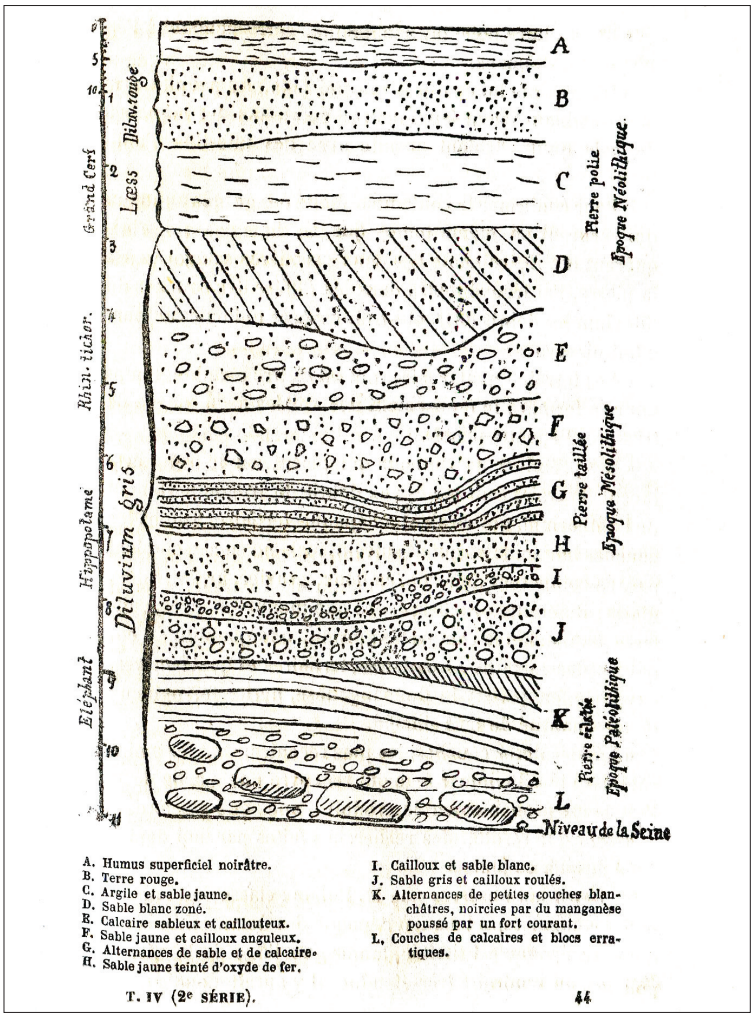

Figure 2. Stratigraphic section at Levallois, with lateral annotations on paleontological finds and on archaeological epochs (Reboux 1869b:689).

4. On stratigraphic superpositions

Questions of stratigraphy, broadly understood, preoccupied Reboux's most tenacious detractor, Anatole Roujou (1841-1904). Coming from the opposite, south-eastern suburb of Choisy-le-roi, Roujou's prehistoric explorations along his neighbourhood's railway sections followed more explicit scientific ambitions. Indeed this enthusiastic if impoverished young scholar was a keen proponent of free-thinking transformist anthropology, on which he notably published in Mortillet's journal *Matériaux pour l'histoire positive et philosophique de l'homme*. From 1867 onwards, Roujou had further opportunity to hone his positivist skills as research assistant, in charge of geological sections across the Seine basin, for Eugène Belgrand, then the chief waterways engineer in Baron Haussmann's reconstruction of Paris and a keen quaternary specialist in his own right (Roujou 1865, and see Charvilhat 1904, Blanckaert 2009:359ff). The opening salvo, in 1867, concerned Reboux's attribution of Neolithic polished implements to the upper quaternary levels at Levallois. For Roujou, such specimens had hitherto been found only in surface soils or in burials: they belonged to a much more recent and possibly different race, which used pottery, domesticated animals, and never encountered any long-migrated reindeer. It could only be concluded that "Mr Reboux did not see these *haches* in place, he bought them from the workers, and consequently he can affirm noting on the position which they occupied [in the stratigraphy]" (Roujou in Reboux 1867:107). This aspersion, incidentally, was further seized

upon by Mortillet, who was then proposing to instil some methodological rigour in archaeological fieldwork: "In practice, he said, one must above all be weary of the facts reported by the quarry workers, and believe only one's eyes (...) Did Mr Reboux himself extracted the flints which he attributes to different layers?" (Mortillet in Reboux 1867:108).

