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Biology and medicine in France and Russia

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Biologie et médecine en France et en Russie / Biology and medicine in France and Russia

Histoires croisées (fin XVIII^{e}-XX^{e} siècle) /
Crossed histories (end of 18th-20th century)

Sous la direction de / Edited by
Jean-Claude Dupont, Jean-Gaël Barbara,
Eduard Kolchinsky, Marina Loskutova

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Introduction

Si la science est une aventure internationale, elle est aussi géographiquement située, travaillée en des lieux déterminés. Le corpus qu'elle constitue, universel par destination, s'élabore au commencement dans des contextes spécifiques. Comprendre ces paradoxes du développement des pensées et des pratiques scientifiques suppose d'entrer dans la structure fine des spécificités géographiques et d'augmenter le grain des analyses dans cette direction.

Ce volume, consacré à deux pays situés aux extrémités du continent européen, la France et la Russie, se veut une contribution au « tournant géographique » (*geographical turn*) des études sur la science[1]. Depuis le XVIIIe siècle, les guerres, les diplomaties, les voyages scientifiques et les voyages d'études relièrent ces deux aires éloignées. Ces relations prirent alors différentes formes. Elles résultèrent d'abord de contacts individuels, issus d'une communauté d'intérêts scientifiques. Les déplacements des individus à l'intérieur de l'espace européen, malgré les vicissitudes historiques, créèrent des réseaux scientifiques et des relations d'amitié. Dans le domaine des sciences de la vie, les relations franco-russes furent peu formalisées, et à quelques exceptions notables près, les initiatives personnelles furent la règle. Des deux côtés, certains acteurs de la vie intellectuelle firent montre d'un véritable engagement personnel pour favoriser ces échanges ; francophilie et russophilie n'étaient pas de vains mots. Parfois même se nouèrent des relations familiales. Certains programmes de recherche communs entre les institutions scientifiques des deux pays pouvaient résulter de ces initiatives individuelles. Enfin, certaines relations scientifiques pouvaient être consécutives à une volonté politique et diplomatique à un plus haut niveau.

Sous ces formes variées, un moment ralenti par la Révolution de 1917 et surtout la période stalinienne, ces relations n'ont jamais réellement cessé. Contrairement à une idée reçue, même pendant la Guerre

1. D. A. Finnegan, « The spatial turn : geographical approaches in the history of science », *Journal of the History of Biology*, 2008, p. 369-388. Ce « tournant spatial » ou « tournant géographique » est présent dans les sciences humaines françaises dès les années 1990, avec Gilles Deleuze, ou Marcel Gauchet qui utilisa le premier l'expression française de « tournant géographique ». Voir M. Gauchet, « Introduction au dossier "Nouvelles géographies" », *Le Débat*, 92, 1996, p. 42.

froide, les échanges furent presque ininterrompus. En témoigne par exemple la vitalité des traductions des œuvres scientifiques en français et en russe au XIX[e] siècle et durant l'époque soviétique. Naturellement, les sciences de la nature, comme les sciences humaines, constituent un univers mental, et les enjeux épistémologiques de la traduction coexistent avec ses enjeux idéologiques et politiques, tout comme aujourd'hui ceux de l'usage d'un anglais rudimentaire, par là universellement compréhensible.

La mise en parallèle des cas français et russes ne doit pas conduire à isoler arbitrairement une relation binaire d'un faisceau de relations constitutives beaucoup plus complexe. Marchands, diplomates, militaires, intellectuels de diverses nationalités européennes sont présents en Russie avant Pierre le Grand. Au XIX[e] siècle, au moment où les échanges scientifiques s'accroissent, les étudiants et les voyageurs des deux pays devaient passer par l'Allemagne. Il est clair que la construction de la science de cette région de l'Europe résulte d'au moins trois aires culturelles imbriquées, celles de la France, de l'Allemagne et de la Russie, sans compter les implications des autres sciences nationales. Il serait d'ailleurs faux de réduire la situation de la science russe de l'époque à un dialogue singulier avec la science allemande, sous prétexte de proximité géographique. Jouant de la rivalité franco-allemande, les Russes voient parfois dans la science française un modèle intellectuel alternatif. Cette nécessité de pousser l'analyse au-delà de relations bilatérales se justifie encore au XX[e] siècle : la situation de la science soviétique et celle des « pays de l'Est » ne peut se résumer à une situation de confrontation réciproque avec la science des « pays occidentaux », bloc contre bloc. Contre la mythologie des isolements nationaux et l'image forte du « rideau de fer », il est désormais nécessaire de mettre en avant des schèmes dynamiques d'hybridations, les résistances causées par les échanges, les réceptions et influences mutuelles, les phénomènes de rejets et d'appropriation et la création d'idées neuves dans un isolement relatif permettant de nouvelles hybridations ultérieures. Les relations peuvent d'ailleurs être aussi considérées dans leurs échecs à un moment donné, ce qui peut leur conférer également un sens.

Chacune des contributions russes et françaises du présent ouvrage constitue une étude de cas dont l'ensemble rend compte de cette situation diversifiée. Il ne s'agit pas, en ce qui concerne le cas franco-russe, d'user d'une démarche comparatiste, à la recherche de similitudes et de différences qui figeraient chaque aire culturelle dans son identité, mais plutôt de rendre compte de croisements, d'imbrications constitutives complexes. Les cultures scientifiques ont certes une spécificité nationale, mais les idées circulent et la notion forte de transfert culturel, spécialement développée dans le domaine franco-germanique des sciences humaines, garde ici toute sa pertinence. Il ne s'agit pas non plus de revendiquer l'idée d'une essence européenne de la science moderne au travers de ses origines, discutable à l'époque de son intégration au niveau mondial. Il s'agit de souligner, à travers le cas franco-russe, la diversité, l'importance et les caractéristiques multilatérales d'interactions à l'intérieur d'un espace géographique et historique commun, celui de l'Europe des sciences.

Les interactions franco-russes, particulièrement importantes et polymorphes, restent peu étudiées dans le domaine des sciences du vivant. Deux sections ont été proposées, respectant leurs deux sensibilités traditionnelles, entre histoire naturelle et médecine, et suivant pour chacune d'entre elles une progression chronologique. Beaucoup de ces histoires croisées mettent en avant les acteurs français ou russes.

Ainsi la contribution de Stéphane Schmitt se concentre sur l'œuvre de Ludwig Heinrich Bojanus, et celle de Jean-Claude Dupont, sur Caspar Friedrich Wolff et Louis Tredern. Toutes deux rappellent l'importance de l'espace baltique au tournant des XVIII^{e} et XIX^{e} siècles, en tant que lieu d'échange pour l'anatomie comparée et l'embryologie. Eduard Kolchinsky traite de l'impact des idées de Georges Cuvier et de Jean-Baptiste Lamarck sur le développement de la théorie de l'évolution en Russie. Alors que les travaux de Cuvier constitueront le fondement de la paléontologie russe, ceux de Lamarck ne seront connus que tardivement en Russie, après la publication des œuvres de Darwin. Les formes particulières des théories transformistes en Russie sont analysées dans la perspective de cet héritage, jusqu'à l'époque de Lyssenko. Igor Popov, quant à lui, décrit l'élaboration de la recherche paléontologique de Vladimir Kovalevsky dans les années 1870 et le développement de ses concepts par Louis Dollo. L'affinité téléologique de cette paléontologie néodarwinienne est discutée. Dans le domaine de la botanique, de la physiologie végétale à l'écologie, Anatoli Polevoi traite de la coopération scientifique de Vladimir Lyubimenko et des chercheurs français tel Gaston Bonnier. Oleg Belozerov rappelle un fondateur oublié de la biologie du développement soviétique, Mikhail Zavadovskii, et compare la perspective de son œuvre avec certaines orientations en ce domaine issues de l'Europe occidentale. Certaines contributions traitent de certains lieux privilégiés de rencontre, telle celle de Tatyana Ulyankina, qui concerne la station zoologique

A. Korotneff à Villefranche-sur-Mer et les curieuses vicissitudes de cette institution russe sur le sol français. Mikhael Konashev décrit les convergences et divergences sur l'évolution humaine entre Theodosius Dobzhansky et Pierre Teilhard de Chardin, suite à la publication par ce dernier du *Phénomène humain*. Étienne Aucouturier et Daria Popova évaluent l'origine et la portée de la pensée de Vladimir Vernadski. Ils situent sa biogéochimie dans la tradition du cosmisme russe et la discutent comme source possible d'inspiration pour l'ethnologue Jean Malaurie, fondateur de l'Académie polaire de Saint-Pétersbourg. Stéphane Tirard analyse la réception des travaux sur l'origine de la vie d'Alexandre Oparine, en suivant le cas de l'édition française de son ouvrage *L'origine de la vie sur terre* édité en 1964 par le biologiste français Pierre Gavaudan, et les réactions consécutives de biologistes français tels que Jacques Monod.

Inaugurant la partie « Physiologie et médecine » de l'ouvrage, Vladimir Samoilov rappelle les origines triangulaires de la physiologie au long du XIX^e^ siècle, entre France, Allemagne et Russie, et les relations fécondes entre les chercheurs des trois pays à cette époque. Nikolai Pirogov incarne typiquement les interactions européennes constitutives de la médecine au XIX^e^ siècle. Céline Cherici analyse son rôle fondamental dans l'histoire de l'anesthésie et dans l'histoire de la chirurgie de guerre en Europe. Natalia Fedounina rappelle à travers le destin particulier de Michel Kourilsky l'importante immigration en France des étudiants russes au tournant des XIX^e^ et XX^e^ siècles, à la fois le produit et le résultat des relations médicales franco-russes. L'organisation sensori-motrice est un champ privilégié du domaine de la physiologie expérimentale en France et en Russie. Jean Massion et François Clarac analysent et comparent de façon approfondie les vues d'Alfred Vulpian et d'Ivan Sechenov sur les mouvements volontaires et involontaires. Toujours en neurophysiologie, Henrique Sequeira, C. Bernard, B. Maitte et Pascal Deren traitent de l'histoire de la réaction électrodermale, fruit d'une rencontre entre Ivan Tarchanoff et Charles Féré.

Les contributions qui suivent concernent la période soviétique, durant laquelle les relations avec l'Europe occidentale se sont poursuivies malgré les vicissitudes de l'Histoire. Tatiana Kursanova nous le rappelle à propos de cas de Nikolai Timofeev-Resovsky, dont les travaux en génétique et en radiobiologie furent marqués, du moins jusqu'en 1949, par des liens étroits avec les physiciens et biologistes français. Les interactions franco-soviétiques ont lieu aussi dans des pays tiers comme la Roumanie, où l'influence culturelle française était traditionnelle. La contribution de Cristiana Oghina-Pavie analyse la situation de la France face à la soviétisation de la science roumaine et l'institutionnalisation du lyssenkisme, dont les principaux acteurs en Roumanie furent des médecins et des biologistes. Les relations avec la France qui échappent à la diplomatie se sont poursuivies avec les contacts entre les partis communistes des deux pays. Les deux dernières contributions concernent la neurophysiologie. Jean-Gaël Barbara explique comment, après la période stalinienne des « études pavloviennes », l'histoire de la physiologie est marquée par une rencontre inattendue entre la physiologie russe de l'activité nerveuse supérieure, l'électroencéphalographie et la neurophysiologie clinique élaborée en Europe occidentale. Le périple s'achève en partie comme il avait commencé, sur les rives de la Baltique. Laurent Reynet traite des interactions entre chercheurs suédois, soviétiques et français sur l'activité rythmique de la moelle épinière et la locomotion, analysant les divergences et convergences des écoles, notamment avec celle de Nikolai Bernstein, depuis l'héritage de Charles Sherrington.

Ce livre collectif est issu d'un travail du groupe de recherche international Epistemology and History of Franco-Russian Exchanges in Neurophysiology in the International Context (GDRI-CNRS), qui a élargi sa thématique aux sciences de la vie en impliquant l'Institut Vavilov (Russian Academy of Sciences, Saint Petersburg Branch of the Institute for the History of Science and Technology), l'université Pierre-et-Marie-Curie et l'université de Picardie – Jules Verne. Il constitue le troisième volet d'une trilogie[2], l'ensemble représentant au total une cinquantaine de contributions de chercheurs russes et français, mais qui ne couvre qu'une partie de ce champ si fécond des études franco-russes. Nos remerciements vont aux personnes et aux institutions qui ont rendu cela possible.

Jean-Gaël Barbara
Jean-Claude Dupont
Eduard Kolchinsky
Marina Loskutova

2. Les références des deux précédents volumes publiés sont : J.-G. Barbara, J.-C. Dupont et I. Sirotkina, *History of the neurosciences in France and Russia. From Charcot and Sechenov to IBRO*, Paris, Hermann, 2011 ; J.-G. Barbara, J.-C. Dupont, E. I. Kolchinsky et M. V. Loskutova, *Russian-French links in biology and medicine*, Saint-Pétersbourg, Nestor-Historia, 2012.

Introduction

If science is an international enterprise, it must nevertheless be located geographically and studied in specific areas. The corpus of scientific knowledge is naturally intended to be universal, although it is first elaborated in precise contexts. Therefore, finer analyses of the geographical specificities are required in order to apprehend the paradoxes of the development of scientific knowledge and practices.

This volume is devoted to two countries situated at both ends of the European continent, namely France and Russia. It is intended to represent a contribution to the "geographical turn" in science studies.[1] Since the 18th century, these two geographical areas have been connected by wars, diplomacy and scientific and study travels, and those relations have taken different forms. Such connections started following contacts between individuals who shared common scientific interests and who created scientific networks and relations of friendship throughout the European space, in spite of contingent historical conditions. In the field of the Life sciences, French-Russian relations were little formalized, and with a few notable exceptions, due to personal initiatives. Some actors of the intellectual life of these countries got personally involved on both sides to promote such exchanges, and Francophilia and Russiophilia were more than mere words. Family relations were sometimes established. Those personal initiatives could create grounds for common research programmes between the scientific institutions of both countries. Lastly, those scientific relations could result from a political and diplomatic engagement at a higher level.

Those various relations never completely stopped, although they slowed down during the 1917 Revolution, and even more so during the Stalinian period. Contrary to common belief, those exchanges remained fruitful even during the Cold War, as reflected by the vitality of the translation of scientific books in France and Russia in the 19th century, and during the Soviet regime. Naturally, like

1. D. A. Finnegan, "The spatial turn: geographical approaches in the history of science," *Journal of the History of Biology*, 2008, p. 369-388. This "spatial turn" or "geographical turn" is present in French Social Sciences since the 1990s with Gilles Deleuze or Marcel Gauchet who coined the French expression "tournant géographique" in M. Gauchet, "Introduction au dossier 'Nouvelles geographies,'" *Le Débat*, 92, 1996, p. 42.

the social sciences, the natural sciences represent a mental space and the epistemological issues of the translation of books coexist with its other underlying political and ideological issues at stake, as can be seen today with the use of a rudimentary scientific English language, comprehensible by all.

The parallel study of French-Russian scientific exchanges should not lead us to isolate a binary model from an ensemble of constitutive relations integrated in a more complex setting. Merchants, diplomats, the military personnel of the various European countries are already present in Russia before Peter the Great. Later, in the 19th century, when scientific exchanges exploded, students and scientific travellers from France and Russia had to travel across Germany. It is indeed clear that the making of the European science in those regions resulted from three interconnected major cultural areas, France, Germany and Russia, without mentioning other countries. It would be wrong to reduce the development of Russian science in the 19th century to a unique dialogue with German science, although close geographical connections are evident. Russian scientists sometimes could take French science as an alternative model while playing with the French-German rivalry. The absolute necessity to analyse those relations beyond a binary level remains true for the 20th century: the status of the Soviet science and that of the sciences of Eastern countries cannot be reduced to the reciprocal confrontation between “Soviet science” and “Western science.” It is now required to propose dynamical schemes of analyses against the myths of national isolations and the strong representation of the “Iron Curtain,” hybridizations and resistances, receptions and mutual influence, rejections and appropriation and the creation of new ideas in relative isolation leading to novel and later hybridizations. Scientific exchanges can also be considered as failures at a given time, which may also be meaningful.

All French and Russian papers of the volume are case studies forming an ensemble which demonstrates a varied perception of French-Russian scientific relations. It is not intended to draw comparisons relying on similitudes and differences which would present each cultural area in a static national perspective. We wished rather to report crossed histories relying on complex and constitutive networks of relations. Scientific cultures do possess national specificities, but ideas circulate and the concept of cultural transfers is as highly relevant as it is, for example, in the study of French-German social sciences. Neither is it meant to claim a European essence of modern science through its origins, which is highly questionable in the worldwide integration of scientific communities. With the French-Russian example, we mean to demonstrate the diversity, the importance and the multilateral specificities of interactions lying within the common geographical and historical space of European sciences.

The French-Russian scientific relations, particularly important and polymorph, remain very little investigated, especially concerning the Life sciences. The book is divided into two sections according to the traditional divide of the Life science studies, between Natural history and Medicine, each of them presenting papers in the chronological order. Most of these crossed histories feature specific French and Russians actors.

Stéphane Schmitt analyses the works of K. Ludwig, Heinrich Bojanus and Jean-Claude Dupont focuses on those of Caspar Friedrich Wolff and Louis Tredern. Both of these contributions demonstrate how important the Baltic area was, at the turn of the 18th century and 19th century, for scientific exchanges concerning comparative anatomy and embryology. Eduard Kolchinsky deals with the impact of the ideas of Georges Cuvier and Jean-Baptiste Lamarck, upon the development of the Evolution theory in Russia. Cuvier's works have clearly laid the basis of Russian paleontology, while those of Lamarck were read, commented on and praised, only after the works of Darwin were translated in Russian and published. The aspects of Evolution theories and transformism in Russia are analysed in the particular chronological perspective of this heritage, until Lyssenko. The paper of Igor Popov describes the paleontological researches of Vladimir Kovalevsky, during the 1870s and the way in which his concepts were further developed by Louis Dollo. The author specifically discusses the teological affinities of this Neodarwinian school of paleontology. In the field of botany, from plant physiology to ecology, Anatoli Polevoi shows how Vladimir Lyubimenko built a scientific cooperation with French scientists, such as Gaston Bonnier. Oleg Belozerov deals with the forgotten Soviet developmental biology of Mikhail Zavadovskii, comparing it to other developmental biologies from Western Europe. The following papers deal with particular privileged places for scientific cooperation, such as the A. Korotneff zoological station at Villefranche-sur-Mer, studied by Tatyana Ulyankina, and showing the rather curious development of this Russian institution in France. Mikhael Konashev describes the converging and diverging ideas of Theodosius Dobzhansky and Pierre Teilhard de Chardin on the evolution of man, after Chardin's

Phénomène humain was published. Étienne Aucouturier and Daria Popova show the origins of the ideas of Vladimir Vernadski and they evaluate their impact. They situate the biogeochemistry of Vernadski in the Russian tradition of Cosmism and they discuss it as a possible source for French ethnologist Jean Malaurie, who founded the State Polar Academy in Saint Petersburg. Stéphane Tirard analyses the reception of the works on the origins of Life by Aleksandr Ivanovich Oparine, with the study case of the French edition of *The Origin of Life on the Earth* (1964), by French biologist Pierre Gavaudan, and the reactions of French biologists such as Jacques Monod.

In the following section on "Physiology and Medicine," Vladimir Samoilov introduces the three origins of physiology in the 19th century, in France, Germany and Russia, and the fruitful relations between the scientists of these countries at that period. Because the Russian surgeon, Nikolai Pirogov, exemplifies the European interactions at the foundation of medicine in the 19th century, Céline Cherici analyses his fundamental role in the European history of anaesthesiology and that of war surgery. Natalia Fedounina highlights the importance of the emigration of Russian students to France at the turn of the 20th century, as a cause and consequence of French-Russian relations in medicine, with the study of the particular destiny of French biologist, Michel Kourilsky. The privileged neuroscientific field of sensorimotor organisation of the nervous system is the focus of the paper by Jean Massion and François Clarac. They analyse and precisely compare the ideas of Alfred Vulpian and Ivan Sechenov on voluntary and involuntary movements. Also in neurophysiology, Henrique Sequeira, Bernard Maitte and Pascal Deren deal with the history of galvanic reaction as a result from a encounter between Ivan Tarchanoff and Charles Féré.

The following papers deal with the Soviet period, when Russian relations with Western Europe were maintained in spite of the vicissitudes of History. This is shown by Tatiana Kursanova with Nikolai Timofeev-Resovsky, whose works on genetics and radiobiology depended on close relations with French physicists and biologists at least until 1949. The French-Soviet relations also occurred in other countries, such as Romania, with its tradition of French cultural influence. The paper by Cristiana Oghina-Pavie shows how France dealt with the Soviet turn of Romanian science and the institutionalisation of Lyssenkism, with the involvement of actors, principally Romanian doctors and biologists.

French-Russian scientific relations often escape traditional diplomacy but continue with interactions between the communist parties of both countries. The two last papers dealing with these aspects concern the field of neurophysiology. Jean-Gaël Barbara shows that this field is marked with an unexpected encounter of the Russian physiology of the higher nervous activity, with the Western European electroencephalography and clinical neurophysiology, after the Stalinian period of orthodox Pavlovian studies. The book ends as it started on the Baltic sea, with the paper of Laurent Reynet on the interactions between Swedish, Soviet and French scientists on the rhythmic activity of the spinal cord and locomotion, with a particular focus on divergences and convergences between scientific schools, especially those involving that of Nikolai Bernstein, following the tradition of Charles Sherrington.

This collective work was part of the research activity of the international research programme on the *Epistemology and History of Franco-Russian Exchanges in Neurophysiology in the International Context* (GDRI-CNRS), with a broadening of the field of study to Life sciences in general, with the collaboration of the Vavilov Institute of the Russian Academy of Sciences, Saint Petersburg Branch of the Institute for the History of Science and Technology, Pierre and Marie Curie Paris University and Université de Picardie – Jules Verne. This book is the third and last part of our research programme which totalises a total of fifty studies by Russian and French researchers,[2] but still only covers part of the complex and prolific French-Russian studies. Our thanks go to the researchers and their institutions that made this project possible:

Jean-Gaël Barbara
Jean-Claude Dupont
Eduard Kolchinsky
Marina Loskutova

2. Previously published books are: J.-G. Barbara, J.-C. Dupont and I. Sirotkina, *History of the neurosciences in France and Russia. From Charcot and Sechenov to IBRO*, Paris, Hermann, 2011; J.-G. Barbara, J.-C. Dupont, E. I. Kolchinsky and M. V. Loskutova, *Russian-French links in biology and medicine*, Saint Petersburg, Nestor-Historia, 2012.

PARTIE I

HISTOIRE NATURELLE ET ÉVOLUTION

I

Les contributions de Ludwig Heinrich Bojanus à l'anatomie comparée et à l'embryologie, entre France, Allemagne et Russie

Stéphane Schmitt

Le personnage de Ludwig Heinrich Bojanus, qui est principalement connu aujourd'hui des zoologistes[1], constitue un cas particulièrement remarquable de la circulation des savants, et donc des idées scientifiques, à travers l'Europe à la fin du XVIII[e] et au début du XIX[e] siècle. En effet, originaire de l'Est de la France, il étudia principalement en Allemagne et poursuivit sa formation à travers différents pays d'Europe, avant de mener une brillante carrière académique dans l'empire Russe. D'autre part, les travaux qu'il effectua dans divers domaines des sciences de la vie, particulièrement en anatomie comparée et en embryologie, illustrent bien les tendances scientifiques et épistémologiques de son temps et témoignent par exemple des tensions entre les tenants et les détracteurs des principes de la *Naturphilosophie* dans le monde germanophone et au-delà. Nous nous proposons par conséquent de donner ici un aperçu de l'œuvre de cet auteur encore relativement peu étudié par les historiens des sciences, et d'en évaluer la portée à l'échelle européenne.

1. Il est célèbre essentiellement pour la mise en évidence de l'organe excréteur de certains mollusques, auquel il donna son nom – l'organe de Bojanus –, ainsi que pour avoir reconnu l'aurochs comme une espèce particulière : *Bos primigenius*, Bojanus, 1827.

Bojanus est né le 16 juillet 1776 en Alsace, plus précisément à Bouxwiller, alors capitale d'un petit État autonome, le comté de Hanau-Lichtenberg[2]. Cette enclave dans le royaume de France, au statut complexe, héritage des ambiguïtés des traités de Westphalie, se trouvait du reste dépendre des landgraves de Hesse-Darmstadt depuis 1736. À cet égard, les origines de Bojanus ne sont pas sans évoquer celles de Cuvier, lui aussi originaire d'une principauté située à la frontière des cultures française et allemande. Son père, fonctionnaire à l'office des forêts du comté, lui fit commencer des études au collège de Bouxwiller, mais l'annexion des micro-États alsaciens à la Révolution lui fit perdre son emploi. Il se tourna donc naturellement vers le prince qu'il avait servi jusqu'alors, et toute la famille émigra à Darmstadt en 1793. Là, Bojanus termina ses études secondaires, après quoi, décidé à devenir médecin, il entra à l'université d'Iéna. Ce choix eut indéniablement des conséquences importantes sur la formation de son esprit scientifique et la suite de sa carrière, car Iéna était l'un des principaux centres intellectuels dans l'Allemagne de l'époque, et les plus importantes figures de la pensée romantique naissante y séjournèrent autour de 1800. C'est là, pour une grande part, que se forma la *Naturphilosophie*, qui allait marquer durablement les sciences allemandes[3]. Bojanus y suivit

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2. Les travaux les plus complets sur Bojanus sont de loin, à ce jour, le chapitre que lui consacre B. E. Raikov dans son ouvrage sur les évolutionnistes russes avant Darwin : *Russkie biologi evoliutsionnisty do Darvina. Materialy k istorii evoliutsionnoi idei v Rossii*, Moscou/Léningrad, Académie des sciences de l'URSS, 1952, vol. 1, p. 365-419, et le petit volume (en polonais) de Z. Fedorowicz, « Ludwik Henryk Bojanus », *Memorabilia Zoologica*, 1, Wrocław/Varsovie, Zakład Narodowy im. Ossolinskich, 1958. La plupart des éléments biographiques proviennent des deux principales notices du xix[e] siècle : L. H. von Bojanus et A. W. Otto, *Nova Acta Physico-medica Academiae Caesareae Leopoldino-Carolinae Naturae Curiosorum*, 1831, vol. 15, partie II, xxxvii-xlv ; C. E. von Eichwald, « Memoria clarissimi quondam apud Vilnenses professoris Ludovici Henrici Bojani : quam jubente amplissimo Academiae medico-chirurgicae Vilnensis Collegio », in *Conventu academico die xvi et xvii februar. Anni mdcccxxxiv*, Vilnius/Glücksberg, 1835. Tout récemment, sont parues plusieurs études : P. Daszkiewicz, « Some remarks about the origin and history of Bojanus "Anatome Testudinis Europaeae" », *Bulletin of the British Herpetological Society*, 76, 2001, p. 6-9 ; *id.*, « Ludwig Bojanus, un naturaliste alsacien à Vilnius », *Bulletin de la Société d'Histoire Naturelle et d'Ethnographie de Colmar*, 65, 2004, p. 95-102 ; P. Edel, « L. H. Bojanus (1776-1827) : un grand scientifique entre Ouest et Est », *Cahiers Lituaniens*, 3, 2002, p. 15-19. Plusieurs autres articles de P. Edel (publiés ou annoncés) sont indiqués sur son site : [http://philippe-edel.blogspot.fr] (consulté le 06/11/2013).

3. Voir O. Breidbach et P. Ziche (éd.), *Naturwissenschaften um 1800. Wissenschaftskultur in Jena-Weimar*, Weimar, Böhlaus Hermann, 2001.

notamment les cours de médecine de Christoph Wilhelm Hufeland, ceux d'anatomie de Justus Christian Loder, et il s'imprégna plus généralement des idées en vogue. Après avoir obtenu son doctorat en médecine et en chirurgie en 1797, il voyagea pendant une année pour achever sa formation, d'abord à Berlin, puis à Vienne, avant de rentrer à Darmstadt, en 1798, afin d'y exercer la médecine.

Sa carrière prit toutefois un tour particulier lorsque le ministre de Hesse-Darmstadt, Carl Ludwig von Barkhaus-Wiesenhütten, suivant une tendance générale dans l'Europe de l'époque, projeta de créer une école vétérinaire. Bojanus fut alors chargé d'une longue mission destinée à recueillir des informations sur les meilleures institutions de ce genre et, subventionné par l'État, passa deux années (1801-1803) à visiter les écoles les plus prestigieuses : Lyon, Alfortville, Londres, Hanovre, Vienne, Dresde, Berlin, Copenhague. Cette période fut pour lui particulièrement intéressante, car outre son travail d'observation, il eut la possibilité de se familiariser avec des approches nouvelles et de fréquenter divers milieux savants. Ainsi, il put assister à Vienne aux cours dans lesquels Franz Joseph Gall exposait ses conceptions phrénologiques, et peu après il en publia à Paris un long compte rendu, qui fut largement diffusé et contribua à faire connaître les travaux du médecin allemand auprès des savants français[4]. Son rôle à cet égard doit d'autant moins être négligé que Bojanus fit partie de la Société des Observateurs de l'Homme, où s'élaborait la science anthropologique[5], et que le fait d'avoir aidé à faire connaître Gall dans un tel contexte a très certainement joué sur les destinées françaises de la phrénologie.

De retour à Darmstadt, il publia un ouvrage qui exposait les conclusions de sa mission[6], ainsi qu'une traduction allemande d'un ouvrage vétérinaire anglais célèbre alors[7] ; mais dans le contexte troublé

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4. L. H. Bojanus, « Encéphalo-cranioscopie. Aperçu du système craniognomique de Gall, médecin à Vienne », *Magasin encyclopédique ou journal des sciences, des lettres et des arts*, 1, A. L. Millin (dir.), Paris, an X (1802), p. 445-472 ; également publié dans le *Journal de Physique, de Chimie, d'Histoire naturelle et des Arts*, 55, par J.-Cl. Delamétherie (éd.), an X (1802), p. 198-215. Le texte fut traduit en anglais : « A Short View of the Craniognomic System of Dr. Gall, of Vienna », *Philosophical Magazine*, 14, 1802, p. 77-84 et p. 131-138.

5. Voir J.-L. Chappey, *La Société des Observateurs de l'Homme (1799-1804). Des anthropologues au temps de Bonaparte*, Paris, Société des études robespierristes, 2002.

6. L. H. Bojanus, *Über den Zweck und die Organisation der Thierarzneischulen*, Francfort-sur-le-Main, Andreäischen Buchhandlung, 1805.

7. E. Coleman, *Grundsätze des Hufbeschlags, aus d. Engl. durchaus umgearbeitet*, Darmstadt, Heyer, 1805.

des guerres napoléoniennes, le projet d'école vétérinaire fut peu à peu abandonné par les autorités. Bojanus se mit alors à la recherche d'une position et obtint la chaire de médecine vétérinaire de l'université de Vilnius, ville située dans l'Empire russe depuis le troisième et dernier partage de la Pologne en 1795. L'université venait de rouvrir en 1802 et était de culture principalement polonaise[8]. Bojanus y arriva en mai 1806 et allait y mener une très honorable carrière jusqu'en 1824. Il y enseigna, en latin, d'abord la médecine vétérinaire, puis aussi l'anatomie comparée à partir de 1815. Il contribua au développement de cette institution dans laquelle il s'impliqua considérablement, obtenant par exemple la construction d'un amphithéâtre d'anatomie. Sur un plan académique, il s'intégra remarquablement dans le réseau des sociétés savantes européennes, devenant membre non seulement de la Société de médecine de Vilnius, mais aussi de plusieurs institutions équivalentes, tant dans l'empire russe (Moscou, Saint-Pétersbourg) que dans d'autres pays d'Europe (Académie des Sciences de Stockholm, Leopoldina…) et correspondit avec de nombreux naturalistes et anatomistes, notamment avec Cuvier[9]. Le fait d'avoir, contrairement à beaucoup de ses collègues de Vilnius, manifesté peu d'enthousiasme lors de l'occupation napoléonienne en 1812, et de s'être alors prudemment retiré à Saint-Pétersbourg, lui valut par la suite les faveurs de la cour impériale. Devenu conseiller d'État en 1821, il fut recteur de l'université en 1822-1823, réussissant tant bien que mal à contenir en douceur l'activisme nationaliste des étudiants polonais. Sa monographie sur la tortue fut récompensée par l'impératrice douairière et l'impératrice régnante, signe de ses excellentes relations avec les autorités. Il put fonder à Vilnius, en 1823, l'école vétérinaire qu'il n'avait pu établir à Darmstadt ; mais sa santé se dégrada, et dès 1824 il dut quitter sa chaire. Il se retira alors à Darmstadt où il mourut trois ans plus tard[10].

Durant toute sa carrière en Russie, Bojanus continua d'entretenir des contacts très étroits avec l'Allemagne, où on lui proposa d'ailleurs

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8. Elle sera pour cette raison un foyer de tensions, souvent réprimées, comme en 1823-1824 ; l'université sera fermée après la révolution de 1830, jusqu'en 1919.

9. Voir P. Edel, G. Cuvier et L. H. Bojanus, « À la recherche de la correspondance de deux grands naturalistes entre Paris et Vilnius », in Marie-France de Palacio (éd.), *Correspondance d'érudits aux XVIII^e et XIX^e siècles. France, Pologne, Lituanie*, Presses universitaires de Rennes, coll. « Interférences », 2014.

10. K. F. Burdach lui rendit visite en 1826 ; il le dépeint dans un état très grave : *Blicke ins Leben*, Leipzig, Leopold Voss, vol. 4 (1842-1848), p. 356-357.

des postes, qu'il refusa[11]. Proche de Burdach, qui se trouvait alors à Königsberg et qu'il rencontra plusieurs fois, il était aussi en relation avec les courants les plus radicaux, schellingiens, de la *Naturphilosophie*, malgré ses propres réserves à leur égard. De manière significative, la plupart des articles d'anatomie et d'embryologie qu'il publia après 1815 parurent, soit dans les recueils de l'Academia Leopoldina, soit, le plus souvent, dans la revue *Isis*, *oder enzyklopädische Zeitung*, publiée par le sulfureux Lorenz Oken et particulièrement engagée dans les mouvements libéraux et nationalistes[12]. Ces liens, *a priori* étonnant au regard tant des divergences scientifiques majeures entre Bojanus et Oken (voir *infra*) que de la prudence de Bojanus sur le plan politique, témoignent de la complexité des réseaux de communication savants et des rapports entre courants antagonistes dans les premières décennies du XIX^e^ siècle.

L'œuvre de Bojanus, outre l'exposé du système de Gall évoqué plus haut, est principalement tournée vers l'étude des animaux. Son principal intérêt était au départ la médecine vétérinaire. Ses investigations dans ce domaine le conduisirent notamment à l'étude des épizooties[13] et de l'amélioration des races[14]. Mais ses recherches s'orientèrent peu à peu vers l'anatomie comparée, selon une trajectoire déjà suivie quelques décennies plus tôt par Félix Vicq d'Azyr[15]. Bojanus s'illustra

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11. *Ibid*., p. 314-315.

12. Sur la revue *Isis*, voir notamment K. Stiefel, « Zwischen Naturphilosophie und Wissenschaftspolitik : Zum Profil der "Isis, oder Encyklopädischen Zeitschrift von Oken" als naturwissenschaftliches Publikationsorgan in den Jahren 1817 bis 1822 », *Berichte zur Wissenschaftsgeschichte*, 26, 2003, p. 35-56 ; C. Taszus, « Lorenz Okens, "Isis" (1816-1848). Zur konzeptionellen, organisatorischen und technischen Realisierung der Zeitschrift », *Blätter der Gesellschaft für Buchkultur und Geschichte*, 12-13, 2009, p. 85-154 ; *id*., « Okens "Isis". Pressefreiheit, Restriktionen und Zensur in Mitteldeutschland in der ersten Hälfte des 19. Jahrhunderts », *Jahrbuch für Europäische Wissenschaftskultur*, 4, 2009, p. 205-241. Bojanus compte parmi les principaux contributeurs à *Isis*, du moins les années où il put envoyer ses articles. Oken indique même que certains textes anonymes sont de lui (*Isis, oder encyklopädischen Zeitschrift*, 1828, col. 601).

13. L. H. Bojanus, *Anleitung zur Kenntnis und Behandlung der wichtigsten Seuchen unter den Hausthieren. Zweite, umgearbeite u. vermehrte Auflage*, Vilnius, Fr. Moritz, et Leipzig, Fr. Fleischer, 1820.

14. *Id*., *Des principales causes de la dégénération des races des chevaux et des règles à suivre pour les relever : discours prononcé à l'ouverture solennelle du théâtre anatomique à l'université impériale de Vilna, le 13 octobre 1815*, Vilnius, Joseph Zawadzki, 1815.

