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# The Theory of Evolution and Its Impact

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stand still, but remakes itself and grows and changes by virtue of the fact that it gives such a terrific foundation. Is Darwinism past its sell-by date? Not by a long chalk yet!"

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# Idola Tribus: Lamarck, Politics and Religion in the Early Nineteenth Century

**Pietro Corsi**

**Abstract** There is no doubt that traditionally the history of evolutionary ideas has been and is Darwin-centred. I have no dispute with this, being a convinced "Darwinian", in spite of years of work I have devoted to study Lamarck and the many non-Darwinian theories of evolution current in Europe and the United States before and after 1859. Whereas historians have paid some attention to post-Darwinian, non Darwinian theories, pre-Darwinian theories have been much neglected. Attention is usually paid to so-called "Lamarckian" attitudes present in European natural history debates from the early 1800s to the 1850s, only to conclude that Lamarck played no role, was almost unanimously neglected and in any case unanimously vituperated. This was hardly the case. However, the aim of my paper is not to vindicate Lamarck, but to argue that even concentration on Lamarck would amount to gross anachronism. After analysing reasons – essentially political and religious – that have been given to explain the alleged oblivion into which Lamarck's works had fallen (if they ever rose to attention) I will examine evidence concerning the wider debate on Lamarck's ideas within the medical literature of the 1810s and the 1820s. This will open up a new research area, focussed on the translation into French of major German authors (Meckel, Tiedemann, Carus, Treviranus, Burdach, Oken) and on the attempts to re-formulate key Lamarckian tenets in the terms of German natural philosophy, comparative anatomy and embryology, and medicine. The debate on the development of life – historical and embryological – was wider and much more interesting than the debate on Lamarck's own theories, which in any case well deserves to be rescued from oblivion.

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All differences taken into account, Lamarck and Darwin shared the common destiny of being often identified with doctrines they never upheld, or not exactly in the form history has attributed to them. Over the last century and one half, wave after wave of the recurrent debate on "Lamarckism" vs. "Darwinism", and the repeated rituals of centenary and other anniversary celebrations have done much to obscure the real contribution of the two naturalists to the debates on evolution. In saying so, I am of course taking for granted several assumptions, some of which will be spelled out and discussed in the following pages. A major assumption which will not be critically scrutinized, and is presented here as a comment at the end of one year of world-wide celebrations, is that in-depth and easily available historical studies on Lamarck and Darwin have been rarely read or consulted by a good number of commentators who during 2009 have been very active explaining who Darwin really was. Nor have they been consulted by the much lower number of those who remembered that 2009 marked not only the bicentenary of the birth of Charles Darwin, but also of the publication of the *Philosophie zoologique*, one of Lamarck's key evolutionary texts. The impression one gets, after reading, viewing, or listening to a statistically relevant portion of what has been said on Darwin during 2009 through several continents and languages, is that his works, as those of Lamarck, are not that well known, and that the work of professional historians who have engaged the primary sources is scarcely taken into account. This is not a novelty, after all: since the early 1800s much of the debate on what we call today "evolutionary" doctrines was carried on without much attention to the actual articulations of the "scientific" arguments under discussion.

Before stating and developing the key themes of my paper, let me provide only one example of what I peremptorily stated above. There is no doubt that the doctrine of the inheritance of acquired characteristics is universally regarded as the cornerstone of Lamarck's theory and the major point of difference with Darwin and Darwinism. Yet, as Jean-Gayon has persuasively argued, and a rapid search by word of the Lamarckian corpus available on line will confirm, Lamarck never spoke of the theory of the inheritance of acquired characteristics.[1] He most surely believed that new needs originate new behaviours, and new behaviours increase or decrease the size and functions of the solicited organs, to the point that new species and genera are formed. Life is thus constantly transformed, since the process is cumulative through inheritance. This was a conviction he shared with many authors active at the end of the eighteenth and at the beginning of the nineteenth Centuries, to the point that early critics of Lamarck rarely complimented or reproached him for this. The key issue when discussing Lamarck was always whether the process of change he had described was sufficient to overcome the species or the genus barrier – a point some were ready to concede – as well as higher divides (family, order) – which very few granted. Fifty years later, the same reaction characterized the early

[1] Jean Gayon, "Lamarck Philosophe", in P. Corsi et al., *Lamarck philosophe de la nature*, Paris, PUF, 2006, pp. 9–35. See P. Corsi, http://www.lamarck.cnrs.fr/ for the complete edition of Lamarck's theoretical works, his manuscripts and herbarium.



(and later) debates on natural selection: many saw it as a plausible mechanism to explain the fixation of varieties, which could in no way put in doubt the constancy of species, or of genera.

The interesting question that emerges from carefully comparing the relevant Lamarckian and Darwinian texts, is that the capability of organisms to change and to pass on to the next generation whatever was gained or lost during their lifetime was severely limited in the case of Lamarck, and less so with Darwin. For Lamarck, only very young organisms, in which the tissues were still very soft, and the circulation of blood, nymph, and the nervous and other fluids was particularly brisk, showed a potential for adaptive change: never the adults. This was not Darwin's opinion. When presenting the ill-fated and little studied theory of pangenesis, among other phenomena of heredity Darwin sought to explain how a change that occurred at a given point in the life of one organism tended to appear again at the same stage of individual development in his progeny. Furthermore, whereas Lamarck simply took up a widely shared, almost commonsensical belief that the characteristics of the parents were passed on to the next generation, Darwin spent time and ink to understand how this was possible, even discussing similar theories put forward by authors such as Georges-Louis Leclerc, Comte de Buffon (1707–1788) and Charles Bonnet (1720–1793), on whom Huxley had called his attention.[2] Lamarck spent much less time on the matter: he simply argued that since the male seminal fluid (akin to electricity and magnetism) acquired specific peculiarities within each type of organism, it was legitimate to infer that the same fluid would take up slightly different anatomical and functional properties by circulating through an organism that had undergone a very slight change during the early phases of its life. Indeed, for Lamarck, fully blown characteristics were the end result of a cumulative process of very minor changes within the fluid dynamics internal to all and every organism. Thus, if a new need was requiring a more pronounced use of a given organ, thereby increasing the flow of blood, nutritional and nervous fluids to that part, what was passed on to the next generation (provided the young individuals that had gone through the same process reproduced together when adults), was not a character that as yet did not exist, but the slight change in the pattern of the fluid dynamics and the slightly modified features of the seminal fluid. On the contrary, Darwin admitted the inheritability of changes occurring in a single parent, and asked himself how these could be maintained through successive generations. This is not to conclude that Darwin was more Lamarckian than Lamarck, but to insist on the fact that the mere reading of the works of Darwin and Lamarck would prevent all hasty and easy generalizations.

Even though rarely read by those who should, the scholarship on Darwin of the last 40 years has been on the whole excellent and has powerfully contributed to a

[2] See C. Darwin, *The Variation of Animals and Plants under Domestication*, 2 vols., London, J. Murray, 1868, vol. 2, Ch. XXVII, "Provisional hypothesis of pangenesis", pp. 357–404.

less anachronistic appreciation of the man, his career and doctrines.[3] The case of Lamarck, on which I will devote the main bulk of my paper, is to some extent quite different. There are of course excellent studies of his work and career, though the writings of the French naturalist have not been translated or edited with the same alacrity and systematic dedication.[4] There is no correspondence left worth mentioning, no notebooks, no autobiographies or diaries. Much of the Lamarckian manuscripts are in fact drafts or final versions of printed works. Lamarck was very parsimonious with information about himself, his life and thoughts, to the point that much of the scarce biographical hints we have are due to members of his family, his enemy Cuvier or to a young medical practitioner who interviewed him in the early 1820s.[5] Moreover, whereas over the last 20 years or so important scholarship has appeared offering insights into the wider natural history scene (institutional, intellectual and social) of the United Kingdom, the same cannot be said of France during the times in which Lamarck was active. In other words, the scholarship on Lamarck has not incited new studies on the wider scientific and institutional context characterizing the life sciences during the early decades of the nineteenth century in France.[6]

The rare albeit excellent exceptions to the rule have not helped us to gain a less anachronistic view of contemporary priorities, actors and debates. The set of traditional assumptions concerning the context of Lamarck's work remain stubbornly unchanged, in spite of growing evidence that should advice historians to enlarge the scope of their research. It is a few of these implicit, often untold assumptions I wish to tackle in the following pages. Basically, they turn around a major conviction, the total or almost total isolation Lamarck lived in. This assumption generates in its turn a host of further assumptions – if not prejudices – asked to perform a causal role in the narrative. They can be ranged, historically and thematically, from the (usually French) patriotic and whiggish explanation that Lamarck was born too early, or that he was seeing too far, to the less charitable (usually Anglo-American, pro-Darwinian) view that he was simply wrong, overwhelmed by top brass of science such as Georges Cuvier (1769–1832), Pierre-Simon Laplace (1749–1827), or Antoine Lavoisier (1743–1794) and his pupils.

[3] I will only refer here to the biographies by A. Desmond and J. Moore, *Darwin*, London, Michael Joseph, 1991 and *Darwin*, Oxford, Oxford University Press, 2007; and J. Browne, *Charles Darwin*, 2 vols., London, Jonathan Cape, 1995–2002.