Beyond such erroneous attributions – common enough allegations, then and since, which Reboux rather ineffectually countered by expressing his full confidence in his labourers – Roujou had also some deeper objections to level at the facts themselves, as it were, at these "*supposed superpositions* at Levallois, to which he is far from attaching the same interest as his contradictor" (Roujou 1870:132, emphasis added). In his detailed paper on the quaternary terrains around Paris, Roujou seemed to doubt the actual order of the superposition proposed at Levallois and, beyond that, the very possibility of finding distinct and overlaying epochs within the Stone Age. In his zeal, Roujou appeared almost poised to reject the idea that the lowermost layers were necessarily the oldest: it was high up in the "*limons des plateaux*" that the more ancient types of flint instruments were found, he argued, whereas in general the *bas-niveaux* of Belgrand are "all the more recent that they are closer to the Seine, and situated at a lower altitude" (Roujou 1870:123, 125ff.). These stratigraphic insinuations – coming two good centuries after Nicolas Stenon, and three generations after William 'strata' Smith or Georges Cuvier and Alexandre Brongniart – prompted a forceful reaction from palaeontologist E.-T. Hamy. An emerging leader of the Société d'anthropologie de Paris, Hamy (1842-1908) was then completing his comprehensive *Précis de paléontologie humaine* as a companion volume to Charles Lyell's 1864 *Ancienneté de l'homme*. Drawing attention to what we would call 'scales of analysis', he set to quell "This in my view far too vast generalisation of the laws of inverse stratigraphy as proposed by Mr Belgrand [and Lyell]": it may be that in a given river bed the most elevated alluvial formations were the oldest,

> But in each alluvial group [- Hamy went on -], in each riverbed considered apart, nature recovers its rights and the oldest layers show themselves to be the lowermost. To another adversary I would recall first of all the excavations of Mr. Reboux at Levallois. But I know that Mr. Roujou will not take into account any of the arguments drawn from the collections of the paleoethnologist from the Ternes. I therefore cross the river, to examine the drawers of Mr. Emile Martin, the tireless researcher of Grenelle, whose discoveries Mr. Roujou does not contest (Hamy in Roujou 1870:134-135).

Chastised at least on this level, Roujou redirected his scepticism at the very possibility of distinguishing epochs and tracking change in quaternary stratigraphy. His argument is worth quoting at length:

> It is quite certain, and this fact is so evident as to require no demonstration, that in the same site the lower layers are older than the upper ones. But who will measure the time span between their formation? Who will trace the precise limits between these zones of similar colour and identical mineralogical composition? Despite prolonged researches, I have not found anything that would permit their rigorous distinction, be it in terms of fossils or of vestiges of human industry. Undoubtedly, time must have elapsed between these successive deposits, but nothing proves that this time span was long enough to bring about a perceptible change in the fauna, or some progress in this nearly stationary and immobile human industry (...) What I reject as absolutely deprived of factual evidence are all these little paleontological and archaeological subdivisions of the upper grey layers in this first bed: what is for me impossible to accept is that the stones reknapped [retouched] by percussion on their edges (*pierres retaillées par percussion sur leurs bords*) are found only in the middle layer of the bas-niveaux alluviums, for the reasons that I have myself found already at the basis of this formation and in such condition that it is impossible to believe in any reworking of the deposits (Roujou 1871b:279-280).