15. S. Schmitt, « From Physiology to Classification : Comparative Anatomy and Vicq d'Azyr's Plan of Reform for Life Sciences and Medicine (1774-1794) », *Science in Context*, 22, 2, 2009, p. 145-193.

particulièrement dans cette discipline et réalisa plusieurs découvertes importantes, notamment chez les mollusques. Son ouvrage resté le plus célèbre est consacré à l'anatomie de la tortue d'Europe[16]. La qualité de cette monographie fut reconnue par Cuvier lui-même, auquel elle était dédiée et qui croyait pouvoir la considérer « comme la plus parfaite de toutes, celle de l'homme excepté[17] ». Dans l'introduction d'une édition en fac-similé parue en 1970, Alfred Romer admettait que les planches n'avaient pas été surpassées un siècle et demi plus tard[18]. Il est vrai que, pour parvenir à ce résultat, Bojanus avait disséqué environ cinq cents tortues (abondantes dans la région de Vilnius) et qu'il avait soigné particulièrement l'illustration. Il s'était lui-même formé aux spécificités du dessin destiné à la gravure sur cuivre, il avait fabriqué ses presses, et il était allé en Hesse à l'été 1817 pour y recruter un graveur compétent, Friedrich Leonard Lehmann (qui allait travailler plus tard pour von Baer)[19]. N'ayant pas trouvé d'éditeur pour soutenir son projet, il l'avait financé sur ses fonds propres. Ce travail fut donc universellement salué, et il fut généralement admis que Bojanus était allé aussi loin qu'il était possible dans la précision de la description anatomique[20]. Il se démarquait nettement à cet égard de certains *Naturphilosophen* comme Oken, moins attachés aux détails et plus prompts aux généralisations et aux spéculations.

De fait, les vues générales et les considérations théoriques sont rares chez Bojanus, qui adopte le plus souvent un style très sobre et s'écarte peu de la description proprement dite. Il lui arrive cependant de s'écarter de cette ligne, comme dans le discours inaugural de son cours d'anatomie comparée, prononcé en 1814 et qui revêt un caractère programmatique[21]. Dans cet opuscule, il insiste beaucoup sur l'absence de discontinuités dans la nature, notamment entre les règnes

16. L. H. Bojanus, *Anatome testudinis Europaeae*, Vilnius, Joseph Zawadski, 1819-1821, 2 vol.

17. G. Cuvier, *Histoire des Sciences naturelles, depuis leur origine jusqu'à nos jours, chez tous les peuples connus*, Paris, Fortin Masson et Cie, 1845, vol. 5, p. 405.

18. A. S. Romer, « Bojanus and the anatomy of the turtle » *Anatome Testudinis Europaeae*, *Facsimile Reprints in Herpetology*, Marceline, mo Society for the Study of Amphibians and Reptiles, 1970, p. iii-v.

19. K. F. Burdach, *op. cit.*, p. 315.

20. Voir notamment les comptes-rendus très élogieux dans *Isis, oder encyklopädischen Zeitschrift*, 1819, p. 1766-1769 ; *ibid.*, 1821, col. 272 ; *ibid.*, 1823, p. 750-763, avec un long extrait en latin.

21. L. H. Bojanus, *Introductio in anatomen comparatam. Oratio academica*, Vilnius, Zawadzki, 1815.

animal et végétal (certains organismes assurant la transition) et sur la gradation des êtres. « C'est une loi et une disposition de la nature qui la fait s'élever d'une marche continue depuis une structure très simple jusqu'à une structure plus composée et élaborée[22] », dit-il par exemple, ce qui a été parfois considéré, notamment par Raikov, comme une preuve de son adhésion à une théorie transformiste. En fait, Bojanus ne fait que reformuler des thèmes très communs depuis le XVIIIe siècle et liés à la notion d'échelle des êtres, et il ne semble pas croire (ou, du moins, il ne le dit pas explicitement) que les espèces vivantes évoluent *réellement* dans le temps.

Plus significatif en revanche est son implication dans les discussions relatives à la théorie vertébrale du crâne. Cette conception, née autour de 1800, supposait que le crâne des vertébrés était fondamentalement composé d'un certain nombre de vertèbres, très modifiées de manière à pouvoir accueillir le cerveau et les organes céphaliques, mais tout de même reconnaissable par une analyse ostéologique précise[23]. Cette idée, qui commençait à connaître un certain succès, spécialement en Allemagne, donna lieu à une vive querelle de priorité entre Oken et Goethe, qui s'en attribuaient chacun la première intuition. C'est d'ailleurs Bojanus, au cours d'un séjour à Iéna, qui aurait appris à Oken que Goethe prétendait avoir fait cette découverte avant lui[24]. Pour Oken, l'enjeu était d'autant plus important que la théorie vertébrale constituait une pièce majeure de son système de *Naturphilosophie* : il voyait en effet dans cette répétition par la tête de l'anatomie segmentée du reste du corps une manifestation éclatante du principe selon lequel tout était répété dans tout et où tout, dans le monde, était lié par un réseau subtil et complexe de correspondances.

Bojanus entreprit lui-même, juste après son retour à Vilnius, de se pencher sur cette question et il publia l'année suivante une première étude à ce sujet dans la revue d'Oken[25]. D'emblée il y proclame son attachement aux faits plutôt qu'aux spéculations et aux suppositions trop vite considérées comme des vérités acquises, présentant ses propres conclusions comme provisoires, ce qui apparaît évidemment comme

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22. Cité par B. E. Raikov, *op. cit.*, p. 406.

23. Sur ces débats, voir notamment S. Schmitt, *Histoire d'une question anatomique : la répétition des parties*, Paris, MNHN, 2004, p. 50-85 et p. 118-130.

24. G. H. Lewes, *The Life and Works of Goethe ; with sketches of his age and contemporaries*, Londres, David Nutt, 1855, vol. 2, p. 157.

25. L. H. Bojanus, « Versuch einer Deutung der Knochen im Kopfe der Fische », *Isis, oder Encyklopädische Zeitung*, 1818, p. 498-510.

une critique implicite de l'approche d'Oken. Surtout, contrairement à ce dernier et à Goethe, il considère qu'il est préférable d'étudier en priorité des vertébrés inférieurs, plutôt que des mammifères, « si l'on doit parvenir à une idée claire des premiers rudiments de la structure du crâne et de son développement progressif jusqu'aux mammifères » (col. 500). Son analyse anatomique le conduit ainsi à admettre l'existence de quatre vertèbres crâniennes (plutôt que trois, chez Oken). Un long paragraphe final est consacré à la vanité des querelles de priorité en science, dans la mesure où celle-ci est une activité fondamentalement collective, ce qui vise, là encore, Goethe et Oken. Ce dernier apprécie d'ailleurs assez peu : il ridiculise la position de principe de Bojanus, réaffirmant sa priorité, et insère dans le texte même de l'article plusieurs critiques ponctuelles. Il n'empêche que deux autres contributions de Bojanus sur la théorie vertébrale seront publiées dans *Isis* au cours des années suivantes, fondées sur l'étude du crâne de tortues et d'autres reptiles, c'est-à-dire là encore de vertébrés inférieurs[26].

Cet épisode est intéressant, en premier lieu, car il montre que les tensions épistémologiques ne se traduisent pas nécessairement par des clivages institutionnels et éditoriaux et que les échanges entre des courants opposés peuvent être relativement actifs : Bojanus, que l'on peut ranger parmi les sceptiques à l'égard de la *Naturphilosophie* schellingienne, non seulement n'hésite pas à s'exprimer dans l'organe le plus radical de cette dernière (qui d'ailleurs lui en laisse largement la possibilité), mais s'empare en outre d'une problématique typique de la *Naturphilosophie*, qu'il constitue en programme de recherche conforme à ses propres conceptions, plus empiriques, de la méthode scientifique. On voit en outre comment un thème privilégié de la *Naturphilosophie* se trouve diffusé de cette manière hors des frontières allemandes, dans l'Empire russe[27].

Bojanus entreprit divers autres travaux d'ostéologie comparée dans le but de caractériser les différences entre espèces, en s'intéressant particulièrement à des espèces présentes en Europe orientale ou en Asie. Il étudia ainsi des crânes d'argali, caprin ramené par des expéditions russes en Extrême-Orient, afin de le comparer à la chèvre

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26. *Id.*, « Bemerkungen in Bezug auf die Deutung der Kopfknochen im Fische », *Isis, oder Encyklopädische Zeitung*, 1818, p. 2095-2096 ; *id.*, « Abermals ein Wort zur Deutung der Kopfknochen », *Isis, oder Encyklopädische Zeitung*, 1821, p. 145-1167.

27. Sur la réception de Schelling en Russie, voir Thomas Bach (éd.), *Schelling in Rußland : Die frühe naturphilosophischen Schriften von Daniil Michajlovic Vellanskij (1774-1847)*, Marburg/Lahn, Basilisken-Presse, 2005.

domestique[28], et décrivit un animal fossile découvert en Sibérie qu'il considéra comme une espèce disparue, le *Merycotherium*[29]. Plus célèbre est sa description du squelette de l'aurochs, qui le conduisit à distinguer totalement cette espèce du bison d'Europe[30]. Tous ces travaux témoignent de l'application des approches les plus modernes de l'anatomie comparée, développées alors en France et en Allemagne par des savants tels que Cuvier, à la connaissance spécifique de la faune de l'Empire russe.

Il reste à évoquer une dernière catégorie de travaux accomplis par Bojanus, ceux qui concernent le développement des organismes. Bien que relativement peu étudiés par les historiens, ils revêtent une importance significative sur un plan descriptif aussi bien que théorique[31]. Les plus remarquables concernent l'organisation des annexes embryonnaires des mammifères, une question très débattue dans les deux premières décennies du XIX^e^ siècle[32]. La topologie des membranes extérieures de l'embryons pose en effet, peut-être plus encore que l'embryon luimême, de considérables problèmes d'observation, compte tenu de sa complexité et de l'extrême délicatesse des tissus, qui sont généralement

28. L. H. Bojanus, « Craniorum Argalidis, Ovis et Caprae domesticae comparatio », *Nova Acta Physico-medica Academiae Caesareae Leopoldino-Carolinae Naturae Curiosorum*, 12, partie I, 1824, p. 291-300.

29. *Id.*, « De merycotherii sibirici seu gigantei animalis ruminantis, antediluviano quodam, dentibus incerto Sibiriae loco erutis, declarato vestigio, commentatio », *Nova Acta Physico-medica Academiae Caesareae Leopoldino-Carolinae Naturae Curiosorum*, 12, partie I, 1824, p. 264-278. Cuvier contestera peu après qu'il s'agisse d'un genre particulier et rapportera le *Merycotherium* au genre *Camelus*.

30. *Id.*, « De uro nostrato ejusque sceleto commentatio », *Nova Acta Physico-medica Academiae Caesareae Leopoldino-Carolinae Naturae Curiosorum*, 13, partie II, 1827, p. 411-473. L'aurochs a disparu au début du XVII^e^ siècle ; les derniers individus vivaient dans des forêts lituaniennes, ce qui explique que Bojanus ait eu plus facilement accès à un squelette.

31. Voir L. Y. Blyakher, *History of embryology in Russia from the middle of the eighteenth to middle of the nineteenth century*, traduit du Russe, Washington, Smithsonian Institution, 1982, p. 227-236.

32. L. H. Bojanus, « De foetus canini velamentis, inprimis de ipsius membrana allantoide, observatio anatomica, iconibus illustrata » in *Mémoires de l'Académie Impériale des Sciences de aint-Pétersbourg* [1812], Saint-Pétersbourg, Imprimerie de l'Académie Impériale des Sciences, 1815, t. 5, p. 302-320 ; traduit en allemand, « Abhandlungen über die Hüllen des Hundsfötus », *Isis, oder Encyklopädische Zeitung*, 1818, p. 1616-1623. Bojanus est aussi l'auteur de plusieurs autres contributions de détail que nous ne pouvons énumérer ici. Mentionnons seulement son étude des cercaires : en suggérant que ces organismes étaient produits par des vers, il ouvrit la voie à l'élucidation du cycle des trématodes (« Kurze Nachricht über die Zerkarien und ihren Fundort », *Isis, oder Encyklopädische Zeitung*, 1818, p. 729-730).

détruits, ou du moins totalement affaissés, dès que l'embryon est extrait de l'utérus maternel. Bojanus dut donc mettre au point un procédé de dissection qui lui permit d'ouvrir le chorion d'un fœtus de chien sans que les liquides contenus dans les différentes cavités ne s'écoulent, et il put donc observer la disposition des structures extra-embryonnaires, entre elles et par rapport à l'embryon. Au cours des années suivantes, il entreprit des investigations similaires chez plusieurs autres espèces de mammifères[33], ainsi que dans l'œuf des reptiles, chez lesquels il établit l'existence d'un sac vitellin équivalent à celui des mammifères[34]. Ces travaux l'amenèrent à entrer en controverse avec divers savants européens, comme Dutrochet et Oken, lequel, une fois encore, ne manqua pas de critiquer vivement certaines contributions de Bojanus parues dans sa revue *Isis*.

Bien que Bojanus lui-même, selon son approche habituelle, n'ait guère proposé de considérations théoriques, ses travaux marquèrent une rupture par rapport aux conceptions d'Oken et d'autres *Naturphilosophen*, qui tentaient de retrouver dans l'architecture des membranes une géométrie idéale (sphérique) et conforme aux principes de similitude et de répétition sur lesquels ils fondaient l'anatomie et l'embryologie. Comme le note Blyakher, ces recherches de Bojanus constituent à cet égard une étape importante, entre Wolff et Pander, dans la naissance de la théorie des feuillets germinaux, en suggérant que la morphogenèse

33. Sur le cheval : L. H. Bojanus, « Bemerkungen aus dem Gebiete der vergleichenden Anatomie », *Russische Sammlung für Naturwissenschaft und Heilkunst*, Riga et Leipzig, Hartmann, 2, 4, 1817 ; reproduit dans *Isis, oder Encyklopädische Zeitung*, 1818, p. 1425-1432 et « Ueber die Darmblase des Pferdefötus », *Isis, oder Encyklopädische Zeitung*, 1818, p. 1633-1636 ; sur le mouton : *id.*, « Ueber die Darmblase des Schaafsfötus », *Isis, oder Encyklopädische Zeitung*, 1818, 1623-1633 ; « Ueber die Darmblase des Schafsfötus, zum Beweise, dass die vesicula umbilicalis mit dem Darm unmittelbar zusammenhängt », *Deutsches Archiv für die Physiologie*, J. F. Meckel (éd.), 4, 1818, p. 34-46 ; « Sur la vésicule ombilicale du fœtus de brebis, pour prouver qu'elle communique directement avec l'intestin », *Journal complémentaire du Dictionnaire des Sciences médicales*, Paris, Panckoucke, 2, 1818, p. 84-94 ; de nouveau sur le chien : *id.*, « Observatio anatomica de fetus canino 24 dierum ejusque velamentis », *Nova Acta Physico-medica Academiae Caesareae Leopoldino-Carolinae Naturae Curiosorum*, 10, partie I, 1820, p. 139-152 ; sur l'homme : *id.*, « Ein Wort über das Verhältniß der membrana decidua und decidua reflexa zum Ei des menschlichen Embryo », *Isis, oder Encyklopädische Zeitung*, 1821, p. 268-270 ; « Anfrage und Bitte wegen der membrana decidua », *Isis, oder Encyklopädische Zeitung*, 1821, col. 1174 ; sur le lièvre : *id.*, « Ueber die Darmblase des Haasenfetus », *Isis, oder Encyklopädische Zeitung*, 1822, p. 1228-1230.

34. *Id.*, « Dottergang im Fetus des Coluber berus », *Isis, oder Encyklopädische Zeitung*, 1818, p. 2093-2094.

pouvait résulter de la croissance de membranes aux plis complexes plutôt que d'agrégation de vésicules. Et là encore, il est significatif que ces travaux, comme ceux de Pander et de von Baer, aient été effectués à la marge des mondes germanique et russe.

Bojanus apparaît donc bien, sur un plan historique, comme une figure intéressante par sa position intermédiaire, d'une part d'un point de vue géographique et culturel, ayant évolué dans des contextes français, allemand et russe, d'autre part d'un point de vue épistémologique, puisque, tout en ayant maintenu des liens étroits avec la *Naturphilosophie*, il a lui-même pratiqué une science beaucoup plus proche de celle de savants plus empiristes et fort critiques à l'égard de la *Naturphilosophie*, comme Cuvier.

II

Une impulsion franco-germano-russe à l'embryologie européenne : Caspar Friedrich Wolff et Louis Tredern

Jean-Claude Dupont

L'embryologie moderne résulte des apports fondamentaux de Caspar Friedrich Wolff (organes embryonnaires transitoires), de Christian Heinrich Pander (théorie des feuillets), de Karl Ersnt von Baer (œuf des mammifères) et de Martin Heinrich Rathke (arcs branchiaux chez les mammifères). Leurs carrières se déroulent sur les rives de la Baltique, à l'université de Königsberg (aujourd'hui Kaliningrad, en Russie), à l'université de Dorpat (aujourd'hui Tartu, en Estonie), et à l'Académie impériale de Saint-Pétersbourg, dans une région transnationale alors ballotée entre la Prusse et l'Empire russe. Dans cet espace germano-balte évoluent aussi quelques rares naturalistes d'origine française, un peu oubliés de l'histoire, comme Ludwig Heinrich Bojanus ou Louis Tredern de Lézérec. On se propose de rappeler l'importance générale de l'impulsion donnée par « l'école de la Baltique » à l'embryologie européenne, en s'attachant plus spécialement à Wolff et à Tredern.

I. LA RENAISSANCE DE L'EMBRYOLOGIE DANS LA RÉGION BALTIQUE

Le point de départ de cette renaissance de l'embryologie opérée par Caspar Friedrich Wolff (1735-1794) est sa thèse de médecine soutenue à l'université de Halle, la *Theoria generationis*[1] (1759). Ses travaux

1. C. F. Wolff, *Theoria generationis*, Halle, Hendel, 1759 ; éd. en fac-similé, Hildesheim, G. Olms, 1966 ; traduit en allemand par P. Samassa, *Ostwalds Klassiker der Exakten Wissenschaften*, Leipzig, Wilhelm Engelmann, 1896, p. 84-85 ; traduit en russe par A. E. Gaissinovitch et E. N. Pavlovski, Moscou, Académie des sciences de l'URSS, 1950.

ultérieurs comprennent outre cette thèse et ses versions rectifiées de 1764 et de 1774, environ une trentaine de monographies publiées dans les rapports de l'Académie des sciences de Saint-Pétersbourg, parmi lesquelles le *De formatione intestinorum* (voir *infra* p. XXX), auxquelles s'ajoutent des manuscrits non publiés, sa correspondance avec Haller et un traité inachevé sur les monstres[2].

Wolff est classiquement reconnu comme l'auteur de la première démonstration de la réalité de l'épigenèse, remettant en cause l'idée alors dominante de la préexistence de l'embryon dans les germes, en même temps de la rectification de l'ancienne l'épigenèse d'Aristote et de Harvey. Mais l'ambition initiale de Wolff exprimée dans la *Theoria generationis* est bien plus grande que de promouvoir une épigenèse même de nouveau style : c'est celle d'une anatomie enfin rationnelle (*anatomia rationalis*) qui soit fondée sur une théorie de la génération. S'il s'intéresse aux conséquences, à la forme et aux processus qui conservent la forme de l'organisme, c'est pour mieux en découvrir les causes les plus profondes, les processus qui la modifient ou qui la créent, c'est-à-dire les lois de la génération.

Les modalités concrètes de la génération sont décrites en trois parties, traitant respectivement des plantes, des animaux, et plus généralement des lois de la génération dans les organismes vivants. Le développement animal et le développement végétal sont traités de manière similaire. Chez les plantes, nous explique Wolff, les fluides sont constamment absorbés du sol, distribués à travers la plante, et évaporés des feuilles. Ce mouvement doit être causé par une force qu'il appelle la « force essentielle » des plantes (*vis essentialis*). Le « germe » est une substance inorganisée sécrétée par les organes génitaux des parents qui s'organise sous l'action de la force essentielle. Par ailleurs les fluides nutritifs ont une tendance naturelle à se solidifier (*solidescibilitas*). Les jeunes plantes et les jeunes animaux naissent d'une substance simple, inorganique et amorphe, sans qu'il soit besoin de postuler l'existence de parties préformées dans l'embryon. Tout est formé graduellement, grâce à la sécrétion et la solidification des fluides sous le contrôle de la force essentielle. Les différentes parties de l'embryon apparaissent successivement sous une forme simple, tout à fait différente de celles qu'elles revêtiront par la suite.

2. Pour une bibliographie des travaux de Wolff, voir S. A. Roe, *Matter, life, and generation. Eighteenth-century embryology and the Haller-Wolff debate*, Cambridge, Cambridge University Press, 1981.

Albrecht von Haller reçoit de Wolff sa *Theoria Generationis* dans l'espoir qu'il abandonne sa défense de la préexistence… avec l'effet inverse : Haller en deviendra un des partisans les plus rigides. Il s'en suivra entre 1759 et 1777 (mort de Haller) une des plus fameuses controverses de l'histoire des sciences, controverse à la fois embryologique et philosophique. Cette controverse est visible dans une série de publications : la correspondance entre Wolff et Haller de 1759 à 1777 (avec neuf lettres de Wolff), les revues de Haller sur chacun des travaux de Wolff, la *Theorie von der Generation* (1764), édition en allemand de la *Theoria Generationis* considérablement révisée et augmentée par Wolff en réponse à Haller, le huitième volume des *Elementa physiologiae corporis humani* de Haller (1766) et l'édition révisée de son traité *Sur la formation du cœur dans le poulet* (1767)[3].

Alors que la théorie de la préexistence domine le monde naturaliste, Wolff éprouve des difficultés à intégrer la vie universitaire. Après sa réquisition dans l'armée prussienne durant la guerre de Sept Ans, il parvient à obtenir la chaire d'anatomie à l'Académie des sciences de Saint-Pétersbourg (1767). La période russe de sa carrière commence alors. C'est là qu'il publiera son chef-d'œuvre, le *De Formatione intestinorum*[4]. Selon Karl von Baer, c'est cet ouvrage, publié à Saint-Pétersbourg en deux parties en 1768 et 1769, traduit en allemand par Johann Friedrich Meckel en 1812, et non la *Theoria generationis* qui représente la plus grande contribution de Wolff à l'embryologie.

Pour comprendre la genèse de cet ouvrage fondateur, il faut se rappeler les arguments théoriques développés par Haller, principalement la transparence, la solidarité des organes, et la nature indéterminée de la *vis essentialis*.

– L'argument de la transparence est que même si les parties sont invisibles, nous n'avons pas le droit d'arguer qu'elles n'existent pas. La couvaison ne fait que rendre visible ce qui était à l'origine invisible.

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3. Les neuf lettres de Wolff à Haller sont traduites dans : S. A. Roe, *op. cit.* Voir en annexe de cet ouvrage les références des revues des travaux de Wolff par Haller et des autres travaux de Haller.

4. Voir C. F. Wolff, « De formatione intestinorum praecipue, tum et de amnio spurio, aliisque partibus embryonis gallinacei, nondum visis », *Novi Commentarii Academiae Scientiarum Imperialis Petropolitanae*, 12, 1768, p. 403-507 ; *ibid.*, 13, 1769, p. 478-530. Traduit en allemand par J. F. Meckel, *Über die Bildung des Darmkanals im bebrüteten Hühnchen*, Halle, Renger, 1812. Traduit en français par M. Perrin et J.-C. Dupont, *Caspar Friedrich Wolff, la formation des intestins (1768-1769)*, Turnhout, Brepols, 2003.

Les structures préformées sont transparentes, semi-fluides, les vaisseaux sanguins sont encore « cachés », transparents, mous ou aplatis et c'est ce qui les dérobe à nos yeux. Il y a même un support expérimental : si on verse du blanc d'œuf dans de l'alcool (esprit de vin), qui n'est pourtant pas la *vis essentialis*, les vaisseaux, les viscères deviennent visibles. Wolff aurait eu le tort d'assimiler l'invisible et l'inexistant.

– Le second argument est l'objection physiologique : l'impossibilité pour des organes solidaires de se former successivement. La préexistence des organes les rendait d'emblée solidaires. La fragmentation de l'organisme que réalisait l'épigenèse conçue comme création successive d'organes définitifs et isolés crée le problème de l'établissement ultérieur d'une solidarité, sans laquelle l'organisme ne peut coordonner le fonctionnement de ses parties.
– Le troisième argument, celui de l'indétermination de la *vis essentialis*, est résumé ainsi par Charles Bonnet, fervent partisan de Haller :

> Mais s'il n'y a rien de préformé dans la matière que la force essentielle organise, comment cette force sera-t-elle déterminée à produire un Animal, plutôt qu'une Plante, et un certain Animal, préférablement à un autre ? Pourquoi encore la force essentielle produira-t-elle dans un certain endroit, un certain organe, et non un autre ?[5]

Les objections en faveur de la préexistence se fondent aussi sur des données observationnelles nouvelles. Le traité de Haller de 1758 sur le cœur était déjà à cet égard l'aboutissement d'un travail considérable[6]. Pour écrire la *Theoria generationis* (1759), Wolff s'était fondé sur l'observation minutieuse du développement de la graine et de l'embryon de poulet. Cependant ses observations sur ce dernier matériel n'égalaient pas celles du grand Albrecht von Haller. De plus, suite aux objections de Wolff, notamment dans la *Theorie von der Generation*, Haller avait informé Bonnet en 1765 qu'il allait refaire ses observations, et réexpérimenter, ce qui l'amènera effectivement à confirmer sa position concernant certaines questions embryologiques, principalement la formation des vaisseaux et du cœur et la continuité

5. Voir C. Bonnet, *Considérations sur les corps organisés, où l'on traite de leur origine, de leur développement, de leur reproduction, etc.*, Amsterdam, M. M. Rey, 1762, 2 vol. Le passage est dans l'édition de 1985, Paris, Fayard, p. 467-468.

6. On en aura une idée en consultant A. von Haller, *Commentarius de formatione cordis in ovo incubato* [1767], éd. M. T. Monti, Basel, Studia Halleriana VI, Schwabe, 2000.

membranaire. Il les publiera dans les *Éléments de Physiologie* (vol. 8, 1766), et *Opera minora* (vol. 2, 1767).

Wolff sent l'impasse que serait une discussion supplémentaire sur la puissance déterminatrice de la *vis essentialis*, problème qu'il abordera encore néanmoins ultérieurement dans des discussions avec Blumenbach comme on le verra plus loin, tout comme s'engager dans une discussion théorique sur l'invisibilité. Il fallait produire sur la genèse d'un organe un traité d'une technicité indiscutable, à la mesure de celle du traité de Haller sur le cœur et de ses observations ultérieures.

Ce sera le *De formatione intestinorum*, qui fera ainsi de Saint-Pétersbourg un des berceaux de l'embryologie européenne. Wolff nous propose un modèle de différentiation qu'on ne peut définitivement plus assimiler à celui d'Aristote et de William Harvey. S'il rectifie l'ancienne épigenèse, c'est que « la formation s'opère par une série de créations distinctes, créations de systèmes (nerveux, musculaires digestifs, etc.) plutôt que d'organes isolés, chacun d'eux formant un tout relativement autonome[7] ». L'épigenèse est d'emblée celle de systèmes connectifs et non d'organes. Dès lors l'objection de Haller concernant la solidarité des organes ne concerne plus Wolff.

L'autre concept-clé est celui d'ébauches ou d'organes embryonnaires transitoires. Les feuilles intestinales qui formeront les parois du tube intestinal fusionnent, et forment une cavité entre elles pour produire toutes les parties du tube intestinal se tenant entre le duodénum et le rectum. Un espace apparaît entre les deux feuillets de la soudure pour former la lumière du canal alimentaire. « Je le demande donc, dit Wolff, ces feuilles sont-elles l'intestin complet ? Personne assurément ne l'affirmera. Je conclus donc à partir de là que les parties n'ont pas toujours existé complètes et formées, mais qu'elles se sont formées en un temps déterminé après la conception[8]. »

Reste la question de la force essentielle. Pour être expliquée, l'épigenèse réclame à la fois une séquence causale en termes mécanistes, doit être dirigée vers un but. Wolff est ici placé devant un problème apparemment insoluble. À son initiative, en 1782, l'Académie des Sciences de Saint-Pétersbourg met la question au concours. Les réponses de Johann Friedrich Blumenbach, Carl Friedrich von Born, et celle

7. G. Canguilhem, G. Lapassade, J. Piquemal et J. Ulmann, *Du développement à l'évolution au XIX^e^ siècle*, Paris, PUF, 1962.

8. C. F. Wolff, « De formatione intestinorum... », art. cit., § 155.

de Wolff sont publiées ensemble en 1789[9]. Wolff y développe des vues vitalistes assez proches de celles de Blumenbach. Mais Wolff conserve pour la *vis essentialis* l'idée d'une force mécanique dépourvue elle-même de pouvoir téléologique, s'appliquant à la matière de l'embryon qui possède une prédisposition à la future organisation. Alors que pour Blumenbach, la *nisus formativus* ou *Bildungstrieb* est elle-même à l'origine de l'organisation, force formative immanente et inhérente à la matière embryonnaire : l'organisation doit être acceptée comme un fait émergent résultant de cette force.

II. LA CONTRIBUTION FRANÇAISE : UN PEU PLUS QU'UN « GRAIN DE SEL » ?

En dépit de ces différences, la force vitale est spécifique du vivant, ce qui disqualifie le modèle d'explication physique cartésien et garantit en droit l'autonomie de l'embryologie comme science du vivant. Après Wolff, les recherches empiriques sur les mécanismes de cette nouvelle « préformation générique » selon l'expression de Kant devenaient possibles[10]. Cependant de son vivant, Wolff aura peu d'appuis, à part ceux de Jiří Procháska[11] et Blumenbach, et encore concernent-ils surtout la question du vitalisme. Ce n'est qu'après la mort de Wolff, que dans les pays germanophones, la préexistence tendra à être abandonnée et que les vues épigénétiques se répandront de plus en plus. Les spéculations concernant les forces vitales se poursuivent, stimulées par la *Naturphilosophie*. Par ailleurs, poursuivant la tradition inaugurée par Wolff de l'observation minutieuse mais délaissant les spéculations sur les causes formatrices et la source de l'organisation, Christian Pander

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9. J. F. Blumenbach et C. F. Born, *Zwei Abhandlungen über die Nutritionskraft welche von der Kayserlichen Academie der Wissenschaften in St. Petersburg den Preis gethheilt erhalten haben. Nebst einer fernern Erlaüterung eben derselben Materie von C. F. Wolff*, Saint-Pétersbourg, Kayserlichen Academie der Wissenschaften, 1789 ; C. F. Wolff, *Von der eigenthümlichen und wesentlichen Kraft der vegetabilischen, sowohl als auch der animalischen Substanz*, Saint-Petersbourg, Kayserliche Academie der Wissenschaften, 1789.

10. E. Kant, *Kritik der Urteilskraft*, Berlin und Libau, Lagarde und Friederich, 1790, § 81. Voir J.-C. Dupont, « Pre-kantian revival of epigenesist », in P. Huneman (éd.), *Understanding purpose : Kant and the philosophy of biology*, Rochester, University of Rochester Press, 2007, p. 37-42.

11. J. Procháska, qui s'est intéressé au développement du système nerveux, prônait l'idée d'une force vitale galvanique présente dans les nerfs (*vis nervosa*) comme cause prédisposante dont l'action nécessite la présence d'un stimulus.

et Karl von Baer ouvrent bientôt à l'embryologie épigénétique de nouveaux horizons[12]. L'épigenèse wolffienne sera rectifiée sur la question des feuillets embryonnaires[13], et des lumières nouvelles sont apportées sur la formation du blastoderme. Ainsi Wolff décrit bien les enveloppes de l'embryon et la continuité membranaire avec l'intestin, mais le lien génétique entre l'embryon et ses enveloppes n'est pas encore précisé. Avec Pander et von Baer, le blastoderme deviendra une membrane germinale à partir de laquelle les enveloppes embryonnaires vont se développer, résultant de la croissance extraembryonnaire des feuillets. Les feuillets germinatifs, dont le nombre sera discuté jusqu'à Robert Remak sont consacrés comme les structures intermédiaires qui donnent naissance à une série d'organes. Remak réinterprétera plus tard la théorie des trois feuillets embryonnaires dans le cadre de la théorie cellulaire (1855).

On dispose désormais de données assez nombreuses sur l'embryologie épigénétique de cette période, attestant toutes d'une circulation intense des hommes et des idées entre les mondes germaniques et russes. En témoignent ainsi les vicissitudes des carrières de Pander, von Baer et Bojanus, pour ne citer que ces auteurs. Toutefois la situation de l'épigenèse au début du XIXe siècle diffère quelque peu selon les pays, ce qui explique peut-être la destinée scientifique particulière du médecin français Louis Sébastien Tredern de Lézérec (1780-1818).

La thèse de Tredern sur l'histoire de l'œuf aviaire et de son incubation (1808)[14] fut retrouvée à Würzburg par von Baer lui-même, alors qu'il recherchait avec Christian Pander des travaux précédents sur l'embryologie du poulet. Von Baer ne tarira pas d'éloge sur ce travail,

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12. Voir J.-C. Dupont et S. Schmitt (éd.), *Du feuillet au gène. Une histoire de l'embryologie moderne (fin XVIIIe-XXe siècle)*, Paris, Éditions Rue d'Ulm, 2004 ; S. Schmitt (éd.), *Les textes embryologiques de Christian Heinrich Pander*, Turnhout, Brepols, 2003.

13. Notons que pour Wolff, la *lamina*, que l'on a traduit ici par « feuillet », est seulement la feuille embryonnaire à partir de laquelle se constitue l'intestin. Mais pour Pander et von Baer, les feuillets germinatifs représentent l'état primitif du germe dans sa totalité. Ces feuillets, par des mouvements de plis sont à l'origine des futures structures organiques. Les concepts de lamina chez Wolff et de feuillets germinatifs de l'embryologie ultérieure ne sont donc pas tout à fait identiques. Nous remercions J.-L. Fischer pour la remarque.

14. L. S. Tredern, *Dissertatio inauguralis medica sistens ovi avium historiae et incubationis et incubationis prodromum*, Jenae, Literis Etzdorfii, 1808.

et publiera même une petite biographie que d'autres données viendront par la suite éclairer[15].

Son père, le capitaine Jean Louis Tredern de Lézérec, noble et militaire breton, avait émigré à la Révolution à Saint-Pétersbourg, emmenant Louis Sébastien. Ce dernier s'engage dans une carrière dans la marine russe basée à Reval (aujourd'hui Tallin, en Estonie) jusqu'en 1801. Il séjourne en France, et en 1804, entreprend des études de médecine à l'université de Würzburg. Il suit entre autres les cours d'Ignaz Döllinger, qui aura aussi pour élève Ernst von Baer, Christian Pander et Lorenz Oken. Tredern part en 1807 à Göttingen où il rencontre Johann Friedrich Blumenbach à qui il présente ses travaux et ses dessins et demande un soutien. Peut-être attiré par Oken, c'est finalement à Iéna qu'il soutient finalement sa thèse de médecine en 1808. Il retourne un temps à Göttingen, puis rentre en France en 1809. Il abandonne alors ses travaux d'embryologie pour soutenir à Paris une seconde thèse qui doit lui permettre d'exercer la médecine dans son pays d'origine et qui a trait à l'organisation hospitalière (1811). Il devient alors assistant-bibliothécaire à la bibliothèque Mazarine, et exerce comme expert médical auprès des tribunaux. Il part en Guadeloupe en 1817 où il meurt de la fièvre jaune l'année suivante.

Deux questions se posent alors. Pourquoi von Baer s'intéresse-t-il à cet auteur assez obscur que l'on ne peut certes comparer à Wolff, et s'attache-t-il à son unique écrit ? Pourquoi Tredern abandonne-t-il l'embryologie après sa première thèse ?

Il apparaît que les réponses tiennent toutes deux à la situation de l'épigenèse à cette époque. La thèse de 1808 est une description brève, mais magnifiquement illustrée du développement de l'œuf de poulet. Tredern y utilise la technique d'incubation que reprendra Pander pour sa propre thèse en 1817. Il y décrit tout particulièrement le développement de la face, du bec et des pattes, peu étudié par son prédécesseur Wolff, et en tire de nouveaux arguments en faveur de l'épigenèse. Pour von Baer, l'embryologie de Tredern produit des arguments raffinés en faveur de l'épigenèse, poursuivant la tradition wolffienne

15. L. Stieda, *Der Embryologe Sebastian Graf von Tredern und seine Abhandlung über das Hühnerei*, Wiesbaden, J.-F. Bergmann, 1901 ; J.-C. Beetschen, « Louis Sébastien Tredern de Lézérec, a forgotten pioneer of chick embryology », *International Journal of Developmental Biology*, 39, 1995, p. 299-308. Voir aussi J.-C. Beetschen et P. Baudrier, « New insight into life and death of the self-styled estonian embryologist, Louis Sébastien Marie de Tredern de Lézérec (1780-1818) », *Trames*, 14, 64/59, 2010, p. 107-119.

de la description fine et l'éloignant de conceptions simplistes. Mais selon von Baer, Tredern éloigne aussi l'embryologie épigénétique des conceptions trop spéculatives et trop théoriques des auteurs allemands, trop influencés selon lui par la *Naturphilosophie.* Il indique précocement et clairement la marche à suivre prenant l'allure d'un véritable programme de recherche pour l'embryologie épigénétique.