[4] See for instance R. Burkhardt, *The spirit of system: Lamarck and evolutionary biology*, Cambridge, MA, Harvard University Press, 1977, 1995.

[5] Georges Cuvier,. "Éloge de M. de Lamarck, lu à l'Académie royale des sciences le 26 Novembre 1832", in *Mémoires de l'Académie royale des sciences de l'Institut de France*, 13 (1831–1833), pp. i–xxx; Isidore Bourdon, "Lamarck", *Dictionnaire de la conversation et de la lecture*, 34 (1837), pp. 265–269.

[6] See for instance J. Secord, *Victorian sensation: the extraordinary publication, reception, and secret authorship of Vestiges of the Natural History of Creation*, Chicago and London, University of Chicago Press, 2000, and J. Endersby, *Imperial nature: Joseph Hooker and the practices of Victorian science*, Chicago and London, University of Chicago Press, 2008.



He had no chance to be listened to in a world moving towards disciplinary specialization and epistemological rigour. A second assumption concerns the inevitability of Lamarck's isolation in the increasingly conservative political climate of the Consulate and the Empire, and in the ultra-conservative intellectual atmosphere of the restored monarchy. His materialistic biology and transformist doctrines (it is claimed) were unacceptable to authorities determined to curb any form of political and intellectual subversion. Finally, the third assumption we are going to examine below is the one concerning the audience of Lamarck's works. Followers of various versions of assumptions one and two will find this third point completely superfluous. To them, Lamarck had no audience worth talking about, at least until the 1820s, and even then the few who paid any attention to him did not, in fact, support his views as the old naturalist would have wished. In France as well as in Europe, Lamarck's materialism found sympathetic hearing only within the radical fringes, thereby adding to the already long list of reasons people had to dismiss him outright.[7]

The way in which the assumptions we have sketched above have been argued by historians does not lack plausibility and evidential support. Yet, consensus has been gained at the price of restricting the research horizon to the point of neglecting major features of natural history practices and debates of the early nineteenth century, in France as well as elsewhere in Europe.

## 1 Lamarck Versus Institutional Science

Very few historians of early nineteenth century life sciences appear to doubt that the major educational and institutional reforms introduced by successive revolutionary governments, the Directory, the Consulate and the Empire deeply changed the social and intellectual practices of research within the complex articulation of disciplines still constituting the "histoire naturelle". To a significant extent, they are absolutely right. In 1792, 1793 and 1799, two naturalists occupying the opposite sides of the epistemological spectrum in the debate over natural history agreed that France was not doing much, after all. Jean-Claude de la Métherie (since 1793 simply "Delamétherie") and the then still little known Cuvier insisted that Germany was better equipped than France in several sub-domains of natural history. Cuvier pointed out that almost every German university town was publishing its own scientific or medical journal and hosted important private and public collections. In France, almost everything was concentrated in Paris, and Parisian naturalists were too happy to sit on the top of their monopolistic privileges to care about sharing their knowledge with colleagues in the provinces and abroad. As a

[7] The best known and best argued representative of the view that Lamarck was acceptable only to extreme radicals is Adrian Desmond, *The Politics of Evolution. Morphology, Medicine and Reform in Radical London*, Chicago and London, University of Chicago Press, 1989.

consequence, France was rather poor in periodical publications, since only a handful had survived the revolutionary years, and there were not that many even before 1789, for that matter.[8]

In the space of a few years the situation changed dramatically and unpredictably. The Revolutionary armies engaged in the systematic plundering (which they called "confiscation") of conquered lands, to finance the huge state deficit and the costs of the war. Cash, precious minerals, paintings and sculptures, natural history and scientific instrument collections took the road to Paris in hundreds of over-charged wagons. The *Muséum national d'histoire naturelle*, established in June 1793, was the ideal place were natural history collections could be hosted, catalogued and studied on behalf of the Republic of knowledge, which did not know of frontiers or wars. Confiscations were undertaken with a higher view in mind, the benefit of mankind, French authorities insisted.[9] By 1802, Paris hosted the largest and richest natural history collections ever assembled in Europe. Naturalists from all over the Continent had to pay frequent visits to the French capital: some, undoubtedly, to pay due homage to the new rulers; others, because they had to keep up with their own work. The local German collections Cuvier had extolled in 1799 were no more sufficient to guarantee cutting edge research.

French scientific publishing also benefited from the new impulse successive governments accorded to the practice of science.[10] The at times purely symbolic

[8] Georges Cuvier, "Extrait d'une Notice biographique sur Bruguière, lue à la société philomathique, dans sa séance générale du 30 nivôse an VII", in *Magasin encyclopédique*, 5th year, vol. 3 (1799), pp. 42–57; Louis Marchant, *Lettres inédites de Georges Cuvier a C. H. Pfaff sur l'histoire naturelle, la politique et la littérature*, Paris, Victor Masson, 1858, p. 78: "Les sciences ont aujourd'hui peu de dignes prêtres en France, et cette pauvreté est d'autant plus pénible, que l'on se souvient encore de l'ancien éclat dont elles ont brillé"; Jean-Claude Delamétherie, "Discours préliminaire", in *Journal de physique*, 42 (1793), p. 7. See also A.-L. Millin, "Journal d'histoire naturelle", in *Magazin encyclopédique*, 1, n. 8 (8 décembre 1792), pp. 57–60, "L'Allemagne voit paroître un grand nombre de collections, et de recueils d'histoire naturelle", p. 57.

[9] For two recent systematic studies of the accumulation of collections in Paris see B. Daugeron, *Apparition-Disparition des Nouveaux mondes en Histoire naturelle, Enregistrement-Epuisement des collections scientifiques (1763–1830)*, Paris, EHESS, Thèse de doctorat, 2007, 2 vols.and P.-Y. Lacour, *La République naturaliste. Les collections françaises d'histoire naturelle sous la Révolution, 1789–1804*, Florence, European University Institute, Ph. D. Dissertation, 2010, 2 vols. It is interesting to point out that in his biographical notice of Bruguière (see n. 8) Cuvier complained that the collections amassed at great public expense were now collecting dust at the Muséum, since no one appeared to work on them. This too was soon to change, but systematic exploitation of the conquered natural history riches only started after 1802, that is, after Cuvier became full professor there. The political innuendos of Cuvier's astonishing biography of Bruguière, his equally astonishing veiled attacks against colleagues working at major State institutions have never been analyzed in detail.

[10] Amongst many, the testimony of Louis Marchant, *Lettres inédites de Georges Cuvier*, is telling, p. 30: "C'était une époque très-favorable pour les sciences et ceux qui les cultivaient; le premier consul se trouvait très-honoré du titre de membre de l'Institut, il le mettait en tête de tous les autres. Les premiers hommes de la science, comme Laplace, Chaptal, Monge, étaient en même temps les premiers aux affaires. L'institution grandiose du Jardin des plantes, à laquelle des savants spéciaux d'une grande célébrité étaient attachés pour chaque branche de l'histoire naturelle, pour la géognosie, la géologie, pour la chimie théorique et pratique, à laquelle se reliaient les grands musées nationaux, avait surtout une grande part dans cette sollicitude et ces encouragements."



encouragement lasted until the end of the 1790s, followed by real investments – especially as far as the Museum d'histoire naturelle was concerned, during the early 1800s. Yet, already in the second half of the 1790s the rhetorical and ideological support for science-oriented activities was sufficient to massively increase the market for natural-history publications: collective works of Buffon, dictionaries and encyclopaedias, textbooks and innovative surveys of new or renewed domains of research (among which Cuvier's *Leçons d'anatomie comparée*), manuals for the new school systems found eager buyers and were exported throughout Europe and the Americas.[11] Unfortunately, this feature of the French publishing market has been little studied and even less appreciated. Furthermore, no attention has been paid to the dense population of naturalists surviving thanks to their pen. Historians have tended to ignore a highly articulated editorial, epistemological and research scene and have instead concentrated their attention on a handful of individuals and institutions.

As it is often the case within the history of science, our contemporary concept of proper scientific practice deeply influences our reading of the past. In spite of the pioneer work of Dorinda Outram, published in 1984, historians insist (at times very ably indeed) that the natural history scene was dominated by Georges Cuvier, who exercised an undisputed leadership, set the standards, and marginalized whoever did not conform to his anti-speculative, matter of fact approach to natural knowledge.[12] The *Muséum national d'histoire naturelle* and the *Institut de France* were the strongholds from where Cuvier, the so-called Napoleon of intelligence, manoeuvred his troops and kept epistemological order. Outram richly documented the difficulties Cuvier experienced, the set-backs he had constantly to suffer, and pointed out that many of the institutional gains the naturalist scored were often the consequence of his political shrewdness and power rather than the indication of his undisputed scientific authority. When in 1818 a distant relative and admirer of Cuvier, Jean-Jacques Coulmann (1796–1870), visited London, he was amazed to notice that the anatomist was better known in the English capital than in Paris.[13] As I have myself documented, Cuvier's leadership was constantly challenged, in some

[11] For further contemporary testimonials, see, among others, F. W. Blagdon, *Paris as it was and as it is*, London, C. and R. Baldwin, 1803, vol. 2, p. 582, and *passim*. See also P. Corsi, "After the Revolution: Scientific Language and French Politics, 1795–1802", in M. Pelling and S. Mandelbrote, eds., *The Practice of Reform in Health, Medicine, and Science, 1500–2000*, Aldershot, Ashgate, 2005, pp. 223–245

[12] D. Outram, *Georges Cuvier: Vocation, Science and Authority in Post-Revolutionary France*, Manchester, Manchester University Press, 1984.