Banlieusard enmity aside, Roujou was by no means alone in feeling misgivings towards all these trifling "*petites subdivisions*". With arguments ranging from the theological to the epistemological, many contemporary scholars found it difficult to give credence to the notion that there were distinct epochs to be distinguished *within* the Stone Age (i.e. prior to the Neolithic), let alone that these epochs could be attested superposed one over another in a single stratigraphic section. Even to such a knowledgeable authority as John Evans, to give a signal example, it appeared just about "possible (…) that we should find that there are two drift periods distinguishable by the positions of their beds, and by the character of the implements they contain. I merely mention this as a suggestion, it may be of the vaguest kind, but still as showing the necessity of the cooperation of archaeologists and geologists on this the neutral ground between the two sciences" (Evans 1862:81). In fact, such assumptions of continuity or stasis within the Stone Age were often made in Evans' circles, notably by the likes of Charles Lyell

or William Boyd-Dawkins – the glaring exception here being of course A.H. Lane Fox (Pitt Rivers) (see on that Schlanger 2010). Moreover, also scholars who did not adhere to the "industrial immobility" implied by Roujou, or who favoured empirical on-site comparisons, had considerable difficulties to give compelling stratigraphic or chronological meaning to their observations. Such a paradox has also been observed by Marc-Antoine Kaeser in his study of Swiss archaeologist and naturalist Edouard Desor. Desor may have claimed allegiance to the "geological method", and mentioned the "distribution" or "association" of finds, but in his actual archaeological explorations and publications he made no recourse at all to stratigraphy (Kaeser 2004:295). Also Desor's longstanding friend Gabriel de Mortillet, as Kaeser notes, proceeded by "determining on homogenous sites the characteristics of an epoch as a whole, before ordering each epoch one vis a vis the other: in this undertaking, the study of archaeological stratigraphy seems to have occupied a meagre position, overwhelmed by the a prioris of unilinear evolutionism" (ibid. 296).

Specifically to the quaternary deposits around Paris, the presence of heavy-duty *haches* in the lower parts of the sequence was explained by the fact that, used as ice picks to open fishing holes on the frozen river, they had occasionally slipped and fallen from their users' numb hands (J. Prestwich), or indeed by the fact that their sheer weight led them to gradually 'sink' into the depths of the sedimentary matrix (H.-J. Gosse). It was again left to Hamy to take matters in hand and set the record straight concerning,

> another fact that Mr. Roujou seems to cast in doubt, and which Mr Reboux had, to my mind, quite well demonstrated. I am talking of the direct superposition in the same locality of the products of two different ages (...) "No serious and really scientific research has yet been attempted, said Mr Roujou, in order to demonstrate the succession of various types of worked flints in the lower alluvial layers". Here again I have regretfully to separate myself from our distinguished colleague, and it is yet again on the researchers of Grenelle that I call for the demonstration of my opinion. The first to arrive, in chronological order, is our colleague from Geneva, Mr H.-J. Gosse. Thus, it appears from the observations of this distinguished archaeologist at Grenelle that the flints of the shape of the Somme do usually occupy the lowest parts of the sand and gravel banks, whereas the knives, arrowheads etc. are more numerous on the top. Mr Gosse attributed to the different weight of the worked stone their relative position in the banks where, according to him, they must have sunk more or less deeply (...). The truth is that Mr. Gosse, without really being aware of it, had found at Grenelle the superposition of two quite distinct industrial ages, which Mr Emile Martin has since rediscovered in the same locality (Hamy in Roujou 1870:135-136, and cf. Hamy 1870:248).

5. On modes of flintworking and types of instruments

What transpires from these suitably muddled debates – alongside Reboux's crucial role in stirring them up – is a surprising entanglement of stratigraphic and artefactual interpretations. Rather than being understood and assessed in turn and independently, many such stratigraphic claims were from the onset evaluated in function of a supposedly related (or not) body of evidence that was stone implements. We may in this respect acknowledge but also reconsider the hypothesis advanced by Alain Schnapp, whereby the emergence of modern archaeology out of antiquarianism implied the 'triangulation' of stratigraphy, technology, and typology into a unified method (Schnapp 1993:321 ff., 2002:139-40, passim). In fact, this calibration was a laborious and halting process, spanning several years and generating various controversies, at the outcome of which consensus could be achieved and methodological rigour established (see also Kaeser 2004:471). Be it as it may, these debates disclose something of the state of the art of ancient stone implement studies in the 1860s and 70s. Once the previously overwhelming questions surrounding these stone artefacts have been by and large resolved – namely their authenticity and their artificiality – their study still entailed an unsuspected array of intriguing conjectures and terminological confusions. Much as Reboux himself adamantly upheld the flaked, knapped and polished flints as the cornerstones of his tripartite scheme, force is to note that the identification and interpretations of these stone implements varied quite considerably between authors and across publications.