Cette voie sera effectivement rigoureusement celle de Pander et de von Baer, mais ne sera pourtant pas suivie par Tredern lui-même. La réponse à la seconde question, celle de l'abandon, met en lumière la situation différente de l'épigenèse en France et en Allemagne. L'idée de la préexistence en France domine encore sous le Premier Empire et même pendant la Restauration, en réaction contre l'épigenèse prônée en Allemagne par les tenants de la *Naturphilosophie*, considérée comme trop spéculative. Telle sera par exemple la position de Cuvier, pourtant lui aussi de culture franco-germanique. À l'appui de cette thèse, Jean-Claude Beetschen invoque les ouvrages utilisés pour enseigner l'embryologie en France : celui d'André-Marie-Constant Duméril (1804-1807) et surtout celui d'Anthelme Richerand (1801-1833), partisan de la préexistence oviste qui subit l'influence de Haller, Spallanzani et Bonnet[16]. Richerand cite les travaux de Jean-Louis Prévost et Jean-Baptiste Dumas de 1824, auxquels il attribue la découverte de l'ovule des mammifères, et non le mémoire Von Baer sur le sujet, pourtant traduit en français par Gilbert Breschet en 1829. Il n'adoptera que tardivement des vues épigénétiques voisines de celles de Meckel (1833). Bien que cette attitude conservatrice n'ait pas été absolument partagée (cas de Breschet), l'épigenèse ne s'imposera pas dans l'enseignement médical. À la Faculté des Sciences et au Muséum, elle est soutenue par Étienne Geoffroy Saint-Hilaire suivi par Antoine Étienne Serres. Mais ceux-ci restent avant tout préoccupés d'anatomie comparée et de transformisme, sur lesquels ils s'opposent aux vues de Cuvier. Le grand ouvrage tératologique d'Isidore Geoffroy Saint-Hilaire ne paraîtra qu'entre 1832 et 1837. Dans la France médicale du moins, la préexistence se maintient plus tardivement que dans les pays germanophones. Vers 1810, la thèse du découragement institutionnel de Tredern est donc tout à fait plausible.

16. Voir J.-C. Beetschen, art. cit., pour la référence à ces ouvrages.

CONCLUSION

Le « miracle baltique » fut reconnu par son artisan le plus fameux, Karl von Baer, comme le résultat d'une tradition initiée par Wolff, un Berlinois qui fit carrière à Saint-Pétersbourg, et relayée par Tredern, un Français de Saint-Pétersbourg et Reval formé à l'université allemande. Tredern ancra un peu plus l'histoire de l'embryologie dans un espace baltique transnational qui deviendra le berceau de l'embryologie européenne. Il reste à comprendre plus avant les conditions matérielles et intellectuelles très particulières qui permirent cette inscription dans l'espace baltique de la rupture opérée par Wolff. D'ores et déjà, il est difficile d'assimiler « l'école de la Baltique », soit comme le fait Léonid Blyakher tout entière à une « école russe »[17], soit de n'en retenir que l'inspiration germanique par ailleurs incontestable[18], soit encore d'en faire les premiers linéaments d'une biologie revendiquée comme « balte »[19]. Dans les trois cas, les apports extérieurs à cet espace « clos » se retrouveraient à tort réduits à un grain de sel. La construction de la théorie épigénétique impliqua en effet des acteurs de toute l'Europe, parmi lesquels nombre d'auteurs francophones ou d'origine française (Maupertuis, Buffon, Bonnet, Cuvier, E. et I. Geoffroy Saint-Hilaire, Tredern, Bojanus…). Les régions européennes où les influences culturelles sont les plus intriquées et les vicissitudes historiques sont les plus complexes cristallisent ainsi des lieux singuliers de découvertes qui doivent être comprises dans le cadre de mouvements historiques plus larges des idées scientifiques.

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17. L. Y. Blyakher, *History of embryology in Russia, from the middle of the eighteenth to the middle of the nineteenth century*, Washington, Smithsonian Institution and the National Science Foundation, 1982.

18. T. Schmuck, *Baltische Genesis : Die Grundlegung der Embryologie im 19. Jahrhundert*, Aachen, Shaker, 2010 ; O. Riha, T. Schmuck, « Das Baltikum als Wiege der Embryologie. Kontingenzen eines transnationalen Wissenschaftsraums », *Würzburger Medizinhistorische Mitteilungen*, 29, 2010, p. 208-240.

19. Voir à ce sujet l'intéressant numéro consacré aux villes baltiques dirigé par Michel Espagne et Thomas Serrier dans la *Revue Germanique Internationale*, 11, 2010.

III

The impact of Georges Cuvier's and Jean-Baptiste Lamarck's ideas upon the development of evolutionary theory in Russia: 1800-1950

Eduard I. Kolchinsky

INTRODUCTION

Prominent French naturalists J.-B. Lamarck (1744-1825) and Georges Cuvier (1769-1832) took antagonistic stances in interpreting the fundamental problems of evolutionary biology and historical geology. Cuvier was a proponent of Catastrophism, a believer in the stability of species, while Lamarck developed the first consistent theory of evolution of life forms and was one of the founders of Uniformism and Actualism. Even the death of his major opponent did not placate Cuvier who wrote his "Eloge de M. de Lamarck" in such a way that the editorial board suggested that he rewrote it.[1] Cuvier refused and his *Eloge* became publicly known only in the aftermath of his death in 1832. It became the main source of information on Lamarck's life and work.

Later on many Russian historians of biology pointed out that the unwillingness of Russian authors to adopt Lamarck's ideas was determined by Cuvier's dislike of Lamarck.[2] However, the influence of Cuvier cannot entirely account for the fact that for many decades the Russian academic community was ignoring Lamarck and his works,

1. G. Cuvier, "Eloge de M. de Lamarck," *Mémoires de l'Académie Royale des sciences de l'Institut de France*, XIII, 1835, p. i-xxxi.

2. L. N. Seravin, "Pokhval'noe slovo Zhanu Batistu Lamarku," *Vestnik Sankt-Petrburgskogo universiteta*, 3/4, 24, 1994, p. 3-17, p. 4.

nor for a tremendous upsurge of Lamarck's popularity in the first half of the 20th century.

In the paper we will demonstrate that the reception of Georges Cuvier and Jean-Baptiste Lamarck by the Russian academic community was in many ways determined by the extent to which Russian biologists were prepared to adapt these ideas in accordance with national academic traditions and the state of evolutionary theory on a world scale. Our observations will be based on the analysis of all the Russian translations and publications of the works by Cuvier and Lamarck, and on the books and articles devoted to them in Russia. We will also explore the impact exerted by dominant paradigms and practices on the various branches of evolutionary biology.

II. RECEPTION OF GEORGES CUVIER: BETWEEN CREATIONISM AND NEO-CATASTROPHISM

The early reception of Cuvier and Lamarck in Russia was determined by traditions established by those scholars who had been working for the Imperial Academy of Sciences in Saint Petersburg, particularly by Peter Simon Pallas (1731-1811). In 1777, when addressing the meeting of the Academy, Pallas suggested a hypothesis which explained a long history of Earth, and the origins of mountains.[3] Later, Cuvier considered this speech as the foundation of stratigraphy and historical geology. Three years later, Pallas delivered a speech to the Academy again, while arguing against the idea of the transformation of species.[4] His arguments formed the foundations of biological Creationism.

Cuvier also used them in 1812, in his introduction to his four-volume *Recherches sur les ossements fossiles de quadrupèdes* (vol. 1). Its expanded version was later reprinted as a separate book under the title *Discours sur les révolutions de la surface du globe*.

Cuvier's major results were rapidly acknowledged in Russia, achieving the status of basic scientific axioms, and being integrated in textbooks. For example, a Russian translation of a French edition

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3. P. S. Pallas, "Observations sur la formation des montagnes et les changements arrivés au globe, particulièrement à l'égard de l'Empire russe; lues à l'Assemblée de l'Académie Impériale des Sciences le 23 Juin 1777," *Acta Academiae Scientiarum Imperialis Petropolitanae*, 1, 1778, p. 21-64.

4. *Id.*, "Mémoire sur la variation des animaux; Première partie, lue à l'Assemblée publique du 19 Septembre 1780, en présence de Msgr. Le Prince Royal de Prusse," *Acta Academiae Scientiarum Imperialis Petropolitanae*, 2, 1784, p. 69-102.

of the *Album on natural history for children*[5] contained more than 500 engravings, which were taken from manuals by Georges-Louis Leclerc Buffon (1707-1788), Bernard Germain Etienne de Lacépède (1756-1825), Georges Cuvier and other naturalists. In 1891, a few engravings from Cuvier were reprinted in an *Atlas for children* composed by Estonian illustrator Eduard Jakobson (1847-1903).[6] Dmitrii Mikhailov's (1824-1890) *Course of natural history* was very popular in Russia in the mid-19th century. It was reprinted eight times (1860-1880). Its supplement, a *Zoological atlas for schools*, contained more than 500 images of various animals,[7] many of which were taken from Cuvier's atlases.

Cuvier's *Discours sur les révolutions de la surface du globe* was published twice in Russia with an interval of about a century. For the first time it was translated by an inspector of elementary schools in Odessa, Timoffei Dymchevich, who used the fourth French edition.[8] The second translation was published in 1939, edited by a leading Russian paleontologist, Aleksei Borissiak (1972-1944).[9] By that time, the Soviet ideology was actively promoting Frederick Engels's judgment of Cuvier and his theory, which was defined as "revolutionary in phrase and reactionary in substance." Therefore the editorial foreword to the second Russian translation warned its readers that Cuvier's theory had been invented to rescue an erroneous doctrine of the stability of species. The publication of this book, at the time when the Stalinist terror reached its peak, was an act of real courage. Aleksei Borissiak wrote a wonderful foreword, in which he analysed Cuvier's biography and his work, and showed their role in the elimination of naïve transformist ideas. Borissiak spoke against a common misperception of Cuvier's theory of catastrophes, as if Cuvier had implicitly acknowledged a multiplicity of the acts of creation.

The first Cuvier's biography was published in Russia in 1835. It appeared in the first Russian magazine that reached a rather sufficiently

5. *Al'bom natural'noi istorii dlia detei*, Moscow, tip. Stepanov, 1843.

6. E. M. Jakobson, *Atlas dlia detei*, Riga, 1892.

7. D. S. Mikhailov, *Uchebnyi zoologicheskii atlas*, Saint Petersburg, tip. I. Ogrizko, 1861.

8. Baron Cuvier, *O perevorotakh ili izmeneniiakh na poverkhnosti zemnogo shara v estestvennoopisatel'nom i istoricheskom otnoshenii*, Odessa, Gorodskaia tipographiia, 1840.

9. G. Cuvier, *Rassuzhdeniia o perevorotahk na poverhknosti zemnogo shara*, Moscow, Biomedgiz, 1937.

broad audience, the *Library for reading.*[10] An unknown author based his essay on French sources: G. Laurillard and P. Flourens (1833-1835). This biography has never been mentioned in the subsequent research on Cuvier in Russia. The first original book on Cuvier in Russia was published in the late 19th century, in a biographical series "The life of famous people."[11] It was written by Mikhail A. Engelgardt (1861-1915) who was known for his translations and popularization of science. The author, while acknowledging Cuvier's contribution to the expanding palaeontological evidence of evolution, nevertheless characterized his catastrophic theory as "monstrous."[12] The last (third) biography of Cuvier was written 85 years later by Ivan I. Kanaev (1893-1984), a historian of biology.[13] Kanaev gave a detailed analysis of all the major works by Cuvier and came to a conclusion: "[…] Cuvier, with his anti-evolutionary views, did more for the evolutionary theory than his contemporaries, who held evolutionary beliefs […] Lamarck, Geoffroy Saint-Hilaire are of lesser importance."[14] His conclusions can be seen as a summary of the debates on Cuvier which had been going around among Russian biologists. Both Engelgardt and Kanaev based their accounts mainly on the French sources and did not address the reception of Cuvier's works in Russia.

In the same year, when the evolutionary theory was expounded by Darwin, the Kazan University published, as a separate book, a speech by zoologist Nikolai Petrovich Wagner (1829-1907) which was devoted to Georges Cuvier and his major opponent in the debates on the changeability of species and Geoffroy Saint-Hilaire.[15] Wagner clearly sympathized with Cuvier. In the 1870s, there were plans to publish a book on Cuvier, in a book series entitled "Famous naturalists," which did not materialize, perhaps because the evolutionary theory captivated the attention of the Russian public.

However, soon after, the non-Darwinian concepts of evolution, including neo-Catastrophism, gained credibility in Russia. Adapting Cuvier's ideas to the evolutionary paradigm, Russian supporters

10. *Biblioteka dlia cheniia*, 1, 1834.

11. M. A. Engelgardt, *Georges Cuvier. Ego zhizn' i nauchnaia deiatel'nost*, Saint Petersburg, Obshchestvennaia pol'za, 1891.

12. *Ibid.*, p. 55.

13. I. I. Kanaev, *Georges Cuvier*, Leningrad, Nauka, 1976.

14. *Ibid.*, p. 195.

15. N. P. Vagner, *Georges Cuvier and Geoffroy Saint-Hilaire*, Kazan, tip. Universiteta, 1860.

of neo-Catastrophism wrote about long periods, when taxa and biological communities led to a stable existence interchanged with short periods of their mass transformations. Many Russian paleontologists and geologists offered their hypotheses suggesting that global physical-geographic or cosmic factors suddenly affected the organic world, radically changing its florae and faunae. Especially popular were the hypotheses about geological revolutions advanced by a palaeontologist, Dmitrii N. Sobolev (1872-1949), a botanist, Nikolai I. Kuznetsov (1864-1932), a geologist, Boris L. Lichkov (1888-1966); global climate changes – advanced by an ornithologist Piotr P. Suchkin (1868-1928), a palaeontologist Nikolai N. Yakovlev (1870-1966); about global poisoning – proposed by paleontologists Aleksei P. Pavlov (1854-1929) and Maria V. Pavlova (1854-1938) and about changes in the insolation of the Earth's surface by botanist Mihkail I. Golenkin (1864-1941).

In the crisis years of Darwinism, Dmitrii N. Sobolev, a geologist and paleontologist from Khar'kov, attempted to develop a synthetic theory of neo-Catastrophism, which he called "historical biogenetics." He tried to combine Lamarck's evolutionary principle with Cuvier's revolutionary principle; that is to combine Gradualism with Catastrophism by assigning them to different stages of the biological cycle.[16] By linking together the processes of tectogenesis, orogenesis, volcanism and metallogeny with the transformations that took place on the Earth's surface under the influence of climate, hydrosphere and biosphere, Sobolev in fact developed a concept of Earth's global evolution related to cosmic rhythms.

Soon after, academic debates became entangled with ideological campaigns. Cuvier's theory was not exempted: Cuvier was accused of incorrect "interpretation of facts" (Karl Marx), of suggesting the idea of the multiple acts of creation, and of "making the miracle an essential natural agent" (Frederick Engels). These casual remarks made by the classics of Marxism became the dogma in the Soviet literature. Cuvier was criticized for his extreme empiricism, metaphysics, mechanicism, and idealism.

Nevertheless, many Soviet biologists rejected the notion that Catastrophism was intrinsically linked to Creationism; they even suggested that Cuvier's ideas could be seen as an early version

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16. D. N. Sobilev, *Nachala istoricheskoi biogenetiki*, Kharkov, Gosizdat, 1924.

of evolutionary theory.[17] In the course of time a more balanced view was adopted, suggesting that Cuvier created a theory of sudden changes in the florae and faunae which took place in a limited area. Therefore this theory could be properly defined as a theory of biosphere evolution with the immutability of species. Thus, Cuvier's theory formed the starting point for the making of many modern concepts of Saltationism and neo-Catastrophism.[18]

II. RECEPTION OF LAMARCK'S IDEAS BY RUSSIAN BIOLOGISTS

Contrary to the reception of the ideas of Cuvier, it took almost a century for the ideas of J.-B. Lamarck to be accepted by the Russian academic community. His works became known in Russia only in the mid-19th century, thanks to zoologist Karl (Charles) Rouillier (1814-1858). However it was only after the victory of Darwin's theory that Lamarck was rescued from oblivion and became the banner of the competing school of thought.

When the sixth edition of the *Origins of species* was released – the edition, in which Darwin partially acknowledged that Lamarck had been right in his conclusions – Russian biologists developed a habit of ignoring his ideas about autonomous causes of progress, while they paid particular attention to his views on the functional basis of animal morphogenesis and on the influence of climate and soils upon the development of plants. Following these statements, botanists Andrei N. Beketov (1825-1902), Kliment A. Timiriazev (1843-1920), Viacheslav R. Zalensky (1875-1923) considered Lamarck's key idea about the inheritance of acquired traits as an important contribution to the theory of natural selection. Even earlier, the Lamarckian principle of gradation became the foundation for all sorts of theological and teleological concepts of evolution, in which the idea of teleological evolution was combined with Saltationism (concepts proposed by embryologist Karl Ernest von Baer [1792-1876] and ichthyologist Nikolai Ja. (Yakovlevitch] Danilevskii [1822-1885]).

17. B. N. Lichkov, *K osnovam sovremmennoi teorii zemli*, Leningrad, Izdatel'stvo universiteta, 1965.

18. K. M. Zavadskii and E. I. Kolchinskii, *Evolutsiia evolutsii*, Leningrad, Nauka, 1977.

All these factors affected the Russian translations of J.-B. Lamarck initiated by biologist and educator, Piotr F. Lesgaft [1837-1909). He edited the Russian translation of Lamarck's *Système analytique des connaissances positives de l'homme* made by educator Valerian Polovtsov (1862-1918) and his young wife, philosopher Varvara Simanovskaia (1877-1936), in 1899.

In 1911, after a century of delay, the major work of Lamarck was published in Russian, translated by S. V. Sapozhnikov.[19] It was edited by a historian of science, Vladimir P. Karpov (1870-1943). This Russian translation was used in a subsequent two-volume publication of *Philosophie zoologique* (1935-1937), edited by V. P. Karpov, who also wrote Lamarck's biography, while introductory articles for the book were written by a future president of the Academy of Sciences of the USSR, botanist Vladimir Komarov (1969-1935) (for the first volume) and zoologist Ilia Poliakov (1912-1992) (for the second volume). Two different forewords to the same edition can be explained by a profound split among Soviet biologists in the 1920s – 1930s: they took different views on the inheritance of acquired traits. V. L. Komarov strove to emphasize positive aspects of Lamarck's legacy accepting that acquired traits could possibly be inherited. Poliakov, on the opposite, harshly criticized Lamarck and his numerous followers.

However after the 1948 session of the All-Union Academy of Agricultural Sciences (VASKhNIL), Poliakov was forced to change his views when the Academy of Sciences embarked on preparing a new authoritative edition of Lamarck's *Selected works* (1955-1959), which he edited jointly with a geneticist, N. I. Nuzhdin, who shared the ideas of Trofim Lysenko. A new translation of Lamarck's *Philosophie zoologique* was needed to present Lamarck as a great predecessor, not only of Darwin, but also of the "Soviet creative Darwinism," that is Lysenkoism. The translation considerably distorted the original. Later Poliakov himself regretted his participation in this project.

The first book on Lamarck and his theory appeared in Russian in 1911, at the time when evolutionary theory underwent a crisis. It was a Russian translation of a German book by A. Wagner.[20] The book was openly apologetic. It was soon followed by a flood of academic, popular and quasi-scholarly biographies of Lamarck written by Russian authors. It reflected the acuteness of the debates waged on Lamarck. In some

19. J. B. Lamark, *Pholosophia zoologii*, Moscow, Nauka, 1911.
20. A. Vagner, *Novyi kurs v biologii*, Moscow, Obrazovanie, 1911.

of these publications, Lamarck was sharply criticized for postulating the immanent expediency of a living matter and the inheritance of acquired traits.[21] Others, who followed the tradition established by P. F. Lesgaft and V. V. Polovtsov, considered these ideas as Lamarck's major contribution to science, or at least, they strove to prove that his views were compatible with contemporary evolutionary theory. Among the latters were the founder of the Darwin Museum in Moscow and V. L. Komarov.[22] Nevertheless Lamarck never enjoyed unqualified support in Russia. Russian Lamarckists considered themselves as Darwinists and advocated the synthesis of the concept of natural selection and the principle of inheritance of acquired traits.

In the mid-1920s ideological arguments were called upon to support this view. According to an author of a secondary-school textbook on evolutionary theory, Fedor F. Duchinskii (1884-?), a refusal to acknowledge the inheritance of acquired traits meant a revision of Darwinism, and that would ultimately make a consistent materialist explanation of evolution impossible and lead to a revision of the "very foundations of Marxism."[23] A Marxist physiologist, Boris M. Zavadovsky (1895-1951) called for the synthesis of Darwinism and Lamarckism that would become the basis of Marxist philosophy.[24] His appeal found an expression in a curious misprint "Lamarxism."

This position was harshly criticized by younger Marxist geneticists Izrail I. Agol (1891-1937), Nikolai P. Dubinin (1906-1998), Solomon G. Levin (1894-1938), Vasilii N. Slepkov (1902-1937), Alexandr S. Serebrovskii (1892-1948) and a few others, who stigmatized Lamarkism for its metaphysics, mechanicism, implicit teleology, vitalism, etc. Most of them perished in the Great Terror.

By the early 1930s the academic debates had come to their end. The discovery of artificial mutagenesis supported the impression that "Lamarckism belongs to the past."[25] However, by that time, in the USSR, the fate of a scientific hypothesis depended on Stalin who had

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21. V. A. Safonov, *Lamark i Darwin*, Moscow, Molodaia Gvardiia, 1930; M. M. Beliaev, *Lamark*, Moscow, Uchpedgiz, 1936.

22. V. L. Komarov, *Lamark*, Moscow, Leningrad, Gosizdat, 1925.

23. F. F. Duchinskii, "Darwinizm, Lamarkizm i Neo-Darwinizm," *Pod znamenem Marksizma*, 7-8, 1926, p. 101-121, p. 106.

24. B. M. Zavadovskii, "Darwinizsm, lamarkizm i nasledovanie priobretennykh priznakov," *Pod znamenem marksizma*, 10-11, 1925, p. 79-114, p. 113.

25. M. M. Mestergazi, *Osnovnye problemy organicheskoi evoliutsii*, Moscow, Kommunisticheskaia Akademiia, 1930, p. 153.

been sympathizing with Lamarckism since 1905. As a result, Lamarckism became one of the main theoretical foundations of Lysenkoism, and its many critics were persecuted. Trofim D. Lysenko and his main ideologist Isai I. Prezent exploited Lamarck's theoretical and philosophical principles for stigmatizing Darwinists and earning an approbation of the Soviet authorities. In this way Lamarckism became discredited in the eyes of the Russian academic community for many years to come.

The number of publications on Lamarck reached its peak in the late 1940s – early 1960s – the time when Lysenkoism was dominant in the Soviet Union.[26] Most authors emphasized the greatness of Lamarck as the scholar who proposed the first theory of evolution. With the fall of Lysenkoism most authors adopted a very critical attitude towards Lamarck; we could find only three publications in which Lamarck and his work are considered in a positive manner.[27] Nowadays, only a historian of science, Yurii Viktorovich Chaikovsky, is still trying to prove that Lamarck's ideas have retained their significance for contemporary biological research.[28]

CONCLUSION

The history of the uneasy reception of Cuvier and Lamarck in Russia confirms that, despite a whole complex of social and cultural factors that affected the reception, Russian scholars were committed to universal norms and values of modern science – a decisive factor in a long-term historical perspective.

Unlike Lamarck, Cuvier always believed that established facts and generalizations based on them were more important for science

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26. E.g. I. I. Puzanov, *Zhan Batist Lamark*, Moscow [1947], Uchpedgiz, 1959 [2nd ed.]; S. S. Stankov, *Linné, Rousseau: Ocherk o botanicheskikh rabotakh*, Moscow, Sovetskaia nauka, 1955; N. I. Nuzhdin, *Lamark, Darwin i sovremennaia biologia*, Moscow, Sel'khozizdat, 1959; I. I. Prezent, *Biolog-materialist Zhan-Batist Lamark*, Moscow, Akademia nauk, 1960; V. M. Korsunskaia, *Podvig zhizni sheval'e de Lamarka*, Moscow, Detgiz, 1961; I. M. Poliiakov, *Zhan-Batist Lamark i uchenie ob evolutsii organicheskogo mira*, Moscow, Vysshaia shkola, 1962; N. M. Shapochka, *Evolyutsionnoe uchenie Lamarka*, Moscow, Moscow University, 1963.

27. G. V. Gegamian, "Lamark, Vernadskii i biosferologiya," *Priroda*, 9, 1981, p. 78-81; L. N. Seravin, "Pokhval'noe slovo Zhanu Batistu Lamarku," *Vestnik Sankt-Petrburgskogo universiteta*, 3/4, 24, 1994, p. 3-17; E. A. Aronova, "Darwinizm, Lamarkism ili chto-to mezhdu nimi," *Priroda*, 4, 2005, p. 75-77.

28. I. V. Chaikovsky, "Jubilei Lamarka-Darwina I revolyutsiya v immunologii," *Nauka i Zhizn'*, 2-3, 2009.

than a hypothesis that lacked a credible empirical base. That was the reason why, for about two centuries, it was Cuvier, a believer in the immutability of species, and not his opponent Lamarck, who exercised an ongoing influence upon the development of evolutionary ideas in Russia, and who became the source of inspiration for a strong intellectual current, neo-Catastrophism. With the loss of support from the Soviet authorities, Lamarckism virtually lost all its influence in the Russian-speaking part of the world.

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IV

Sources of the Darwinian palaeontology: Vladimir Kovalevsky and Louis Dollo

Igor I. Popov

INTRODUCTION

Russian scientists often noted that, in the 1870s, a new stage in the development of palaeontology was set, that is the formation of Darwinian palaeontology, which is still in use. Russian scientist, Vladimir Kovalevsky (1842-1883), greatly contributed to this "revolution." His activities were rich in extraordinary events, which is why they attracted the attention of historians and writers.[1] The work of Kovalevsky exerted some influence outside Russia; he especially significantly influenced French-Belgian scientist, Louis Dollo (1857-1931).

Historians and biologists usually only glorified the successes of the new Darwinian palaeontology. However, the situation is more complicated since the origination of this palaeontology contributed to researches on evolution and, at the same time, contributed to the penetration of teleology into biology. It implemented the ancient idea that, in order to understand a phenomenon, it is necessary to reveal its rational purpose which it originated from. With some delay, a paradox appeared in biology: on the one hand, teleology contradicts that science is close to religion, but on the other hand, it is actively used in evolutionary studies. Modern evolutionists still analyse such

1. Y. M. Gall and V. Kovalevsky "The variation of animals and plants under domestication" [Vladimir Kovalevsky as translater and editor of Darwin's treatise], *Vestnik VOGiS*, 11, 1, 2007, p. 40-44; S. E. Reznik, *Vladimir Kovalevsky (tragedia nigilista)* [*Vladimir Kovalevsky. Tragedy of nigilist*], Moskva, Molodaya gvardia, 1978; D. V. Todes, *V. O. Kovalevsky: vozniknoveniye, soderzhaniye i vospriatiye ego rabot po paleontologii* [*origination, contents and acceptance of his works on paleontology*], Saint Petersburg, Nestor-Historia, 2005; E. E. Milanovsky and V. O. Kowalevsky, *Vestnik Moskovskogo universiteta*, 5, 2003, p. 69-74 and 2, 2004, p. 81-88.

a paradox. Some authors claim that Darwin, or his active advocates, were real teleologists,[2] while the others claim that it is just an illusion.[3] Addressing the works of Kovalevsky and Dollo will in a way clarify the sources of this discussion.

I. LIFE OF VLADIMIR KOVALEVSKY

Vladimir Kovalevsky was born in 1842 in Latvia. He had secondary and higher education in Saint Petersburg, studying in a private school, and then in a college of jurisprudence. Kovalevsky mastered the main European languages, and this knowledge greatly influenced his following activities. While a student, he took an interest in natural sciences and especially in chemistry. His brother, Alexander Kovalevsky, studied natural sciences, and later became an eminent zoologist and embryologist. After the completion of his education, Vladimir Kovalevsky was assigned to the department of heraldry of senate, but he did not work there, asking for a sick leave to Europe. Later he lived in various European countries perfecting his education and looking for an occupation to engage in.

The self-reliant life of Kovalevsky started as some liberalization took place in Russia. In 1855, Tsar Nikolay I died. He personified reaction, strong power, discipline, severity, absolutism, etc. The next Tsar, Alexander II, turned out to be liberal. He completed various progressive reforms, but educated people were not satisfied, requiring more and more. Their activities provoked the reaction of authorities. Political confrontation became stronger. Kovalevsky belonged to the "progressive" part of society and he participated in the activities of Russian emigrants, but in a careful manner. He was never arrested. Revolutionists paid attention to this fact, they falsely accused him of treachery and his conspiratorial activity stopped.

In 1863, Kovalevsky returned to Saint Petersburg and he decided to become a doctor. He started to attend lectures on anatomy and medicine,

2. J. G. Lennox, "Darwin was a Teleologist," *Biology and Philosophy*, 8, 1993, p. 409-421; J. O. Reiss, *Not by Design: Retiring Darwin's Watchmaker*, Berkeley, California, University of California Press, 2009.

3. E. Mayr, "The idea of teleology," *Journal of the History of Ideas*, 53, 1992, p. 117-135; M. T. Ghiselin, "Darwin's language may seem teleological, but his thinking is another matter," *Biology and Philosophy*, 9, 4, 1994, p. 489-492; F. Ayala, *Teleological explanations in evolutionary biology, Nature's purposes: Analyses of Function and Design in Biology*, Cambridge, MA, The mit Press, 1998.

but he abandoned them soon after, starting to deal with the translation and edition of books. He mainly edited classic treatises of natural sciences. Meanwhile, he visited Europe and met several scientists. He negotiated the edition of their papers. Between 1863 and 1869, he published more than 60 voluminous books. Although they were popular, the commercial benefits of this enterprise were doubtful. Kovalevsky borrowed money too easily, then he was enabled to repay in time. He tried to stop this business and he elaborated new projects. Being a correspondent of Russian newspapers, he participated in the campaigns of Garibaldi in 1866. However, he came back shortly after. Then he moved to Europe again, visited Darwin, impressed him favourably and successfully negotiated the translation and edition of his next treatises in Russia.

In 1868, Kovalevsky married Sofia Korvin-Krukovskaya. Initially it was a marriage of convenience and a fine gesture for getting Sofia and her sister the possibility of a higher education in Europe. At that time, young women were allowed to travel abroad only with relatives or their husbands, and such marriages were sometimes arranged among "progressive" persons. Later Sofia Kovalevskaya (1850-1891) succeeded in science and became a well-known mathematician. Soon after their marriage, Kovalevsky and his wife moved to Europe. The sale of books and his business income, as well as the dowry, probably covered their expenses. After some years, they had a daughter.

Together with his wife, Kovalevsky visited several European scientific centres and he learned various sciences: geology, chemistry, crystallography, mathematics. He continued to bustle and look for a main occupation. At some point, he decided to focus on palaeontology. Such a decision was related to his enthusiasm for Darwin's theory. Since Darwin and his first advocates did not present enough data from fossil records, Kovalevsky decided to fill in this gap. From his point of view, some fossils – like those of molluscs – did not provide any significant information on evolutionary mechanisms, because they do not demonstrate a rapid transformation in the process of adaptation. He looked for a zoological group which could demonstrate this point, and he took an interest in extinct ungulates. He explored various collections, visiting all the significant European museums. Kovalevsky rapidly prepared a thesis on palaeontology and successfully defended it in Jena. The thesis was devoted to detailed research on the genus *Anthracoterium*, primitive ungulates considered as the ancestors of horse, and to the revision of the large family *Hypopotamidae*.

During that period, Kovalevsky lived in poverty and debts. His books sold slowly; so money came in slowly and irregularly. His brother Alexander helped him, but he himself was poor. However Kovalevsky continuously travelled over Europe, bought collection specimens and ran into new debts.

Kovalevsky worked on fossil collections in France during French-German war and the Paris Commune. He turned out to be slightly involved in these events: the sister of his wife Anna was married to the French communard, Viktor Jacklard, who was put in prison after the Commune. Kovalevsky came to Paris with his wife to try to have Jacklard released and to take care of Anna, also threatened with prison. They helped Anna to move abroad, but Jacklard remained in prison. Finally the father of Sofia, an old general who knew Louis-Adolphe Thiers personally, came to Paris and asked for the release of Jacklard. Thiers rejected his demand, but, probably, helped, because Jacklard escaped from prison soon after. The details of these events are not well-known, but no doubt that Kovalevsky was ready to help. Jacklard waited for exile to New Caledonia, and Kovalevsky planned to follow him with their wives.

In 1873, Kovalevsky decided to do a master's degree in Russia, but his first attempt was unsuccessful. He decided that he was able to overcome any academic obstacle and he went on to take exam although in a situation of conflict of interests. For some reason Odessa city was chosen for the exam, while one of the board members was at odds with Kovalevsky and his friends. The situation during the exam was questionable, but Kovalevsky himself required to taking it. His opponent carefully prepared and obtained Kovalevsky's failure. Kovalevsky wrote and published a brochure about this event.

In 1874, Kovalevsky came to Saint Petersburg with his wife and, in 1875, he took an exam successfully. He continued to struggle to cope with business and science starting new commercial activities. Together with his wife, Kovalevsky decided to become rich quickly and to deal with science peacefully afterwards. They bought up building plots in Saint Petersburg, starting development projects (in parallel Kovalevsky worked with a new newspaper), but they went totally bankrupt in 1880. Surprisingly Kovalevsky managed to settle this problem: he was invited to direct a firm dealing with the production of lubricants from petroleum (it was a new and a promising trend of industry) and at the same time he obtained a position in Moscow University. However Kovalevsky continued to bustle and get involved in new affairs.

He invented a new technique for oil transportation: by means of hermetic barrels travelling under the ice in rivers during winter. In such a way Kovalevsky tried to solve the serious economic problem of the seasonality of transportation. Since in Russia rivers are frozen during several months, big breaks in intensive transportation of loads existed. Kovalevsky patented his idea, but no transport under ice was realized.

In 1882, Kovalevsky went to the US to get to know the American petroleum industry and he explored collections of fossils. He met with the greatest American paleontologists, E. Cope and O. Marsch, and admired their rich collections. His knowledge of palaeontology progressed, but his activities did not. The situation in his firm became worse and worse. Nevertheless, being sure of its stability, Kovalevsky raised numerous new loans. He parted with Sofia because he worried about his inability to support the family. His scientific work worsened too. He gave lectures improperly, because he was very often absent. Finally, Kovalevsky complained that his memory was failing, he could no longer remember the contents of his lectures. In 1884, he committed suicide.[4]

Kovalevsky dealt with paleontology for just four years, but he was impressively successful in publishing series of books and articles.[5] His scientific approach was in contrast to the traditional ones. While palaeontologists usually described numerous fossils over years or even decades, Kovalevsky rapidly chose the objects which were interesting to him, and rapidly described them in terms of his scientific concept. In such a way he demonstrated that, when one is clear on objectives and methods, it is possible to get rewarding results rapidly. His style of work often provoked the perplexity of his colleagues, because he continuously moved from museums to museums never stopping for a long time. His scientific carrier also progressed in reverse direction: he first received excellent theoretical knowledge, then learned to work with material, then became a recognized specialist, and only after then went to take exams in Russia. Despite spending a long time in Europe,

4. S. E. Reznik, *op. cit.*; E. E. Milanovsky, "V. O. Kovalevsky," *Vestnik Moskovskogo universiteta*, 5, 2003, p. 69-74 and 2, 2004, p. 81-88.

5. The most significant are the following: V. O. Kovalevsky, "Sur l'Anchitherium auerelianense Cuv. et sur l'histoire paléontologique des chevaux," *Mémoire de l'Académie imperial des Sciences de Saint Petersbourg*, 5, 20, 1873, p. 1-73; *id.*, "On the Osteology of the Hyopotamidae," *Proceedings of the Royal Society of London*, *Philosophical Transactions of the Royal Society of London*, 21, 1873, p. 147-165; *ibid.*, CL XIII, 1874, p. 19-94 (six plates).

he was never going to settle there. He was offered positions in Vienna and other European cities, but he always rejected these invitations. In spite of the disorderliness of his activities, his main aspiration was evident: he continuously tried to contribute to progress and to the enlightenment of his motherland.

II. PALAEONTOLOGY OF KOVALEVSKY

Kovalevsky studied the extinct relatives of horses, trying to prove that they formed a gradual series of ancestors and descendants evolving towards the modern forms. He presented abundant material of bones, describing in detail teeth, legs and other parts of skeletons. In such a way, he rather successfully demonstrated a fact of evolution, but his interpretations of evolutionary mechanisms were not so convincing. He wrote about bones thoughtlessly: in his writings, he perceived the parts of fingers and other bones as communities of persons, which behaved either rationally or not rationally (as they had obligations and rights, concluded agreements with each other or violated them, etc.). Such a "juridical" anatomy was absolutely teleological. It was as if somebody had set up the bones an objective, and they fulfilled it in different ways. The main objective was to adapt mammals to the life in grasslands. Kovalevsky believed that forests were replaced by grasslands over large territories, and this transformation forced animals to evolve: they had to run rapidly and to eat grass.[6] (This scheme is still widely accepted, although the opposite one also can be substantiated: the appearance of ungulates created grasslands, because they ate and trampled down the plants, that is why the plants most resistant to such conditions progressed.) In such a way Kovalevsky demonstrated an algorithm of biological study, which is still in use: he advised to look for adaptation in every event of evolution and every detail of organisms; if this were problematic, he recommended to look for such explanations at any price, not considering any other interpretation. This is a demonstration of a procedure, which was appropriately

6. V. O. Kovalevsky, "Monographie der Gattung Anthracotherium Cuv. and Versuch einer naturlichen Classification der fossilen Huftiere," *Palaeontographica*, 22, 1873-1874, p. 3-5 and p. 131-346 (Tat II); *id.*, "Osteologie des Genus Entelodon Aym," *Palaeontographica*, 7, 1876, p. 415-450; *id.*, "Osteologie des Genus Gelocus Aym," *Palaeontographica, XXIV,* 24, 5, 1887, p. 145-162 (Tat II).

called the "Panglossian paradigm,"[7] by Stephen Gould and Richard Lewontin, in honour of the character of Voltaire, Doctor Pangloss, who always found teleological explanations for any phenomenon and any event: the nose exists to wear glasses, two legs – to wear brushes, etc.; if Doctor Pangloss turned out to be sentenced to the gibbet or sent in servitude to the galleys, it was also a demonstration of the general harmony of everything.