[13] J -J Coulmann, *Réminiscences*, Paris, Michel Lévy frères, 1862–1869, vol. 1, p. 233: Coulmann was the younger brother of the wife of General Frederic-Louis-Henri Walther (1761–1813), Cuvier's first cousin. Outram, 1984, has called attention to the role Walther played in introducing Cuvier to powerful figures in Paris in early 1795, when Walther was already a significant figure within the army.

cases openly derided, and the publications directed to the general public he sponsored failed miserably (his famous and very successful specialized works were too expensive to reach the ordinary reader), to the joy of his opponents. Cuvier did try his best to convince contemporaries that he was the depositary of true scientific values and achievements. His *éloges* of deceased academicians, his reports to the Emperor on the progress of science in France and Europe spared no effort to turn celebration and highly selective information into prescription. Yet, year after year the *Journal de physique* devoted the entire January issue to review scientific publications and achievements throughout Europe. Delamétherie, its editor, made no secret of the fact that he was challenging Cuvier's rival accounts. For instance, Cuvier never mentioned current debates on spontaneous generation, to him a non-subject, whereas Delamétherie informed readers – and the historians – of how many variants of spontaneous generation doctrines were available in Europe during the 1800s and the 1810s.

I mentioned above the highly competitive market of dictionaries and collective works. During the 1790s, Charles-Nicolas-Sigisbert Sonnini de Manoncourt (1751–1812), a former collaborator of Buffon, launched an edition of his master's work, to which scores of volumes were added, to turn it into a "Complete course" of natural history. 127 volumes were published and sold to over 1,400 subscribers throughout France and Europe.[14] In 1803 the same editorial team launched the *Nouveau dictionnaire d'histoire naturelle*, completed in the space of 2 years.[15] The 24 volumes, relatively cheap set was sold to some 2,300 readers at Continental level.[16] Cuvier reacted with anger, as I have already documented elsewhere. In 1804 he issued an announcement inviting readers to subscribe to the forthcoming *Dictionnaire des sciences naturelles*, placed under the editorship of his brother Frédéric (1773–1838). Adopting a tone of ease and superiority, Cuvier warned readers not to buy the rival publication, which was not issued by the Professors of the Museum as his own dictionary was, but by amateurs without much experience.

[14] Charles-Nicolas-Sigisbert Sonnini de Manoncourt, ed., *Histoire naturelle, générale et particulière, par Leclerc de Buffon. Nouvelle édition, accompagnée de notes, dans lesquelles les supplémens sont insérés dans le premier texte, à la place qui leur convient. [. . .]*, Paris, F. Dufart, Year VII (1798)-1808. The list of more than 1400 subscribers does not take into account direct sales from the printers and publishers.

[15] Julien-Joseph Virey, *Nouveau dictionnaire d'histoire naturelle appliquée aux arts, à l'agriculture, à l'économie rurale et domestique, à la médecine, etc., par une Société de naturalistes et d'agriculteurs*, 1803–1804. 1st ed., 24 vols., Paris, Déterville. 1816–1819; 2d ed., 36 vols., Paris, Déterville. Already in 1807 the publishers were considering a second edition.

[16] For an interesting contemporary comment on the *Nouveau dictionnaire* see H. Redhead Yorke, *Letters from France in 1802*, 2 vols., London, H. D. Symonds, 1804, vol. 2, p. 333: "This precious work is published at so reasonable a price, that the sale will scarcely defray the expenses of paper and printing. It is essentially a patriotic undertaking by Sonnini, Virey, Parmentier, Huzard, Bosc, Olivier, Latreille, Chaptal, Cels, Thouin, Du Tour, and Patrin, men possessed of great knowledge of the subjects of which they treat".



Cuvier badly lost the war of dictionaries. Only volume 5 of the *Dictionnaire des sciences naturelles* appeared and only a handful of subscribers received copies of volume 6. In 1816, the publishers of the *Nouveau dictionnaire* started a new expanded edition. This time, 36 volumes were distributed in the space of 3 years to almost 3,000 subscribers. Cuvier immediately retorted by changing the cover of the first 6 volumes left unsold in 1804–1805, and launched the *Dictionnaire des sciences naturelles* a second time over. Volume 60 was issued only in 1830, and a careful perusal of the entire set (a task historians have surprisingly failed to undertake) reveals that Cuvier and his brother did not, on the whole, spend much time on it. Cuvier's dictionary is a rather disappointing publication, lacking a firm editorial line and, at time, even basic supervision. Embarrassingly, determined enemies of Cuvier were allowed to write entries that contradicted what the famous naturalist was preaching since his coming to Paris, as it was the case with the entry "Matière verte", confided to Jean-Baptiste Bory de Saint-Vincent (1778–1846), who did not miss the ironic chance of promoting spontaneous generation on the pages of a publication nominally under the tutelage of Cuvier.[17] One final point needs to be emphasized. In the printed announcement we mentioned above, Cuvier insisted that his dictionary represented institutional science, and his rivals were only members of the unruly crowd of unreliable if not desperate amateurs. If one compares the list of contributors to both dictionaries, one will immediately realize that Cuvier did not tell the truth. The rival dictionary could boast contributions by Jean-Antoine Claude Chaptal (1756–1832), member of the Institut and until 1804 Minister of the Interior; of André Thouin (1746–1824), the famous head gardener of the *Jardin des Plantes* and himself member of the Institut; and of Antoine-Augustin Parmentier (1737–1813), the ultra-popular pharmacist, lover of potatoes, and expert in food preservation, among many other accomplishments. Parmentier, it is appropriate to mention, was the main sponsor of the *Nouveau dictionnaire d'histoire naturelle*: in no way could he be considered an amateur. Thus, even Cuvier was at pain to draw a clear line between the "institutional" science he was supposed to lead and the actual articulations of natural history practices and publications over which he clearly exercised little control.

During the 1810s and the 1820s, a series of popular generalist encyclopaedias and of more specialized medical and natural history dictionaries did not hesitate to openly challenge Cuvier's authority. By 1816, Cuvier almost completely withdrew from all teaching engagements, paid assistants and secretaries to do his job, and became a full time member of the political establishment. Authors ranging from Louis Antoine Desmoulins (1794–1828), Étienne Geoffroy Saint-Hilaire (1772–1844) and François-Vincent Raspail (1794–1878) did not refrain even from alluding to the fact that Cuvier was limiting himself to the writing of prefaces to the various, very expensive volumes of the *Règne animal* or the *Histoire naturelle des poissons* compiled by a host of ghost writers and researchers: he

[17] J.-B. Bory de Saint Vincent, "Matière verte", in *Dictionnaire des sciences naturelles*, 29 (1823), pp. 314–336.

had become an author of coffee table books.[18] The question is not whether the allegations (undoubtedly exaggerated) were true or not. The point to be emphasized is that during the 1820s Cuvier's authority was challenged at various institutional and editorial levels, and the only power he could exercise was on his own salaried staff: within certain limits, as we shall see below.

Thus, the view that Lamarck was silenced by Cuvier's dominant and domineering position within the hierarchy of contemporary natural sciences cannot be substantiated. Cuvier hardly managed to silence anyone, even colleagues he hated, such as the geologist and monarchist Barthélemy Faujas de Saint-Fond (1741–1819, whom he nick-named "Sans fond" in reference to his colleague's constant need for money), Geoffroy Saint-Hilaire (who was writing with the style of a "cook", Cuvier mused), Lamarck (a lonely man dominated by his imagination), or wizards of the natural history publishing industry such as Delamétherie (editor of the then famous *Journal de Physique*) or Sonnini de Manoncourt. The latter repeatedly and publicly made fun of his junior colleague, who pretended to discipline all the rest of the community of naturalists by waving at them his pathetic school teacher's stick: people knew better, and bought the works he, Sonnini, was producing.[19]

If one pays close attention to Cuvier's career after the very early 1800s, one is struck by the decreasing time he spent teaching and undertaking first hand research. He was often absent from Paris, and when in town he devoted much of his seemingly boundless energy to perform administrative duties and to keep his many State jobs. Staying in power is never a given, and Cuvier, as contemporary witnesses such as Stendhal, de Chateaubriand, or Cuvier's distant relative we mentioned above testify, worked very hard to keep surfing the treacherous, incessantly breaking waves of contemporary power.[20]

The assumption that Cuvier exercised a decided and unquestioned authority over the natural history disciplines of his time can be maintained only at the price of ignoring four fifths of the natural history scene of the time. As we will argue below, Cuvier did not exercise full authority even on his direct collaborators, who were wholly dependent on his good will to continue to earn a salary. They obeyed, did what they were told, but in turn actively supported naturalists who did share their admiration for the master, but felt free to pursue lines of research Cuvier was very critical of, or openly despised.