At the 1867 CIAAP meeting, as we recall, Reboux had added the ("simply") 'flaked' stones to the already well established categories of 'knapped' and 'polished' stones. Leaving aside the 'polished' stones, unanimously attributed to Neolithic times, the debate focused on the two other terms. To judge by his illustrations (Fig. 1a, b) and by his detractors' comments, what mainly distinguished knapped from flaked stones was that the later had been "retouched" – that is, that their edges have un-

dergone some secondary modifications ("*retaillées par percussion sur leurs bords*") after they had been detached from their core. That granted, it did not follow for architect and archaeologist Louis Leguay that these were the products of distinct industries from different time periods: "Indeed, all worked flint must begin by being a flake. Thus all the epochs of the Stone Age have had they flaked flint. Therefore, it does not belong any more to one [epoch] than the other, and, as it carries no sign that would permit it to be ranked in one of them, it is only in the site (le *gisement*) that we must search for the proof of a more or less high antiquity" (in Reboux 1867:108). Worsaae was equally emphatic: "Science cannot admit such a profound distinction between so close products of the same industry. We should therefore admit only two distinctions: 1) the flaked and knapped flints, 2) the polished flints" (ibid). A learned demonstration on the very pieces presented by Reboux was undertaken by Mortillet, who concurred with these objections (ibid). Finally, also Anatole Roujou found here another opportunity to disagree with his contradictor,

> Various types of flint succeeded themselves during the formation of the *bas-niveaux* alluviums? I am little disposed to admit it, for the reason that I am very certain that the *hache* of Saint-Acheul had been employed since the beginning of this period. As for the other types [i.e. the type of Moustier], I would be less affirmative, since no serious and scientific research has yet been undertaken on the subject (...) As I have said, the *hache* of Saint-Acheul had been in use from the beginning of the epochs when the *bas-niveaux* alluviums were deposited: striking flint by percussion was therefore known since that time. If there had been an age of simply flaked stone, it is towards the beginning of the tertiary epoch that we should seek it, given that the Miocene flints of the abbé Bourgeois already show retouches (Roujou 1870:126-127).

Thus was Reboux's proposal at Levallois entwined with another distinction, then becoming topical, between the types of Saint-Acheul and of Moustier. Although intimated earlier in the century (notably by François Jouannet and Casimir Picard), this distinction was considerably reinforced and disseminated upon Boucher de Perthes' 1859 vindication. In the Paris basin, for example, the aforementioned Swiss medical student H.-J. Gosse could the very next year claim to have found both "*formes de la Somme*" and "*couteaux*" (Gosse 1860, Hamy 1870:248). In their famous explorations of the cave of Moustier in the Dordogne, Edouard Lartet and Henry Christy recorded that the same "spearhead type convex on its two faces" frequently observed in the diluvium of Abbeville and Saint-Acheul was found alongside "big spears with a plane or slightly concave face on the one side, the opposite side displaying longitudinal ridges, or being simply rounded, with sharp edges shaped more or less into festoons" (Lartet & Christy 1864:238). Three years later this distinction was readily endorsed by Mortillet for the Stone Age displays at the Musée de antiquités nationales and the Exposition universelle of 1867. Together with the *type de Saint-Acheul*, namely "these kind of *haches* in flint, of amygdaloid shape, more or less oval, more or less elongated, to which the workers of the Somme have given the name of *Langue de Chat* (Cat's tonge)" were also found "points with a uniform face on one side and reshaped with care on the other side (...) of the type of *Moustier*" (Mortillet 1867:183, 191, 1868:80, 1869a:173). While some chronological patterning between these types was occasionally hinted at, at least until the early 1870s they were most often considered as occuring *together* in the lowermost levels of both the *alluvium* terrains in the North and cave-sites to the South.