Surprisingly, in addition to adaptive evolution Kovalevsky described inadaptive forms of evolution. However, from his viewpoint, inadaptive evolution still means that evolution is adaptation, because the inadaptive forms become extinct. These speculations were tautological: animals were considered as inadaptive because they became extinct, while their extinction was explained by inadaptive evolution. Meanwhile, inadaptive and adaptive animals often coexisted and still coexist. According to Kovalevsky this fact was caused by differences in competition: animals having competitors evolved (horses), while the animals not having competitors did not (for example, hippopotami). If, in the modern fauna, inadaptive animals having competitors still exist, this means, that they will be extinct soon. For example, Kovalevsky claimed that rhinoceros escaped in refuges along the rivers, while in the main part of the grasslands they were forced out by more perfect ungulates. If the primitive and progressive animals lived simultaneously in the past, this means that our knowledge of fossil records is incomplete or erroneous. Such claims were not enough substantiated or evidently erroneous, but the lack of evidence was compensated by abundant descriptions of bones and speculations in terms of Darwin's theory. However even such a firm epistemological base had not freed these speculations from contradictions. For example, the main idea was that horse-like odd-toed ungulates are the perfect forms which will impede the other ungulates, but Kowalevsky himself noted that, in some cases, even-toed mammals are more viable than odd-toed ones: sometimes in Russian villages in lean years horses died, while the cows survived because they could feed upon the coarsest fare, *i. e.* straw, which was used to cover roofs. This was possible because cows are ruminants; they have a more complicated digestive system. This means that ruminants are better adapted to a diet of grass.

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7. S. J. Gould and R. C. Lewontin, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme," *Proceedings of the Royal Society of London*, Serie B, 205, issue 1161, 1979, p. 581-598.

Kovalevsky probably speculated so easily about the competition and habitats of mammals because he had never observed animals in nature and did not like to look for biological data himself. His life style made it problematic. Meanwhile he wrote and told several times, that he preferred to work with books and museum collections. During his periods of successes, he was offered to explore some Russian territories, but he rejected these plans. He elaborated new palaeontology rather speculatively. The conditions for such work in Russia were favorable to some extent: palaeontologists in Russia were not numerous, this was why there did not exist any pressure from other concepts and traditions. Biological palaeontology was a relatively new science for Russia. It is partly explained by the geologic character of Russian territory. Close to Saint Petersburg, where the scientific community concentrated at that time, there are just four locations where the stone-like fossils are well visible. These fossils are from Paleozoic time – orthoceratids, bivalves, trilobites, gastropods, brachiopods. They are not big and not very remarkable. The remains of mammoths and other impressive objects rarely arrived from the remote parts of the Russian Empire. In many European countries stone-like fossils are much more numerous. For example, a house in south-eastern France was built with rudists – the Mesozoic molluscs, which do not have modern analogies. Such an abundance of remarkable fossils caused the early birth of palaeontology. This science had rich traditions before Darwin, this why a rapid formation of Darwinian palaeontology in Europe was problematic.

III. DEVELOPING DARWINIAN PALAEONTOLOGY BY LOUIS DOLLO

French-Belgian scientist Louis Dollo (1857-1931) fully accepted the procedure of the paleontological research of Kovalevsky. Dollo stressed this link with several times considering himself as a student of Kovalevsky.[8] They never had direct contacts, but Dollo wrote a letter to his "supervisor" once but the contact was not established. The brother of Vladimir Kovalevsky, Alexander, received the letter

8. For example, he devoted one of his treatises to Kowalevsky and put his portrait there : L. Dollo, "La paléontologie éthologique," *Bulletin de la Société belge de géologie, de paléontologie et d'hydrologie*, 23, 1909, p. 377-421.

in 1880, but he was late forwarding it.[9] Probably Kovalevsky had no time to answer it and the letter was lost. However it is still informative since it indicates that Dollo accepted the palaeontology of Kovalevsky since the beginning of his scientific career.

In many other respects the life and work of Dollo represented a contrast to those of Kovalevsky. Dollo was born in Lille (France). He was trained as a mining engineer at the University of Lille, while taking some additional courses in biology. After graduating from the University, he did not pursue a professional career in that field. In 1879, Dollo moved to Brussels, he worked as an engineer in a gas plant for a while, and then he found a position in a Belgian museum in 1882, and worked there for more than 47 years without leaves, being a solitary person (he was married and had a son, but parted with his family soon after moving to Belgium). He almost never left Brussels. Sometimes he moved over to Belgium giving popular lectures, but usually worked in his museum.[10]

Dollo achieved great advances in his research on anatomy and palaeontology, as well as in popularizing science. During the first years of his scientific career, he published several hundreds of papers. One of his works was devoted to iguanodons, which were discovered at that time in Belgium. Dollo discovered them while working on a public museum exhibit. Describing iguanodons, Dollo cited Kovalevsky and compared their evolution to the evolution of horses, finding many similarities such as the fact that Iguanodons had to adapt to grasslands in the same manner as horses did.[11]

Partly because of his many scientific achievements, he was in conflict with his colleagues in his Museum. His direct superior pointed out which objects Dollo should study, and those which he should not. Sometimes Dollo was not allowed to work within the museum collections or even to leave his room. Dollo overstated the importance of this, focusing on his research and his scientific activities. Some of his colleagues noted that Dollo exaggerated these difficulties, and that he was a touchy person.[12] However his conditions of work

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9. L. S. Davitashwilli and L. Dollo, *Voprosy istorii estestvoznanya i tehniki*, 3, 1957, p. 108-120.

10. L. K. Gabunia, *Louis Dollo (1857-1931)*, Moscow, Nauka, 1974.

11. L. Dollo, “Cinquième note sur les dinosauriens de Bernissart,” *Bulletin du Musée Royal d’Histoire Naturelle de Belgique*, 3, 1884, p. 129-146.

12. V. Van Straelen, “Louis Dollo (1857-1931),” *Bulletin du Musée royal d’histoire naturelle de Belgique*, 9, 1, 1933, p. 1-6 ; P. Brien, “Notice sur Louis Dollo”, *Annuaire de l’Académie Royale de Belgique. Notices biogéographiques*, 3, 1951, p. 69-138.

were undoubtedly difficult: he worked for years and years in a small basement room reminding of a prison cell[13] (he kept the portraits of Kovalevsky and Darwin there[14]).

Unlike Kovalevsky, Dollo tried to write papers as short as possible. His texts often represented series of numbered statements. One of his articles had a title "laws of evolution." Such a precious paper is just two pages long.[15] Thanks to Austrian palaeontologist, Otenio Abel, one of these laws became known as the "law of Dollo,"[16] *i.e.* the law of irreversibility: "[an] organism cannot return even partly to the condition of his ancestors." The "laws of Dollo" and his ideas were not always in accordance with the theory of Darwin, but Dollo agreed with main postulate of purposeful evolution. So, according to Dollo, some turtles aimed to adapt to the life in Open Ocean. This is why they underwent some transformations. Later on turtles aimed to adapt to coastal environment, and they suffered new transformations. If some turtles did not demonstrate necessary changes, this means that they did not have competitors. (Although no data on competition among turtles was available.) Developing such a procedure, Dollo elaborated and developed an additional algorithm of research, which is still very popular: the search for secondary evolutionary changes of animals. For example, from the viewpoint of Dollo, an arboreal kangaroo is a "secondary form": at first cursorial kangaroos originated from arboreal mammals, then some cursorial kangaroos "decided" to adapt back to arboreal lifestyle. Moreover, the whole group of marsupials also suffered secondary transformation: they are not primitive mammals without placenta, they lost their placenta because it was not useful to them. Snake-like fishes, cephalopod molluscs, tetrapod dinosaurs and many other animals are also secondary forms.[17] It was difficult to prove

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13. O. Abel and L. Dollo, "Zur Vollendung seines siebzigsten Lebensjahres," *Palaeobiologica*, 1, 1928, p. 10.

14. N. N. Yakovlev, *Vospominaniya geologa-paleontologa*, Moscow, Nauka, 1965.

15. L. Dollo, "Les lois de l'évolution," *Bulletin de la Société belge de géologie, de paléontologie et d'hydrologie*, 7, 1893, p. 164-166.

16. O. Abel, "Die Bedeutung der fossilen Wirbeltiere fuer die Abstammungslehre," in *Die Abstammungslehere. Zwoelf Gemeinverstaendliche Vortraege ueber die Deszendenztheorie in Licht der neuren Forschung*, Jena, Verlag von Gustav Fisher, 1911, p. 198-250.

17. L. Dollo, "La paléontologie éthologique," *Bulletin de la Société belge de géologie, de paléontologie et d'hydrologie*, 23, 1909, p; 377-421; *id*., "Sur la phylogénie des dipneustes," *Bulletin de la Société belge de géologie, de paléontologie et d'hydrologie*, 9, 1895, p. 79-128; *id*., "Les ancêtres des marsupiaux étaient-ils arboricoles?" in *Miscellanées*

such scenarios, but such speculations are continuously in progress. Now many phyla of simple animals are considered as "secondary simplified" ones. The interpretations of primary and secondary events in evolution are fascinating and could seem well-founded, but such a procedure allows absolute arbitrariness in genealogical schemes.[18]

Like Kovalevsky, Dollo divided animals into two groups: significant and insignificant ones for research on evolution. Thereafter Dollo divided palaeontology into two sciences: geologic and biologic ones. Geological palaeontology deals with the fossils which clearly mark the geological layers irrespective of their evolutionary transformations (Mollusks), while biological paleontology explores the animals demonstrating evolution and adaptation (Vertebrates). In such a way any paleontological data contradicting the composed schemes of evolution could easily be removed (into geology).

IV. THE DESTINY OF THE NEW PALAEONTOLOGY

The posterity of the paleontology of Kovalevsky over the first years after his death is not well traced in Russia. Paleontologists were still not numerous, however, their number continuously increased, and some decades later they remembered Kovalevsky. In the 1930s, he was considered as the founder of a new science. His writings were republished in the 1940s and 1950s and presented to the Russian audience by several scientists, including eminent Russian academicians. Louis Dollo was also often recalled approvingly.[19] The main reason of such popularity was the fact that, in the Soviet era, Darwinism occupied the same place in Russian biology as Communism in society. A single serious heresy appeared in the Russian palaeontology of the time: palaeontologist Dmitry Sobolev resolutely declared

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biologiques dédiées au Professeur Alfred Giard à l'occasion du XXV^e anniversaire de la fondation de la station zoologique de Wimereux, 1874-1899, Paris, L. Danel, 1899, p; 188-203; *id.*, "Sur l'évolution des chéloniens marins (considérations bionomiques et phylogéniques)," *Bulletin de l'Académie royale de Belgique. Classe des sciences*, 8, 1903, p. 801-830.

18. For a discussion, see I. Popov, *Periodical systems and periodical laws in biology*, Moscow, Saint Petersburg, KMK, 2008, in Russian.

19. V. O. Kovalevsky, *Paleontologia loshadey* [*Palaeontology of horses*], Moskva, AN SSSR, 1948, in Russian; *id.*, *Sobraniye nauchnikh trudov v triokh tomakh* [Collection of scientific papers in three volumes], Moskva, 1950, 1956, 1960, in Russian.

his orthogenetic concept in the 1920s.[20] Sobolev dealt with extinct cephalopod mollusks – this very group, which was "excluded" by Kovalevsky and Dollo from evolutionary biology. Unlike ungulates, the "objectives" to adapt to some environment were not traced easily among them. Especially cephalopods often caused heresies in evolutionary biology until recently. This statement characterizes well the modern knowledge on their evolution:

> What influence, if any, environmental factors can have had in driving this evolution, in accordance with the classical Darwinian adaptive canon, remains wholly unknown. Numerous claims in the literature to the contrary are almost invariably tautological. In short, we now know rather well how the ammonites evolved, but not why.[21]

The heresy of Sobolev was soon erased by ideological criticism.[22] Afterwards, the orthogenetic concepts sometimes appeared in Russian biology, but the authority of Kovalevsky and Dollo was still high. Soviet researchers often addressed the problem of irreversibility and other related topics.[23] However they did do so in a rather philosophical context, not mentioning directly the papers of Dollo. So, in 2012, the pages on "Ethological palaeontology" by Dollo remained uncut in the book kept in the library of the Russian Academy of sciences. Probably Dollo was known mainly from a good biographical book written by Georgian-Soviet palaeontologist L. Gabunia.[24]

When glorifying Kovalevsky, his followers referred to the fact that several foreign scientists also recognized his merits: Albert Gaudry, Edward Cope, Henry Osborn. Kovalevsky was soon forgotten in Europe and the US though. He is hardly cited and mentioned in the lists of

20. D. N. Sobolev, *Nachala istoricheskoy biogenetiki* [*Bases of historical biogenetics*], Simpheropol, 1924, in Russian.

21. J. Calloman, "Jurassic ammonites: real phylogeny in real time" in *The Lyell Meeting: Approaches to Reconstructing Phylogeny*, The Geological Society, Burlington House, 2002, p. 7.

22. For details, see I. Popov. Orthogenesis versus Darwinism: the Russian case, *Revue d'histoire des sciences*, 2, 2008, p. 367-397.

23. S. A. Orlov, "Neobratimost evolutsii" [Irreversibility of evolution], *Razvitie evolutsionnoy teorii v SSSR*. L., Naika, 1983, p. 399-405, in Russian; L. N. Seravin, "Printsip protivonapravlennosti morphologicheskoy evolutsii i analiz zakona Dollo" [Principle of counter-directionality of morphological evolution and analysis of Dollo], *Evolursionnaya biologia: istoria i teoria*, SPb, Ran, 1999, p. 81-92, in Russian.

24. L. K. Gabunia, *op. cit*.

outstanding scientists. The studies on extinct ungulates progressed after Kovalevsky. Kovalevsky was cited sometimes in this context, but the following studies by O. Marsch and other palaeontologists became much better known. Dollo was better known also, but he was cited mainly as one of the authors of the law of irreversibility and for numerous morphological descriptions, and not as a student of Kovalevsky and co-author of a new palaeontology. Many paleontologists expressed respect to Dollo in the 1920s, but they did not share his enthusiasm for Darwin's theory. In the end of 19th – beginning of 20th century – paleontologists expressed rather non-Darwinian views on evolution. Usually they considered evolution as a spontaneous transformation reminding the growth of crystals, which is just partly related to adaptation. It happened because fossil records were still very difficult to use for evolutionary analysis, and it was difficult to describe evolution using simple traditional schemes by selection. Inadaptive forms, extinction of well-adapted forms, simultaneous extinction of several big groups and many other complicated phenomena were hardly explainable. However Darwinian paleontology was formed in the middle of the 20th century in Europe and the US during the process of the formation of the synthetic theory of evolution under the pressure from genetics.

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V

Scientific cooperation of Vladimir Lyubimenko with French researchers

Anatoli V. Polevoi

One of the greatest botanists, physiologists, ecologists of plant in Russia – Vladimir Nikolaevich Lyubimenko (1873-1937) was a student of the famous botanist and plant physiologist I. P. Borodin (1847-1930) – follower of the ideas of the school of Academician Alexander Famintsyn. Borodin was head of the Department of Botany at the Forest Institute (now Saint Petersburg Forest Technical Academy), where Lyubimenko studied from 1894 to 1898.[1] Under the leadership of Borodin, already on the third course (1896), he began the study of the accumulations of oxalic acid calcium crystals in the organs of plants, which was published in 1900 and awarded a gold medal. At that time he first turned to the study of the formation and growth of dormant buds of trees and their role in the adaptation of plants to the influence of environmental factors.

Lyubimenko's work in Fontainebleau was determining factor in his formation as a botanist and plant physiologist, with a wide ecological approach for research on individual plants and interactions of plants in ecosystems under the influence of changing environmental factors. He worked in the laboratory of G. Bonnier, which was a branch of the plant physiology laboratory of the Sorbonne during five summer seasons from 1904 to 1908 and he later came to Paris in 1923 and in 1926.

Gaston Bonnier (1853-1922) was a student of famous French evolutionist, A. Girard, and he entered the constellation of the greatest world's scientists, the founders of ecological experimental physiology of plants.[1] The evolutionary approach of Bonnier in the study of the general biological processes of the formation of the forms of plants led

1. K. V. Manoylenko, *V. N. Lyubimenko: evolyutsionnye, ekologo-fizologicheskie, istoriko-nauchnye aspekty deyatel'nosti*, SPb, Nauka, 1996, p. 168.

Lyubimenko to the comparative study of the photosynthetic activity and the content of pigments in light-demander and shade-enduring tree species in different lighting conditions.[2] The study of the features of photosynthesis in plants under the influence of light, with different intensity and qualitative composition, temperature, geographical factors of the origins of plants, became a major area of research for Lyubimenko. In France also and in the following years, he studied the effect of light on the germination of seeds[3],[4] and the growth of fruits of different tree species. In France, he devised a spectrophotometric method for the analysis of small quantities of pigments (chlorophyll, xanthophylls).[5,6] He showed the formation of chlorophyll of Coniferae in the dark, and the existence of protochlorophyll in the inner shell of some pumpkin seeds (Cucurbitaceae) and he discussed its role in the synthesis of chlorophyll.[7] Investigating the formation of chlorophyll, he discussed the role of rodoksantin and lycopene in the process. As a result of his work in Bonnier's laboratory, Lyubimenko developed a plan for future long-term studies of photosynthesis in environmental and ecological directions.

The main results of his work on photosynthesis in Bonnier's laboratory were published in 1909 in two papers in Russian[8] and French.[9] In 1910, Lyubimenko also discussed the relationship between chlorophyll concentrations in the plant tissues and the energy of photosynthesis.[10]

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2. V. N. Lyubimenko, "Sur la sensibilité de l'appareil chlorophyllien des plantes ombrophiles et ombrophobes," *Revue Générale de Botanique*, 17, 1905, p. 381-415.

3. *Id.*, "Action directe de la lumière sur la transformation des sucres absorbés par les plantules du *Pinus pinea*," *Comptes rendus de l'Académie des Sciences*, 143, 15, 1906, p. 516-519.

4. *Id.*, "Variations de l'assimilation chlorophyllienne avec la lumière et la température," *Comptes rendus de l'Académie des Sciences*, 143, 17, 1906, p. 609-611.

5. *Id.*, "Etude spectroscopique des pigments verts des graines mûres," *Comptes rendus de l'Académie des Sciences*, 142, 25, 1906, p. 1432-1435.

6. *Id.*, "La concentration de la chlorophylle et l'énergie assimilatrice," *Comptes rendus de l'Académie des Sciences*, 143, 22, 1906, p. 837-839.

7. V. N. Lyubimenko and N. A. Monteverde, "O zelenom pigmente vnutrennej obolochki semyan nekotorykh Cucurbitaceae i ego otnoshenii k hlorofillu," *Izv. SPb. botan. sada*, 9, 2-3, 1909, p. 27-44.

8. V. N. Lyubimenko, "Vliyanie sveta razlichnoy napryazhennosti na nakoplenie suhogo veschestva i hlorofilla u svetolyubivyih i tenevyinoslivyih rasteniy," *Truly po lesnomu opyit. delu v Rossii*, 13, 1909, p. 1-110.

9. *Id.*, "Production de la substance sèche et de la chlorophylle chez les végétaux supérieurs aux différentes intensités lumineuses," *Annales des Sciences Naturelles; Botaniques*, 7, 1909, p. 321-415.

10. *Id.*, "Soderzhanie hlorofilla v hlorofilnom zerne i energiya fotosinteza," *Trudyi SPb obschestva estestvoispyitateley*, 41, 3, 1910, p. 1-267.

In 1923, Lyubimenko published his major work in German and Russian on the "Course of General Botany" [11] which was then translated into French in 1927. [12] The peculiarity of this book was a comprehensive data analysis of plant anatomy, their functional activity and the influence of environmental factors, including ecological and geographical factors. In the book, he analyzed the embryology of plants, the doctrine of heredity and the adaptation of plants in evolutionary directions.

Once again, V. N. Lyubimenko visited the laboratory of Fontainebleau in 1923, after attending in the International Congress of the Protection of Nature, in which he participated together with V. I. Vernadsky. Before his trip to the 4th International Botanical Congress in Ithaca (usa) in 1926, Lyubimenko worked in a laboratory in Boulogne, where he prepared reports for the Congress and waited for the registration of his documents for America. His reports about the work of the Congress were published in 1926 in Russian and in 1927 in the French press. [13] In the same year, Lyubimenko published a few reports about a new French Botanical Journal. [14] In 1925, Lyubimenko reported on the usefulness of the work of French botanist M. Molyar (1866-1944) in order to analyze the most recent data in the field of experimental morphology, to the creation of which French botanists significantly contributed (G. Bonnier, M. Molyar, etc.).[1] Lyubimenko was also interested in Molyar's experiments dealing with the mechanisms of the perception of external stimulus by plants and morphological responses. Molyar considered metabolism was a key factor, in changing environmental conditions, in determining the structure of plants. Lyubimenko stressed the essential organizational work carried out by Molyar which led to the creation of the department of plant physiology of the Sorbonne. He noted the importance of Molyar's attempts "to penetrate to the mysterious area of physiology of the shaping of plant." [15]

In the 1930s, other links were established between V. N. Lyubimenko and French science. In 1936, Lyubimenko was presented as a Corresponding

11. *Id.*, *Kurs obschey botaniki: Rukovodstvo dlya slushateley vyisshih uchebnyih zavedeniy i dlya samoobrazovaniya*, Berlin, 1923, p. 1056.

12. *Id.*, *Traité de botanique générale*, Paris, 1927, vol. 1-2, p. 1227.

13. *Id.*, "IV^e^ Congrès international de botanique," *Archives Botaniques: Bulletin mensuel*, 1, 1927, p. 82-86.

14. *Id.*, "Novyiy frantsuzskiy botanicheskiy zhurnal," *Zhurnal russkogo botanicheskogo obschestva*, 12, 1-2, 1927, p. 239-240.

15. *Id.*, "Originalnyiy frantsuzskiy issledovatel," *Zhurnal russkogo botanicheskogo obschestva*, 9, 1925, p. 203-208.

Member of the Paris Academy of Sciences. He also received a proposal to write a monograph about vernalization and geotropism. This work was stopped by the sudden death of the scientist on August 14th, 1937.

In carrying out the program planned in France, Lyubimenko studied the chlorophyll content of leaves of 226 species of plants from Crimea, and compared these data with those, obtained from plants from the tropics (Java island) and Arctic plants from the Kola Peninsula. He also made a table of chlorophyll concentrations in leaves in different geographical latitudes.

He first noted and began to study the possible role of enzymes (in particular peroxidase) in the conversion of plant pigments in different geographical areas and reported about the relationship of chlorophyll and proteins of plastids. Following G. Bonnier, Lyubimenko studied the characteristics of adaptation that occurred in plants from the valleys after moving them to the mountains. Lyubimenko believed that this transfer led to a reduction in the length of the growing season and the appearance of dwarf forms with short stems and a reduction in size and in the number of leaves. Lyubimenko studied the phenomenon of chromatic adaptation, analyzing the pigments of algae, the purple bacteria plastids of higher plants. It was shown that the yellow pigment, lycopene, was present in all the investigated objects. Lyubimenko supposed a common origin of the yellow pigment system in living organisms. Following Famintsyn's ideas, Lyubimenko believed that plant plastids were independent organisms[16], [17] adapted to life within the leaf cells. He was inclined to believe in the symbiogenesis nature of the origin of green plant plastids. Lyubimenko developed a method for the isolation of plastids from leaf tissues and the culture media for the study of their function in the isolated state.

Being a follower of the ecological ideas of G. Bonnier, Lyubimenko studied the effect of the environmental factors on photosynthesis, respiration, transpiration of plants, promoted the importance of the idea of the synthesis between, in the one hand, plant physiology and ecology, and evolutionary theory, in the other hand. In his works, Lyubimenko wrote about the necessity to study the appearance of adaptive characteristics of plants in different environmental conditions. He stressed the principles

16. *Id.*, "O prevrascheniyah pigmentov plastid v zhivoy tkani rasteniya," *Zapiski Akademii Nauk*, Serie 8, 33, 12, 1916, p. 1-274.

17. A. S. Famintsyin, "O roli simbioza v evolyutsii organizmov," *Zapiski Imperatorskoĭ. Akademii Nauk*, Serie 7, 20, 2, 1907, p. 1-14.

of the integrity of plant interactions with the environmental factors and the increasing organism resistance to stress factors during development. According to the conclusions of Lyubimenko, plant resistance to cold depended on the stage of the plant development and on its physiological state. Therefore it was necessary to have in mind the ontogenic stage of the experimental plant, in the study of environmental factors. In the 1920s and 1930s, Lyubimenko studied the photoperiodic response of plants depending on plant age, the phase of development, the environmental temperature, the light quality and intensity, and the geographical factors – such as their latitude of origin. It was concluded that the main physiological difference between short- and long-day plants is found in the ratio between the activity of respiration and photosynthesis: in long-day plants, respiration rate is higher in comparison with the process of photosynthesis, than in short-day plants. In 1932, Lyubimenko introduced the term "photoperiodic induction," showing that it is sufficient to act by light on the leaves of plants, in a short time, in the particular sensitive period of their life before the formation of flower organs.[18] Thus, Lyubimenko first examined the question of the adaptive significance of the photoperiodic response in plants.

The original researches of V. N. Lyubimenko continued with the development of the problems of photosynthesis in evolutionary directions, taking into account the character of the formation of pigment systems during the interaction of plant organisms with environmental and geographical factors.[19, 20, 21] In his experimental analysis of the adaptive responses of plants in response to environmental factors, Lyubimenko creatively developed the ideas of the school of A. S. Famintsyn – I. P. Borodin and the French school of morphologist and plant physiologists G. Bonnier. The tendency of V. N. Lyubimenko to study the morphological and physiological responses of plants in their interactions as individuals and in coenoses with environmental factors is increasingly being used in the modern ecological physiology of plants.

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18. V. N. Lyubimenko and E. D. Buslova, "O vliyanii svetovoy induktsii na razvitie Perilla ocymoides," *Trudy AN SSSR*, 14, 3, 1937, p. 145-148.

19. K. V. Manoylenko, "Rol V.N. Lyubimenko v razvitii evolyutsionnogo napravleniya v fiziologii rasteniy," *Iz istorii biologii*, 5, 1975, p. 188-196.

20. *Id.*, "Vadimir Nikolaevich Lyubimenko (K 120-letiyu so dnya rozhdeniya)," *Botanicheskii zhurnal*, 73, 11, 1993, p. 116-123.

21. E. M. Senchenkova, "Issledovaniya hlorofilla v trudah V.N. Lyubimenko i ih sovremennoe razvitie," *Fizicheskie i himicheskie osnovyi zhiznennyih yavleniy*, Moscow, 1963, p. 147-213.

VI

A forgotten root of the Soviet developmental biology: Mikhail Zavadovskii and the developmental dynamics of the organism

Oleg P. Belozerov

Historians of science, interested in the origins of developmental biology in the USSR, usually seek them in the activities of the group of investigators who worked during the 1920s-1940s in the paradigm of developmental mechanics. The most prominent representatives of this group were Dmitri Filatov, Ivan Schmalhausen, Boris Balinsky, Nikolai Dragomirov, Vera Danchakova, Julius Schaxel, and some others. Developmental mechanics was really a very important root of the Soviet developmental biology, but not the only one, a fact which is curiously neglected in the specialized literature. In the 1920s-1930s in the USSR there existed an alternative program of the study of development, presented by Mikhail Zavadovskii and named developmental dynamics of the organism. It was founded on different grounds: if, in the study of the form of living beings, the roots of developmental mechanics rested on morphology, those of developmental dynamics laid in physiology. This paper analyzes the short history of developmental dynamics, the causes of its neglect, and its influence on the emergence of developmental biology in the USSR.

The founder of developmental dynamics, Mikhail Zavadovskii, was a well-known figure in the Soviet biology of the first half of the 20th century. He was educated at the Imperial Moscow University and at the Shaniavsky Moscow City People’s University, where he was most influenced by Nikolai Koltsov – who later became one of the principal organizers of Soviet science. Zavadovskii also worked under Koltsov’s patronage for five years after graduating from university. Zavadovskii’s initial research interests led him into the area of physicochemical biology – his first original research, carried out in Koltsov's laboratory, was devoted to the study of the physicochemical properties of the Ascaris egg shell.

In the late 1910s, he changed his research priorities, having become interested in the issues of development. But his approach to the topic was different from the one of Wilhelm Roux and other developmental mechanists: instead of studying the early stages of development he focused on the postnatal development of mammals and birds, and first of all on the role of endocrine glands in the process. Being in the years 1919-1921 in the famous Askania-Nova reserve, located in the steppes of modern South Ukraine, and at the University of Tavria (Crimea), working predominantly on chickens and to a lesser extent on some mammals, he tried by means of the grafting of sexual glands to induce robust and constant secondary sex characters. From his observations he drew a conclusion that all secondary sex characteristics can be divided into "independent" (emerging without influence of sexual glands, though the latter could modify their development) and "dependent," or truly secondary sex characteristics, emerging only under the influence of sexual glands. Further he assumed that the testes produced a special male hormone called "masculinisin," that the ovary produced another called "feminisin" and that sex was determined by such a hormone.[1]

In the 1910s and the 1920s, it was hoped that the nascent discipline of endocrinology would solve some of the riddles of the science of development, as for instance, the question of how hereditary germs (genes) control development – hormones could potentially fit the role of a transmission mechanism in this process. Zavadovskii was among those who tried to convert these vague hopes into a coherent theory. In the 1920s, he devised an ambitious research program named after a number of experiments in the developmental dynamics of the organism. Defined tautologically, developmental dynamics was conceived of as a science of development. But the fact that Zavadovskii launched his project when the study of development had had quite a long history, raised the question of why Zavadovskii decided to inaugurate a new discipline of development.

Zavadovskii expounded his motives, albeit confusedly, in the preface to his monograph "Developmental dynamics of the organism."[2]

1. M. M. Zavadovskii, *Pol i razvitie ego priznakov*, Moskva, Gosgiz, 1922, p. 53.
2. *Id.*, *Dinamika razvitiia organizma*, Moskva, Gosmedgiz, 1931.

He claimed that since the end of the 19th century experimental biology had accumulated vast amounts of evidence which needed systematization. The realization of this task demanded the formulation of the main idea that could unify all these evidence. From Zavadovskii's viewpoint such an idea could be the one of development, interpreted in a broad sense. He believed that all biological disciplines could be divided into "dynamic" and "static" ones. "Dynamic" sciences – physiology, developmental mechanics, and evolutionary teaching – employed experimental methods and their aim was the study of the causes of the changes of living beings. The task of the "static" sciences – anatomy and embryology – was the study of the form of living beings and those sciences employed descriptive methods. Zavadovskii believed that the three "dynamic" sciences were so closely interconnected, due to their emphasis on the changeability and fluidity of living beings, that they should be united into a larger science namely– "developmental dynamics."

As we will see further, the above-mentioned reasons for devising developmental dynamics did not seem convincing to Zavadovskii's contemporaries. It looked as if the aim of this new discipline was to facilitate the teaching due to the better organization of the instructional material. But Zavadovskii also gave more comprehensive explanations of his discipline-building. He pointed out that:

1. his views had little connection with Roux's ideas – here Zavadovskii probably meant that he came to the study of development from physicochemical biology and physiology and paid more attention to the physiology than to the morphology of developing organisms;
2. according to the widespread opinion in the USSR developmental mechanics covered only embryonic stages, whereas he was interested mainly in postembryonic ones;
3. he paid a lot of attention to the demarcation between heredity and development, genetics and embryology. With the hope to overcome it, he advanced the idea of a special sub-discipline within developmental dynamics, namely "morphogenetics," which was especially devoted to this problem. Inspired by his works on birds, he assumed that genes could control development by means of "morphohormones." If so to speak "ordinary" hormones rather regulated the physiological functions of the organism, while morphohormones shaped its form;
4. he believed that developmental dynamics could solve some of the problems of evolution and, as a starting point, analyzed the problem of the inheritance of acquired characters through the prism

of developmental dynamics. He argued that such inheritance was impossible or unlikely, for the acquired characters usually were due to changes in somatic cells, not in germ ones.

Although he only declared what he intended to do concerning developmental dynamics (he could demonstrate but few achievements in this field) it quickly gained official recognition. In the end of the 1920s – beginning of the 1930s – the Moscow University underwent a radical reorganization. In 1930, a new Biology Faculty was created with a number of new departments including the Department of Developmental Dynamics. In 1933, the State Scientific Council approved the specialty "developmental dynamics of animals," though with the reservation that it would be taught only at the Moscow University for a while due to the absence of a trained staff in the other universities.

The reaction of Zavadovskii's colleagues involved in the study of development was less enthusiastic. The discussions in the end of the 1920s – beginning of the 1930s – and some publications, first of all reviews of Zavadovskii's book "Developmental dynamics of the organism" demonstrated a rather critical attitude to his creation.

First, the division of biological sciences into "dynamic" and "static" ones met a strong objection. It should be mentioned that at first Zavadovskii counted genetics among "static" sciences because, in his opinion, it employed a "static" method of crossing and worked with mature characters. In response geneticists pointed out to him that genetics also studied such undoubtedly "dynamic" phenomena such as mutational and population processes, and later Zavadovskii moved genetics to the domain of the "dynamic" sciences.

Second, the attempt to unite different biological disciplines on the grounds of the idea of development was considered as artificial, for, as Zavadovskii's critics noted, the idea of development was inherent in all biological sciences. Zavadovskii's claim that biological evolution can be studied by the methods of developmental mechanics attracted special criticism, all the more so because in one of his books Zavadovskii expressed this idea in an ultimate form: "I have no doubt that developmental mechanics of species is a special case of developmental mechanics of the individual."[3]

Third, Zavadovskii's attempt to single out a special class of hormones – morphohormones – met with a skeptical reception.

3. *Id.*, *Vneshnie i vnutrennie factory razvitiia*, Moskva, Gosgiz, 1928, p. 211.

Zavadovskii's own disciple Mikhail Mitskevich ironically wrote that if morphohormones were the hormones controlling the formation of the structures then "ordinary" hormones should be called "functiohormones." And in Pavel Svetlov's opinion Zavadovskii failed to demonstrate convincingly that hormones could play an important role in development except in the cases of sex characteristics development. In addition Svetlov pointed out that Zavadovskii avoided using Roux's terminology but his own was very tangled. As a result most of developmental biologists saw no reasons for inventing a new discipline on development, different from developmental mechanics.

The negative reception of developmental dynamics is one of the most probable reasons why Zavadovskii stopped theorizing about developmental dynamics after 1936. The other one could be a shift in Zavadovskii's research interests: in the beginning of the 1930s, he was captivated by a new theme, the working out of the methods of experimental multifetality in agricultural animals. Meanwhile developmental dynamics kept developing as an officially recognized scientific and teaching discipline: departments of developmental dynamics functioned at the Moscow and Saratov Universities, the "Proceedings in the developmental dynamics," the new incarnation of "Proceedings of the Laboratory of Experimental Biology" had been published in 1931, academic degrees on developmental dynamics were awarded. But in spite of all these successes the fall of developmental dynamics was not far away. In 1948 there took place the notorious August session of the All-Union Academy of Agricultural Sciences (VASKHNIL) which entailed the ban of classical genetics in the USSR. Zavadovskii was not a geneticist but as an academician of VASKhNIL and its vice-president from 1935 to 1938, he took part in all the genetic discussions of the 1930s-1940s and supported geneticists in their struggle against Lysenko; in addition he was a close friend of Aleksandr Serebrovskii, one of the leading Soviet geneticists. This is why after the session Zavadovskii was discharged from the university, his department was dismantled, its employees at best found asylum in other departments, at worst – were also discharged. Zavadovskii's book "Developmental dynamics of the organism" was purged from the university libraries along with his books on genetics.

After 1948 Zavadovskii was a jobless pensioner for six years. He was allowed to return to research only in 1954, when he reestablished his laboratory in the All-Union Institute for Animal Breeding, where he worked until his death in 1957.

Although developmental dynamics gained the support of only the minority of Soviet embryologists and existed as a formally recognized discipline for a relatively short time, it contributed a lot to the formation of developmental biology in the USSR. Thanks to Zavadovskii's efforts the study of development was officially recognized as a separate university specialty, there emerged the first university departments, which housed the new science. And even more significant, Zavadovskii's department and laboratories became important centers for training professionals in the area of development. Zavadovskii was one of the mentors of such prominent Soviet developmental biologists as Leonid Blyakher, Maria Vorontsova, Anatoly Peredelskii, Tatiana Dettlaff, Boris Tokin, Mikhail Mitskevich. It is worth mentioning also that a number of his disciples succeeded in other areas of biology, first of all endocrinology and physiology.

VII

New archival data about the Aleksei A. Korotneff Russian Zoological Station in Villefranche-sur-Mer, France (1920-1930)

Tatyana I. Ulyankina

With the October 1917 Revolution many Russian institutions located in other countries lost their sources of financial support. Most Russian consulates and diplomatic missions of Tsarist Russia were closed. The A. A. Korotneff Russian Zoological Station in Villefranche-sur-Mer (France) was among those Russian research establishments, which suffered in these years. The Station was opened in 1886 by a Kiev professor, Aleksei Alekseevich Korotneff (1851-1915), in the building of a former Russian naval base. In 1857, the government of the Kingdom of Sardinia leased this building to Russia for 99 years.[1] Korotneff, who became not only the owner but also the director of the Zoological Station, raised funds for the reconstruction of the naval base. The Station was very popular among zoologists from all over the world; it was one of the major centres for the study of the Mediterranean coastal and pelagic fauna. According to Korotneff's wishes, his deputy Professor, Mikhail Mikhailovich Davydoff (1896-1933), was promoted to the position of Station director after Korotneff's death in 1915.