As a client of power, Cuvier was hardly in a position to challenge power, even when decisions concerning his own career and appointments were taken that forced

[18] Antoine Desmoulins, *Histoire naturelle des races humaines*, Paris, Méquignon-Marvis, 1826, p. viii; François-Vincent Raspail, "Coteries scientifiques", *Annales des sciences d'observation*, 3 (1830), pp. 151–159, p. 157; on Geoffroy Saint-Hilaire's repeated attacks against Cuvier, see Corsi, *The Age of Lamarck*, ch. VIII.

[19] I have discussed Sonnini's defiant attitude against Cuvier in *The Age of Lamarck*, pp. 36–38.

[20] Stendhal, *The Life of Henry Brulard*, translated and with an introduction by Jean Stewart and Bert C. J. G. Knight, London, 1958, p. 180



him to swallow very unsavoury compromises. One single example will suffice. When the highly respected former collaborator of Buffon, Louis-Jean-Marie Daubenton, died at the end of December 1799, Cuvier aspired to succeed him to the Chair of mineralogy at the *Collège de France*. This was to him an important step forward, and a prestigious, highly symbolic promotion. Delamétherie, who had spared no ink in publicly deriding his junior colleague, whom he depicted as an able yet arrogant social climber, felt the chair belonged to himself: he was after all (in his own eyes at least) one of Europe's most prestigious mineralogists. Politics played a key role in the appointment. Delamétherie was supported by General Bonaparte's independently minded brother Lucien (1775–1840), and by his own brother Antoine (1751–1804), an opaque yet faithful "yes-man" of the First Consul Bonaparte. Antoine had helped Lucien to orchestrate and execute the coup-d'état of Brumaire 1799 that imposed the Consular regime and established Bonaparte's leadership. Almost on the day of the appointment Lucien and Napoleon quarrelled, and Lucien's authority was greatly diminished. The chemist and successor to Lucien to the Ministry of Interior, *ad interim* in the fall of 1800, and fully in 1801, Chaptal, and his friend Bernard-Etienne de Lacépède (1756–1825), a naturalist also very close to General Bonaparte, favoured Cuvier, who was duly appointed. Yet, political debts to Antoine Delamétherie had to be paid as well, and the letter of appointment specified that Cuvier was granted the chair, but Jean-Claude Delamétherie was accorded the position of assistant. His salary was going to be one-third of the salary allotted to the full chair, which Cuvier had to pay directly to the hated rival.[21] It was only in November 1802 that Cuvier was finally given a full chair at the *Muséum*, the same year in which he was appointed perpetual secretary to the First Class of the Institute. As Dorinda Outram pointed out several years ago, Cuvier would have been very pleased to know that historians would make him the undisputed emperor of natural sciences. This would have consoled him of the enormous efforts he kept producing to maintain his many jobs and his many salaries in the real world of politics. He had little patience with academic rituals – which he did nevertheless perform as an accomplished master – and did his best to revenge himself against his intellectual and political enemies. Whether he succeeded is of course another matter, and to assume that he managed to condemn Lamarck to a bitter isolation is precisely a groundless assumption.

## 2 The Politics and Religion of Science

As we mentioned above, Lamarck has left precious little archival material; the thousands of pages of manuscripts still preserved are in fact, for the most part, drafts of his published works. Thus, we have only his printed works to try to understand

[21] H. D. de Blainville, *Observations sur la chaire d'histoire naturelle du Collège de France*, Paris, 1832.

his standpoints on dramatic and burning issues such as politics and religion before and after the French revolution and during the Empire and the Restoration. As far as politics is concerned, Lamarck kept a low profile, though in 1794 he dedicated his *Recherches sur les principaux faits physiques* to Jean-Paul Marat, refusing, he added, earlier suggestions to inscribe the work to Louis Capet, the deceased King. As far as religion is concerned, some commentators have insisted on his mentioning the Deity here and there as evidence that Lamarck was at least a deist, though they failed to notice that reference to a Superior Being increased with the increasingly conservative intellectual and political climate of the Consulate, the Empire and the Restoration. As an eternalist who claimed that nothing can be created or destroyed in nature, since all elements exist since eternity; as someone who wrote that religious ideas were created and spread by self-interested elites aiming at dominating the majority of the population; and as someone who at the end of his life affirmed that there was no metaphysical principle sustaining human life and consciousness – no soul, in other words, there is little doubt that he was not a fervent nor even a lukewarm Christian, and I personally doubt he was even a deist. What we know for sure is that he waited a relatively long time before baptizing his children, and did so only in 1808.[22] This was well into the Empire, when it was abundantly clear to everyone that the regime would not tolerate open profession of atheism.

Lamarck's post-1800 views, his transformist doctrines in particular, were indeed accused of leading to, or even of advocating atheism. Julien-Joseph Virey (1775–1846) explicitly linked Lamarck to atheism in the successful *Nouveau dictionnaire d'histoire naturelle*, and repeated his charges several times until the 1830s. A precious testimony has recently been unearthed by Hervé Ferrière in his ground-breaking doctoral dissertation on Bory de Saint-Vincent.[23] In 1804 Bory's best friend, the entomologist Jean-Marie Léon Dufour (1780–1865) was in charge of seeing through the press Bory's *Voyage dans les quatre principales îles des mers d'Afrique* since the author was away from Paris on military duties. Dufour unsuccessfully tried to convince Bory to suppress a chapter, in which the young naturalist sketched a non-Lamarckian evolutionary model for the history of the Earth and of life, inspired by current debates on the "Théorie de la Terre": was he not aware, Dufour insisted, of what priests and bigots ("prêtraille" and "bigotaille") were saying against Lamarck? Prudence was called for, to avoid unnecessary danger and polemic. Though further research is required to establish the actual foundation and origin of this rumour, there

[22] Raphaël Bange, "Les ressources de l'état civil parisien pour l'histoire des sciences. L'exemple de Lamarck", *Bulletin de la Société d'histoire et d'épistémologie des sciences de la vie*, 1 (1994), pp. 30–41.

[23] H. Ferrière, *Bory de Saint-Vincent (1778–1846): naturaliste, voyageur et militaire, entre Révolution et Monarchie de Juillet; essai biographique*, Thèse de doctorat, 2 vols., Université Paris 1, Panthéon-Sorbonne, 2001 and *Bory de Saint-Vincent: l'évolution d'un voyageur naturaliste*, Paris, Syllepse, 2009.



is no doubt that Dufour is referring to something people talked about within natural history circles.

Historians have often insisted on the atheistic tendency of Lamarck's tenets to account for his alleged isolation, and I have myself pointed out instances which show Lamarck's awareness of the risks he was facing. Though Lamarck has been credited to be one of the earliest proponents of the word "biologie" (yet by no means the first, as some historians love to repeat), the systematic analysis of the Lamarckian corpus reveal that the proud announcement of his new project, the establishment of biology, in January 1802, was followed only 6 months later by a stern disclaimer: his age, commitments and bad health would prevent him from carrying the project forward. In the Preface to the *Philosophie Zoologique*, the famous two-volume work he published in 1809 as textbook for the new Imperial University, Lamarck reassured readers that his biology was completely abandoned. Surprisingly, in the preface to the first volume of the *Histoire naturelle des animaux sans vertèbres* (1815) Lamarck informed his readers that he was working to establish a new discipline, for which not even a name existed, which he proposed to call "biology". In a study I published a few years ago, I reconstructed the political reasons that made Lamarck aware, in July 1802, that his project, and the word he chose, "biologie", could be associated to another word and project, "*idéologie*", then at the centre of intense political debate. As the ideologues wished to reform philosophy and dispense with metaphysics and religious tutelage, Lamarck wished to establish a science of life equally free from religious and philosophical preconceptions, and solidly grounded on a set of explicitly materialistic assumptions. In 1803 the ideologues were severely punished by General Bonaparte because of their republicanism, but also for their opposition to the partial re-establishment of the Catholic religion in the country, thanks to the Concordat of 1801. The second section of the Institut, devoted to the moral and political sciences, was closed down, and its members dispersed throughout the other sections. Many *idéologues* retained a certain measure of influence, mainly through their work within the field of education and legislation, but were kept at a safe distance from any form of political influence and power.[24]

A man too proud to court powerful patrons, or simply socially inept, as Cuvier suggested, Lamarck well knew that he was in a much weaker position than representatives of *idéologie*. A campaign against his tenets and philosophical propensities could have led to his dismissal, which would have meant total destitution for himself and his family. He therefore spared no words to reassure his readers and potential critics that he had learned the lesson, and would not persist in pursuing a line of research people saw as contrary to the sound principles of religion. In 1815, Lamarck believed, as many Frenchmen did, that the Chart the restored King Louis XVIII granted to the French people guaranteed full freedom of opinion and of expression. He thus took up again his biology project, though the language was now

[24] Pietro Corsi, "Biologie", in P. Corsi *et al.*, *Lamarck, Philosophe de la nature*, Paris, PUF, 2006, pp. 37–64.

more guarded and prudent. The last works he composed from 1816 through 1820 expanded upon his materialistic interpretation of psychological and intellectual phenomena, though here and there he paid lip service to the superior truths of Revelation, which were guiding and correcting research when the highest moral and metaphysical concerns were involved. During the 1820s, Lamarck's health deteriorated, his blindness became total after 1818, and he withdrew from public life and teaching. The *Institut* granted him the privilege of getting the token extra-compensation accorded to members who attended sessions: the old naturalist could not afford missing one.