A somewhat different view on the matter was taken by Hamy, who divided the stone instruments of the Mammoth Age "into instrument shaped on the two faces [*haches*, disks], and into instruments which bear only on one side the traces of human work [knives, spearheads, flakes]" (Hamy 1870:183-184). Given his arbitrating position, Hamy's dual and hierarchical description was taken on board – so much so that Roujou undertook on its wake a spectacular, if short lived, volte-face: he who had seen the Moustier type as emerging later than Saint-Acheul now believed, following Hamy, that it actually reached back much earlier. After all,

> From the purely theoretical point of view, the type of Moustier must be considered as more ancient than that of Saint-Acheul, for it is simpler, easier to knapp, and requiring less intelligence for its fabrication. To make a *hache* of Saint-Acheul, one must necessarily knapp a type of Moustier, which is effectively that instrument half done and knapped on only one of its faces, which is infinitely easier to do, as know all persons who have worked flint (Roujou 1871a:169).

With this implacable and yet implausible logic, Roujou can hardly count among these knowledgeable experimentators – Reboux on the contrary was one of their front-runners, to judge by his understanding of flintworking in his 1873 paper on "*Des trois époques de la Pierre*". Early man, so Reboux

conjectured there, had begun to use sharp stone fragments, hand held or hafted, and then learned to strike stone on stone and consequently to prepare cores from which to detach flakes and long blades with a hammerstone: this was the first human industry, the flaked stone of the Palaeolithic epoch (Reboux 1873a:523-4, see also Reboux 1871). The subsequent phase, the knapped stone of the Mesolithic epoch, saw considerable developments in both form and manufacture. Quaternary man now worked with his hammerstone around a core or nodule, removing small flakes to shape his tool into a circular disk-like form, an amygdaloid shape, or an elongated *hache*: "One has to remark", added then Reboux,

> That this instrument was never detached from the core; whereas, once the operation over, there remained to the man of the flaked stone epoch a shaped instrument, a core and a hammerstone, the man of the second [knapped stone] epoch had in his hands only a tool and the hammer with which he just fabricated it (Reboux 1873a:525-528).

Reboux's interpretation of the "*Mésolithique*" 'knapped' stone had actually changed considerably between the "retouched" flakes of 1867 and the "core-tools" of 1873 – a change left implicit, and further confused by the continuing reuse of now obsolete illustrations (Fig. 1 b here). Nevertheless, it must be recognised that Reboux's perspective was first and foremost *technological*. With his precocious distinction between *débitage* and *façonnage* flint knapping systems (see also Evans 1872:245), Reboux was reaching towards a range of *behavioural* questions regarding processes of raw material selection, transformation and use. Indeed the experimental programme he had initiated by the early 1870s – the term is hardly too strong – touched on precisely such issues as flint knapping replications, testing modes of hafting stone implements, experimenting with their use for butchering animal carcasses, skinning hide or cutting wood, and calling on ethnoarchaeological analogies from the arctic and the Americas to interpret them – all the while engaging in public demonstrations ("*véritables conférences en action*") in such learned venues as the Sorbonne and the *Société d'anthropologie de Paris* (e.g. Reboux 1873b, 1874a, 1874b).

6. Debating Mortillet

In comparison with these promising research vistas, there appears to be something of a narrowing down in the approach that subsequently came to dominate Palaeolithic studies – namely Gabriel de Mortillet's famous "chronological" or "industrial" classification scheme, with its successive Stone Age epochs (Acheulean, Mousterian, Solutrean, Madgalenian, etc.) identified through their characteristic tool types (see Mortillet 1869, 1872b, 1883 for various developments). Before the basic assumptions and eponymous terminology of this scheme gained worldwide ascendency – notably due to their simplicity, their operational potential, their natural science allure, and their institutional anchoring and demonstration at the Saint-Germain museum –, Mortillet's proposals did not go without challenges from his contemporaries, including … Jules Reboux.

Reboux's first objection concerned the idea that the earlier phases of the Stone Age were dominated by a single implement, the type de Saint-Acheul, to the apparent exclusion of all others: "I am far from sharing the opinion [of Mortillet] which pretends that the ancients had only a single instrument for all their needs. For my part, I find in the quaternary of Paris twenty three distinct forms of weapons, instruments and tools, which are well characterised and repeated several times over" (Reboux 1873b:279). Reboux seems here to rejoin Hamy's regret that the Saint-Germain nomenclature applied the term 'type' to "a form of tool, and not, as we understand it, to an ensemble of instruments representing the industry of a country at a given epoch" (Hamy 1870:195ff, 226).