In 1921, a small group of Russian émigré scientists from Paris and Prague managed to establish a special Committee for Support of the Russian Zoological Station in Villefranche-sur-Mer (Comité de patronage et de direction de la station zoologique russe à Villefranche-sur-Mer) to deal with complex financial and legal problems. For many

1. P. E. Kovalevsky, "Russians in Villefranche (From the report at the Russian Scientific Institute in Paris)," *Russian thought*, Paris, p. 533 (March 4, 1953); S. Fokin, *Russkie uchenye v Neapole*, Saint Petersburg, Aleteiya, 2006.

decades the Committee's archives from the 1920s-1930s were considered lost.

In 2008, a Russian historian, Sergei Fokin, wrote:

> […] over sixty letters from 1925 to 1931 curiously disappeared from the Station, as well as Tregouboff's personal archives.[2] These documents, if they were found, could perhaps elucidate the final years of the Russian Zoological Station at Villafranca.[3]

Recently I found the documents of the Russian Zoological Station in the Archives of the Alexander Solzhenitsyn Centre for the Study of Russian Emigration (Moscow). The collection contains about 500 letters and documents. It was received from the national archives of France where it had been kept as a special collection (no. 69) named "Kovalevsky's family collection."

Evraf Petrovich Kovalevsky was the general secretary and treasurer of the Committee of Patronage. The information about these documents is provided in my paper. It reflects a complicated situation in the Russian Zoological Station in the 1920s-1930s and tells us about a few outstanding Russian émigré scientists who defended Russian interests in Villefranche-sur-Mer in 1921-1935.

In 1921, Nikolai Ivanovich Andrusov (1861-1924) was elected the chairman of the Committee of Patronage. He was an outstanding geologist, paleontologist, a full member of the Russian Academy of Sciences (elected in 1914) and of the Ukrainian Academy of Sciences (from 1919 on). After his emigration to France, Andrusov worked as a fellow at the geological office in the Sorbonne. In 1922, he moved to Prague where he received a position at the geological cabinet of the Charles University. Two members of the Committee of Patronage were working at the Pasteur Institute in Paris: zoologist and immunologist Professor Sergei Ivanovich Metalnikoff (1870-1946) and physician and biologist Alexander Tikhonovich Vassilieff (1875-1945). Other members were lawyer and politician Valerian Konstantinovich Agafonov (1866-1941), the widow of a former Station director, Sophia Ivanovna Korotneff, a new director of the Station, Professor Mikhail Mikhailovich

2. G. S. Tregouboff was the last Russian director at the Russian Zoological Station in Villefranche-sur-Mer (1943-1956).

3. S. I. Fokin, "Russian biologists at Villafranca," *Proceedings of the California Academy of Sciences*, Serie 8, 59, 1/11, 2008, p. 170.

Davidoff, and his assistant, Grigory Semenovich Tregubov (Grégoire Tregouboff, 1886-1969).

Professor Mikhail Mikhailovich Novikov (1876-1965), a zoologist, comparative anatomist and former rector of the Imperial Moscow University, started his work at the Committee of Patronage in 1923, after he had moved to Prague. In 1924, when Andrusov died, Novikov was elected as the Committee chairman. Another Russian botanist who worked in Prague, Vasilii Sergeevich Il'in (1888-1957), became a Committee member at the same time. Novikov was not new to Villefranche: in his student years he visited the Station many times. In 1913, Novikov helped Korotneff put pressure on the Russian Duma (the Parliament) for passing a resolution authorizing the transfer of the Russian Station to the jurisdiction of the Ministry of Public Education and the Russian Academy of Sciences.[4] Professor S. S. Krym (born in 1867), an agronomist, joined the Committee of Patronage some years later. Before emigrating to France, Krym was a member of the First Duma (1906); he was elected a delegate of the Taurida Province.

In Prague "Novikov quickly became a respected member of the Czech academic establishment."[5] He was elected a member of the Czechoslovak Society of Zoologists and of the Czech Royal Scientific Society. From 1923 to 1925, thanks to the initiative of Novikov, a Russian zoological seminar functioned at the Zoological Institute of the Czech Charles University. The Russian Station was the only site where Czech zoologists could carry out experimental research on oceanic organisms. On Novikov's recommendation, a few outstanding Czech scientists joined the Committee of Patronage – Professor Bohumil Nemez (1873-1966), a botanist and rector of Charles University in 1921-1922, Professor Edward Babak (1873-1926), a physiologist from the school of medicine at the Masaryk University and at the Czech University in Brno, and Professor Zdenek Bazant (1879-1953), rector of the Higher Technical School in Prague in 1923 and 1924.

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4. M. M. Novikov, "Report on the bill," *On the projects of the situation and the state of the Russian Zoological Station in Villafranca*, unpublished manuscript dated May 20th, 1913. No. 21850, in The Archives of the Solzhenitsyn House of the Russian Abroad (hereafter AHRA), *fond* 69 (collection) (Kovalevsky's family archive), *opis'* 2 (inventory), *delo* 12 (file), *listy* 1-4ob. (folios) (verso side).

5. T. Hermann and K. Kleisner, "The five 'homes' of zoologist Mikhail M. Novikov (1876-1965). Analogy and adaptation in one's life and as a principle of biological investigation," *Jahrbuch für Europäische Wissenschaftskultur*, 1, 2005, p. 107.

Their illustrious academic reputation helped the Station to receive financial assistance from the Czech Academy of Sciences and Arts. In 1923 and 1932, the Czech Academy paid the Station for a few positions reserved for Czech scientists. From 1923 to 1929, it supported twelve workplaces; then in 1929-1930, the payments were interrupted (the reason will be discussed below). In 1930, they were resumed for a short while, although on a lesser scale: the Czech Academy covered four workplaces in 1931 and just one place in 1932. In these years more than one hundred Czech scientists and students were able to attend the Russian Station. The Czech Academy also purchased some equipment. For example, thanks to a generous donation made by Professor Carl Cori, a future Noble Prize laureate, the Station was equipped with a cabinet for experimental research.

Novikov was also able to establish cooperation with the Polish, Yugoslavian and Bulgarian Academies of Sciences. As he wrote, "at that time the Station was an extremely strange, you can even say – a paradoxical, institution. Its owners were beggars, Russian refugees who extended, however, their hospitality to the whole academic world, since the Station attracted scientists from everywhere. I remember, once our motorboat accommodated representatives of six nations – Russians, French, Americans, Germans, Poles, and Chinese. It was impossible to start a conversation that would engage everyone in such a company, yet their interest and enthusiasm… were unanimous."[6]

The Committee of Patronage raised more than 500 000 francs. This money was used to pay regular salaries to the Station administration.[7] The Committee consisted of renown scholars with impeccable reputation with an exception of one man, deputy director Gregory (Grigorii) Semenovich Tregouboff (1886-1969). Sophia Korotneff wrote about him: "I do not know what you think of Tregouboff? True, he works hard enough but he is a very unpleasant, disagreeable, quarrelsome man. I am afraid, the Station will lose its good reputation, if he runs it."[8]

According to Professor Aleksei Vassiliev, "presently, the Station belongs to a zoologist who seems not to be interested in his research,

6. M. M. Novikov, *Ot Moskvy do Niyu-Yorka. Moya zhizn' v nauke i politike*, Moscow, mgu, 2009, p. 280.

7. The Magazine of the Patronage Committee of the Russian Zoological Station in Villefranche, Paris, February 10, 1932, in AHRA, f. 69, op. 2, d. 9, l. 23.

8. Unpublished letter sent by S. I. Korotneff to M. M. Novikov on February 11, 1925, in The State Archive of the Russian Federation (hereafter garf), Moscow, f. R-6767, op. 1, d. 49, ll. 1-3ob.

since for about two years he has not written a single line, even having all his time free, a very rich fauna and everything that might be needed for his work!"[9]

Tregouboff was also negatively characterized in the memoirs left by A. U. Davydoff, the widow of a well-known Russian embryologist Konstantin N. Davydoff (1870-1960), a distant relative of M. M. Davydoff.

> In early May 1925, we went to Villefranche-sur-Mer where K.N. has been working in the Laboratory and where we could get a room for living… At that time M.M. Davydoff, a distant relative of K.N., was appointed director but then he was already completely senile. The real director was Tregouboff, a rather unpleasant man; he was always drunk and very rude, especially with Russians. His wife was also a rather unpleasant French woman, hostile to Russians, particularly to us. Fortunately, at that time there were a few other scientists at the Station; thanks to this circumstance, we felt much better… On the other hand, we were on very friendly terms with young scientists from Poland, as well as with a son of Professor Novikoff [Novikov], Vladimir, who worked as a mechanic on the Laboratory motorboat… When the head [of the Station] went to Paris, the Laboratory changed from the very first day of his departure: whistles, laughter and bustle were heard in the corridor, many kinds of jokes were made. Russian dinner was cooked in some laboratories, or people shared expenses and made common tea.[10]

Tregouboff was born in Kiev; he graduated from the University of Montpellier where he developed an interest in protistology. He studied under Professor Octave Duboscq. In late 1914 Tregouboff appeared at the Russian Station in Villefranche. A year later he was appointed to the position of Station librarian. Since he had no Russian diploma, he could not obtain a permanent position at the Station that was governed by Russian laws. It seems that Korotneff agreed to find Tregouboff some employment at the Station under a pressure exercised by Tregouboff's father, in he thus saved Tregouboff from military conscription.

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9. A. T. Vasiliev to E. P. Kovalevsky on November 26, 1931, in AHRA, f. 69, op. 2, d. 4, ll. 1-2.

10. S. I. Fokin, "Russian biologists at Villafranca," *Proceedings of the California Academy of Sciences*, Serie 4, 59, 1/11, 2008, p. 189.

Unfortunately, in the mid-1920s when Davidov became seriously ill, Tregouboff took the command of the Station. He was rather skeptical about the efforts made by the Committee of Patronage in order to preserve the Station as a Russian institution. When members of the Committee protested against his policies, he replied that the only realistic way to secure the future of the Station would be to hand it over to France.

Novikov wrote about Tregouboff:

> […] a nephew of a well-known member of the State Duma Archpriest Tregouboff, as it often happens in the families of our clergy, he boasted of his atheism. Being a very irritable man, he often quarreled with the Station employees, so that I had to use my diplomatic skills in order to overcome grave misunderstandings.[11]

Chronologically, the process of denationalization of the Russian Station started with a court resolution from January 4, 1925, no. 24278, that authorized the administrative sequestration of the Station as a foreign property. There are many reasons to believe that the resolution was instigated by negotiations between Tregouboff and his former mentor at the University of Montpellier, Professor Octave Duboscq. Tregouboff carried them on his own initiative, without an authorization from the Committee of Patronage. He justified them by claiming that the Station needed protection from the Bolsheviks. In fact subsequent events proved that the Bolsheviks were a lesser evil compared to Tregouboff: with support from the Czech Academy of Sciences, the Station could remain a Russian or Russo-Czechoslovak, or Slavic institution for a long time. Yet Tregouboff had different plans for its future.

From 1924 on, Tregouboff started ousting Czech scientists from the station: he refused them positions and left their letters unanswered. When they complained, he made false excuses. He claimed that the letters had not been delivered by the post or that the funds he received from the Czech Academy of Sciences were insufficient to cover the expenses or there were no vacant positions. His actions were aimed at disrupting cooperation with Czechoslovakia: evidently, he expected that the disruption of financial assistance from the Czech Academy

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11. M. M. Novikov, *Ot Moskvy do Niyu-Yorka. Moya zhizn' v nauke i politike*, Moscow, mgu, 2009, p. 280.

would make it easier to hand the Station over to France. Tregouboff harassed not only the Czechs and the Germans but also those Russian scientists who displeased him.

In 1929, the Czech Academy of Sciences protested against his unethical behavior towards Czech scientists and refused to provide funding for their positions for the next year. Novikov resigned from his position as chairman of the Committee of Patronage and even left the Committee. Serge Metalnikoff was elected as the new chairman. The desperate financial situation of the Station, when it lost support from the Czech Academy, was solved by a French court official, Pierre Jaudon, who arranged that 50 000 francs were allocated to the Station by the French Ministry of Foreign Affairs. A year later, Novikov managed to renew the agreement with the Czech Academy of Sciences, which restarted paying for positions for Czech scholars, even if Czechoslovakia experienced a severe financial crisis. Novikov did everything possible to save the Station. He considered its transfer to France as a heavy blow that "would be painful for national consciousness."[12] Novikov asked his colleagues in Paris to spread the news among Russian émigré elite of the problems the Station was experiencing.[13] He hoped that such influential persons as Vasilii A. Maklakov, baron B. E. Nolde and a few others could "raise a question at the Commission on Intellectual Cooperation of the League of Nations."[14] Novikov personally addressed the Board of the Union of Russian Academic Organization imploring the Board to complain to Professor Duboscq. He believed that Octave Duboscq "who had already been the director of a Mediterranean biological station in Banyuls was the key figure in the proposed project that meant taking over Russian and Czech property and inflict new humiliation on our national dignity."[15] Novikov assumed that Tregouboff's was principally motivated by his expectations for a higher salary if the station were taken over by the French administration. Novikov hoped that Duboscq might abandon his project under the pressure of the Russian émigré elite, and thus the Station would remain a Russian institution.

12. M. M. Novikov to S. I. Metalnikoff, April 12, 1930, in AHRA, f. 69, op. 2, d. 4, ll. 1-2.

13. *Ibid.*

14. *Ibid.*

15. *Ibid.*

However, the negotiations failed and on July 9, 1930, the Russian Zoological Station was taken over by the Sorbonne. Still, a major concession was made: technically, the Station remained a Russian institution that was temporarily maintained and administered by the Sorbonne. It retained its name – Professor A. Korotneff Russian Zoological Station[16]. However, the money that the Station received from the Czech Academy of Sciences and other national academies were transferred to the Sorbonne, and not directly to the Station administration, as it had been the case earlier.

Unfortunately, Tregouboff was not content with this arrangement and went on with his struggle with the Committee of Patronage. He openly abstained from contacts with the Committee and its members on the grounds that he was an employee of the French administration. In this way he ignored all the documents and requests that he received from the Committee. Without the Committee authorization, Tregouboff hands over to the Sorbonne internal documentation of the Committee (including private correspondence), translating it from Russian to French. He closed the door of the Station to the Committee general secretary, E. P. Kovalevsky, on the pretext that Kovalevsky was not a biologist. He removed the reference to Korotneff from the name of the Station, changing it to the "Russian Zoological Station of the University of Paris."

The Committee members were unable to do anything about these policies. In 1935 Metalnikoff wrote to Kovalevsky: "Unfortunately, the activities of our Committee have been reduced to naught, as we have virtually no means to help the Station."[17] Russian emigrants put their hopes in the Franco-Russian Society but it could not protect the Russian interests at the Station.

When Davydoff died in 1933, Professor Duboscq was officially appointed director of the Station. Tregouboff remained in office as deputy director. It was only with the death of Duboscq in 1943 that Tregouboff finally assumed the directorship of the Station. He was the last Russian head of the Zoological Station in Villefranche-sur-Mer, and he occupied this position till his retirement in 1956.

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16. E. P. Kovalevskii to M. M. Novikov, August 11, 1930, in garf, f. R-6767, op. 2, d. 46.

17. S. I. Metalnikoff to E. P. Kovalevsky, November 17, 1935, in AHRA, f. 69, op. 2, d. 1, l. 51.

In 1986 the Station received a new name – the Ocean Observatory in Villefranche-sur-Mer (Observatoire Océanologique de Villefranche-sur-Mer) affiliated with the Pierre and Marie Curie University in Paris and the National Centre for Scientific Research (CNRS).

For many years the Paris Academy of Sciences has been bestowing the Tregouboff Prize to eminent scientists who do research in marine biology. In 1984 the prize was awarded to a Soviet scientist, a corresponding member of the Ukrainian Academy of Sciences, a researcher at the A. Kovalevsky Institute of South Seas Biology and the Sebastopol Biological Station Tamara Sergeevna Petipa (1927-1992) who was a major specialist in planktology and marine ecology. The prize was awarded within the framework of the Soviet-French scientific cooperation.

On the internet an interested reader can find a short entry on Grigorii Tregouboff written by a science writer Catherine Gouseff: "Gregory Tregouboff was a scientist of Russian origin, a pioneer in the field of marine biology, an author of several books on Mediterranean planktology. For thirty years Tregouboff was the director of the Oceanological observatories at Villefranche-sur-Mer. He did not hesitate to spend most of his personal fortune trying to preserve the scientific activities of this institution." Perhaps the members of the Paris Academy who set up the Tregouboff Prize believed in this story and were not aware of Tregouboff's abominable conduct towards his colleagues and compatriots. Hopefully, the truth about this person will sooner or later be common knowledge.

VIII

Theodosius Dobzhansky and the "phenomenon" of Pierre Teilhard de Chardin

Mikhail B. Konashev

Among evolutionary scientists, Theodosius Dobzhansky (1900-1975) and Pierre Teilhard de Chardin (1881-1955) occupy a special place.[1] Undoubtedly, their writings had a special value for the development of the evolutionary theory, including the evolutionary concept of man and its reception outside of evolutionary biology in the 20th century. In many respects, thanks to them, the evolutionary concept of man became a part not only of Western or European culture, but of the whole world.[2] Teilhard's works have been apprehended by many as a message to all the people of the world in which Teilhard tried to restore the meaning of human existence and to call people for joint creation in the best of worlds. It is how it was understood by Dobzhansky who was inspired by this message, and became, probably, one of the best defenders and interpreters of Teilhard. He demonstrated convincingly its synthetic character and its importance for the understanding of the unique role of man in his own evolution and in the evolution of the whole universe.

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1. See in detail: C. Cuénot, *Teilhard de Chardin*, Paris, Le Seuil, 1968; J. Arnould, *Teilhard de Chardin*, Paris, Perrin, 2005; F. J. Ayala, "Theodosius Dobzhansky," *Biography Memoirs. National Academy of Sciences*, usa, 55, 1985, p. 163-213; M. B. Konashev, "Rovesnik genetiki, rovesni veka: F. G. Dobrzhanskii (1900-1975)," in *Deayteli russkoi nauki xix-xx vekov*, Saint Petersburg, Nestor-Istoriya, 2008, p. 193-228.

2. See in detail: M. B. Konashev, "Pierre Teilhard de Chardin and Theodosius Dobzhansky: 'synthesis' of the evolutionary theory and belief, and its reception," in J.-G. Barbara, J.-C. Dupont, E. I. Kolchinsky and M. V. Loskutova (eds.), *Russian-French links in biology and medicine*, Saint Petersburg, Nestor Historia, 2012, p. 158-174.

Teilhard's book "The Phenomenon of Man"[3] has brought him a worldwide fame after its translation into English.[4] Dobzhansky read it in French in 1958, and discovered that Teilhard had told much of what he himself wanted to say.[5] At the same time, Dobzhansky pointed out his basic divergences with Teilhard about which he wrote later in several articles. Besides Teilhard's main book, Dobzhansky read some other works by Teilhard and on Teilhard, as well as Teilhard's correspondence published in English.[6] Moreover Dobzhansky participated in several projects devoted to Teilhard and to his works, conferences, including the "conference on Hope and the Future of Man," the symposium "Teilhard de Chardin: In Quest of the Perfection of Man" in 1971, a session of the British Teilhard de Chardin Association,[7] and also in the sessions of the American Teilhard de Chardin Association,[8] which he presided in 1969.[9] Being in Paris in October 1969, Dobzhansky, together with French geneticist and friend, E. Boesiger, visited the *Foundation Teilhard de Chardin*.[10]

According to Dobzhansky, Teilhard's writings "have met with a mixed reception, ranging from hero worship to outright hostility."[11] The reason of such a divergence of opinions was that he was "too much a scientist to set forth his conclusions in a manner wholly to please the theologians, and too much a theologian to make himself altogether a scientist." In addition he "was a poet who often wrote in symbols and metaphors."[12] As he tried in his writings to connect science, religion and poetry, he "was severely criticized from all sides."[13] Moreover

3. P. Teilhard de Chardin, *The Phenomenon of Man*, New York, Harper, 1959.

4. *Id.*, *Le phénomène humain*, Paris, Le Seuil, 1955.

5. apsl. B:D 65. Dobzhansky Papers. Notebooks.

6. P. Teilhard de Chardin (ed.), *Letters to two friends*, New York, New American Library, 1968.

7. apsl. B:D 65. Dobzhansky Papers. Notebooks.

8. *Ibid*.

9. T. Dobzhansky, "Career Summary and Bibliography," in M. K. Hecht, W. S. Steere and B. Wallace (eds.), *Evolutionary biology*, New York, London, Premium Press, 1976, vol. 9, p. 410.

10. apsl. B:D 65. Dobzhansky Papers. Notebooks.

11. T. Dobzhansky, "An essay on religion, death, and evolutionary adaptation," *Zygon*, 1, 1966, p. 325-327.

12. *Id.*, "Pierre Teilhard de Chardin as a scientist," in P. Teilhard de Chardin Pierre (ed.), *Letters to two friends*, New York, New American Library, 1968, p. 221.

13. *Id.*, "Teilhard and Monod – two conflicting world views," *The Teilhard Review*, 8, 1973, p. 36.

"Teilhard was eager to communicate his insights and endeavoured to make them unambiguous and easily assimilated."[14]

Teilhard's concepts were not understood just because there were a combination of science and religion, *i.e.* namely their "synthesis." Teilhard's proponents thought "that, since Teilhard was a scientist, he was able to give a scientific demonstration of his mystical vision, and Teilhard himself nurtured such an illusion. The opponents, among whom G. G. Simpson has given the best reasoned critique, rightly pointed out that the vision does not necessarily follow from the scientific evidence."[15] This misunderstanding "made Teilhard the target of some not wholly unjustified criticisms by other scientists."[16] In this connection Dobzhansky wrote that it "is easier to criticize books for not being what their authors did not intend them to be. By the same token, it is most unwise for overenthusiastic supporters to claim for their favorite authors achievements which these authors were not aiming for."[17] He also concluded that it is precisely "Teilhard's unprecedented many sidedness that has raised him to a position of one of the foremost thinkers of our time. […] In an age of analysis he dared to essay a synthesis."[18]

A supporter, a follower and the ideological successor of Teilhard, Dobzhansky aspired to explain his evolutionary concept. The same evolutionary world view as in "The Phenomenon of Man," without claiming to be purely and simply scientific treatises, can be seen in other Teilhard's books. However, it would be a mistake "to see in Teilhard's writings attempts to derive his religious beliefs from, or to prove them by his science. What he is trying to do is rather to include his science in his total world view which is basically a religious one. Such an attempt is of interest to scientists."[19] Anyway, this attempt to combine and to reconcile science and religion, or, at least, to include science in the general religious outlook has pleased Dobzhansky himself.

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14. *Id.*, , "Pierre Teilhard de Chardin as a scientist," *op. cit.*, p. 221-222.

15. *Id.*,, "An essay on religion, death, and evolutionary adaptation," *op. cit.*, p. 329.

16. *Id.*,, "Teilhard de Chardin and the orientation of evolution", *Zygon*, 3, 1968, p. 247.

17. *Ibid.*, p. 225.

18. *Ibid.*, p. 222.

19. T. Dobzhansky, "Evolution and transcendence," *Main Currents in Modern Thougth*, 22, 1965, p. 8.

However he needed to introduce rather religion in a scientific outlook as a whole rather than science into a religious one.[20]

Teilhard's synthesis, as well as its limitations, was conditioned by the circumstances of the scientific career and of the biography of its author. The four years of First World War represented for Teilhard a time of spiritual tension and maturation. His different activities included his working on some general papers on evolutionary themes, after he had returned to Paris, which "led to his first difficulties with his religious superiors, difficulties that intensified and continued to the end of his life."[21] Later, in 1946, after his return from China to France, his ecclesiastic superiors discouraged him from accepting a professorship at the *Collège de France* in Paris and, at last, in 1950, the authorities in Rome withheld their permission to publish his major works *Le Phénomène Humain* and *Le Groupe Zoologique Humain.*[22]

Dobzhansky wanted not only to explain Teilhard's "synthesis," but also to justify it. From his point of view Teilhard's "synthesis" was necessary, because both science and religion give only an incomplete, limited and unilateral picture of the world and of man in it. The relation of science and religion, according to Dobzhansky, can be understood in three ways. One group holds that science is the sole and only valid source. At the exact opposite, there are those who dismiss science because it deals with impersonal objects, and is consequently irrelevant to the problems of personal existence and selfhood. There is also a middle ground. Knowledge gained from science is as necessary as it is by itself insufficient. It must be supplemented by the insights of poets, artists, mystics, and by religious experience. Dobzhansky was sure that Teilhard just like himself stood firmly on this middle ground, but wrote that Teilhard's position was not quite the same as his own.[23]

In Dobzhansky's opinion, Teilhard did not seek to deduce his religious convictions from, or even to support them by his scientific findings. Teilhard's writings are not natural theology; they deal with a theology of nature. Neither are his books only scientific monographs, and his major work, "The Phenomenon of Man" is "more than that: an attempt to formulate a world view, a *Weltanschauung.*"

20. See in detail: M. B. Konashev, *Evolutsionisty i religiya*, Saint-Petersburg, Nestor-Istoriya, 2011, p. 24-43.

21. T. Dobzhansky, "Teilhard de Chardin and the orientation of evolution," *op. cit.*, p. 223.

22. *Ibid.*, p. 224.

23. *Ibid.*, p. 242.

Therefore his world view "includes science as one of the components, though one of cardinal importance."[24]

Thus, Dobzhansky defended Teilhard from unreasonable and unfair criticisms, first of all those from his compatriots. Having mentioned discussions in France during the autumn of 1965 on the validity of Teilhard's theory of biological evolution, Dobzhansky pointed out that its chief protagonist was Jean Rostand who "has shocked some admirers of Teilhard by saying that there is simply no such thing as Teilhard's theory of biological evolution." Certainly, Rostand was right in the sense that Teilhard deliberately ignored embryology and genetics, but for Dobzhansky "the present-day views concerning these matters happen to be even more favorable for the Teilhardian synthesis than were the views that he himself accepted."[25]

Even less rightful was from Dobzhansky's point of view a criticism of J. Monod, world famous French biologist. In an article especially devoted to the comparison of concepts and outlooks of Teilhard and Monod, he wrote that Teilhard de Chardin and Monod had little in common. Though both were adherents of evolutionary biology, they viewed the evolutionary process in quite different perspectives, and their world views were irreconcilable. If Teilhard embraced the whole range of human experience – scientific, philosophical, esthetic, religious, and mystical – in one coherent world-view, Monod dismissed religion and any philosophy except his own. Therefore he attacked Teilhard, and did so "with extraordinary bitterness."[26] However the Teilhardian synthesis was for many people more attractive and successful.

The key point which determined the opposition between Teilhard's and Monod's concepts was the issue of the direction of evolution. Teilhard was convinced that the universe has a direction and that it could result in some sort of irreversible perfection. To Monod, all ideas of this kind were anathema and evolution had no direction. Dobzhansky agreed that this "problem of direction or 'necessity' vs. 'chance' deserves most careful consideration," but it can be shown "that this dichotomy, applied by Monod to biological as well as to human evolution, is spurious." Because evolution is "neither necessary, in the sense of being predestined, nor is it a matter of chance or accident.

24. *Ibid.*, p. 244.

25. *Ibid.*, p. 226.

26. T. Dobzhansky, "Teilhard and Monod – two conflicting world views," *op. cit.*, p. 36.

It is governed by natural selection, in which ingredients of chance and antichance are blended in a way which makes the dichotomy meaningless, and which renders evolution a creative process."[27]

Certainly, Dobzhansky not only stated, explained and defended Teilhard's "synthesis," but also strongly criticized it, and first of all its biological part, especially Teilhard's understanding, or, more precisely, misunderstanding of the evolutionary role of natural selection. Teilhard "did not realize that natural selection is not building perfect organisms out of piles of unrelated genes; selection acts on a succession of parental and descendant generations modifying the organisms to fit their environments."[28] Dobzhansky also disclosed the contradiction in Teilhard's concept between the mechanism of evolution and orthogenetic character of evolution. He explained that any orthogenetic theory of evolution postulates preformation when all that happens was bound to happen. But this is completely contrary to Teilhard's basic philosophy of universal evolution being a creative process. So any predetermination is "foreign to Teilhard's thinking. If all that happens in evolution is only a long strip tease act, all evolution becomes meaningless."[29]

One of Teilhard's mistakes according to Dobzhansky consisted in that he did not understand that evolution can well be directional without being orthogenetic. Consequently, orthogenesis is only a possible, but neither a necessary nor even a very plausible, explanation of the progressive trend observed in the history of the biosphere and culminating in the emergence of the noosphere. The explanation preferred by most evolutionists is "more in accord with the spirit of the Teilhardian system."[30]

Later Dobzhansky added that Teilhard "seemed to accept, without considering them closely and critically, the orthogenetic and finalistic interpretations of evolution which were rather popular, at least in continental Europe, during the 1920s."[31] It should also be added that these orthogenetic concepts were especially influential in France.[32]

27. *Ibid*.

28. T. Dobzhansky, "Evolution and transcendence," *op. cit*., p. 9.

29. *Ibid.*, p. 9.

30. T. Dobzhansky, "An essay on religion, death, and evolutionary adaptation," *op. cit.*, p. 330.

31. *Id*., "Teilhard de Chardin and the orientation of evolution," *op. cit.*, p. 250.

32. See in detail: V. I. Nazarov, *Finalizm v sovremennom evolutionnom uchenii*, Moscow, Nauka, 1984, p. 284; C. Grimoult, *Histoire de l'évolutionnisme contemporain en France, 1945-1995*, Genève/Paris, Droz, 2000, p. 107-115.

Nevertheless Dobzhansky emphasized the value of his concept and declared that it required development just like "synthesis" did. He agreed with J. Rostand's statement that the work of Teilhard had given splendour to a curious spiritual compound of French literature that was hitherto unknown, but emphasized that Teilhard had no intention of establishing anything resembling a new orthodoxy. Therefore his admirers, scientists and non scientists, "are called upon to develop the work to the beginning of which he devoted his life."[33] Such a revision is patently necessary for the biological side of Teilhard "synthesis" and for all its other components. For Dobzhansky the attractiveness of an evolutionary theodicy created by Teilhard is "that it postulates the growth of freedom as the inner meaning of the history of the cosmos."[34]

The evolutionary concept of Teilhard, his "synthesis" in spite of its lacks and limitations was in Dobzhansky's opinion optimistic. Teilhard's optimism contradicted the general pessimism of an epoch and was based not so much on his science, but on his belief. Dobzhansky quoted Teilhard's words: "Mankind as an organic and organized whole possesses a future: a future consisting not merely of successive years but of higher states to be achieved by struggle. Not merely survival, let us be clear, but some form of higher life or super-life." And then he continued:

> An individual humanizes conscious of his own fragmentariness. However, it is up to him, to some extent at least, to make his existence something more than a "passing whiff of insignificance." He can contribute, if he so chooses, to the achievement of a higher life for himself and for the world of which he is a part.[35]

Just like Teilhard, Dobzhansky wished that everyone would be inspired by this optimism as well as by a sense of responsibility for the future. Therefore it is the reason why he concluded some of his articles and even his book by his favourite quotation from Teilhard:

> The outcome of the world, the gates of the future, the entry into the super-Human – these are not thrown open to a few of the privileged, nor to one chosen people to the exclusion of all others. They will open to an advance

33. T. Dobzhansky, *op. cit.*, 1968, p. 227.
34. *Id.*, "Evolution: implication for religion," *Christian Century*, 19, 1967, p. 940.
35. *Id.*, "An essay on religion, death, and evolutionary adaptation," *op. cit.*, p. 331.

> of *all together*, in a direction in which *all together* can join and find fulfilment in a spiritual renovation of the earth.[36]

Perhaps therefore Dobzhansky who on his 70th anniversary quoted those words of Teilhard most precisely expressed the sense of phenomenon of Teilhard de Chardin:

> Don't get lost in vain inner self-examination about your capacities and value. But tell yourself, categorically, that for the success of the enormous work of creation, God only needs one thing: that you should do your best. As soon as you give what you are capable of giving you are united in maximum measure to the creative Act; you couldn't be a more useful servant.[37]

36. See, for example: *id.*, "Evolution and transcendence," *op. cit.*, p. 9; *id.*, *The Biology of Ultimate Concern*, New York, The New American Library, 1967, p. 137.

37. APSL. B:D 65. Dobzhansky Papers. Notebooks.

IX

Origine et portée du cosmisme de Vladimir Vernadski

Étienne Aucouturier et Daria Popova

INTRODUCTION

Depuis la redécouverte des travaux de Vernadski par l'écologie moderne[1] – biosphérale et écosystémique[2] – et la reconnaissance institutionnelle par l'Unesco en 1960 de la notion de *biosphère*[3], les aspects scientifiques de ses travaux sont bien connus et étudiés. Le cadre conceptuel de ses travaux sur la Biosphère et la *noosphère*, est cependant marqué par un certain mysticisme et un irrationalisme, propres au courant cosmiste russe de son temps, un courant de pensée, ou une tradition métaphysique peu connue hors de Russie. C'est en effet dans une perspective cosmiste qu'il faut replacer les travaux scientifiques de Vernadski, qui avait en vue d'établir par les sciences de la nature, que la vie n'est pas un phénomène seulement terrestre, mais aussi et premièrement cosmique. Son approche pluridisciplinaire et globale de la vie, qui lui a valu bien des décennies plus tard d'être considéré comme un pionnier de l'écologie contemporaine, a cependant cela de commun avec des ensembles théoriques tels que le positivisme d'Auguste Comte, de tenter de rendre compatibles entre eux trois éléments en apparence opposés : un réductionnisme s'agissant de la compréhension des phénomènes naturels, une métaphysique ou un versant religieux, et un finalisme, biologique et social. Nous examinons premièrement, dans notre article, le cadre métaphysique cosmiste russe dans lequel

1. J. D. Oldfield et D. J. B. Shaw, « V.I. Vernadskii and the development of biogeochemical understandings of the biosphere, c.1880s-1968 », *The British Journal for the History of Science*, 46, 2, 2013, p. 287-310.

2. P. Acot, « Biosphère et biodiversité », *Aménagement et Nature*, 128, 1998, p. 31-35.

3. V. I. Vernadski, *La Biosphère* [1926], Paris, Le Seuil, 2002.

Vernadski a constitué sa biogéochimie. Nous entrons ensuite un peu plus dans le détail de la conceptualisation des deux notions centrales de sa biogéochimie, la Biosphère et la noosphère. Puis nous nous demandons brièvement si la biogéochimie de Vernadski peut être qualifiée, comme elle l'est souvent, de holiste. Enfin nous proposons une brève analyse comparative de la métaphysique de l'ethnologue français Jean Malaurie (1922), fondateur de l'Académie polaire de Saint-Pétersbourg.

I. LA MÉTAPHYSIQUE COSMISTE RUSSE ET LA NOTION DE COSMOS CHEZ VERNADSKI

Le cosmisme russe est un composé de croyances, mêlant à l'atomisme grec ancien et au rationalisme occidental – mais aussi au romantisme allemand en particulier le « cosmisme naturaliste[4] » de Friedrich Schelling –, des traditions orientales[5] et issues de la culture russe médiévale. Ce mouvement a cela d'étonnant qu'il s'inspira des avancées scientifiques de son temps, qu'il mobilisa pour tenter d'appuyer en retour – avec une dose de scientisme – des hypothèses métaphysiques. Il est marqué par un syncrétisme liant les sciences avec la religion, la métaphysique, la poésie, ou encore les arts plastiques. Le cosmisme russe est particulièrement représenté dans des textes et des œuvres d'ordre métaphysique de savants, de philosophes et d'artistes. Il est très marqué par la croyance en une catastrophe planétaire future, une crise globale mais remédiable, que l'humanité pourrait surmonter, par la raison : les sciences de la nature y sont ainsi conçues comme un moyen d'atteindre l'une des finalités naturelle de l'homme, qui est de protéger l'humanité et la vie. Cette ambition ou cette fin que l'on peut qualifier de cosmopolitique, est dès lors incluse, initialement, dans le projet vernadskien de caractériser par les sciences de la nature l'unité cosmique de l'homme et des conditions physico-chimiques de son existence.

On distingue au sein du cosmisme russe trois traditions ou courants : scientifique, théologico-philosophique et esthétique. Cela est

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4. G. P. Kovaleva, « Naturfilosofskii kosmizm F. Shellinga » [Le cosmisme naturaliste de F. Schelling], *Mezhdunarodnyi nauchno-issledovatel'skii zhurnal*, noiabr, 2012 [notre traduction].

5. V. Boudina, « L'atelier créatif des jeunes savants », *Sodroujestvo*, 9, février-mars 2003.

conventionnel car la plupart des scientifiques, penseurs et artistes s'en réclamant, interagissaient dans ces trois domaines. Les principaux représentants du courant scientifique sont Nikolaï Fedorov (1829-1903), Constantin Tsiolkovski (1857-1935), Vladimir Vernadski et Alexandre Tchijevski (1897-1964). La tradition religieuse est principalement représentée par Vladimir Soloviev (1853-1900). Le peintre Nicolas Roerich (1874-1947) est, quant à lui, l'une des figures principales du courant artistique cosmiste russe[6].