The question to be asked, one I did not ask myself in my article of 2006 on the politics of *biologie*, is the following: can we generalize the situation Lamarck experienced, and conclude that the practice of natural history was subjected to close scrutiny if not censorship during the Consulate, the Empire and the Restoration? The answer is complex, and all the evidence at our disposal suggests great prudence before embarking upon hasty generalizations. That Lamarck was afraid for his job and livelihood does not mean that others were as well. Then, as now, the danger of defending a minority or a fringe position was inversely proportional to the social and political weight of the single individual going public. A few examples will suffice to clarify this point.

We have already referred to Delamétherie in connection with his appointment as assistant to Cuvier at the *Collège de France*. Delamétherie was known as a die-hard materialist. Though he often used a language compatible with main stream traditional eighteenth century deism, Delamétherie liked to entertain foreign visitors on the different shadows of contemporary French atheism.[25] In one particular instance, when accompanying a party of Englishmen on a private visit to the *Muséum national d'histoire naturelle*, he made fun of one of his guests, who, astounded by the beauty of the display of the richness of living nature, had made a comment on the clear indication the splendid collections offered of the existence of God: "He smiled and returned for answer, that I ought to recollect I was in an ecstasy", his guest Henry Redhead Yorke (1772–1812) recalled.[26] During the 1800s and the early 1810s, Delamétherie closely mirrored Lamarck's publications, taking up almost the same topics and issues, to the point that a visitor like the Italian geologist count Giuseppe Marzari Pencati (1779–1836) could mistake works by Lamarck for works published by Delamétherie, as Cuvier himself did (for different reasons, needless to say). The differences between the two authors were significant, especially as far as the doctrine of the transformation of life throughout the history of the Earth was concerned. Delamétherie believed in a primeval Ocean in which rocks

[25] On Delamétherie's atheism, see H.. D. de Blainville, "Notice historique sur la vie et les écrits de J.-C. Delamétherie", in *Journal de Physique*, 85 (817), pp. 78–107, p. 89.

[26] H. Redhead, *Letters from France in 1802*, vol. 1, p. 225. See also J. A. C. Sykes, ed., *France in Eighteen Hundred and Two Described in a Series of Contemporary Letters by Henry Redhead Yorke*, London, William Heinemann, 1906, p. 93. Sykes appeared to ignore the original work. His edition is marred by hilarious spellings of the names of the main actors of Parisian life in 1802.



and life were formed through countless ages thanks to processes of crystallization, and maintained that all life forms known to man developed from a restricted number of prototypes equally generated by specific forms of crystallization – all of which Lamarck firmly denied. Yet, there is no doubt that Delamétherie thought that Lamarck had copied some of his ideas, and wrote extensively on the action of habits in giving new shapes to organs – within the well defined limits of the original crystallization process that had established the prototypic form, one should hasten to add. On this subject his discussion was more detailed and sustained than Lamarck's. Delamétherie too was a civil servant, not very well off, having decided to repay the heavy debts his brother incurred at gambling. Yet, his political connections and his high European reputation as editor of the *Journal de physique* – a periodical historians are very unwise to ignore – bought him a degree of free expression, higher than the one Lamarck felt was allotted to him.

We have already mentioned the dark prophecy Léon Dufour addressed to his friend Bory de Saint-Vincent. The Catholic party would not forgive him for his materialist account of the history of the Earth and of life on it. It is interesting to note that Dufour too, as Marzari Pencati and Cuvier, appeared to believe that Lamarck endorsed the hypothesis of a primeval Ocean. What is more interesting from the point of view of our discussion is that no Catholic reviewer took up the challenge. As Hervé Ferrière has brilliantly shown, Catholic reviewers were not nice to Bory, and even accused him of having deserted the famous expedition to the South Seas led by Captain Nicolas-Thomas Baudin (1754–1803) – which was indeed true, since Bory (together with other naturalists of the expedition) jumped ship at the Île de France, today's Mauritius, and refused to continue the journey. Yet, no one mentioned the materialist and possibly atheistic tendency of the book. Favourable reviewers also avoided any comment on the incriminated chapter, though only 3 years earlier the debate on the Theory of the Earth had aroused generous comment, and Delamétherie was never tired of providing new evidence for his own geological and biological views in the pages of the *Journal de physique*. So, why, contrary to the prediction made by Dufour, and in spite of religious attacks against Lamarck and his doctrines, friends and foes alike avoided commenting on Bory's equally objectionable doctrines? We cannot know for sure and here again further research is required. Yet, the fact that Bory's uncle and surrogate father (the young naturalist was an orphan), Bernard Journu-Auber (1745–1815), was one of the richest men in the country, and a close collaborator of General Bonaparte as Regent of the Central Bank of France, may explain why no one dared to attack a naturalist placed under such a tutelage. Even the serious accusation of desertion came to nothing: indeed, Journu-Auber even proposed that his protégé should be promoted to the rank of Captain for the services rendered to the State during the expedition he deserted.

A final example, concerning an amateur naturalist well known in his time throughout Europe, and completely ignored by historians, will reinforce the point that during the Consulate and the Empire the danger of maintaining unsavoury philosophical or scientific tenets was inversely proportional to the social status of the proponent. Jean-Baptiste Fray-Fournier (1764–1835) was a surgeon and

amateur naturalist who lost the use of his right hand and opted for a lucrative career as a purveyor for the army (*Commissaire Ordonnateur des Guerres*) and organizer of military hospitals for the *Grande Armée*.[27] He travelled and worked through several German States, and resided for sometime in Berlin, Magdeburg and Ulm, from where he engaged in correspondence with leading German naturalists. Fray's passion, and in his mind his title to consideration, were his experiments on the spontaneous generation of organic molecules. He got Pierre Jean Georges Cabanis (1757–1808) interested in his work, and even performed at Arcueil, under the eyes of Claude Louis Berthollet (1748–1822) and his assistants. Fray, a believer and a Christian, was convinced that organic molecules were formed thanks to the action of solar rays, and they could combine to give birth to elementary forms of life. Once life started developing, everywhere in the world it would climb the ladder of complexity from monad to man. Though the process was the same everywhere on Earth, the end result (as well as the intermediate steps) was marked by the physico-chemical peculiarities of the locality were the process started. Thus, he explained, on the top of the Pyrenees one can find trees that are specific to the locality, since they developed from spontaneous generations made up of basic molecules typical of the physico-chemical constitution of those mountains. Though Fray did not doubt that the process had been providentially designed by an all-benevolent creator, his account of the origin and development of life on Earth bore here and there close resemblance to points Delamétherie, Bory and other materialists advocated. Fray's work attracted favourable attention in Germany. Top naturalists such as Friedrich Tiedemann (1781–1861), Johann Friedrich Meckel (1781–1833) and Wilhelm August Eberhard Lampadius (1772–1842) praised his work, and adapted it to their own views of the origin and development of life forms. In France, Antoine Desmoulins quoted with approval – without mentioning the name of Fray – the explanation for the peculiar flora and fauna showed by isolated geographical areas such as the picks of the Pyrenees, whereas during the 1820s Bory de Saint-Vincent and others denied that Fray had preceded them in elaborating the theory of the spontaneous generation of organic molecules. In the British Isles, Fray's work was well known to John Barclay (1758–1826), a famous teacher of anatomy at Edinburgh, who in 1822 devoted a long chapter of his *An Inquiry Into The Opinions, Ancient And Modern, Concerning Life And Organization* to a refutation of the Frenchman's work (whom he considered a better thinker than Erasmus Darwin), filled with quotations in French. Barclays even bothered to reproduce the paragraphs Cabanis devoted to the first experiments performed by Fray in the early 1800s.[28] I do not need to expand upon the fact that in France no one appears to

[27] Joëlle Jezierski, "De fleur et de sang. Parcours d'un herbier napoléonien", in *Machine à feu. Revue du livre et de la lecture en Limousin*, 25 (2007), pp.40–41

[28] Jean-Baptiste Fray, *Essai sur l'origine des corps organisés et inorganisés, et sur quelques phénomènes de physiologie animale et végétale*, Paris, Mme Ve Courcier, 1817. An earlier and shorter version of the work, *Nouvelles expériences extraites d'un manuscrit qui a pour titre: essai sur l'origine des substances organisées et inorganisées* had been published in Berlin, L. Quien, and Paris, chez Nicolle, 1807. J. Barclay, *An Inquiry Into The Opinions, Ancient And Modern, Concerning Life And Organization*, Edinburgh, Bell and Bradfute, 1822, pp. 126–142; for the quotations from Cabanis, see pp. 127–128.



have challenged Fray's tenets or asked him to distance himself from current materialist and atheist explanations for the history of life on earth. His trusted role as a pillar of the medical services of the Grande Armée acted as a very effective shield.