Next, alongside his interest in studying complete assemblages, Reboux could not accept that the 'all-purpose' *coup-de-poing* Saint-Acheul type singled out by Mortillet was predominantly hand held, rather than hafted. As we saw earlier, this question of hafting (*emmanchement*) was an important element of Reboux's technological and experimental approach. To Mortillet's insistence that these *haches* fitted perfectly the hand of their users, Reboux retorted with the ethnographic example of Australian aborigines, whose transversal hafting of such axes enabled them to use their two extremities – a comparison in turn rejected by Mortillet on the grounds that these Australian axes were actually polished (see Reboux in Mortillet 1874:344, Reboux 1874b, and figures therein).

Lastly, also these polished axes, or more precisely the manner of their appearance, proved to be a stone of contention. While Mortillet notoriously conceived of a radical 'hiatus' between Palaeolithic and Neolithic times, marked by (among other things) the introduction of the technique of stone polishing, Reboux doubted that this mode of stone-working had appeared all of sudden, as some suggested, brought into Gaul by some wandering Ari-

ans, who would have also (he ironised) transmitted them also to the Caraïbes, the Javanese, and indeed the Australians. It was rather the case that "all these people, without knowing each other, having the same passions and the same needs, have fabricated instruments for the same uses, while giving them different forms and perfecting them continuously" (Reboux 1874c:67, 1876a:90).

Turning upon this rather principled statement back to Mortillet's perspective, it might appear at first sight that the professional grand synthesiser took little notice of his eclectic and amateurish contradictor. Mortillet did of course include many finds from Levallois and the Paris basin in the prehistoric displays he curated during the late 1860s, and much later he also credited Reboux with the "type Levallois" (Mortillet 1880:56) – while at the same time he consistently deplored the latter's undue reliance on untrustworthy quarry workers.

The truth of the matter, however – and this argument will lead us to the conclusions of this paper – is that Mortillet was fundamentally if unavowedly *indebted* to Reboux's conceptual breakthrough, with its unprecedentedly clear-sighted and purposeful demonstration of the continuous progress of human productions. In the summer of 1867, when Reboux was advancing his first propositions, Mortillet simply did *not* believe that stone implements could serve as chronological or diagnostic indicators. Out of conviction, or possibly out of obligation or allegiance vis à vis the more senior Edouard Lartet, Mortillet had then endorsed the latter's paleontological classification, with its ages of the Cave Bear, the Elephant and Rhinoceros, the Reindeer and the Auroch. When the CIAAP delegates visited together the Saint-Germain museum of which Mortillet was the curator, for example, they could see that "so far as the simply flaked stone is concerned, the various display cases *are dated paleontologically*. At the centre of the display can be seen some remains of characteristic animals of the epoch" (Rhoné 1867;127, emphasis added). Likewise at the earliest Stone Age display in the *Galerie de l'histoire du travail* of the *Exposition universelle*, the delegates found that "on the upper part of these display cases have been placed selected specimens of now extinct or migrated animals that *serve to caracterise this epoch* (…)" (Louis Lartet 1867a:45, emphasis added). Finally, to cap it all, we find that Mortillet himself iterated this very standpoint specifically against Reboux, during the CIAAP debate: "to determine the age of a flint tool" – Mortillet unambiguously castigated the later's Levallois claims – "it is necessary to pay more attention to the accompanying fauna than to the very form of the objet" (de Mortillet in Reboux 1867:108).