Cet étrange mélange des genres scientifique, théologique, artistique et politico-moral, ainsi que cette étrange peur partagée entre des scientifiques, des penseurs, des théologiens et des artistes, d'un *Armageddon*, sont éclairants pour comprendre à la fois l'œuvre scientifique de Vernadski, mais aussi ses succès ultérieurs et contemporains, tant dans la constitution de l'écologie des écosystèmes[7] dans les années 1950, que dans ses utilisations politiques ultérieures. La tradition scientifique, qui nous intéresse particulièrement concernant les travaux de Vernadski, fut dans ce cadre culturel une tentative de construire une image du monde cosmique, s'appuyant sur les méthodes et les données des sciences de la nature, afin de chercher une issue à la crise à venir, conçue comme inévitable.

Parmi les nombreuses références de Vernadski, l'œuvre du naturaliste allemand Friedrich von Humboldt (1759-1869), en cinq volumes, intitulée *Cosmos*[8] (1845-1859) tient une place de choix. Vernadski l'a lue et s'en est inspiré – particulièrement de sa formulation de la notion de sphère de la vie, la *lebensphere*, dont Vernadski s'inspira pour conceptualiser sa *biosphère*. L'une des influences majeures de Vernadski fut aussi l'œuvre du célèbre naturaliste russe Mikhaïl Vassilievitch Lomonossov (1711-1765), dont l'atomisme l'inspira.

À titre d'exemple et afin d'éclairer le cadre métaphysique dans lequel Vernadski élabora sa biogéochimie, illustrons-le par une brève description des croyances de Constantin Tsiolkovski, le père de l'astronautique, qui a pensé non seulement la possibilité d'un transport

6. V. N. Dudenkov, *Filosofiia kosmizma v Rossii rubezha 19 i 20 vekov* [La philosophie du cosmisme en Russie à la limite des XIXe et XXe siècles], Izd-vo Sankt-Peterburgskogo gosudarstvennogo texnologicheskogo instituta, Spb, 1998.

7. J. D. Oldfield et D. J. B. Shaw, « V.I. Vernadskii and the development of biogeochemical understandings of the biosphere, c.1880s–1968 », art. cit., p. 287-310.

8. A. von Humboldt et E. Buschmann, *Kosmos, Entwurf Einer Physischen Weltbeschreibung*, Stuttgart, J. G. Tübingen, Cotta'scher Verlag, 1845.

interplanétaire et les premières fusées d'exploration spatiale, mais a aussi élaboré une métaphysique hylozoïste, atomiste et progressiste.

Selon lui, il existe une sensibilité de tout l'univers, inséparable de la matière. Tsiolkovski conçoit l'univers comme un système doué d'une sensibilité, à différents niveaux, et postule l'inexistence de matière inerte[9]. La matière de l'univers est spirituelle et une[10]. Ses propriétés sont l'immortalité et l'étendue infinie. Tous les êtres vivants sont, de ce point de vue, des unités issues d'un atome originel d'éther[11], dont elles ont en partage des éléments. La mort de ces unités ne conduit pas à celle des atomes les composant, qui migrent alors dans une autre forme. En outre, il y a selon lui une progression naturelle des formes vivantes, c'est pourquoi il soutient que « la matière se rétablit et donne une autre vie, qui selon la loi du progrès sera plus parfaite[12] ». Toute la matière de l'univers, des bactéries jusqu'aux entités astronomiques, étoiles et planètes comprises, est mêlée et vivante : les corps simples se transforment en corps plus complexes, qui à leur tour se décomposent pour composer d'autres formes. De même l'organisme humain se compose d'atomes, de parties de matière, qui se trouvaient autrefois en des lieux différents : cette union d'atomes est temporaire et la mort n'est ainsi qu'une illusion de la sensibilité humaine.

Vernadski fondait aussi ses représentations du monde sur l'atomisme et sur la croyance principale des cosmistes russes en l'unité des processus cosmiques et terrestres. Outre les travaux de Pierre Teilhard de Chardin et la métaphysique bergsonienne, souvent cités en référence, l'une de ses références scientifiques fut aussi l'œuvre du géologue nord-américain

9. K. E. Tsiolkovskii, *Kosmicheskaia filosofiia* [La philosophie cosmique] [1923], Cfera, 2004, p. 32-33 [notre traduction]. « Comme toute la matière peut toujours dans des circonstances favorables passer à l'état organique, nous pouvons dire que toute la matière non-organique est virtuellement vivante […]. Toute la matière est unie et ses particules élémentaires doivent être semblables dans tout l'univers. Elles se caractérisent par les trois propriétés suivantes : le temps, l'espace et la sensation. Attribuer la sensation à une partie du cosmos contredirait les principes du monisme cosmique. Chaque animal est un petit univers. Le cosmos ne diffère que par sa dimension. »

10. *Ibid*.

11. Notons ici la très grande proximité des postulats de Tsiolkovski avec ceux des atomistes anciens, tel Épicure, qui dans sa « Lettre à Hérodote » (§ 40-41) postule que les corps dont les composés sont faits, sont à la fois indivisibles et immuables, et que l'âme elle-même est un de ces composés (§ 63).

12. K. E. Tsiolkovskii, *op. cit.*

James Dwight Dana (1813-1895)[13], en particulier sa géologie dynamique, et sa métaphysique, exposée dans l'introduction de son *Manuel de géologie*[14] (1862), avant que celui-ci n'adhère, avec des réserves, à la théorie darwinienne de l'évolution[15]. La planète terre y est conçue par Dana comme un individu comparable à un animal terrestre, mais appartenant à un « règne[16] » non terrestre, déterminée par un être divin qu'il nomme « Esprit Infini[17] ». L'humanité à la surface de cette planète est elle-même soumise à ces niveaux de détermination, car la terre « œuvre à son but final[18] » qui est notamment d'aider à « faire avancer le progrès de l'homme en tant que race[19] ».

III. BIOSPHÈRE ET NOOSPHÈRE

Selon Vernadski les sciences naturelles – en particulier la géochimie dont il fut l'un des fondateurs – montrent que la matière vivante dans son ensemble est organisée à la surface de la terre selon des cycles,

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13. V. I. Vernadskii, *Biosfera i noosfera* [Biosphère et noosphère], Aiiris Press, 2004, p. 478.

14. J. D. Dana, *Manual of geology*, New York, Blakeman, Taylor & Co., 1862 [4e éd.]. « Science, in her survey of the earth, has recognized three kingdoms of nature […] the animal kingdom, the plant kingdom, and the crystal kingdom. An animal in either kingdom has its systematic mode of formation or growth. […] But the earth also, according to Geology, has been brought to its present condition through a series of changes or progressive formations, and from a state as utterly featureless as a germ. Moreover, like any plant or animal, it has its special systems of interior and exterior structure, and of interior and exterior conditions, movements, and changes; and, although Infinite Mind has guided all events toward the great end, – a world for mind, – the earth has, under this guidance and appointed law, passed through a regular course of history of growth. Having, therefore, as a sphere, its comprehensive system of growth, it is a unit or individuality, not, indeed, in either of the three kingdoms of nature which have been mentioned, but in a wider, – a World Kingdom. Every sphere in space must have had a related system of growth, and all are, in fact, individualities in this Kingdom of Worlds. […] the sphere before us is an individual, as much as a dog, or a tree; and, to arrive at any correct views on these subjects, the world must be regarded in this capacity. The distribution of man and nations, and of all productions that pertain to man's welfare, comes in under the same grand relation; for, in helping to carry forward man's progress as a race, the sphere is working out its final purpose. »

15. W. F. Sanford, « Dana and Darwinism », *Journal of the History of Ideas*, 26, 4, 1965, p. 531-546.

16. *Ibid.*

17. *Ibid.*

18. *Ibid.*

19. *Ibid.*

et qu'il y existe des échanges constants entre l'organique et l'inorganique. La Biosphère est l'une des régions terrestre, que Vernadski caractérise comme un espace biogéochimique de l'écorce terrestre se développant selon ses propres lois, par rapport à la lithosphère. Ses recherches et ses thèses en biogéochimie – mais il fut aussi un historien des sciences prolifique – sont principalement exposées dans deux opuscules datés de 1926, dont le premier est central, réunis sous le nom de *La Biosphère* et comprenant *La Biosphère dans le cosmos* – notons ici ce complément, *dans le cosmos* – et *Le domaine de la vie.*

L'énergie solaire est selon lui une force fondamentale qui forme la Biosphère et permet que s'accomplissent les cycles géochimiques. La vie sur terre est un phénomène impossible sans le rayonnement du soleil. La Biosphère, qui comprend l'hydrosphère, la partie haute de la lithosphère et la partie basse de l'atmosphère, est pénétrée par des émissions cosmiques. Les éléments de la Biosphère accumulent et répartissent l'énergie reçue de ces émissions en les transformant en énergie électrique, chimique, mécanique, thermique. Selon Vernadski ces émissions cosmiques proviennent non seulement du soleil, mais aussi de tous les corps célestes :

> Il est certain que [ce sont] les rayons du soleil qui déterminent les traits principaux du mécanisme de la Biosphère, […] dans la vie des organismes, dans le mouvement et l'activité des vents ou des courants marins, dans l'activité des glaciers, dans le mouvement des rivières et leur formation, et dans le travail colossal des dépôts de neige et de pluie.[20]

Il s'agit de cycles biogéochimiques, qui sont des cycles de répartition des éléments chimiques dans la Biosphère. Ces éléments chimiques migrent constamment d'un organisme à un autre et d'une région à l'autre : du sol, de l'atmosphère et de l'hydrosphère, ils passent dans les organismes vivants, lesquels vivent, meurent, et se décomposent, s'ajoutant ainsi à la matière non-vivante de la Biosphère.

Parmi les phénomènes biogéochimiques terrestres, Vernadski inclut aussi la raison humaine. Il soutient – et en cela les tenants contemporains de la thèse de l'existence d'une ère géologique nouvelle, l'*anthropocène*[21], n'ont fait que reprendre la thèse vernadskienne –, que la raison humaine est une force géologique, qui s'est développée

20. V. I. Vernadski, *La Biosphère*, éd. cit.
21. P. J. Crutzen, « Geology of Mankind », *Nature*, 415 (6867), 23, 3 janvier 2002.

au cours de très nombreuses années, mais qui se manifeste tardivement – à l'ère industrielle –, quand l'homme commence à coloniser l'intégralité de la Biosphère. Cet homme moderne est une force géologique car en un temps court relativement à l'échelle géologique, et par la science et la technique, il est en mesure de modifier significativement à lui seul ce que Vernadski appelle la « migration biogène[22] » : le déplacement de masses d'éléments chimiques, par les déplacements et les activités du vivant, dans la Biosphère. Cette idée que l'esprit de l'homme est non seulement issu – et fait partie – du cosmos, mais qu'il est en outre une force géologique sur terre, est ce que Vernadski a dénommer la *noosphère*, ou sphère de la pensée, de l'intellect ou de l'esprit. Il caractérise cette sphère de la pensée comme étant intrinsèquement liée à la Biosphère : « Une nouvelle force géologique sans précédent, celle de la raison et de la technique, s'est créée dans la matière vivante, nous semble être illimitée et sortira peut-être des limites de notre planète[23]. »

Ainsi la noosphère peut-elle être conçue comme un état du développement de la Biosphère qui s'oriente dans le sens des intérêts de l'homme, de sa survie, mais aussi comme une force physico-chimique : « Notre pensée existe comme un événement réel mondial, pas seulement en qualité de contenu de la conscience, d'image du monde[24]. »

Vernadski a ainsi une compréhension réaliste et naturaliste – nous pourrions dire même matérialiste – de la raison humaine et soutient en même temps que cette raison est une partie du cosmos, s'étudiant lui-même et contribuant à la poursuite d'une fin cosmique, qui est en même temps un bien cosmique. Cette idée est issue principalement des thèses de James Dwight Dana – et de Joseph Le Conte (1823-1901), un autre géologue américain – sur la céphalisation[25]. De la complexification historique des systèmes nerveux que révèlent les données empiriques paléontologiques, Dana autant que Vernadski concluent à une progression et à la position privilégiée et pionnière de l'homme parmi les autres vivants.

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22. V. I. Vernadski, *La Biosphère*, éd. cit.

23. V. N. Doudenkov, *La philosophie du cosmisme en Russie à la limite des* XIX^e^ *et* XX^e^ *siècles*, Spb, Maison d'édition de l'Institut technologique d'État de Saint-Pétersbourg, 1998.

24. F. I. Girenok, *Uskol'zaiushchee bytie* [L'entité fuyante], Rossiiskaia akademiia nauk, Institut filosofii, Moskva, 1994, p. 196 [notre traduction].

25. V. I. Vernadski, « Some Words about the Noosphere », *American Scientist*, 1945.

Dans son opuscule intitulé *L'autotrophie de l'humanité* (1925), empreint de « socialisme scientifique[26] », Vernadski va ainsi jusqu'à soutenir que l'humanité doit pouvoir accéder et que ce serait un progrès, par la science, à l'autotrophie :

> Il existe dans l'écorce terrestre une grande force géologique – peut être cosmique – dont l'action planétaire n'est généralement pas prise en considération dans les concepts du Cosmos, concepts scientifiques ou basés sur la science. […] Cette force c'est l'entendement humain, la volonté dirigée et réglée de l'homme social. […] L'homme est un animal social hétérotrophe. Il ne peut exister qu'en présence des autres organismes, des plantes vertes spécialement. […] L'entendement, qui le distingue dans les cadres de la matière vivante, crée dans cette dernière des traits étonnants, change profondément son action sur le milieu ambiant. […] Par son intermédiaire l'homme utilise la matière ambiante - brute ou vivante – non seulement pour la construction de son corps, mais aussi pour sa vie sociale. Et cette utilisation devient une grande force géologique. L'homme est l'*Homo faber* de M. H. Bergson. Il change l'aspect, la composition chimique et minéralogique du milieu de son habitation. Ce milieu de son habitation est toute la surface de la Terre. […] Il y a introduit des masses immenses de nouveaux composés chimiques inconnus et des formes nouvelles de la vie - les races des animaux et des plantes. […] Pour résoudre la question sociale, il est nécessaire de toucher aux fondements de la puissance humaine – de changer la forme de la nourriture et les sources de l'énergie que l'homme utilise. […] La synthèse des aliments, libérée de l'intermédiaire des êtres organisés, quand elle sera accomplie, changera l'avenir humain. Que signifierait une synthèse pareille des aliments dans la vie humaine et dans la vie de la Biosphère ? Par son accomplissement, l'homme se libérerait de la matière vivante. D'un être social hétérotrophe il deviendrait un *être autotrophe*.[27]

Ainsi Vernadski (1863-1945) affirme-t-il en 1943, pendant la deuxième guerre mondiale, que « la noosphère est le dernier de nombreux stades de l'évolution de la Biosphère dans l'histoire géologique[28] ». Nous précisons cela car Vernadski continue d'affirmer, au crépuscule de sa vie, que ses recherches en géologie ont à voir avec la politique

26. *Id.*, « L'autrotrophie de l'humanité », *Revue générale des sciences pures et appliquées*, 36, 1925, p. 495-502.
27. *Ibid.*
28. *Ibid.*

et les processus historiques, que « la première guerre mondiale a eu des effets décisifs sur [son] travail scientifique [et qu'elle] a radicalement changé sa *conception géologique du monde*[29] ».

IV. VERNADSKI EST-IL VRAIMENT HOLISTE ?

Ses thèses sont couramment qualifiées de holistes, notamment par le philosophe et historien Jacques Grinevald[30], en particulier car Vernadski a réussi à introduire la notion d'une « matière vivante[31] », afin de cesser de différencier qualitativement l'inorganique de l'organique : on le comprend, la matière peut devenir vivante, sous certaines conditions physico-chimiques. Mais Vernadski ne soutient pas, comme un Tchijevski, que le cosmos est un et vivant. Pourtant, il se défend aussi de soutenir la thèse de l'abiogenèse, selon laquelle le vivant est issu de l'inerte. Dans *La Biosphère*, il se défend de vouloir rechercher des causes génératrices de la vie et dit s'en tenir aux généralisations empiriques disponibles en son temps :

> Les deux représentations dominantes de la vie, vitaliste et matérialiste, sont aussi les reflets d'idées philosophiques et religieuses […] et non de déductions tirées de faits scientifiques. Ces deux représentations entravent l'étude des phénomènes vitaux, et troublent les généralisations empiriques[32].

Il soutient en ce sens que la question de l'origine de la vie peut au mieux faire l'objet d'hypothèses, et que l'hypothèse de l'origine extra-terrestre de la vie est biochimiquement aussi tenable que les autres, s'il y a une continuité de l'inorganique à l'organique dans la biogenèse et si le rayonnement solaire est indispensable à ce processus considéré comme universel.

Chez Vernadski, le vivant est ainsi à la fois massivement déterminé dans ses formes et ses propriétés par des contraintes physiques extra-terrestres – en particulier pour ce qu'il peut en démontrer en son temps, par le rayonnement solaire –, mais dispose aussi, par cette même détermination, de propriétés distinctes de la matière non-vivante.

29. *Ibid.*
30. J. Grinevald, « On a holistic concept for deep and global ecology : The Biosphere », *Fundamenta scientiae*, 8, 2, 1987, p. 197-226.
31. V. I. Vernadski, *La Biosphère*, éd. cit.
32. *Ibid.*

Il existe donc pour Vernadski au moins une distinction entre l'organique et l'inorganique. Mais cette distinction est selon lui quantitative et non qualitative. Elle provient de la révolution méthodologique qu'il entend mener quant à la compréhension des phénomènes terrestres, y compris pour sa partie vivante, ou dirons-nous plutôt pour sa partie conditionnant la possibilité du vivant, la Biosphère :

> […] la théorie de l'évolution […] ne joue presque aucun rôle en géochimie. […] Ainsi l'espèce est habituellement considérée dans la biologie du point de vue *géométrique* ; la forme, les caractères *morphologiques*, y occupent la première place. Dans les phénomènes biogéochimiques, au contraire, celle-ci est réservée au nombre et l'espèce est considérée du point de vue *arithmétique*. […] Dans les processus biogéochimiques il est indispensable de prendre en considération les constantes numériques suivantes : le *poids* moyen de l'organisme, sa *composition chimique élémentaire moyenne* et l'*énergie géochimique* moyenne qui lui est propre, c'est-à-dire sa faculté de produire des déplacements, autrement dit « la migration » des éléments chimiques dans le milieu vital. Dans les processus biogéochimiques ce sont la *matière* et l'énergie qui sont au premier plan au lieu de la forme inhérente à l'espèce. […] Vu sous cet angle, l'espèce du biologue [sic] peut être envisagée comme une *matière vivante homogène*, caractérisée par la masse, la composition chimique élémentaire et l'énergie géochimique[33].

Son appareil théorique est donc un réductionnisme – semblant désenchanter totalement ce que nous pourrions attendre d'une conception cosmiste russe du monde – selon lequel la connaissance scientifique du vivant implique de le considérer initialement comme un ensemble de masses inanimées de matière : Vernadski inclut le vivant dans un champ théorique plus englobant que celui de la biologie, car le vivant est déterminé selon lui dans ces formes au niveau de la chimie et de la physique, plutôt que par les lois – qui sont plutôt des règles – de la biologie. Il pose aussi simultanément l'existence de forces de progrès, émergeant de la matière, telles l'entendement, entendu comme une force géologique, ce qui peut laisser penser que ce réductionnisme n'est que méthodologique et non ontologique. Mais rien n'interdit cependant de penser, en particulier parce que Vernadski insiste sur la nécessité de s'en tenir aux généralisations empiriques des sciences de la nature dans la démarche scientifique, qu'il considère ces propriétés du vivant comme

33. *Ibid.*, p. 252-253.

totalement déterminées par leurs propriétés physico-chimiques, en particulier puisqu'il rejette explicitement le vitalisme dans *La Biosphère*.

La connaissance de l'unité de la nature, chère à Humboldt et Vernadski, passe donc chez ce dernier par une réduction préalable à ses particules élémentaires. Vernadski substitue ainsi, d'un point de vue méthodologique, à la spécialisation des sciences de la nature de son temps une réunion sous un champ qu'il nomme biogéochimie. Il semble que c'est en cela, et en cela seulement, qu'il pourrait être métaphoriquement qualifié de holiste, car il considère qu'il manque un instrument théorique unique qui permettrait d'analyser la complexité des grands phénomènes terrestres, un instrument qu'il tente de créer en examinant les intersections entre les champs de la biologie, de la géologie, de la géographie, de la pédologie de son maître Vassili Vassilievitch Dokoutchaïev (1846-1903), de la physique et de la chimie. Il semble donc que les notions de biosphère et de noosphère soient instrumentales au sein de sa théorie biogéochimique et ne désignent pas des entités ontologiquement homogènes. Cependant, ses écrits politiques et métaphysiques laissent parfois penser le contraire, à savoir qu'il existe un être vivant de la taille de la Biosphère identique à elle.

V. BIOGÉOCHIMIE ET ETHNOLOGIE : LA MÉTAPHYSIQUE D'INSPIRATION COSMISTE DE JEAN MALAURIE

Il nous a semblé intéressant d'établir finalement et brièvement un lien théorique, autant que géographique et probablement culturel, entre la métaphysique de l'ethnologue et géographe français Jean Malaurie (1922-.), qui fut parmi les fondateurs de l'Académie polaire de Saint-Pétersbourg, et les enjeux métaphysiques cosmistes sous-jacents de la biogéochimie de Vernadski. En effet c'est une référence peu souvent citée parmi les héritiers des conceptions cosmopolitiques de Vernadki, qui cependant, dans ses textes politiques, utilise une métaphysique proche de celle de Vernadski. Comme lui, Jean Malaurie inaugure, dans ses études admirables de son objet de prédilection, les terres arctiques et ses habitants, une méthode à la fois scientifique et couplée à une métaphysique des liens entre une expérience humaine primitive ou première et les différents niveaux d'étude des phénomènes terrestres. Il conçut en outre, au plan politique mais sur la base de cette métaphysique, une des personnifications ou vitalisations de notre planète, la terre, telle qu'en conçut à peu près à la même époque le cybernéticien et biologiste anglais James Lovelock (1919-.), avec l'hypothèse « Gaïa[34] », selon laquelle la terre est elle-même un être vivant.

Par cette autre personnification, que Jean Malaurie nomma « Terre mère[35] », dans un discours prononcé à l'Unesco en 2007, celui-ci vise semble-t-il à reprendre des aspects du discours politique pionnier – et matérialiste historique – de Vernadski, notamment dans l'*Autotrophie de l'humanité*, concernant la nécessité vitale pour l'humanité de contrôler son propre usage des ressources – alimentaires et généralement énergétiques – terrestres, qui y sont en quantité limitée :

> Nous sommes des veilleurs de nuit face à une mondialisation sauvage, à un développement désordonné ; et si nous n'y prenons pas garde, ce sera un développement dévastateur. La Terre souffre. Notre Terre Mère ne souffre que trop. Elle se vengera. Et déjà les signes sont annoncés.

Nous pouvons remarquer dans cette citation à la fois l'idée que la planète terre est dotée d'une sensibilité, et en même temps, qu'une catastrophe est imminente pour l'humanité, thèmes chers aux cosmistes russes.

Parallèlement et parfois au sein de ses très nombreux travaux de géographe et d'ethnologue incontournables pour qui veut connaître l'arctique et ses populations, Jean Malaurie évoque aussi une « prescience primitive[36] » des hommes qu'il a pu rencontrer, principalement des Inuits, avec lesquels il a vécu en totale immersion pendant de longues périodes, et qu'il a défendus contre les menaces que faisait peser sur leur habitat les sociétés industrielles, en particulier les projets militaires américains dans l'arctique :

> Penser avec ses mains, ses pieds et ses cinq sens, ainsi procède l'hyperboréen qui, en s'insérant dans la nature jusqu'au plus profond, peut, par le chamanisme, en appréhender le sens et le cours. De proche en proche le chercheur découvre ainsi des géosystèmes géographiques et historiques. […] Le géosystème des hautes latitudes se retrouve sur le plan ethnologique : les chasseurs hyperboréens n'ont développé leur histoire – dans le cadre hostile qui est le leur – qu'en s'inscrivant dans une connaissance intime et inconsciente des lois

34. J. E. Lovelock, *La Terre est un être vivant : l'hypothèse Gaïa*, Paris, Flammarion, 2010.

35. J. Malaurie, *Terre Mère*, Paris, éd. CNRS, 2008.

36. *Ibid*.

> des milieux naturels de l'écosystème, dans une organisation et des alliances rigoureuses qui en sont l'expression écosystémique[37].

Nous pouvons comprendre cette croyance de Jean Malaurie en l'existence d'une prescience de leur unité, ou de leur très forte interdépendance avec la terre, chez des populations épargnées par l'industrialisation, comme une idée lamarckienne, ou du moins progressiste, selon laquelle les pensées de populations humaines n'ayant pas atteint le stade industriel, seraient en quelque sorte chronologiquement plus proches de l'origine, et donc d'un lien presque organique entre l'inanimé et l'animé, dans l'hypothèse d'une abiogenèse, et d'une évolution graduelle. Cette « préscience primitive[38] » ou cette « innocence native[39] » nous fait défaut et nous met en péril, nous êtres humains rationnels de l'ère industrielle, selon Jean Malaurie :

> […] il est d'autres représentations de la nature et de l'espace qui ne répondent pas, ou répondent partiellement à ces principes de causalité [de la physique contemporaine]. La pensée dite « prélogique » des peuples « primitifs », dans leurs « pensées sauvages », si méprisée par la science rationnelle est une image de la nature ressentie comme cohérente par des générations de peuples traditionnels. Serait-elle d'un enseignement moderne ? Pourrait-elle nous donner une clef de la géographie sacrée des constructions de ces peuples ? Ne convient-il pas de se glisser dans leur structure de pensée pour tenter de saisir leur organisation spatio-temporelle ? Telle est la question posée[40].

Il nous semblerait intéressant de chercher à reconstituer – mais cela nécessiterait un travail de recherche bien plus approfondi en histoire et philosophie de l'écologie que celui dont notre article est issu – la généalogie de ces thèses, en apparence au moins issues du cosmisme russe, au sein de l'écologie contemporaine, ou plus généralement des sciences de la vie et de la terre. Il nous semble que cette métaphysique est demeurée très influente pour l'écologie, qui s'est elle-même montrée en outre régulièrement récalcitrante à s'appuyer définitivement

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37. J. Malaurie, *Les derniers rois de Thulé : avec les Esquimaux polaires, face à leur destin*, 5e édition et suivie d'un dossier *Débats et Critiques*, Paris, Plon, coll. « Terre Humaine », 1989.

38. *Id.*, *Terre Mère*, éd. cit.

39. *Ibid.*

40. J. Malaurie, *Les derniers rois de Thulé : avec les Esquimaux polaires, face à leur destin*, éd. cit.

sur la théorie darwinienne de l'évolution et sur la synthèse moderne, contrairement aux autres champs de la recherche en biologie.

Il serait tout aussi important d'examiner en détail le degré de surinterprétation ou plus simplement d'incompréhension des travaux scientifiques de Vernadski, qui leur ont valu d'être parfois contre employés, ou à contresens. Il est en effet possible de considérer aussi, par l'ampleur cosmique de la vie, que la terre n'est selon Vernadski en aucun cas une limite à l'expansion de la vie dans l'univers. Il n'y a donc pas nécessairement de lien direct entre la connaissance des grands systèmes terrestres et l'injonction contemporaine selon laquelle faudrait les préserver. Aussi des thèmes politiques et moraux contemporains comme la préservation de la diversité ou la lutte contre les pollutions physico-chimiques ne sont pas nécessairement des thèmes vernadskiens, bien que ses travaux soient parfois mobilisés à ces fins. Car nous pouvons trouver dans son œuvre une forme d'optimisme et de croyance en l'avènement d'un homme doté d'un entendement qui lui permettra de surmonter et dominer les conditions de possibilité biologiques de son existence. C'est en substance ce que Vernadski soutenait dans son texte sur « l'Autotrophie de l'humanité ». N'excluant pas, en somme, que nous puissions manger un jour des cailloux pour survivre, lorsque nous serons arrivés à un niveau suffisant de connaissance des processus physico-chimiques déterminant la vie, Vernadski ne considérait pas les modifications physico-chimiques de la Biosphère comme ayant particulièrement de valeur morale au regard de la préservation de la vie.

CONCLUSION

De la même manière que l'alchimiste prolifique Newton[41] n'a pas convaincu ses héritiers physiciens relativement à cet aspect – quantitativement majeur – de ses recherches, le cosmiste Vernadski peine à convaincre quant à ses thèses métaphysiques. Cependant, si cet aspect de ses recherches éclaire avec étrangeté les raisons ou motivations intimes de sa volonté d'intégrer les phénomènes terrestres, il n'en demeure pas moins que ce projet a été admirablement mené, a fait des émules, et non des moindres, puisque sa notion de la Biosphère est aujourd'hui internationalement reconnue comme étant d'une importance scientifique

41. C. Chalquist, « Sir Isaac Newton, Alchemist », *Psychological Perspectives*, 52, 2, 2009, p. 199-218.

autant que politique majeure. Il est en un sens un exemple caractéristique de ce que l'irrationalité et la croyance ne sont pas incompatibles avec la rationalité et la science, et qu'elles peuvent être même, au contraire, considérées d'un point de vue de l'histoire des sciences, comme de bonnes raisons de raisonner. C'est en tout cas ce que nous permet de considérer la recherche historique sur la genèse des concepts scientifiques, à savoir qu'il existe des raisons non scientifiques de faire de la science, une forme de causalité irrationnelle déterminant la production d'énoncés rationnels.

X

La réception des travaux sur les origines de la vie d'Alexandre Oparine. Le cas de l'édition française de son ouvrage *L'origine de la vie sur la Terre* (1964) par Pierre Gavaudan

Stéphane Tirard

INTRODUCTION

Alexandre Ivanovitch Oparine (1894-1980) compte parmi les grandes figures de la recherche sur les origines de la vie au XX^e siècle. Il formula sa théorie pour la première fois en 1924 et se spécialisa sur cette question durant toute sa carrière. En 1965, son œuvre connut une réception élargie en France par la parution, en français, d'un de ses plus importants ouvrages, *L'Origine de la vie sur la Terre*[1], édité par le biologiste français Pierre Gavaudan (1905-1985).

Le présent article se propose de rappeler quelle fût la teneur de la théorie d'Oparine qu'il publia en 1965 et comment dans les années d'après-guerre, il tentait de proposer une théorie compatible avec le contexte lyssenkiste que subissait la biologie de son pays. Oparine centra sa théorie sur l'étude de l'origine du métabolisme, en délaissant la question du support de l'hérédité sans considérer le rôle des acides nucléiques à ce propos.

Après avoir rappelé certaines étapes marquantes de la carrière d'Oparine, nous présenterons l'édition française de 1965 de son ouvrage, en donnant notamment une brève analyse des aspects portant sur les acides nucléiques. Nous présenterons ensuite les remarques et compléments donnés par Gavaudan, avant de terminer par des éléments

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1. A. I. Oparin, *L'origine de la vie sur la Terre*, trad. P. Gavaudan, M. Guyot et A. Synge, préface, remarques et commentaires de P. Gavaudan, Paris, Masson, 1965.

sur les échanges que celui-ci eu en 1965 avec Jacques Monod au sujet des travaux d'Oparine.

I. ALEXANDRE IVANOVITCH OPARINE ET LES ORIGINES DE LA VIE[2]

Physiologiste végétal de formation, Oparine donne en 1923, devant la Société de Botanique de Moscou, une conférence sur les origines de la vie. Celle-ci est publiée un an plus tard sous la forme d'un opuscule[3] contenant une conception des origines de la vie qui sera la base de tous ses travaux ultérieurs sur ce thème. Après avoir précisé les conditions régnant sur la Terre primitive, Oparine décrit les étapes d'une évolution de la matière, dans l'atmosphère puis la mer primordiale, conduisant à la formation de gouttes de gel organique qui préfigurent les cellules primitives. C'est donc une théorie de l'évolution progressive de la matière qu'Oparine expose.

Cinq ans plus tard, indépendamment des travaux du Soviétique, le Britannique John Burdon Sanderson Haldane (1892-1964) propose lui aussi une théorie des origines de la vie, fort comparable à celle d'Oparine, mais plus empreinte de biologie contemporaine et le processus décrit aboutit à la formation de molécules « semi-vivantes[4] ». Si Haldane ne se pencha ensuite qu'occasionnellement sur la question des origines de la vie, Oparine pour sa part, s'y consacra pendant toute sa carrière.

En 1936, reprenant pour l'essentiel le cadre théorique de 1924, le scientifique soviétique publie un ouvrage de synthèse dans lequel il étudie avec précision les étapes du processus. Ce livre, résolument interdisciplinaire, rassemble des données de planétologie comparée, de géologie, de chimie, de biochimie et de biologie. Il s'agissait dans un premier temps pour Oparine d'analyser avec précision quelles conditions régnaient sur la Terre primitive lorsque la matière évolua vers de complexes organiques microscopiques. Il établit alors que l'atmosphère primitive devait être réductrice et exempte de CO_2. Sa présentation

2. S. Tirard, « Les origines de la vie, un problème historique », in F. Raulin-Cerceau et S. Tirard (éd.), *Cahiers François Viète : Exobiologie, aspects historiques et épistémologiques*, 4, 2002, p. 35-48.

3. A. I. Oparin, *The Origin of Life* [Proiskhozhdenie zhizny], Moscow, 1924 ; J. D. Bernal, *The Origin of Life*, trad. A. Synge, London, Weidenfeld and Nicholson, 1967.

4. J. B. S. Haldane, « The Origin of Life » [1929], in *On Being the Right Size and other essays*, Oxford, Oxford University Press, 1991.

de l'étape ultime de la production des structures microscopiques s'appuie sur les travaux de H. G. Bungerberg de Jong, qui décrivit au début des années 1930 la formation de coacervats, des globules microscopiques qu'il avait observés dans des solutions de colloïdes hydrophiles.

Cet ouvrage d'Oparine semble avoir connu une assez bonne diffusion. Il fut traduit dans plusieurs langues et notamment en anglais en 1938. Le physicien et astronome Harold Urey (1893-1981) le cite en 1952 lorsqu'il propose que des expériences de synthèse soient réalisées à partir d'un mélange d'eau et de méthane, en présence de lumière ultraviolette[5]. Quelques mois plus tard une telle expérience est tentée par l'un de ses étudiants, Stanley Miller (1930-2007)[6]. Celle-ci, publiée dans *Science* en 1953, participa à fonder ce que l'on appela dès lors la chimie prébiotique, qui consistait donc à tenter des synthèses chimiques en respectant des conditions expérimentales correspondant aux conditions de la terre primitive[7].

La question des origines de la vie mobilisa à partir des années 1950 une communauté internationale de plus en plus structurée. En 1957, un symposium international sur les origines de la vie est organisé à Moscou et contribue à confirmer Oparine dans sa posture internationale.

II. L'ÉDITION FRANÇAISE DE L'OUVRAGE EN FRANÇAIS DE 1964

La publication de l'édition française de l'ouvrage d'Oparine s'inscrit dans un contexte dans lequel peu de travaux français marquants sur les origines de la vie se sont imposés. La production la plus remarquable fut sans nul doute celle de l'astronome Alexandre Dauvillier, qui publia, parfois avec Étienne Desguins, plusieurs synthèses sur les origines de la vie[8]. Sa théorie photochimique de l'origine de la vie était fondée sur l'action de la lumière en tant que principal agent

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5. U. Harold, « On the Early Chemical History of the Earth and the Origin of Life », *Proceedings of the National Academy of Science of the United States of America*, 38, 1952, p. 351-363.

6. Il ajoutera de l'ammoniaque aux composés suggérés.

7. M. L. Stanley, « A Production of Amino Acids under Possible Primitive Earth Conditions », *Science*, 117, 1953, p. 528-529.

8. A. Dauvillier et E. Desguins, « La genèse de la vie, Phase de l'évolution chimique », *Actualité Scientifique et Industrielle, Biologie Générale*, 917, 1942 ; *id.*, *L'Origine Photochimique de la Vie*, Paris, Masson, 1958 ; *id.*, « Origine de la Vie », *Encyclopédie de la Pléiade, Biologie*, Paris, Gallimard, 1965, p. 1841-1878.

dans la complexification primordiale de la matière organique contenue dans les étendues liquides.

L'ouvrage d'Oparine, publié en français en 1965[9], *L'Origine de la vie sur la Terre*, est la traduction en français de la troisième édition revue et élargie du livre publié initialement en russe par Oparine en 1936 et traduit en anglais en 1938[10]. La traduction de 1965, due à Gavaudan et à son collaborateur, M. Guyot, Maître de conférences à la Faculté des Sciences de Poitiers, a été réalisée à partir de l'édition anglaise de 1957, publiée sous le titre *The Origin of Life on Earth*[11] et traduite du russe par Ann Synge[12].

Gavaudan[13], biologiste et pharmacien, a soutenu en Sorbonne en 1930 une thèse de biologie intitulée : « Recherches sur les cellules des hépatiques ». Lorsqu'il publie le livre d'Oparine, il est alors professeur de botanique à la Faculté des Sciences de Poitiers. Sa préface livre quelques éléments quant à ses intentions et au contexte de cette publication. Gavaudan souhaite notamment faire la preuve que le problème des origines de la vie appartient à part entière à la science et ne relève plus de la métaphysique. Il évoque l'intérêt qu'ont représenté pour lui la visite d'Oparine à la Faculté des Sciences de Poitiers en 1959 et les trois conférences qu'il y donna à cette occasion[14]. Il dresse également dans sa préface un bilan de la richesse des travaux menés sur la question des origines de la vie à l'échelle internationale et n'omet pas de citer les noms des scientifiques les plus marquants en la matière, tels que Stanley Miller ou Sydney Fox.