What about the Restoration? After an initial tolerant attitude towards the freedom of the press and of opinion, successive ultra-monarchic administrations introduced increasingly restrictive measures. Censorship was exercised with the utmost severity on theatre productions, or on the teaching of history in schools preparing students for the *Agrégation*.[29] In 1825 an anti-blasphemy legislation was passed, so extreme that even a famous writer and right wing politician such as François-René de Chateaubriand (1768–1848) felt that Chares X had done what no King of France had ever dreamt of. One might expect scientific doctrines openly or implicitly favouring atheism and materialism to be repressed, denounced or at least censored, Lamarck's *in primis*. Systematic perusal of periodicals, encyclopaedias, dictionaries and single works published during the Restoration reveal that this was not the case. Conservative writers, including Cuvier, denounced as leaning towards pantheism, if not atheism, Lamarckian transformism, the doctrine of the unity of composition prevailing – according to Geoffroy Saint-Hilaire and his allies – throughout the animal and vegetable kingdoms, or the theory of embryonic recapitulation, according to which the phases of development of the embryo summed up the major steps in the development of life throughout the history of the Earth. Cuvier's or Virey's strictures only added to the success of "dangerous" doctrines with the reading public. From the pages of the internationally successful *Dictionnaire classique d'histoire naturelle* (which travelled with Darwin on the *Beagle*) he edited from 1822 to 1832, Bory de Saint-Vincent blasted against the Jesuits and the theory of the immortality of the soul, as well as against Cuvier's conservatism, and extolled the virtues of Lamarck's dedication to scientific truth and his colleague's transformist doctrines. Bory vigorously campaigned in favour of polygenism, and his many works provided readers in Europe and the United States with accurate summaries of the various standpoints debated by French naturalists. He avoided mentioning, needless to say, that Lamarck believed in the unity of the human species.

During the 1820s, the golden decade for Lamarck's reputation in France and Europe, his doctrines were subjected to a variety of criticisms and only a few commentators insisted on the dangerous leaning of his teaching and theorizing. From Edinburgh to Göttingen, from Turin to Paris, it was common to pay homage to the old naturalist, who had left a monument of taxonomic achievement such as the *Histoire naturelle des animaux sans vertèbres* (7 vols., 1815–1822). The fact

[29] V. Granata, *Politica del teatro e teatro della politica: censura, partiti e opinione pubblica a Parigi nel primo Ottocento*, Milano, Unicopli, 2008.

that Lamarck had become blind in 1818, and was now too sick to take part in public and scientific life, only added to the respect surrounding him during his last years. It is therefore clear that the assumption that Lamarck was isolated because of the religious and philosophical implications and consequences of his transformist doctrines has no foundation, and can be maintained only by ignoring the actual state of affairs in contemporary French and European scientific, political and cultural life.

## 3 Authors and Audiences

Historians of science have traditionally shown a remarkable reluctance to accept the simple fact that at every given moment, the production of knowledge in a given society is as varied and diversified as the social, political or the religious scene. Since some individuals or groups of individuals appear to share our concept of science, or to approach it the most, they are taken as the only ones worth spending one's time on. In doing so, a host of very interesting phenomena and events are completely ignored, even the ones that should appeal to the historians of the development of scientific "truth". The case of Cuvier and his collaborators we mentioned above, or the composite nature of the natural history scene during the decades in which Lamarck acted his own life, deserve comment. I will deal in particular with selected features of debates within the medical profession; the latter's changing political and intellectual allegiances; and the editorial ventures that represented the viewpoint of prominent factions within it. This will open up new and fascinating perspectives on the relationship between France and Germany during the first three decades of the nineteenth century, and on how Lamarck was read by different audiences.

I hinted above that Cuvier was hardly in a position to dominate the natural history scene of his time, and experienced some difficulty even with his own collaborators, who depended on his good will more than anyone else. I will limit my comments to the two young authors who put Cuvier's lectures on comparative anatomy in good form and published it. As is well known, André Marie Constant Duméril (1774–1860) took care of volumes 1 and 2 of the *Leçons d'anatomie comparée* (5 vols, 1800–1805), whereas his colleague Georges Louis Duvernoy (1777–1855) edited vols. 3–5. Duméril was undoubtedly the more independent of the two. A brilliant and precocious anatomist and medical lecturer, he arrived in Paris in 1800, and was immediately recruited by Cuvier to help him in all sorts of tasks, including the editing of his lectures. Duméril also engaged in teaching in the school of medicine of the capital that had taken the place of the abolished Medical Faculty. Duvernoy was coming from Montbéliard, Cuvier's home town, and was a distant relative of the naturalist. He was not as brilliant as Duméril, and his career was less conspicuous. Yet, even Duvernoy played a role we still know little about, and an important one, for that matter. As I pointed out years ago, his views of life



before entering Cuvier's service were not as Cuvierian as one might assume. He saw an important role the medical philosopher could play when reflecting on the properties of living beings. He appeared to see life in terms of a fluid dynamics not dissimilar to the one Lamarck endorsed, though he rejected Delamétherie's and Lamarck's materialism. He also expanded upon the role of use and lack of use in shaping organs.[30]

We pointed out at the beginning of this article that the gathering of important collections in Paris forced naturalists from all over Europe to spend time in the French capital in order to take advantage of the unprecedented wealth of specimens. A young ambitious German anatomist, Johann Friedrich Meckel (1781–1833), spent the years 1804–1805 working in Cuvier's laboratory. Back to Germany, Meckel translated the *Leçons d'anatomie comparée* before embarking on his own ambitious editorial projects. The role of Meckel in early nineteenth century German comparative anatomy and embryology is now well known, thanks to important studies published during the last 20 years.[31] Yet, it is always assumed that during his stay in Paris the young anatomist worked with Cuvier. Now, this is again an easy and reasonable assumption, albeit one that it is difficult to prove. During the very early 1800s Cuvier was dividing his time between extensive travel through the French provinces, as Inspector General of higher education, and his growing political commitments. What we know for sure is that Meckel embarked upon the careful dissection of a limited number of human foetuses under the direct supervision, and collaboration, of Duvernoy.[32] If we follow Duvernoy's highly interesting narrative of events, published in 1849, we learn that Meckel worked on nine foetuses (a rare specimen to obtain even in Germany, though less so in Paris), and called upon Duvernoy to attest the reliability of his observations. Back home, in 1806 (in fact, already in 1805)

[30] G.-L. Duvernoy, "Réflexions sur les corps organises et les sciences dont ils sont l'objet", in *Magasin encyclopédique*, 5th year, vol. 3 (1799), pp. 459–474. See Corsi, *The Age of Lamarck*, pp. 75–76.

[31] S. Gliboff, *H.G. Bronn, Ernst Haeckel, and the Origins of German Darwinism: A Study in Translation and Transformation*, Cambridge, MA, MIT Press, 2008. P. Hunemann, ed., *Kant and the Philosophy of Biology*, Rochester N.Y., University of Rochester Press, 2007. R. Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe*, Chicago, University of Chicago Press, 2002. T. Lenoir, "Kant, Blumenbach, and Vital Materialism in German Biology", in Isis, 71 (1980), pp. 77–108, and *The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology*, Dordrecht, Reidel, 1982. T. Bach, *Biologie und Philosophie bei C. F. Kielmeyer und F. W. J. Schelling*, Stuttgart, Bad-Cannstatt, 2001. S. Schmitt, *Les forces vitales et leur distribution dans la nature: un essai de "systématique physiologique". Textes de Kielmeyer, Link et Oken traduits et commentés*, Paris, Brepols, 2007.

[32] G.-L. Duvernoy, "Ovologie", in *Dictionnaire universel d'histoire naturelle*, 9 (1849), pp. 281–353.

Meckel published in German an essay, *Fragments sur l'histoire du développement du foetus humain* (we are of course following Duvernoy, who gave titles and texts in French), which ended with the conclusion:

> Je suis loin de regarder comme une idée simplement ingénieuse, celle de Kielmeyer, qui pense que le foetus humain passe par les divers degrés de développement auxquels s'arrêtent les animaux inferieurs. Un trop grand nombre de faits viennent le confirmer.[33]

One can therefore surmise that Meckel and Duvernoy discussed several issues in comparative anatomy and embryology, and it is possible that Meckel told Duvernoy of the many new ideas Carl Friedrich Kielmeyer (1765–1844), whom he had met and listened to, was expanding upon in his lectures and manuscripts. Of course, Cuvier too knew rather well what Kielmeyer was speculating upon. Yet, Duvernoy, who in his contributions to the *Dictionnaire universel* took pains even to show that Geoffroy Saint-Hilaire's transcendental anatomy was in the last analysis due to Cuvier, is completely silent on the role his master played in directing Meckel's work. Indeed, Cuvier does not appear to have played *any* role in Meckel's research. As hinted above, the young German anatomist asked Duvernoy to witness his anatomies: he would not have missed the chance to say that the mentor's role had been fulfilled by the already famous Cuvier, who by the way never paid much attention to embryology and deeply opposed the doctrine of embryologic recapitulation. It is possible that Antoine Étienne Renaud Augustin Serres (1786–1868), the French main proponent of embryologic recapitulation, discussed recapitulation with Meckel, whom he met in 1805. Serres was a consumer of German works and became a keen reader and admirer of Lorenz Oken (1779–1851). The point which is important to stress is that Meckel became proficient in the French language and forged close personal links with several French colleagues, especially among the young aspiring naturalists and medical researchers courting notoriety through their anatomical research. Further research is required to reach a better understanding of the workings of Cuvier's laboratory – and the laboratories of other Professors of the Museum – and the modalities of exchange with foreign visitors and researchers.