The radical turnaround that Mortillet soon realised on this crucial point – forsaking from then on the "accompanying fauna" in favour of the "very form of the object" – cannot admittedly be attributed solely to Reboux's catalytic challenge[4]. It is perhaps understandable, from both personal and disciplinary standpoints, that Mortillet would chose to present these ideas as his own inferences or deductions, emanating from the meticulous observations and comparisons of securely collected evidence. During the quaternary period, so he could now assert, the fauna has little varied and cannot therefore provide clear-cut demarcations: "The same does not go for the products of industry. Its products have been profoundly modified, on several occasions and in a general manner: it is therefore on them [these products of industry] that *I have attempted* to establish a chronological and methodical classification of the caves and rock-shelters" (Mortillet 1869:172, emphasis added), and again; "For lack of fauna, only the industry can serve as the basis of a chronological classification of the caves. This is the result to which *I have arrived* long ago [i.e., in 1869]" (Mortillet 1871:171 emphasis added), or again, in its most elaborate and influential early formulation, at the 1872 CIAAP in Brussels:

> Leaving aside the paleontological method, I have turned to the archaeological method. In effet, in archaeology, is it not always by the industrial products that are determined the epochs? The Etruscan epoch, the Greek epoch, the Roman epoch, the Merovingian epoch, the middle ages, the renaissance, are they not characterised, and without question, by their diverse products? In any case, what is it we are looking for? We aim to retrace the diverse phases of the development of the history of man. Is it not therefore more natural to characterise these phases by the productions of man himself rather than by external facts? (Mortillet 1872b:435).

7. Ending with the image of progress

That, precisely, had been Jules Reboux's creed all along – the importance of material productions, of artefacts, as both the embodiment and the representation of human progress. Already in his first report on his prehistoric research at Levallois, in 1866, he clearly claimed to have found "All the intermediaries between the most coarsely knapped stones found in the base of the diluvium until the polished axes that are encountered near the surface of the soil, such that it is possible *to follow the progress of industry*" (Reboux 1866: 130-131, emphasis added,

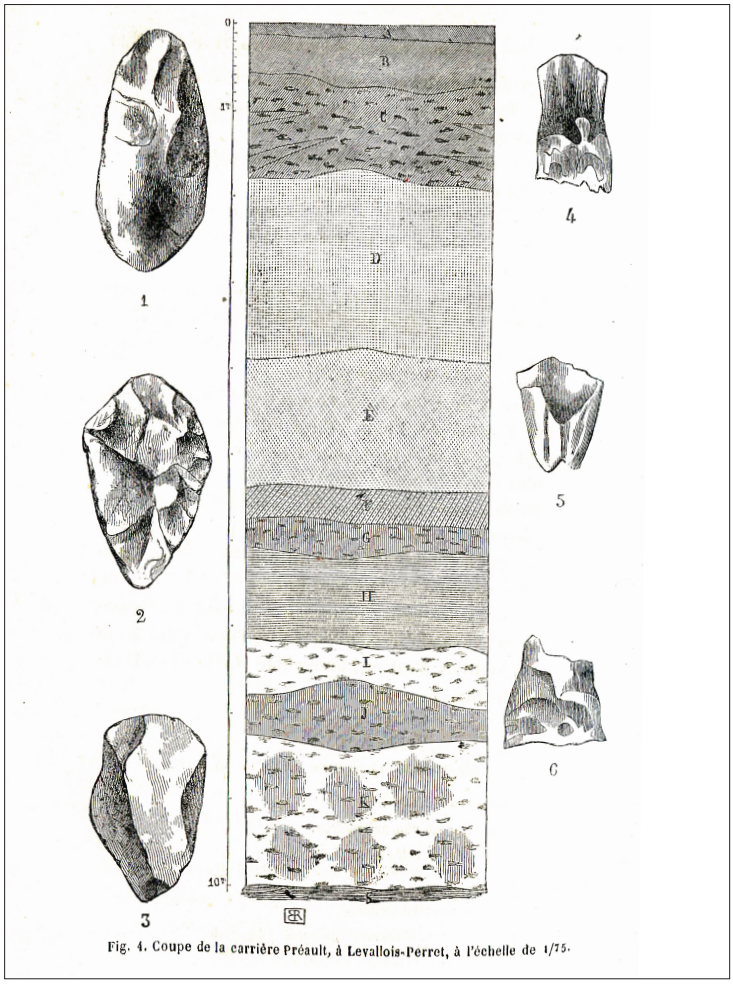

Figure 3. Section at the Préault quarry, at Levallois-Perret (scale 1/75) (Reboux 1873a:530). 1. Polished stone; Neolithic epoch; Dolmen age. 2. Knapped stone; Mesolithic epoch; Reindeer age. 3. Flaked stone; Palaeolithic epoch; Mammoth and Cave Bear age. 4. Tooth of *bos primigenius*. 5. Molar of hippopotamus. 6. Tooth of *rhinoceros tichorinu*s.