Gavaudan se dit intimement lié à l'URSS et, fort de son amitié pour Oparine, il a décidé de se charger de faire connaître son œuvre

9. Il est indiqué dans l'ouvrage que les droits ont été reservés en 1964 et qu'il a été imprimé au premier trimestre 1965.

10. A. I. Oparin, *The Origin of Life*, traduit avec annotations par Sergius Morgulis, New York, The Macmillan Company, 1938.

11. *Id.*, *The Origin of Life on the Earth*, traduit du russe par Ann Synge, Edinburgh/London, Oliver and Boyd, 1957 [3e éd.]. Gavaudan précise dans sa préface que la concordance entre la traduction française et le texte russe original a été vérifiée par M. Frolov, maître de recherches au CNRS.

12. A. Synge est également la traductrice, en anglais, du texte d'Oparine de 1924, dans l'ouvrage de Bernal mentionné plus haut.

13. Pour sa biographie voir D. Girard (éd.), *Biologistes et naturalistes français du xxe siècle*, Paris, Hermann, 2012.

14. À l'issue de cette visite Oparin reçut le titre de docteur *honoris causa* par l'université de Poitiers.

en France[15]. D'emblée cette préface donne au volume un statut particulier, car l'intention de Gavaudan s'avère double. Il tient à faire connaître en France les travaux d'Oparine. Mais, il annonce également ses propres remarques et commentaires qui ne constituent pas moins du cinquième de l'ouvrage[16].

III. PRÉSENTATION DE L'OUVRAGE

Lorsque paraît cet ouvrage dans sa version russe et dans ses traductions anglaise et française, Oparine occupe une position importante dans la biologie soviétique. Il est alors en effet Directeur de l'Institut de Biochimie Bach à Moscou et est indéniablement le chef de file de la recherche sur les origines de la vie dans son pays. Le contexte de la science biologique russe est à cette époque profondément marqué par l'emprise du lyssenkisme qui la maintient en marge des développements de la génétique et de la biologie moléculaire occidentale.

Le livre est divisé en neuf chapitres. Les trois premiers ont une nature historique. Le premier chapitre porte sur la théorie de la génération spontanée qu'Oparine rejette radicalement. Le deuxième porte sur l'éternité de la vie. Il y est question de la théorie de la panspermie à laquelle Oparine n'adhère pas non plus. Le chapitre suivant intitulé : « Histoire de l'approche scientifique du problème de l'origine de la vie » présente notamment les travaux du XIX^e siècle de Haeckel et Pflüger qui ont formulé les premières théories des évolutives de l'origine de la vie. C'est précisément dans cette lignée que s'inscrit la réflexion personnelle du biologiste soviétique qu'il présente dans la suite de l'ouvrage. Les chapitres suivants sont une présentation, étape par étape, du processus d'évolution de la matière qui, selon Oparine, a mené à la vie :

a) chapitre IV : « Formation initiale des substances organiques simples[17] » ;
b) chapitre V : « L'évolution abiogénique des composés du carbone » ;
c) chapitre VI : « Structures et fonctions biologiques des protéines et acides nucléiques et problème de leur origine » ;
d) chapitre VII : « Développement des systèmes organiques multimoléculaires : organisation dans l'espace et le temps[18] » ;

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15. Gavaudan eut par ailleurs une activité de traducteur scientifique.
16. 112 pages sur 551, bibliographies comprises.
17. Dans lequel il est question des conditions du milieu terrestre primorodial.
18. Il s'étend longuement sur les coacervats.

e) chapitre viii : « L'origine des premiers organismes » ;
f) chapitre ix : « Évolution ultérieure des premiers organismes ».

Le processus est donc marqué par trois étapes :

a) phase 1 : L'évolution de composés moléculaires de base (chapitres iv et v) ;
b) phase 2 : La formation des macromolécules (chapitre vi) ;
c) phase 3 : La structuration de systèmes complexes préfigurant des cellules primitives (chapitres vii et viii).

Ce modèle en trois phases s'impose logiquement en raison du déroulement du processus qui va dans le sens d'une augmentation des échelles. Cependant, si cette progression en trois étapes est très courante à l'époque, elle peut avoir suivant les auteurs des fondements variés, voire opposés. Ainsi, lorsque nous la retrouverons sous la plume de Jacques Monod dans *Le Hasard et la Nécessité*, les étapes sont alors caractérisées successivement par les petites molécules, les macromolécules, protéines et acides nucléiques, essentielles pour un biologiste moléculaire comme lui, et enfin la cellule[19].

IV. L'ABSENCE DE LA GÉNÉTIQUE

L'extrait suivant du chapitre sur la biosynthèse des protéines donnera une idée de la rhétorique mise en œuvre par Oparine pour contourner les faits que proposait alors la biologie moléculaire.

Ainsi que nous l'avons vu plus haut le rna joue un rôle direct dans la synthèse des protéines. Mais, bien que beaucoup d'ouvrages scientifiques l'admettent, nous trouvons peu de faits confirmant l'idée d'une action déterminante du DNA du noyau sur la structure spécifique des acides ribonucléiques. Pour utiliser le langage adopté aujourd'hui par les physiciens, l'information concentrée dans les molécules de dna passe d'abord sur les molécules de rna, après quoi la synthèse des molécules de protéines se poursuit en accord avec l'information qui est relayée par la séquence des nucléotides dans la chaîne du rna.

Cependant, même si nous supposons que cette hypothèse est juste, cela ne nous avance pas beaucoup, car nous nous demandons maintenant comment a été créé dans le dna l'arrangement rigoureusement déterminé des nucléotides[20].

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19. J. Monod, *Le Hasard et la Nécessité*, Paris, Le Seuil, 1970.
20. *Ibid.*, p. 250.

Sans faire aucune référence aux propriétés autoréplicatives de l'adn dont l'intérêt génétique est souligné dès 1953 par Francis Crick et James Watson[21], Oparine poursuit en affirmant :

> Il est difficile de penser aujourd'hui que le dna ne participe pas aux activités métaboliques, qu'il ne subit aucune transformation au cours du développement de la cellule, et se contente tout bonnement de se reproduire de telle façon que chaque molécule nouvelle provienne directement de par autocatalyse d'une molécule préexistante.

Oparine s'attache donc à réduire les polynucléotides à des molécules, au métabolisme complexe, certes, mais en rien différents dans le principe aux polysaccharides par exemple. Leur synthèse, « comme celle des autres composés qu'on trouve dans le protoplasme, est réalisée au moyen d'un appareil enzymatique complexe[22] ».

Oparine indique ensuite qu'en 1957, à la fin de la conférence de Moscou, Bernal lui a posé la question suivante : « [...] lesquels des acides nucléiques ou des protéines sont apparus les premiers ? » C'est à ce questionnement qu'il s'intéresse ensuite, délaissant la question de l'origine des acides nucléiques dans les cellules actuelles pour la question de leur origine première, sans donc avoir soutenu la possibilité d'un quelconque rôle de ces molécules dans l'hérédité. Quant à leur origine première, il rejette la possibilité qu'ils soient apparus indépendamment des protéines[23].

V. LES COMPLÉMENTS DE GAVAUDAN

Gavaudan a donc choisi, avec l'accord d'Oparine, d'adjoindre au texte du Soviétique ses propres remarques et commentaires[24]. Dans sa préface il le remercie d'avoir accepté cet ajout, d'autant qu'il est amené

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21. J. D. Watson et F. Crick, « Genetical Implications of the Structure of Deoxuribonucleic Acid », *Nature*, 171, 1953, p. 964-967. Par ailleurs, la publication des travaux de Matthew Meselson et Franklin W. Stahl sur la semi-conservativité est postérieure à la publication de l'ouvrage d'Oparin : M. Meselson et F. W. Stahl, « The Replication of dna in Escherichia coli », *Proceedings of the National Academy of Science of the United States of America*, 44, 1958, p. 671-682. Ils ne sont néanmoins pas évoqués par Gavaudan dans ses commentaires et complements.

22. *Ibid.*, p. 251.

23. *Ibid.*

24. *Ibid.*, p. 419-512.

à le contredire parfois. Gavaudan évoque par ailleurs ses échanges fréquents avec ses collègues, notamment M. P. Schutzenberger (1920-1996), sur les problèmes qu'il traite dans ses remarques et commentaires. Notons cependant qu'il n'a pas lui-même publié sur cette question. Gavaudan est en fait intéressé par de nombreux champs de la biologie, notamment de la biologie théorique. Les origines, malgré les données expérimentales qui peuvent être rassemblées, sont un problème qui conserve une forte teneur théorique et c'est sans doute ce qui le passionne, en plus de la nature interdisciplinaire du domaine.

Dans un premier temps Gavaudan n'hésite pas à élargir le débat sur les générations spontanées et à rappeler que de nouveaux questionnements ont émergé depuis Pasteur, notamment à propos de l'origine des virus, de celle des granules ribonucléoprotéiques ou encore des bioblastes[25]. Gavaudan est très intéressé par le mode de multiplication des organites cellulaires et par la continuité génétique et il évoque la possibilité d'une loi biogénique qui à chaque génération cellulaire récapitulerait les mécanismes de la biogenèse primordiale.

Gavaudan complète l'historique des théories de l'origine de la vie dressé par Oparine. Il insiste pour sa part sur celle de Dauvillier et Desguins et décrit également le travail de Ermano Giglio Tos (1865-1926), de l'université de Cagliari qui publia *Les problèmes de la vie* entre 1900 et 1910. Gavaudan formule finalement la comparaison suivante :

> Bien qu'évolutive comme celle d'Oparine, la théorie de Giglio Tos en diffère seulement sur un point important ; [...] le savant soviétique a recherché le moteur principal de la biogénèse dans la sélection naturelle Giglio Tos l'a reconnu dans la symbiose permettant d'associer de façon complémentaire les possibilités métaboliques des premières particules vivantes[26].

Gavaudan aborde ensuite « Le rôle des coacervats et le problème de l'état physique de la matière vivante » et poursuit immédiatement par une réflexion sur « l'être vivant, comme système thermodynamique ouvert ». Parmi les autres thèmes importants il aborde également la définition de la vie, les difficultés selon lui de l'application de la notion de sélection naturelle dans les théories de la biogenèse, ainsi que le problème de l'aspect téléologique des phénomènes vivants. Il conclut

25. Les actuelles mitochondries.
26. *Ibid.*, p. 441.

enfin par une « Dernière question : l'avenir du problème de la synthèse des systèmes vivants ».

Nous ne pouvons donner ici le détail de ce qui constitue une réflexion très personnelle de la part de Gavaudan sur cette question des origines de la vie, mais soulignons cependant que l'ouvrage publié comporte bien deux conceptions distinctes : celle d'Oparine, sur laquelle vient tout à la fois s'appuyer et se distinguer celle de Gavaudan.

VI. LA RÉCEPTION DE L'ŒUVRE D'OPARINE, LE CAS DE JACQUES MONOD

Il n'est pas aisé de connaître la réception effective du livre d'Oparine en France en 1965. Il apparaît néanmoins que l'ouvrage édité par Gavaudan fut bien représenté dans les bibliothèques universitaires ; on en compte en effet actuellement une cinquantaine d'exemplaires répartis dans autant d'institutions académiques. Ceci est tout à fait remarquable puisque les ouvrages antérieurs du soviétique sont présents en rarement plus de trois ou quatre exemplaires. Deux facteurs peuvent expliquer cette différence. Premièrement, il s'agit du premier ouvrage d'Oparine en français ce qui facilite sans doute son introduction dans les fonds universitaires[27]. Deuxièmement, il est publié chez Masson, un éditeur scientifique de premier plan en mesure de lui assurer une large diffusion[28].

La réception de l'œuvre d'Oparine n'en fut pas pour autant moins délicate. En témoigne une correspondance entre Monod et Gavaudan, l'année même de la parution de l'ouvrage. Dans une lettre datée du 12 janvier 1965, avant que l'ouvrage ne soit en librairie, Gavaudan invite Monod à se joindre à un colloque sur les origines de la vie qu'il souhaite organiser cette même année. Avant de se réjouir de la présence probable de Oparine lui-même, Gavaudan indique que :

> Le titre choisi pour ce Colloque doit lui conférer un caractère souple, éclectique et général. En effet, dans les termes de systèmes biologiques élémentaires on comprendra non seulement l'étude encore conjecturale des éobiontes

27. On ne trouvait effectivement en français que des textes de vulgarisation et de propagande signés par Oparine : A. Oparine, *L'origine de la vie*, trad. E. Bronina, Moscou, Éditions en langues étrangères, 1955.

28. Le catalogue de cet éditeur montre qu'il était enclin à publier des ouvrages scientifiques sur des questions d'évolution et notamment sur les origines de la vie. C'est en effet chez Masson que l'on trouve également l'ouvrage de Dauvillier de 1958.

> ou protobiontes primitifs, mais encore celle des problèmes posés par les diverses unités biologiques élémentaires actuelles conceptuellement ou matériellement isolables, qu'il s'agisse des virus ou des constituants cellulaires. Par ailleurs, l'étude des problèmes généraux de la biogenèse appelle non seulement le concours des biophysiciens, des biochimistes et des biologistes, mais aussi celui, vivement sollicité, des chercheurs qui s'intéressent à ce problème sur le plan théorique, quelle que soit leur position philosophique[29].

Monod lui répond dès le 19 janvier en lui signifiant son intérêt pour ce colloque auquel il compte participer. Mais, il formule de rudes critiques à l'égard d'Oparine : « Je me permets de faire les plus expresses réserves en ce qui concerne les travaux d'Oparine qui, fort ignorant semble-t-il de la biologie moléculaire moderne, apparaissent à beaucoup comme d'une extrême naïveté. »

La réponse de Gavaudan ne tarde pas, le 23 janvier il tente de soutenir Oparine, indiquant qu'il le connaît bien puisqu'il est son traducteur. Il en appelle à l'indulgence de Monod en soulignant que l'inspiration d'Oparine remonte à une période bien antérieure à la seconde guerre mondiale et insiste sur ce qu'il a, selon lui, apporté au domaine des origines de la vie : « Je pense sincèrement que nous devons à Oparine la théorie hétérotrophe et la tentative d'application de la sélection naturelle à la biogenèse (à laquelle d'ailleurs je ne m'associe pas personnellement sans sérieuses réserves). »

Peut-être, faisant allusion dans votre lettre à l'extrême naïveté des travaux d'Oparine, évoquez-vous ces recherches sur les coacervats ? En effet, Oparine n'a sans doute pas échappé aux pièges bien connus dont furent déjà victimes tous ceux qui s'essayèrent à réaliser des modèles plus ou moins simplistes de systèmes vivants.

Il est évident que l'état actuel des connaissances dans les domaines de la biologie moléculaire et de la physicochimie des macromolécules permet d'ouvrir d'autres horizons. C'est bien pour cette raison que j'ai sollicité votre concours que j'espère voir se matérialiser sous la forme active d'un rapport dans lequel votre spécialisation devrait vous permettre de critiquer ou de préciser certaines hypothèses relatives à la nature des particules ou systèmes élémentaires. C'est d'ailleurs dans le même esprit constructif que j'ai fait appel à d'autres spécialistes ; notamment d'éminents cytologistes ; en effet, là aussi, les hypothèses

29. Cette lettre et les suivantes sont issues du Fonds Jacques Monod – Institut Pasteur de Paris, Correspondance : MON.Cor.07.

des théoriciens reposent parfois dans le vide lorsqu'elles font table rase des réalités cytologiques actuelles.

Ce soutien à Oparine semble exercer sur Monod l'effet contraire de celui escompté. Il se rétracte et conseille à Gavaudan d'inviter à sa place André Lwoff (1902-1994), plus intéressé que lui par ces questions, non sans avoir précisé :

> J'avoue ne pas connaître l'œuvre d'Oparine très bien et évidemment beaucoup moins bien que vous. Il est exact que l'« extrême naïveté » dont je faisais état dans la précédente lettre concerne ses idées sur les coacervats. Quant à la théorie hétérotrophe, elle m'a toujours paru tellement naturelle en s'imposant de façon si évidente qu'il m'est difficile d'en faire un très grand mérite à Oparine.

Cet échange montre comment l'œuvre d'Oparine, dans le contexte de la jeune biologie moléculaire, pouvait être facilement critiquée. En effet, Monod conclut sa dernière lettre en précisant que c'est sur la question de l'origine du code génétique qu'il est important de se pencher.

> Permettez-moi de préciser que parmi les questions touchant à l'origine de la vie, un problème précis et très difficile me paraît aujourd'hui dominer tous les autres : c'est celui de l'origine du code génétique et de « l'invention » des mécanismes qui assurent la transcription des séquences polynucléotidiques en séquences polypeptidiques. Parmi les physico-chimistes ou les biologistes moléculaires qui ont réfléchi tout particulièrement à cette question et même entrepris des recherches à ce sujet, je ne connais que le Dr. Leslie Orgel, actuellement au Salk Institute à La Jolla (Californie).

Connaissant la position institutionnelle d'Oparine et étant par ailleurs extrêmement critique à l'égard des thèses lyssenkistes[30], Monod sait qu'il disqualifie Oparine puisque celui-ci ne peut que contourner le problème.

CONCLUSION

L'ouvrage d'Oparine, publié en 1964 par Gavaudan offrit au lectorat français les thèses sur les origines de la vie du biochimiste

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30. S. Tirard, « Les biologistes français et l'affaire Lyssenko, à l'automne 1948 », in J.-J. Becker (éd.), *Historiens et Géographes, numéro spécial : La IV^e^ République. Histoire, recherches et archives*, 358, 1997, p. 95-106.

soviétique. De l’aveu même de Gavaudan, il s’agit d’un texte daté, dont on comprend en raison de la situation de son auteur qu’il est contraint par les conditions politiques dans lesquelles ces travaux sont développés en URSS. Oparine s’intéresse surtout au métabolisme primitif et délaisse, ou contourne, tous les problèmes d’hérédité qui renverraient à la biologie moléculaire dont les grands concepts ont été formulés durant les vingt-cinq dernières années.

L’originalité du livre tient sans doute dans le fait que son éditeur scientifique, Gavaudan, a donné une large place à ses commentaires et à son propre point de vue. Cet ouvrage devenant ainsi une tribune pour ses propres réflexions qu’il voudrait partie prenante dans le développement d’un débat sur les origines de la vie dans la communauté scientifique française.

II

PHYSIOLOGIE ET MÉDECINE

I

Franco-German-Russian physiological triangle in the 19th century

Vladimir O. Samoilov

The first Russian physiologist, doctor of philosophy and medicine, P. V. Postnikov, studied at the University of Padua (1692-1697) where he defended his doctoral thesis.[1] In what follows, Italian physiologists had no significant effect on the development of physiology in Russia.

In the second half of the 18th century, the institutionalization of physiology and anatomy took place. In other words, a division occurred in a huge field of knowledge into two separate disciplines. The reason for it was different methodologies – observation in anatomical studies and experimentation in physiological research. A. V. Galler is thought to be the initiator of this institutionalization. Unfortunately, the contribution in the process of I. F. Shreiber, Galler's friend and classmate at the University of Leiden, is now neglected, despite the fact that he had implemented institutionalization of physiology and anatomy in Saint Petersburg medical schools in 1754[2] and was the first professor of medicine in Russia.

However, the introduction of experimentation in physiological research had been strongly resisted by representatives of natural philosophy, then dominant at German universities and merging with vitalism. Famous historian of medicine G. G. Skorichenko wrote:

> In the late 1830s and early 1840s graduates of Saint Petersburg Medical Surgical Academy having met a backward, bogged down in the mud of metaphysical

1. K. S. Koshtoyants, *Ocherki po istorii fiziologii v Rossii*, Moscow/Leningrad, Publishing House of the Academy of Sciences of the USSR, 1946, p. 496.

2. V. O. Samoylov, "Prepodavanie fiziologii v Sankt-Peterburgskikh lekarskikh shkolakh, Glavnom Vrachebnom uchilishche i Mediko-khirurgicheskoi akademii do osnovaniya samostoyatel'noi kafedry," *Vestnik Rossiiskoi Voenno-meditsinskoi akademii*, 4, 24, 2008, p. 154-163.

utopias outlook of German doctors indicated in their overseas internship reports the low level of education of the German medical world.[3]

Unlike the majority of Russia's doctors, some Russian physiologists (e. g. D. M. Vellansky) promoted natural philosophy rejecting experimentation, yet most of the researchers supported the experimental research of physiological processes. In the beginning of the 19th century, they tended to follow the French physiological approach since distinguished physiologists in France defended the priority of experiments in science.

Among physiologists, this applies first of all to François Magendie,[4] and his follower Claude Bernard[5], who was considered the most skillful vivisector of Europe in the middle of the 19th century. Ivan Glebov, the teacher of I. M. Sechenov, professor of physiology at the Moscow University, being a student at the time together with his university colleague, Alexei Lunin, translated Magendie's book "Brief basics of physiology"[6] into Russian in 1830. According to I. M. Sechenov, his teacher's lecture demonstrations in the 1850s "were mainly long lines of pigeons with brain pinholes."

In his textbook, F. Magendie wrote that in Germany there were still some psychologists who wished to demonstrate the science of life completely independently of experiments *a priori*.[7] He called it "natural philosophic physiology" or "romantic physiology."

In the *Journal of Experimental Physiology and Pathology* published by F. Magendie, Kazan astronomer, Simonov, published an article in 1824 about the mechanism of the accommodation of the eye, a work which can be considered the first scientific research on the physiology of sensory system in Russia.[8]

French physiologists have had an impact on German scientists. The great reformer of physiology, J.P. Müller, who was a natural philosopher at the beginning of his scientific life, later bought his early

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3. N. P. Ivanovskiy (ed.), *Istoriya Imperatorskoi Voenno-meditsinskoi (byvshei Mediko-khirurgicheskoi) akademii za 100 let (1798-1898)*, Saint Petersburg, 1898, p. 393.

4. F. Magendie, *Leçons sur les phenomènes physiques de la vie*, Paris, 1836.

5. L. N. Karlik, *Klod Bernar*, Moscow, Nauka, 1964, p. 270.

6. F. Magendie, *Kratkoe osnovanie fiziologii*, Moscow, 1830, p. 934.

7. *Id.*, *Leçons sur les phenomènes physiques de la vie*, *op. cit.*

8. M. Simonov, "De la prétendue nécessité mathematique du déplacement du cristallin pour conserver constente la distance focal de l'œil," *Journal de physiologie experimentale et pathologique*, 4, 3, 1824, p. 260-263.

works in bookshops and destroyed them. He quickly became a devotee of experimental physiology and trained a galaxy of eminent scientists who approved the methodology in physiology and related fields of natural science once and for all. We will name only the most famous of Müller's students: H. von Helmholtz, E. du Bois-Reymond, E. Brücke, K. Ludwig, T. Schwann, R. Virchow, E. Haeckel, J. Henle, F. Bidder, R. Remak, I. Claparede, A. M. Filomafitsky, N. I. Pirogov, I. T. Glebov.

In the second half of the 19th century, Germans began to dominate physiology, although P. Flourens and C. Bernard were working in France. I. M. Sechenov wrote on that matter that C. Bernard "was not such a teacher as the Germans [...] it was impossible to learn anything from him in the laboratory in a short time."[9] However, his ideological influence on Russian physiologists and physicians remained very strong. As acknowledged by I. P. Pavlov, as a student, he had learned French to read C. Bernard's lectures in the original and considered him a true inspiration in his physiological life.[10] Yet Pavlov carried out his two-year overseas training not with Bernard, but in the German laboratories of Karl Ludwig and Rudolf Heidenhain.

The unification of the small German states into one power under the aegis of Prussia led to a better funding of scientific research; in the setting of the rapid development of industry, the techniques of experimental work were improving; physiologic institutes with excellent conditions for work were founded. And if C. Bernard's laboratory in the Collège de France was, in his own words, "the tomb of scientists" (his work there actually led him to serious illness and premature death), German physiological institutes (especially in Leipzig and Breslau) looked like palaces. There, psychologists not only worked, but also lived (C. Bernard said that in order to become a physiologist, one should live in one's laboratory).

The institute of physiology at Leipzig University became an international scientific training school for physiologists after E. Weber had been replaced by K. Ludwig at the university faculty in 1865. In ten months, a two-story building was erected with several laboratories equipped for a variety of experiments and microscopic studies. Young people, with a medical background, flocked to the institute from all over

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9. I. M. Sechenov, *Avtobiograficheskie zapiski*, Nizhny Novgorod: Nizhny Novgorod Lobachevsky University, 1998, p. 136.

10. V. O. Samoilov and A. S. Mozzhukhin, *Pavlov v Petersburge – Petrograde – Leningrade*, Leningrad, Lenizdat, 1989, p. 29.

the world, with the dream of a prestigious route opening up before them to the solution of many problems in physiology. The life of the institute research was the life of its leader and the achievements of the teams were the achievement of K. Ludwig.[11]

During his training in the physiological institutes of Leipzig and Breslau, I. P. Pavlov wrote: "Overseas travel was important for me, first of all, because it introduced me to the type of scientists like Heidenhain and Ludwig who live their lives with all joy and sorrow in science and nothing else."[12]

With good reason the following words of C. Bernard can be applied to German physiologists; he considered that by the middle of the 19th century:

> [...] the German language has become the dominant international language of science and German professors have established a kind of scientific empire covering the whole of the northern, central and eastern Europe that had a serious impact on science in Russia, the USA, Japan [...] The German professor becomes a model for scientists around the world, the man keeping great traditions.[13]

The libraries and laboratories of the German universities in the second half of the 19th century became the best in Europe.

Germany utilized the national ("sovereign") approach to scientific and educational development. It was consistent, according to H. von Helmholz,[14] with the "freedom to teach and freedom to learn."

More than 300 scientists, including Germans, Russians, Swedes, Finns, English, Americans, Italians, Poles, Dutch, Austrians, Hungarians, Spaniards, and Japanese received advanced training and consultation in the physiological institute of K. Ludwig. Russians (more than 50) were the most significant part, behind Germans of course. There were eleven U.S. citizens and sixteen British people.[15] Sechenov proposed

11. S. A. Chesnokova, *Karl Ludwig*, Moscow, Nauka, 1973, p. 256.

12. V. O. Samoilov and A. S. Mozzhukhin, *Pavlov v Petersburge – Petrograde – Leningrade*, *op. cit.*, p. 29.

13. J. Bernal, *Nauka v istorii obshchestva*, Moscow, Publishing House of the Academy of Sciences of the USSR, 1956, p. 308.

14. G. F. Helmholtz, *Ob akademicheskoi svobode germanskikh universitetov*, Moscow, 1879, p. 23.

15. V. O. Samoilov and A. S. Mozzhukhin, *Pavlov v Petersburge – Petrograde – Leningrade*, *op. cit.*, p. 29.

considering Ludwig "the founder of physiology in Russia" but that "title" was later given to himself.[16] According to authoritative historians of physiology,[17] M. Foster founded the English school of modern physiology in the late 19th century and as for the United States, there were no major systematic physiological studies until the 1920s.

From 1866 to 1875, nearly half of all the articles published in the Leipzig Journal were the works of Russian scientists. In the late 1870s, the number of works decreased because Russia had its own scientific journals by then. Strong physiological departments were created at the Military Medical Academy, the Women's Medical Institute, Saint Petersburg, Moscow, Kazan, Dorpat, Vilna, Kharkov, Tomsk universities. In 1890, the Institute of Experimental Medicine was created and in the beginning of the 20th century – the Saint Petersburg Institute of Physiology. Many foreign physiologists began to visit Russian scientists for training and consultation.

To summarize the foregoing, we can say that, at the beginning of the 19th century, France specified the right vector for the development of physiology which had the decisive role to overcome the natural philosophical outlook among the physiologists of Russia and Germany. Later, having abandoned natural philosophy and developed experimental approach in physiological science, German physiologists occupied a dominant position. In the second half of the 19th century it had a beneficial effect on Russian physiology, which, together with German and French physiologies, began to take its rightful place among the three leaders. The physiological studies of British and American scientists were still inferior.

Doing justice to the influence of the German and French medical schools on the development of Russian medicine the Scientific Council of the Saint Petersburg Medical-Surgical Academy, which employed such eminent physiologists as I. M. Sechenov, I. P. Pavlov, I. F. Tsion, N. M. Yakubovich, I. R. Tarhanov, elected Claude Bernard and Rudolf Virchow its honorary members on the same day – March 25th, 1861. This event emphasizes the undeniable fact of the fruitful relationships between Russian, French and German physiologists in the 19th century.

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16. V. O. Samoilov, *Istoriya fiziologii v portretakh, gravyurakh, fotografiyakh* Saint Petersburg, InformMed, 2008, p. 64.

17. J. F. Fulton and L. G. Wilson, *Selected readings in the History of Physiology*, Springfield, 1966; K. E. Rothschuh, *Geschichte der Physiology*, Berlin, Gottingen, Heidelberg, Springer Verlag, 1953; L. A. Orbeli, *Izbrannye trudy*, Leningrad, Nauka, 1968, vol. 5, p. 129-130.

II

Nikolaï Ivanovich Pirogov (1810-1881) : son rôle fondamental dans l'histoire de l'anesthésie en europe

Céline Cherici

Nikolaï Pirogov, personnage clé pour comprendre le développement de la chirurgie sans douleur en Russie et en Europe[1], est l'auteur de travaux importants qui sont restés longtemps relativement méconnus. Qui était Nicolas Pirogov ? Quels ont été ses liens avec l'Europe occidentale ? Dans quelle mesure a-t-il participé à l'histoire de la chirurgie ? Pirogov fut un des premiers scientifiques à pressentir l'importance des techniques d'éthérisation et à les introduire en Russie. Dans quel contexte politique développa-t-il ces techniques ?

La Russie se dota au XIXe siècle de plusieurs importantes écoles de médecine parmi lesquelles l'université de Moscou, l'Académie médico-chirurgicale de Saint-Pétersbourg, l'université de Dorpat, institutions que fréquentera Pirogov à des titres divers. Une fois diplômé de l'université de Moscou en 1828, où il obtient la qualification de médecin, Pirogov intègre l'université de Dorpat en vue d'accéder au grade de professeur[2]. L'université de Dorpat représente alors le lieu d'ouverture de l'empire russe sur les différentes communautés scientifiques européennes. Pirogov attire rapidement l'attention par son implication dans les disciplines de la chirurgie et de l'anatomie topographique. Il se spécialise dans ces domaines et devient l'un des premiers en Europe à faire de l'expérimentation animale en vue de poser les fondements à l'application à l'homme des techniques chirurgicales. Il recevra

1. G. Altschuller et W. A. Keenan, « History of the development of painless surgery in Russia », *Bulletin of the New York Academy of Medecine*, 58, 5, New York, 1982, p. 493-502.

2. M. Toomsalu, *Professors of the old anatomical theatre of University of Tartu*, Tartu, University of Tartu, 2006, p. 175-183.

une distinction dès 1829, une médaille d'or pour ses articles portant sur la ligature des artères.

Pirogov voyage jusqu'en 1835, notamment à Berlin et à Göttingen, avant de retourner en Russie. Il se consacre sans relâche à l'anatomie pratique, la dissection et écrit plusieurs articles de chirurgie. Au début de l'année 1838, il voyage en France où il peut confronter ses compétences chirurgicales avec la chirurgie parisienne d'alors. Il rencontre le grand professeur d'anatomie et de chirurgie Alfred Velpeau[3] qu'il trouve en train d'étudier ses propres travaux de chirurgie anatomique des artères. Il fait également la connaissance du chirurgien Jacques Lisfranc[4], et en donne cette description peu flatteuse : « Lisfranc en tant que professeur était, au sens strict du terme, Français, effronté, et bavard, grand, les épaules larges et doté d'une voix qui pouvait être entendue sur des verstes[5],[6]. »

En dépit du portrait qu'il en fait, Pirogov reconnaît à Lisfranc un immense talent en tant que chirurgien et clinicien. Il est également passionné par les travaux portant sur les maladies internes des cliniciens français Cruveilhier[7] et Bouillaud[8]. Son séjour d'étude parisien est ponctué des visites de structures hospitalières, de théâtres anatomiques ainsi que d'abattoirs où l'on pratique la vivisection des animaux malades. Il regrette que de nombreux praticiens ne puissent mener davantage de démonstrations diverses sur les cadavres ou sur les préparations anatomo-cliniques.

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3. Alfred Velpeau (1795-1867) est l'auteur de nombreuses publications sur la chirurgie, l'embryologie, l'anatomie et l'obstétrique, parmi lesquelles un *Traité élémentaire de l'art des accouchements* paru en 1830. De 1833 à 1867, il occupe la chaire de chirurgie clinique à la Faculté de médecine de Paris. Il a, par ailleurs, donné son nom à un pansement, la bande Velpeau.

4. Le nom de Jacques Lisfranc de Saint-Martin (1787-1847) a été retenu suite à sa concurrence avec Guillaume Dupuytren (1777-1835). S'il ne reste pas grand-chose de ses travaux, il est connu pour avoir été le chirurgien en chef de l'hôpital de la Pitié à Paris pendant vingt-deux ans.

5. La verste est une ancienne mesure de longueur russe valant 1 066,8 mètres.

6. N. Pirogov, *Questions of life*, *Diary of an old physician*, India, Watson publishing International, 1991, p. 402.

7. Jean Cruveilhier (1791-1874) est un médecin, chirurgien, anatomiste et pathologiste français. Membre de l'Académie de médecine, il est le premier titulaire de la chaire d'anatomo-pathologie de la Faculté de médecine de Paris.

8. Jean-Baptiste Bouillaud (1796-1881), médecin français, identifie le rhumatisme articulaire aigu en lien avec les troubles cardiaques. Il est également le premier à localiser le centre du langage dans les lobes frontaux du cerveau.

Pirogov obtient en 1841 la chaire de chirurgie à l'Académie Médico-Chirurgicale de Saint-Pétersbourg, d'où il démissionnera en 1856. Après cette période, scientifiquement la plus active, il occupera divers postes, entrecoupés de voyages à l'étranger, notamment sur les champs de bataille. Il meurt en Ukraine en 1881.

On peut considérer que l'œuvre de Pirogov a contribué à la naissance d'une physiologie russe autonome, notamment grâce à ses expériences sur la ligature de l'aorte, les effets de l'anesthésie à l'éther et les blessures par armes à feu, en particulier sur les champs de bataille. Tout d'abord, nous analyserons les conditions dans lesquelles Pirogov effectue ses premières recherches sur l'anesthésie par éthérisation. Puis nous étudierons l'évolution de ses recherches sur les champs de bataille ; enfin, nous questionnerons l'importance de ses travaux au sein de l'histoire russe de la chirurgie sans douleur.

I. LES RECHERCHES SUR L'ÉTHÉRISATION

Pirogov commence ses expériences avec l'éther, utilisé comme anesthésiant dès 1846[9]. L'application nouvelle de ce produit dans le domaine de la chirurgie sans douleur s'était répandue comme une traînée de poudre à travers les États-Unis et l'Europe après les démonstrations de William Morton. Après ses voyages scientifiques en France et en Allemagne, Pirogov importe cette nouvelle technique dès son retour au pays, notamment l'équipement permettant d'administrer l'éther, qu'il introduit en Russie. Après avoir essayé sur lui-même, sur un grand nombre de ses assistants, ainsi que sur des animaux de laboratoire les effets des inhalations d'éther, il écrit dans ses « Recherches pratiques et physiologiques sur l'étherisation » :

> Les expériences que j'ai faites sur des individus malades et sains, sur moi-même et sur des animaux vivants, m'autorisent à prononcer un jugement sur la valeur pratique et la manière d'agir de l'éther sur l'organisme animal […]

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9. Il semble que l'éther ait été synthétisé pour la première fois par Jebbar, savant arabe du VIIIe siècle. Mais ce produit est connu depuis le XIIIe siècle en Occident par des savants tels que Raymond Lulle (1234-1315) et Arnaud de Villeneuve (1238-1316). Paracelse (1493-1541) et son disciple Valerius Cordus en définissent le mode de préparation à partir d'acide du vitriol ou d'acide muriatique, oxygénés par de l'esprit de vin en vue de produire le vitriol doux. Celui-ci avait la propriété d'assoupir les poules. Le chirurgien-dentiste de l'hôpital de Boston William Morton permit la première démonstration officielle de l'utilisation de l'éther en anesthésie humaine en 1846.

> je me suis convaincu que la vapeur de l'éther est sans contredit un remède digne de toute notre attention, un remède qui même, sous un rapport, peut transformer toute la chirurgie[10].

Pirogov fait ses premiers essais d'absorption de l'éther par inhalation dès 1846. Dans ses « Recherches pratiques et physiologiques sur l'éthérisation[11] », publiées en 1847, il décrit les expériences durant lesquelles il administre de l'éther à pas moins de cinquante patients et quarante volontaires. Il utilise d'abord un appareillage primitif constitué d'une bouteille, d'un tube nasal et d'un appareil dit de Charrière, du nom du technicien français spécialiste de la mise au point du matériel médical et de sa fabrication. Mais ce matériel ne le satisfait pas dans la mesure où le masque, trop petit, est jugé inconfortable. C'est pourquoi il fabrique lui-même un appareil destiné à l'inhalation d'éther. La conclusion de ses expériences sur l'homme est résumée de la façon suivante :

> Quoique les inspirations de l'éther soient indiquées dans toutes les opérations importantes et doulouruses, la diversité de l'effet de l'inspiration sur le système musculaire, qui ne peut jamais être prévue, nécessite un essai préparatoire, tant pour examiner dans chaque cas individuel l'influence du remède, que pour donner plus de confiance dans l'application et l'effet de ce procédé aux malades méfiants, ignorants et déraisonnables. Dans les cas, qui exigent quelque opération moins douloureuse que délicate, et où l'expérience préparatoire a signalé un effet particulier d'excitation, produit par la vapeur de l'éther sur le système musculaire (mouvements convulsifs, catalepsie, mouvements automatiques), il est naturellement plus conforme au but de ne pas appliquer l'inspiration de l'éther[12].