Duméril was apparently more successful than Duvernoy in attracting young talent, though what follows only constitute the result of a first survey in need of more sustained research. A precocious anatomist and medical teacher at 19, Duméril, as we have already pointed out, reached Paris at the beginning of 1800. He was quickly noted by Cuvier, who associated him to his *Leçons d'anatomie comparée*, asked him to teach his class at the *École Centrale du Panthéon* – one of the many jobs Cuvier was starting to hand out to faithful pupils – and in 1804 even

[33] Duvernoy, "Ovologie", p. 348. J. F. Meckel, "Fragmente aus der Entwicklungsgeschichte des menschlichen fœtus", in *Abhandlungen aus der menschlichen und vergleichenden Anatomie und Physiologie*, Halle, Hemmerde und Schwetschke, 1805, pp. 277–381, now available at Edition Classic VDM Verlag Dr. Muller, 2007. See L. Göbbel and R. Schultka, "Meckel the Younger and his Epistemology of Organic Form: Morphology in the pre-Gegenbaurian Age", in *Theory in Biosciences*, 122 (2003), pp. 127–141.



passed on to him the task of writing a textbook on natural history he had no time to engage in, commissioned by the Government for the *Lycées*. Duméril showed himself a reliable follower. In the textbook, he never mentioned Lamarck as an expert on invertebrates, but only Cuvier. All innovation in the classification of invertebrates was due to Cuvier's anatomical research – which was to a great extent true- and Lamarck had played no role in reforming this important branch of zoology. Thus, no mention was made of the *Système des animaux sans vertèbres* Lamarck had published in 1801, and his name appeared briefly in the text in the sections devoted to botany.[34]

Yet, if one lists the pupils Duméril himself took under his wings, and one considers what he himself published, one can see that he kept a good measure of independence from his master. In the very early 1800s he taught a young doctor, Jean Burdin, author of a *Cours d'études médicales* (1803) which contained the first published hint that the cranium could be seen as formed of expanded vertebrae.[35] The three volumes work was translated into English and German within the year, even though we have no idea of who paid for this – possibly Henri de Saint-Simon (1760–1825), then still quite wealthy, and apparently close to Burdin. Early in 1808, Duméril himself proposed a vertebral theory of the cranium to the Institut, but was laughed down when someone in the audience uttered the ironic comment "here is the thinking vertebra".[36] He also endorsed the theory of the unity of plan prevailing throughout the animal kingdom, about which Cuvier was expressing growing reservations. Another pupil of Duméril was the irascible, highly original Antoine Desmoulins, who during the 1820s became a fierce and relentless opponent of Cuvier. It must be said that Desmoulins managed to quarrel with everybody in Paris, and even succeeded in making Geoffroy Saint-Hilaire defend Cuvier in print, so outrageous were his attacks against the famous naturalist.

The last pupil Duméril coached and protected of whom I am aware is Antoine Jacques Louis Jourdan (1788–1848), an author deserving close attention. After studying medicine and opting at first for the lower qualification of surgeon, Jourdan left France to join the French army in Germany. He was mainly based in Königsberg and Berlin, and remained in the German states from 1808 until 1814 (with the exception of short stays in Paris, when training at the military hospital of *Val de Grâce*). We have no idea of whether he had met Meckel in 1804–1805, when he was already a pupil of Duméril. What is certain is that he forged personal links

[34] C. Duméril, *Traité élémentaire d'histoire naturelle*, Paris, Crapelet, 1804, 2nd ed., 2 vols. Paris, Déterville, 1807. It is to be pointed out that in 1803 Duméril was appointed assistant to Lacépède, who had taken up heavy political and administrative duties.

[35] C. G. Carus, *Traite élémentaire d'anatomie comparée, suivi de recherches d'anatomie philosophique ou transcendante [...] traduit de l'Allemand par A.-J.-L. Jourdan*, 3 vols., Paris et Londres, J.-B. Baillière, 1835. See pp. 4–5 for comments on Burdin, and on the contemporary development in France and Germany of ideas about the vertebrae composing the cranium. Carus also pointed out that Italy and England had contributed precious little to the "philosophical" developments in anatomy.

[36] See Corsi, *The Age of Lamarck*, pp. 237–238.

with many German anatomists and medical researchers, Meckel included, and started a systematic and gigantic project of translating German works into French. In his long career, he signed more than 70 major translations, including the works of Christian Friedrich Samuel Hahnemann (1755–1843), the founder of Homeopathy, a medical doctrine Jourdan endorsed and introduced to France.

From the point of view of Cuvier, Jourdan showed a remarkable propensity to translate all the authors the great anatomist deeply disliked, Karl Friedrich Burdach (1776–1847) Gottfried Reinhold Treviranus (1776–1837) or Carl Gustav Carus (1789–1869) among others. Jourdan became a very active collaborator to the *Dictionnaire des sciences médicales* (60 vols. 1812–1822) a remarkable achievement of the French publishing industry. In 1818 he also became one of the chief authors and probably one of the editors of the *Journal complémentaire du Dictionnaire des sciences médicales*, a periodical that, as the title suggests, was designed to update the entries already published in the dictionary, as well as to provide welcome summaries of medical progress throughout Europe. It is significant that the editorial statement printed in issue n. 1. (signed by the publishers, but bearing clear marks of Jourdan's style) listed 30 medical periodicals published in German, 6 in Italian, 5 in English and 1 in Dutch. Last but not least, Jourdan and the *Journal complémentaire* paid particular attention to Meckel and his innovative, systematic and truly impressive attempt to unite the study of comparative embryology and the study of comparative anatomy.

No polemical hint was made – at least until the mid-1820s – but medical people well understood the meaning of the translation campaign from German and the policy pursued by the very successful *Journal complémentaire* and other medical publications The fact is that several representatives of the medical profession did not like Cuvier's attempt to establish his own superiority in comparative anatomy and human anatomy. Many openly extolled the work of Marie François Xavier Bichat (1771–1802), the true reformer of French anatomical studies: no need to add that, in their view, Cuvier was not. During the late 1810s, Geoffroy Saint-Hilaire early incursion in and statements on transcendental anatomy, supported by eminent representatives of the medical profession, such as Serres and members of the *Société d'anatomie*, as well as Geoffroy's work on teratology, were perceived as constituting a satisfactory vindication of the autonomy of medical research with respect to research conducted within natural history disciplines. An alliance could be forged with Geoffroy, because of his strong links with the medical profession, and his growing confrontation with Cuvier. Moreover, starting with the early-1820s, severe repressive measure against leading members of the medical community, including the then old and extremely respected Philippe Pinel (1745–1826), threw leaders of the profession and ranks and file doctors into determined opposition to the Government. Medical journals and encyclopaedias, pamphlets and reports of meetings of medical organizations joined naturalists such as Geoffroy and his son Isidore or Bory de Saint Vincent in deprecating the poor state of natural sciences in France, mirroring the poor state of political freedoms.

What about Lamarck in all this? And what about the larger picture of Franco-German scientific relationships during the early decades of the nineteenth century?



Jourdan expressed his admiration for Lamarck in a review of the *Histoire naturelle des animaux sans vertèbres* and in a long entry, "Germe", devoted to the question of epigenesis and preformation, published in volume 18 of the *Dictionnaire des sciences médicales*.[37] Contrary to his colleague Virey, who also wrote extensively for the dictionary and kept insisting that Lamarck's doctrines lead to atheism, Jourdan summed up Lamarck's main tenets with fairness and admiration, though he felt free to disagree on several key points. Jourdan, who had attended Lamarck's lectures in 1806, was reading his teacher's work with German eyes, so to speak. Thus, for instance, he argued that Lamarck's insistence on the role of organic fluids and on fluid dynamics in explaining biological phenomena was very similar to the doctrines put forward by Johann Christian Reil (1759–1813), the founder of the famous *Archiv für die Physiologie* (1796–1815).[38]

Jourdan's account of the doctrine of spontaneous generation proposed by his former teacher Lamarck correctly included reference to the different origins of the plant and animal kingdoms, and possibly of several branches of main types of animal organization (p. 161). His interpretation of the progress of organization made no reference to the "tendency" of life to develop more complex anatomical and functional structures. Jourdan rightly stressed that, according to Lamarck, from time to time the dynamic interaction between the organism and the environment required a change in the distribution pattern of fluids and nutrients, thereby favouring those parts that were solicited by a change in the habits of animals. In its turn, the change of habits was the consequence of a change in the physical or biological environment. More diversified and complex distribution channels for the fluids also implied a more specialized, and in the end more rapid circulation. The nervous system in humans constituted the best example of extremely fast movements of fluids inside a very sophisticated network of nervous fibres (pp. 167–180). Thus, the only "progressive" change in living bodies was constituted by the inevitable increase of the speed at which fluids moved within organisms, due to the specialization and refinement of conveying networks and ultimately of the fluids themselves. This did not, however, determine the way in which the organism was going to develop, since the creation of new organs depended on the actual circumstances in which animals found themselves and the actual challenges they had to face (pp. 163–164). Jourdan was not a convert, however. He expressed his conviction that the species barrier could not be overcome. Advanced experiments in domestication showed that even though much could be achieved by breeders, never a zebra had become a horse, he ironically concluded.