and see also 1873a, 1874a). This furthermore was the very message he undertook to illustrate in his 1873 publication. While this fairly elaborate plate (see figure 3 here) has apparently sunk without trace in the sediments of disciplinary historiography, it is in my view one of the most theory-laden images ever produced in the history of prehistoric archaeology, at once destabilising and liberating in its very boldness. The central stratigraphic column in this tripartite image conveys with its minutia of coded distinctions the empirical and observational aspirations of the discipline. It provides a solid framework on which rest, on its either sides, two distinct classificatory paradigms.

To the right, the teeth of three fossil mammal species, bolstered by their italicised Latin designations, feature as specific markers or indicators in a "paleontological stratigraphy" including the Mammoth and Cave bear age, the Reindeer age and the Dolmen age. As this image clearly exposes, however, the perceptible dissimilarities between these specimens cannot hide their blatant *equivalence*, with regards to their distance, their ordering or their relations to one another.

In marked contrast to these essentially arbitrary or interchangeable icons of natural history, the products of flintworking assembled on the left-hand column show quite vividly a succession, a *series*, a "*superposition industrielle*" as Reboux called it, whose thrust we can at last fully appreciate. We can see the simply detached flake tools of the first epoch (Palaeolithic, n°3 here) followed

and superseded by the core tools *haches* of the second epoch (Mesolithic, n°2 here), leading in turn to the polished axes of the third epoch (Neolithic, n°1 here), in a pattern of incremental continuity across the Stone Age. While Jules Reboux's feeling for progress was actually of a spiritualist, non-materialist kind – interspersed with references to the "human mission" and the "god-given gift of perfectibility" (e.g. Reboux 1871, 1876a) – it was also resolutely technological, driven by the "logic of facts". Even if some of his empirical claims could not be sustained, it was nevertheless our ingenious amateur from Levallois who proved best able to grasp the "LOI DU PROGRÈS DE L'HUMANITÉ" – so grandiloquently proclaimed by the tenor of transformism during the *Exposition universelle* of 1867 (Mortillet 1867:368, original caps) – and transform it from an abstract petition of principle into a productive research programme on the nature and pace of human (pre)historic development.

8. Coda

Upon this recovery of Reboux and his role in the crucible of prehistory, historians of archaeology will want to address two further questions: firstly, "how can we account for his inventive and original approach?" and secondly, "why have no traces of his work been retained in disciplinary memory and historiography?" These questions will be addressed in depth elsewhere, but here I can only suggest that the answer to both may well hinge on a single observation, namely that Jules Reboux was essentially an antiquarian.

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Notes

1. First identified as a 'type' (see Hamy 1870:195 ff., Mortillet 1883), the Levallois technique has been associated with Mousterian, Middle Palaeolithic, and pre Homo sapiens occurrences, and given considerable chrono-cultural, technological and, more recently, cognitive significations (see Schlanger 1996, Chazan 1997, and references there).

2. Gilbert 1942. The circulation of finds across collectors and collections in that period represents a research topic in its own right: see some orientations in Van Rebrouck et al. 2009, Chazan 2009, Petraglia & Potts 2004.

3. Without being aware of each other's work, Westropp divided the Stone Age in a relatively similar way: "1. The flint implements of the gravel-drift, evidently used by man in his lowest and most barbarous grade. 2. The flint implements found in Ireland and Denmark, which belong to a people who lived by the chase. 3. Polished stone implements, which mark a more advanced stage, perhaps a pastoral age. The following terms may be used to distinguish them: –Palæolithic, Mesolithic, Kainolithic" (Westropp 1866:291, and see 1872:65).

4. In addition to a certain disenchantment from paleontological stratigraphy as such, another key factor results from the administrative and ideological constraints surrounding the 1867 *Exposition universelle*, as conceived by Frédéric Le Play and Michel Chevalier (see Schlanger, in preparation).

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