[37] A.-J.-L. Jourdan, "Histoire naturelle des animaux sans vertèbres", in *Journal universel des sciences médicales*, 2 (1816), pp. 145–181, and "Germe", *Dictionnaire des sciences médicales*, 18 (1817), pp. 226–277.

[38] Jourdan devoted a long article to the career and doctrines of Reil, again pointing out the similarities with Lamarck, "Littérature médicale allemande. Sur la connaissances et le traitement des fièvres, par Jean-Chrétien Reil", in *Journal universel des sciences médicales*, 2 (1816), pp. 217–239.

The entry in the medical dictionary, published a few months after the review we have alluded to, adds interesting dimensions to Jourdan's attitude towards Lamarck. As in his review of the *Histoire naturelle*, the tone was respectful and sympathetic. Lamarck was to Jourdan one of the chief supporters of epigenesis, one who had convincingly shown the weakness of the preformist doctrine. Jourdan summarized with favour the theories that saw embryos and individual adults develop thanks to the addition of parts made possible by nutrition or produced thanks to environmental chemical and physical agents. Jourdan stressed that both preformism and epigenesis impinged upon our conception of the origin and the development of animal and plant life on Earth. Whereas preformists tended to embrace a strong or a weak version of creationism (all germs of all animals were created at the beginning of time, or successive creations of new germs occurred to fill gaps and losses), the followers of epigenesis were open to the idea that organisms were formed in succession through endless ages and endlessly changing environments. Lamarck, according to Jourdan, was the most coherent representative of the latter view.[39]

Once again, Jourdan was not a convert. His grasp of the niceties and complexities of the Lamarckian theoretical corpus is often well above what historians and other commentators have customarily said of Lamarck. Yet, he was not convinced that use and lack of use, and more generally the dynamic interaction between organisms and their environment was sufficient to explain macro-evolution. The Lamarckian mechanism accounted for the fixation of varieties into good species, perhaps for the production of new genera, but could not explain the emergence of new and more complex anatomical and functional structures. For this, one had to call upon the still unknown laws of development, at the level of the embryo, as well as at the level of the history of life on earth. In later years, Jourdan agreed with Meckel, Tiedemann, and his colleague Serres, that the development of the embryo showed the steps life had to climb in order to produce increasingly perfect beings. Once life was created, indeed, as Lamarck had pointed out, once several forms of spontaneous generation had appeared, each endowed with its own specific structural properties, each form could climb only according to the potential for growth its structure allowed. The laws of development were the key factor, both in ontogeny and in phylogeny. Lamarckian mechanisms only explained how birds adapted to all the environments they were found in, not how the anatomical and functional type "bird" had originated, or, better, had *developed* from less complex vertebrates.

Jourdan commented on Lamarck's ideas on several occasions, and always with the utmost respect. To him, as well as to Bory or Geoffroy, Lamarck deserved the full respect of the scientific community. He had dared to be wrong, whereas Cuvier simply censored other people's ideas. And he had left a scientific legacy which Cuvier's compilations could never match. There is of course no space to analyse the attitudes of other members of the medical community towards Lamarck – authors

[39] A.-J.-L. Jourdan, "Germe", *Dictionnaire des sciences médicales*, 18 (1817), pp. 226–277.



such as Nicolas Philibert Adelon (1782–1862), for instance, who wrote one of the rare reviews of Lamarck's last work, the *Système analytique des connaissances positives de l'homme* (1820), and kept referring to Lamarck in every successive edition of his successful medical textbook.[40]

It is by now abundantly clear that the restriction of the analysis of the reception of Lamarck's doctrines to what Cuvier or a handful of naturalists had to say seriously distorts our understanding of debates on the life sciences during the early decades of the nineteenth century. Not only the vast majority of practitioners of natural history and authors of natural history publications have been denied any hearing, but the vociferous and politically very active population of medical writers and practitioners has been completely ignored. As far as France is concerned, it is not uninteresting to mention that among the most prolific medical authors of the late 1810s and the 1820s a good number were former medical officers of the Napoleonic armies, left without pay, as all former army senior staff had been, by a vindictive decree of the Restoration Government. Many wrote as much as possible simply to implement their income. Their economic needs, their need for recognition and their wounded pride contributed to create an explosive mixture that added to the mounting tension leading to the July 1830 revolution. It is not by chance that after July 1830 many medical radicals of the 1820s returned to their professional occupations, lowered their tone and relented their assaults against "official" science. Many journals, such as the *Journal complémentaire*, even abandoned their campaign in favour of German medicine and anatomy. It was time to stop imitating foreigners. True, other journals did take up the fight, and dictionaries of the 1830s, 1840s and 1850s kept discussing transcendental anatomy or Lamarckian doctrines. Simply, those discussions had lost the political pregnancy and urgency they had taken on during the 1820s.

One final comment is called for. The question of the relationship between French medical authors and their German colleagues cannot be looked at as a parochial anecdote or a minor episode within the larger picture of European debates on life of the early nineteenth century. Several historians have recently argued for the importance of German anatomical, embryological and medical doctrines for the debates on the life sciences that marked the period 1820–1850 in Scotland and England, for instance, and for the formation of Charles Darwin's view of nature and of life.[41] It is perhaps useful to recollect that Jourdan's translations of Meckel, Tiedemann, Carus, Burdach, Treviranus were in fact destined to the British book trade as well

[40] N. P. Adelon, "Système analytique", in *Revue encyclopédique*, 9 (1821), pp. 257–267, and *Physiologie de l'homme*, 4 vols., Paris, Compère jeune, 1823–1824, see vol. 4, pp. 3–4, 103–104, 114–115, 232, for favorable summaries of Lamarck's ideas. Adelon was one of the editors of the *Dictionnaire des sciences médicales*.

[41] See R. Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe*, Chicago, University of Chicago Press, 2002, for an authoritative presentation of this argument and the relevant bibliography. See also Philip F. Rehbock, *The philosophical naturalists: themes in early nineteenth-century British biology*, Madison, University of Wisconsin Press, 1983.

as to the French one. It was the entrepreneurial genius of Jean-Baptiste Marie Baillière (1797–1885) that saw the opportunity for profit to be gained in England, and from his shop in Regent Street the French translations of German works were sold to private and public medical libraries. More than that: the English language edition of Tiedemann's seminal work *Anatomie du cerveau: contenant l'histoire de son développement dans le foetus* was undertaken from the French translation, not from the German original.[42] The introduction by Jourdan, as well as his annotations to the text, was probably considered a kind of added value, though one cannot exclude that during the 1820s and the 1830s it was easier to find in London a translator from the French language rather than from the German one. Meckel's works circulated in England in the French editions, rarely in the original German. Even famous anatomists who knew German, such as the polyglot Robert Edmund Grant (1793–1874), or Robert Knox (1791–1861), owned Meckel in the French edition, not the German one. An English language edition of the handbook of anatomy by Meckel was nevertheless published in the United States from the French edition edited by Jourdan, though the translator informed his readers that he had consulted a German speaking medical man, in order to correct a few mistakes and inaccuracies present in the French edition.[43]

The set of easy assumptions concerning the place and reputation of Lamarck within the French natural history community of the early decades of the nineteenth century has traditionally acted as true *Idola tribus*, preventing research and limiting in considerable ways our understanding of the complex intellectual, social and political dynamics of contemporary natural history practices and publishing. The almost total lack of interest for the state of affairs in the publishing industry of the period under consideration, and the total lack of interest for what books, dictionaries, encyclopaedias actually said, has made us blind to major debates of great significance for the history of the life sciences at European level during the early decades of the nineteenth century. The reconstruction of the ways in which Lamarck was read, admired, criticized or denounced cannot be undertaken without reconstructing the actual articulations of the contemporary natural history and medical scene, in all its institutional, social and political dimensions.

[42] The French translation of Tiedemann appeared in 1823; the English language edition, *The anatomy of the fœtal brain: with a comparative exposition of its structure in animals [...] Translated from the French of A. J. L. Jourdan, by William Bennett, M. D.*, appeared in Edinburgh in 1826, J. Carfrae and Son.

[43] *Manual of general, descriptive, and pathological anatomy, by J. F. Meckel [...] Translated from the German into French, with additions and notes, by A. J.L. Jourdan and G. Breschet. Translated from the French, with notes, by A. Sidney Doane*, 3 vols., Philadelphia, Carey & Lea, 1832. Doane, a graduate from Harvard University, had studied in Paris during 1830–1832. He translated several French medical textbooks and specialized monographs into English.



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