De plus les indications de l'éthérisation varient selon la nature de l'opération. Concernant les opérations oculaires l'éther peut faciliter les choses ou les rendre plus pénibles ; il ne doit pas être utilisé lors des opérations de la bouche et du pharynx ; pour la réduction des pierres il est aussi utile que pour les autres opérations ; il facilite l'extraction des pierres à cause de la relaxation musculaire des muscles de l'abdomen et du périnée. Durant les accouchements, la rupture du périnée devient

10. N. Pirogov, *Recherches pratiques et physiologiques sur l'éthérisation*, Saint-Pétersbourg, Bellizard, 1847, p. 1 et p. 5.

11. *Ibid*.

12. *Ibid*., p. 36.

évitable. Dans tous les cas, il peut provoquer des spasmes urinaires de la vessie ; par-dessus tout l'éthérisation est pleinement utile dans le cas de hernies incarcérées, de dislocations et d'ankyloses, à cause de son effet relaxant sur les muscles ; l'expérience montre que l'inhalation d'éther ne doit jamais être utilisée sur des patients avec des dispositions à l'hémoptysie et aux maux de tête. Enfin, à des fins thérapeutiques, l'éther peut être mélangé avec d'autres substances solubles.

Pirogov pratique également des expériences où l'éther est appliqué directement sur les nerfs périphériques, la moelle épinière et le cerveau de chiens et de lapins, tandis qu'il stimule ces zones mécaniquement et électriquement. Il tente ainsi d'étudier les réactions du système nerveux central et périphérique à l'introduction de l'éther. Par exemple, après avoir inoculé cette substance sur un chien de petite taille, il rend compte ainsi :

> Moëlle épinière mise à nu : injection d'eau (trois quarts de la petite seringue) sous la dure-mère : une espèce de coma : dilatation de la pupille ; extension des extrémités antérieures. L'animal se remet. Injection d'éther deux fois (un peu plus d'une demi seringue) : d'abord point de changement – plus tard profond coma. Pendant toute l'expérience (et aussi après la mort) la moëlle épinière reste sensible à l'irritation galvanique. Le nerf axillaire est mis à nu. Application locale de l'éther : les irritations mécaniques sur le nerf ne provoquent aucun effet, mais il reste sensible au galvanisme et, sous les endroits éthérisés, sensible aussi à l'irritation mécanique. Section : taches verdâtres dans les poumons, dans le cœur droit, vapeur d'éther dans le sang, – le sang est liquide. Répétition de cette expérience : même résultat[13].

Son approche expérimentale et son emploi de la vivisection animale sont ici originaux dans la mesure où il utilise l'éther à des fins fondamentales, en vue d'étudier et de comprendre les mécanismes physiologiques du système nerveux et non plus comme simple anesthésiant, à des fins médicales. Il fait, par ailleurs, le lien entre la chirurgie clinique et les effets de l'éther sur le système nerveux humain et animal.

Il pratique également d'intéressantes expériences sur le lapin :

> L'hémisphère gauche du cerveau d'un lapin mis à nu. Application d'éther au moyen d'éponges (pendant 10 à 12 minutes avec intervalles) : point de symptômes de narcotisation ; mais toutes les fois des symptômes d'irritation après

13. *Ibid.*, p. 49-50.

> l'arrosement de l'éponge ; l'animal saute et veut s'échapper. Une demi-heure après que l'animal se fut remis, et que la masse du cerveau fut un peu enflée, elle sortit de la plaie ; des éponges imprégnées d'éther produisirent, après une application aussi prolongée qu'avant, une narcotisation complète, dans laquelle la respiration sent très fortement l'éther. Après quelque temps, l'animal se remit ; mais il fut très agité et vécut encore 48 heures[14].

Pirogov tente de répondre précisément à la question suivante : Quels sont les seuils toxiques et thérapeutiques de l'éther pour l'organisme ? Pour apporter des éléments de réponse, il effectue de nombreuses expériences sur des animaux vivants, à qui il injecte toutes sortes de doses d'éther afin d'en suivre les effets toxiques et pharmacologiques. Son approche expérimentale appartient à la tradition toxicologique issue des physiologistes français Magendie et Flourens, poursuivie par les recherches de Claude Bernard sur les effets du curare sur l'organisme animal, dont voici à titre de comparaison une citation extraite d'un de ses carnets de laboratoire rédigé en 1851 :

> Sur un gros chien bien portant, au commencement de la digestion et ayant servi quelques jours auparavant à une fistule du pancréas, on introduit sous la peau du dos du curare en dissolution. Après 10 à 12 minutes environ, les effets du curare se manifestent et la respiration s'arrête. Alors on place l'animal sur la table et on l'insuffle avec un soufflet dans la trachée pendant environ 2 heures et demie en cessant seulement par quelques intervalles. Voici ce qu'on observe pendant ce temps. Le sang est très bien poussé par les artères et le cœur et le pouls va bien. Quand on n'insuffle pas, le sang est complètement noir, aussitôt que l'insufflation recommence le sang redevient rutilant. Pendant ce temps, les pupilles vont toujours et oscillent en se dilatant et en se contractant quand on fait arriver la lumière, […]. Cette expérience prouve que le curare éteint le système nerveux de la vie animale, mais pas celui de la vie organique. (Mais cependant pourquoi disparaissent les mouvements respiratoires[15] ?)

Pirogov se questionne sans relâche à propos du mode d'action de l'éther et celle du sang sur le système nerveux après son éthérisation. L'action extrêmement prompte des vapeurs d'éther sur le système

14. *Ibid.*, p. 51-52.
15. C. Bernard, Carnet de laboratoire manuscrit conservé à l'IMEC, carnet 8 J, 1850-1852, en date du 29 septembre 1851.

cérébro-spinal le conduit à penser que les effets de cette substance sont transmis par le biais des nerfs et non par la circulation sanguine. Il énonce une théorie selon laquelle le sang éthérisé agit d'abord sur les parties centrales du système nerveux, avant d'agir sur les autres parties et sur le système nerveux périphérique. Il remarque également que l'action initialement sédative de la vapeur d'éther circulant dans le sang artériel se fait sur le cerveau : les premiers signes apparents de l'éthérisation comme la dilatation des pupilles ou la brillance particulière des yeux sont d'abord cérébrales. L'inconscience se produit plus tard, et graduellement, de façon plus ou moins périphérique, se produit la diminution de la sensibilité accompagnée presque simultanément par l'affaiblissement des mouvements volontaires. Pour tester ses hypothèses, il administre également l'éther par voie intraveineuse ou intra-artérielle avant de stimuler les nerfs périphériques. Pour étudier plus avant l'influence des voies d'administration et de l'emploi de l'éther sous forme liquide ou sous forme de vapeur, un premier groupe d'expériences est pratiqué après respiration de la vapeur d'éther (trachée-artère), tandis qu'un second groupe d'étude concerne l'injection rectale.

Les conclusions de ses études sont les suivantes. Pour obtenir une anesthésie, deux conditions doivent selon lui être remplies : l'éther doit être absorbé par la masse sanguine, et le sang doit entrer en contact avec le système nerveux. Les résultats de ces expériences d'éthérisation mettent en évidence l'importance de la forme et de la voie d'administration. Le sang est l'agent de transmission de la vapeur et communique son action assoupissante au système nerveux. Cependant, si la solution d'éther est directement injectée dans une veine, le décès est immédiat, même si la veine n'est pas proche du cœur. Le même résultat est obtenu si la solution est injectée dans la carotide. Les animaux doivent donc être anesthésiés par des administrations de vapeurs d'éther faites avec prudence.

Pirogov établit quatre degrés d'anesthésie :

1. Le premier degré se déclare aussi longtemps que l'éther est uniquement présent dans le sang veineux, l'éthérisation influence seulement la circulation et la respiration. Le même résultat est obtenu tant que le sang artériel n'est pas saturé d'éther.
2. Au second degré, le cerveau est affecté mais pas la moelle et ses fibres motrices ou sensitives.
3. Le troisième degré manifeste l'influence de l'éther sur la moelle : les réflexes disparaissent, la stimulation mécanique sur les nerfs est

sans effet, mais la stimulation galvanique fonctionne encore et peut provoquer des convulsions.

4. Finalement, au quatrième degré, on ne relève plus de sensation ou de mouvement et on note une relaxation totale. Seul le cœur bat.

Concernant le mode d'action de l'éther :

– L'action dépressive de l'éther sur le système nerveux a lieu par le biais du sang artériel au sein duquel les vapeurs d'éther pénètrent par le système capillaire.
– Les modifications physiques et chimiques produites dans les fibres cérébrales ont pour effets une dépression des capacités d'action du cerveau. Ces effets de l'éther se dissipent plus rapidement que dans le cas d'autres substances anesthésiantes.
– Bien que l'éther liquide appliqué directement sur les nerfs ait pour effet particulier l'inhibition des forces sensitives et motrices, les méthodes ordinaires d'éthérisation à travers les poumons et le rectum ne suppriment pas la sensibilité à moins que l'action du cerveau n'ait été plus ou moins abolie.
– Les fibres nerveuses demeurent sensibles à la stimulation galvanique même quand elles sont privées de leurs capacités motrices et sensitives.

Les quatre degrés de l'anesthésie décrits par Pirogov sont similaires à ceux trouvés par Flourens durant les expériences faites en collaboration avec François Longet en février et mars 1847 en vue d'étudier les effets de l'éther et du chloroforme sur le système nerveux central des animaux. Mais Pirogov étudie les effets de ces substances non seulement dans le cadre expérimental du laboratoire, mais également sur les champs de bataille. Quelle a été l'influence de ses observations et expériences menées sur le terrain ? Dans quelle mesure le contexte de guerre lui permet-il de développer l'éthérisation comme technique d'anesthésie ?

II. L'IMPORTANCE DE LA GUERRE SUR LES TECHNIQUES D'ANESTHÉSIE DE PIROGOV

Au début du printemps 1847, Pirogov a l'opportunité d'utiliser et d'étudier les effets de l'anesthésie par éthérisation sur l'homme dans les conditions des champs de bataille. Il est envoyé à Piatigiorsk, où s'affrontent l'armée russe et les tribus du Caucase alors en pleine rébellion. Il arrive en juin de cette même année en apportant son matériel anesthésique. Entre les mois de juin et septembre, il s'occupe de nombreux blessés et publie en 1849 un *Rapport médical d'un voyage* *au Caucase*[16] au sein duquel se trouve un long chapitre à propos de l'utilisation de l'éther dans des conditions de guerre. Il relate l'anesthésie de plus de cent blessés parmi lesquels seulement deux eurent une injection rectale à cause de la rudesse des conditions d'exercice sur le champ de bataille. Il y pratique également les premiers essais, en Russie, avec du chloroforme. Il compare les effets des deux substances et recueille de nombreuses observations. En seulement trois mois, plus de 690 anesthésies sont réalisées dont plus de 208 avec du chloroforme. Il conclut, à partir de ces expériences, que le chloroforme est plus facile à utiliser, notamment, à cause de la simplicité avec laquelle il s'administre et sa rapidité d'action, ce dernier point étant crucial pour un chirurgien de guerre. En outre, il souligne que le choix de la technique anesthésique utilisée a peu d'influence sur la réussite ou non de l'opération chirurgicale. Il faut noter que Pirogov est un des premiers à établir des listes statistiques reprenant la fréquence des échecs ou des réussites des opérations chirurgicales faites sous anesthésie ou non et menées durant les guerres. Lors de son retour à Saint-Pétersbourg, en 1854, il rédige un manuel de chirurgie intitulé *Klinische chirurgie*[17]. Puis, la guerre de Crimée[18] commence en mars 1854 et Pirogov est envoyé en octobre à Sebastopol où il demeure jusqu'en février 1856. Entre 1854 et 1856, il coordonne le service ambulancier et introduit la notion de tri des blessés selon le critère de la gravité de leurs blessures établie après un examen minutieux et sépare les plaies propres des plaies infectées. À peu près dix mille opérations sont réalisées pendant ce conflit. Sa notoriété dans le domaine de la chirurgie de guerre devient considérable.

Il cesse toute pratique chirurgicale en 1866 et devient alors l'observateur attentif de plusieurs conflits pour la Croix-Rouge. En 1870-1871, il est envoyé en tant qu'observateur des structures hospitalières françaises et allemandes mises en place pour répondre aux besoins

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16. N. Pirogov, *Rapport médical d'un voyage au Caucase*, Saint-Pétersbourg, Imprimerie du journal de Saint-Pétersbourg, 1849.

17. *Id.*, *Klinische Chirurgie*, Leipzig, Druck und Verlag von Breitkopf & Härtel, 1854.

18. La guerre de Crimée se déroule de 1853 à 1856. En février 1854, la France et la Grande-Bretagne demandent à la Russie de quitter les principautés de la Moldavie et de la Valachie. Le 27 mars 1854, sans réponse de la part de la Russie, les nations françaises et britanniques lui déclarent la guerre. L'enjeu de ce conflit est avant tout géopolitique dans la mesure où, si la Russie gagne cette guerre, elle s'assure le contrôle du commerce maritime entre la mer Noire et la Méditerranée.

du conflit. Il voit, non sans satisfaction, que nombrent des indications données dans ses livres pour une meilleure chirurgie et une meilleure organisation des soins sont mises en pratique. Il est également envoyé comme observateur pendant la guerre russo-turque en 1877 et 1878. Il relatera ses expériences de la chirurgie de guerre dans plusieurs livres et rapports de voyages. Il y répertorie les opérations de nombreux patients et établit plusieurs résultats à propos de l'utilisation de l'éther comme outil thérapeutique, notamment les cas où l'endormissement par éthérisation échoue que ce soit à cause des appareils utilisés pour l'inhalation ou à cause de la quantité d'éther inhalée.

Sa pratique depuis la campagne du Caucase et le siège de Sébastopol constitue un chaînon intermédiaire entre la chirurgie septique des guerres napoléoniennes et la chirurgie antisepto-aseptique inaugurée pendant la guerre russo-turque de 1877-1878. C'est le contexte de guerre qui va lui permettre de passer de l'expérimentation animale au développement rapide de la chirurgie clinique humaine.

III. LA PLACE DE PIROGOV DANS L'HISTOIRE RUSSE DE L'ANESTHÉSIE

On ne peut détacher les travaux de Pirogov du contexte russe du xix^e^ siècle. Les circonstances de l'usage de l'éther en Russie et les développements d'une chirurgie sans douleur doivent être rappelés. Nicolas I^er^, empereur de Russie entre 1825 et 1855, « le plus logique des autocrates », se montre très sensible à l'image de son pays dans le reste de l'Europe. Le génie national russe doit s'exprimer au niveau scientifique. C'est donc en 1847, par sa volonté d'accroître le prestige des sciences médicales russes aux yeux du monde, qu'est ordonné un programme de recherche expérimentale sur l'usage de l'éther.

Il faut rappeler que l'éther est utilisé dès 1842 par le chirurgien américain Crawford Long[19] en prévention de la douleur chirurgicale. Cette substance chimique était déjà employée comme une drogue euphorisante depuis la fin du xviii^e^ siècle. Ses effets physiques et mentaux étaient même surnommés les *folies à l'éther.* De véritables orgies d'éther étaient organisées dans de nombreux pays, provoquant des addictions. Crawford Long, alors jeune étudiant usant de cette substance, fit

19. C. W. Long (1815-1878) est un pharmacologue américain connu pour avoir pensé à utiliser l'éther comme anesthésiant à l'occasion de l'opération d'une tumeur en 1842.

le lien entre l'état d'euphorie et le sommeil profond qui faisait suite. Long fit des essais dans la petite agglomération rurale où il exerçait (Jefferson, Georgia) et ne publia pas immédiatement ses résultats. Le concept même de chirurgie sans douleur serait resté enterré en Géorgie sans les travaux d'un dentiste américain, William Morton. Ce dernier fit la démonstration publique en 1846 des effets anesthésiants des inhalations d'éther mais échoua à faire reconnaître ses droits sur la découverte dont il fit une de ses obsessions. Les toutes premières anesthésies eurent cependant lieu dans les mois qui suivirent à Londres, Paris, Erlangen, Leipzig, etc.

Quand le ministre russe de l'Éducation annonce la nouvelle de la découverte d'une utilisation médicale de l'éther au directeur à Moscou du département de l'éducation, le 25 février 1847, un programme de recherche sur les nouvelles méthodes de chirurgie sans douleur est initié. Filomafitsky, professeur de physiologie et directeur de la faculté de médecine de Moscou est désigné pour organiser des recherches d'envergure. C'est ainsi que Pirogov rejoint ce programme et c'est dans ce cadre qu'il commença ses nombreuses expériences. Ses voyages en France et en Allemagne lui ont ensuite permis d'acquérir une notoriété certaine.

Les traités et articles sur l'histoire de la chirurgie le décrivent régulièrement comme un des premiers à avoir expérimenté de façon systématique les différents usages médicaux et physiologiques de l'éther. Ce furent ses essais cliniques et chirurgicaux en conditions de guerre, la fabrication de son matériel d'inhalation, son approche statistique concernant l'influence des anesthésies sur les résultats des opérations chirurgicales qui le rendirent célèbres en Europe. Sa méthode d'administration rectale de l'éther est reprise par le chirurgien Oskar Wanscher en 1884 et adoptée en France, cette même année, par Daniel Mollière. En 1905, sa méthode est essayée à Boston par John Henry Cunningham et Frank Howard Lahey, développée entre 1910 et 1914 et encore utilisée au Danemark dans les années 1930.

CONCLUSION

Nikolaï Pirogov incarne typiquement les interactions européennes constitutives de la médecine au XIX^{e} siècle. Il a beaucoup influencé comme il a été également très influencé par les chercheurs européens, notamment allemands et français. Ses travaux ont également pu bénéficier de l'intérêt russe pour les perspectives médicales ouvertes par les techniques occidentales de chirurgie sans douleur. De nombreux auteurs tels que Altschuller et Keenan dans leur fameux article *History of the development of painless surgery in Russia*[20] soulignent l'importance du rôle qu'il a joué dans les développements et la diffusion des techniques de l'anesthésie, en Russie et sur les champs de bataille européens. Bien que son nom ait eu tendance à être oublié pendant la Guerre froide, Pirogov est aujourd'hui reconnu en Russie et dans le reste de l'Europe comme un des fondateurs de la chirurgie moderne.

20. G. Altschuller et W. A. Keenan, art. cit., p. 493-502.

III

Michel Kourilsky : un destin extraordinaire et ordinaire

Natalia Fedounina

La famille Kourilsky est bien connue aujourd'hui en France. Raoul Kourilsky – professeur de médecine, membre de l'Académie de Médecine, héros de la deuxième guerre mondiale, père de six enfants ; Raymond Kourilsky, polytechnicien, ingénieur civil des mines, père de cinq enfants ; Andrée Kourilsky épouse Porta, un industriel melunais, mère de trois enfants. Parmi les petits-enfants, des directrices de recherche au CNRS, une directrice de compagnie théâtrale, de hauts fonctionnaires, des industriels, des professeurs de médecine, un polytechnicien, professeur au Collège de France, ex-directeur de l'Institut Pasteur, un ancien directeur général du CNRS. Nombre d'entre eux ont reçu des décorations, souvent de grade élevé à titre civil ou militaire (croix de guerre, Légion d'honneur, ordre du Mérite, Arts et Lettres). Néanmoins le fondateur de cette famille extraordinaire, Mottel (dit Michel) Kourilsky est venu de Russie à l'occasion d'un flux d'immigration étudiante. Dans ce sens, le destin de Michel Kourilsky a les caractéristiques communes de beaucoup d'autres descendants provenant de Russie.

Mottel Kourilsky a été le deuxième des trois enfants de Gerchon Movchovitch Kourilsky, directeur de la classe de violon du Conservatoire de musique d'Odessa et de Slata Aizikovna, son épouse, son père étant originaire de Sokolka, province de Grodno et sa mère originaire d'Ananief en Ukraine. Il est né à Odessa le 28 juin 1868 et fait ses études secondaires au lycée impérial d'Odessa. Il émigre en France en octobre 1889 et fait ses études de médecine à Paris. Il était accompagné ou rejoignait plusieurs amis de familles juives comme lui-même, notamment Moundlic, Moguilevsky, Rabinovitch. En effet, l'origine familiale a été le facteur principal de migration des étudiants juifs de Russie[1]. Si jusqu'aux années 1860 les jeunes juifs ont été habituellement éduqués à l'intérieur de la communauté, dans le mode traditionnel, dans les années 1880 la situation a changé avec le mouvement de la Haskala (les Lumières juives), quand l'éducation est devenue un moyen de s'émanciper et de conquérir la liberté. Le nombre de juifs dans les écoles supérieures a augmenté de cent vingt-neuf (3 %) en 1865 à 1856 (14,5 %) en 1886[2]. Ce processus a coïncidé avec la restriction de l'accès de juifs à l'éducation. En 1887, on a introduit les quotas d'admission (numerus clausus) des juifs dans l'université[3]. En conséquence, le pourcentage des juifs parmi les étudiants émigrés a considérablement augmenté (notamment après la première révolution russe, mais c'est vrai aussi pour la fin du XIX[e] siècle)[4]. Les Russes ont choisi le plus fréquemment les universités de Paris, de Nancy et de Montpellier[5]. Il n'y avait ni quotas ni pogroms en France. De plus, le contrôle policier y était beaucoup moins strict qu'en Allemagne, par exemple. Vers la fin du XIX[e] siècle, la France attira beaucoup de médecins russes. On note l'afflux des Russes dans l'enseignement supérieur français dans le dernier quart du XIX[e] siècle. En 1882-1883, à la Faculté de médecine, trois hommes étrangers sur dix sont russes ; quant aux femmes, sur quarante-cinq étudiantes, quatorze sont russes (31 %). À la fin de la décennie 1880, les Russes représentent 87 % des étrangers[6]. Après

1. A. E. Ivanov, « Rossiiskoe studencheskoe zarubezh'e. Konets 19 nachalo 20 veka », *Voprosy istorii estestvoznaniia i texniki*, 1, 1998 ; V. Karady, « La migration internationale d'étudiants en Europe, 1890-1940 », *Actes de la recherche en sciences sociales*, 145, 5, 2002, p. 47-60 ; I. Gouzevitch et D. Gouzevitch, « Étudiants, savants et ingénieurs juifs originaires de l'Empire russe en France (1860-1940) », *Archives juives*, 35, 1, 2002, p. 120 et p. 128 ; (auteur ?), *Russijskoe zarubezhie vo Francii 1919-2000, Biograficheskij slovar'*, Moskva, Nauka, Dom-muzej Mariny Tsvetaevoj, 2008.

2. I. Gouzevitch et D. Gouzevitch, art. cit.

3. A. E. Ivanov, art. cit.

4. *Ibid.*

5. *Ibid.*

6. P. Moulinier, « Les étudiants étrangers à Paris au XIX[e] siècle. Origines géographiques et cursus scolaire », in H. R. Peter et N. Tikhonov, *Universitäten als Brucken in Europa. Les universités : des ponts à travers l'Europe*, Francfort-sur-le-Main, P. Lang, 2003 ; *id.*, *Les étudiants étrangers à Paris au XIX[e] siècle : origines géographiques et cursus scolaires*, 2002, [http://barthes.ens.fr/clio/revues/AHI/articles/preprints/moulinier.html] (consulté le 25/04/2016) ; *id.*, *Les premières doctoresses de la Faculté de médecine de Paris (1870-1900) : des étrangères à plus d'un titre !*, communication au colloque Histoire/Genre/Migration, Paris, mars 2006.

avoir obtenu le diplôme d'études supérieures, beaucoup de Russes continuèrent leur formation.

Mottel est donc venu en France à l'automne 1889 avec ses amis et a fait ses études de médecine à Paris. On ne connaît pas beaucoup de détails sur ses études. Il est retourné en Russie en octobre 1890 pour y accomplir un an de service militaire volontaire, une condition importante pour l'obtention des demandes de naturalisation. L'acquisition de la nationalité nécessitait dix années de séjour en France et l'accomplissement du service militaire[7]. Après plusieurs voyages en Russie, il revient définitivement en France en 1893. Il a été externe des hôpitaux de Paris, médaille de bronze de l'Assistance publique et a soutenu une thèse de doctorat le 2 juillet 1896 « De la polyurie hystérique » sous la présidence de Fulgence Raymond.

Le titre de sa thèse est assez caractéristique. Elle a été soutenue trois années après la mort de Charcot, mais le choix du thème – l'hystérie – peut indiquer l'influence de ce neurologue célèbre. Charcot a en effet été très populaire parmi les étudiants de Russie. Dix-sept thèses sur quarante (42,5 %), soutenues par les étrangers sous la présidence de Charcot, furent écrites par des Russes et autres originaires d'Europe de l'Est[8]. Les théories de l'hystérie et des symptômes hystériques de la fin du XIX^e^ siècle ont fortement subi son influence. Beaucoup de thèses sur l'hystérie ont été faites sous la direction de Charcot ou portent sa marque, après sa mort. On ne peut citer que quelques thèses de médecine russes liées à l'hystérie : celle de Rachel Bychoffski (1893), « Contribution à l'étude de l'hystéro-traumatisme » (président : Potain), de Glafira Abricossoff (1897), « L'hystérie aux XVII^e^ et XVIII^e^ siècles (étude historique) » (président : Joffroy), Elie Léon Rabiner, « Contribution à l'étude clinique du mutisme et du bégaiement chez les hystériques », 1896 (président : Straus), Marie (Malea) Goldman (1899), « Confusion mentale chez les hystériques » (président : Raymond), etc.

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7. La question de la citoyenneté a été liée au statut économique et juridique d'émigrants en France, avec la possibilité d'obtenir des emplois publics. G. I. Liubina, « Russkoe nauchnoe zarubezh'e v Parizhe vo vtoroi polovine 19 – nachale 20 veka », in *Rossiiskie uchenye i inzhenery v emigratsii*, Moskva, PO « Perspektiva », 1993, p. 13-27 ; G. I. Liubina, « Russkaia nauchnaia emigratsiia 19 veka v Parizhe: obshchii vsgliad i uroki », *Voprosy istorii estestvoznaniia i texniki*, 2, 2002.

8. T. Gelfand, « Charcot médecin international », *Revue neurologique*, 150, Paris, 1994, p. 517-523.

Après le concours d'externat et le doctorat de médecine, il s'établit comme médecin généraliste au Châtelet-en-Brie puis à Bombon (Seine-et-Marne). La demande de naturalisation de Michel Kourilsky est satisfaite en juin 1899. Il épouse Eugénie Oudart (1880-1958) en 1898. Elle perd de ce fait la nationalité française et acquiert la nationalité russe. Elle redeviendra française en juin 1899 en vertu du décret de naturalisation de son mari, un mois avant la naissance de son premier enfant. La famille s'établira à Paris vers 1908 avec ses trois enfants. Michel participe à la guerre de 1914-1918 en tant que médecin militaire. Epuisé et malade, il est rapatrié à Paris en 1917 où il décède en décembre des suites d'une opération chirurgicale. Il est enterré à Blandy-les-Tours aux côtés de son épouse.

Remerciements

Je remercie les descendants de Mottel Kourilsky qui m'ont apporté des précisions utiles pour cet article.

IV

Involuntary and voluntary movements: mostly common views of Alfred Vulpian (1826-1887) and Ivan Sechenov (1829-1903)

Jean Massion and François Clarac

INTRODUCTION

During the course of the 19th century, the concepts of sensori-motor organisation by the central nervous system were progressing quickly, especially in physiology. The concept of reflexes and their support by the spinal cord was well established in the description by Jiří Procháska (1749-1820), and the evidence of distinct sensory and motor roots by Charles Bell (1774-1842) and François Magendie (1783-1855) represented a great step forward in the understanding of their mechanism. The reflexes were called involuntary movements, because their performance was observed in decapitated frogs, in the absence of the brain and of consciousness. Marked progress was made by the description of the specific automatic functions in organic life, such as respiration and in animal life, such as locomotion. They were also included among the involuntary movements, because their activity was observed after a lesion of the brain, which was proposed as the seat of the voluntary movements. In contrast, the main difficulty for physiologists was the understanding of the mechanisms of voluntary movements because they were defined in psychological and philosophical terms, and thus escaped the field of Physiology. According to a materialist approach, these mechanisms were considered dependent on the integrity of the brain. How is it possible to explain the problem of the will, the concepts of ideas, memory and consciousness in physiological terms? In his book "The reflexes of the brain"[1] (1863), Ivan Sechenov

1. I. Sechenov, *Reflexes of the brain* [1863], Cambridge, MA, The MIT Press, 1965.

(1829-1903) took a new insight into this problem by describing what he called "psychical reflexes" at the origin of voluntary movements.

At about the same time, the lessons given in 1863 by Alfred Vulpian[2] (1826-1887) are worthy of interest. Vulpian replaced Pierre Flourens (1794-1867) and he was in charge of the chair in comparative physiology of the Museum of Natural History in Paris. These lessons were published in 1866 under the title "General and comparative physiology of the nervous system." Although this publication, as well as that of Sechenov, were made before the findings by Gustav Fritsch (1837-1927), Eduard Hitzig[3] (1838-1907) in 1870 and David Ferrier[4] (1843-1928) in 1876, concerning the excitability of the motor cortex, at a time when the localisation of functions in the hemispheres was still uncertain, both scientists developed a materialist approach of brain functions based on physiology and psychology, and took the model of reflexes to explain the mechanisms of psychical functions such as "volition." In addition, Sechenov introduced the concept of inhibition as a specific tool to modulate the machine-like functioning of the reflexes under the influence of the psychical factors.

Vulpian and Sechenov most probably met during Sechenov's stay in Paris in 1862. Vulpian was well aware of the work of Sechenov on central inhibition which he criticized in his book on the basis of new experimentation. Both Sechenov and Vulpian quoted the papers by Brown-Séquard (1817-1894), who was in Paris at that time: "*Deux mémoires sur la physiologie de la moelle épinière,*" (Two memoirs on the physiology of the spinal cord). Even if they had no close contact during Sechenov's stay in Paris, it is interesting to compare their views on involuntary and voluntary movements suggesting large reciprocal influences between the French and Russian physiologists.

I. WHO WAS ALFRED VULPIAN (1826-1887)?

Alfred Vulpian was at the same time an outstanding physiologist, an excellent neurologist and an anatomo-pathologist. He was born in Paris in 1826.

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2. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveu*x [Lessons on General and comparative Physiology of the nervous system], rédigées par Ernest Brémond, Paris, Baillière, 1866.

3. G. Fritsch and E. Hitzig, "Über die elektrische Erregbarkeit des Grosshirn," *Archiv für Anatomie und Physiologie*, 37, 1870, p. 300-332.

4. D. Ferrier, *The functions of the brain*, London, Smith, Elder & Co, 1876.

Vulpian's training in physiology started with his job as "préparateur" (technician) in the laboratory of Flourens, who held the chair in comparative physiology at the Museum of Natural History of Paris from 1838 until 1867. Flourens was impressed by Vulpian's qualities as an experimenter and as a physiologist, and encouraged him to become a graduate student in medicine. Vulpian obtained his doctoral thesis in 1853, on the origin of the cranial nerves. He was appointed "médecin des Hopitaux" in 1857 and "agrégé" of the Faculty (docent) in 1860. For three years, he taught as Flourens' professor deputy of comparative physiology at the Museum and published his lessons in 1866.

Vulpian was also a neurologist and an anatomo-pathologist. He was a contemporary and very close friend of Jean-Martin Charcot (1825-1893) (see Bogousslavsky[5] *et al.*, 2011). They were both interested in the anatomo-clinical method and were both appointed the same year, 1862, at the Hospice of the Salpêtrière hospital in Paris. Vulpian acknowledged that the microscope was not given as much attention in France as it was in Germany, with Rudolf Virchow, (1821-1902) and, together with Charcot, he played an important role in developing microscopy in Paris. One of his main findings was a description, in parallel with Charcot, of multiple sclerosis.[6, 7] In 1867, he succeeded Léon Jean Baptiste Cruveilhier (1791-1874) as professor of pathological anatomy, and in 1872, after the resignation of Charles-Edouard Brown-Séquard, he switched to the chair of experimental and comparative pathology.

His other main fields of interest were the degeneration and regeneration of the nerves and the vaso-motor apparatus. He was also a pioneer in neuro-pharmacology. From his histological observations on the adrenal gland, he assumed that the medulla of this gland synthesized a substance,[8] later identified as adrenalin, in 1901 by Jokishi Takamine[9]

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5. J. Bogousslavsky, O. Walusinski and T. Moulin, "Alfred Vulpian and Jean-Marie Charcot in each other's shadow? From Castor and Pollux at La Salpêtrière to Neurology Forever," *European Neurology*, 65, 2011, p. 215-222.

6. A. Vulpian, "Note sur la sclérose en plaques de la moelle épinière," *Union médicale*, 30, 1866, p. 459-465, p. 475-482 et p. 541-548.

7. M. Charcot, "Histologie de la sclérose en plaques," *Gazette des Hôpitaux de Paris*, 141, 1868, p. 554-555 et p. 557-558.

8. A. Vulpian, "Note sur quelques réactions propres aux substances de la capsule surrénale," *Comptes rendus de l'Académie des Sciences*, 43, Paris, 1856, p. 663-665.

9. J. Takamine, "The isolation of the active principle of the suprarenal gland," *Journal of Physiology*, 27, London, 1901, p. xxix-xxx.

(1854-1922). Together with Charcot and Brown-Séquard, he founded the "*Archives de physiologie normale et pathologique.*"

Well-known French neurologist, Joseph Babinski (1857-1932), was an "interne" of Vulpian, before becoming the "Chef de clinique" of Charcot at the Salpêtrière. Member of the Academy of medicine in 1867, dean of the faculty of medicine at the university of Paris, in 1875, and member of the Academy of sciences in 1876, Vulpian died in 1887 from pneumonia, at age 60.

II. VULPIAN'S VIEWS ON INVOLUNTARY MOVEMENTS

Vulpian was influenced in his analysis of the involuntary movements by the views of his mentor, Flourens, based on a hierarchical organization of the brain functions identified by rostro-caudal sections of the Brain. Flourens experienced mainly on birds (pigeons), and made a distinction between voluntary movements, depending on the hemispheres, and involuntary movements based on lower brain stem structures. Vulpian analyzed four main categories of involuntary movements in the lessons: the reflexes, the sensitive-motor reactions, the automatisms and the instinctive behaviours. In his analysis, he was very attentive to the evolutive aspects as Flourens himself was.

Vulpian[10] defined the reflexes with anatomo-functional criteria: "a reflex phenomenon," as seen in the decapitated frog, is a movement provoked in a given part of the body by a stimulation originating from that part and acting through a nervous centre other than the so-called brain, *i.e.* without the intervention of the will." He follows in this regard the analysis by Johannes Müller (1801-1856), the well-known German physiologist of Berlin University. For Müller:

> The central organs are submitted to the effects of sensitive nerves, and either reflect them onto the origin of the motor nerves – without consciousness being informed – which produces reflex movements, or transmits them to the *sensorium commune* – in such a way that consciousness is informed.[11]

10. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 394.

11. J. Müller, *Manuel de Physiologie*, translated from German by A. J. L Jourdan, reviewed by E. Littré, Paris, Baillière, 1851 [2nd ed.], t. 1, p. 733.

The old concept of *sensorium commune* (first proposed by Aristotle) refers to the site in the brain, which collects information from the proper sense organs in order to produce sensations, which are the source of consciousness.

The reflex action in spinal animals is, generally speaking, considered to be a fixed prewired circuit that always produces the same response.[12] The original aspect of Vulpian was to consider the reflex as a goal-directed movement. For example, pinching a toe of a hind limb in a decapitated frog provokes a flexion of the limb and has a functional significance, *i.e.* the withdrawal of the stimulated part of the body from the stimulus. When the stimulus intensity increases a bilateral limb extension takes place instead of an ipsilateral flexion, which should either propel the animal forward or push the irritating agent away. These reactions seem aimed at protecting the animal against external aggressions. As Vulpian wrote: "Thus, the spinal cord enables each part of the body to evade irritative causes through appropriate reflex movements,"[13] and thereby contributes to preserving the constancy of the internal environment of the animal (see Claude Bernard (1813-1878);[14] see also Jeannerod[15]).

One of the most surprising observations reported by Pflüger (1829-1910) and mentioned by Vulpian was that, in addition, adaptation took place in the reflex activity of the spinal cord. A stimulation of the dorsal part of the skin on the back, by a drop of acid, elicits a movement of the ipsilateral distal hind limb toward the stimulated part of the skin, followed by rhythmic movements of the distal part of the limb around the stimulated area in order to eliminate the source of the nociceptive action. This resembles more a motor sequence triggered by a stimulus than a simple reflex. Pflüger[16] showed that this so-called reflex is adaptable. After amputation of the ipsilateral hind limb, the movement toward the stimulated skin area is now performed by the intact contralateral hind limb. For Pflüger, this adaptation suggested that

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12. M. Hall, "On the Reflex Function of the Medulla Oblongata, and Medulla Spinalis," *Philosophical Transactions of the Royal Society*, 123, 1833, p. 635-665.

13. A. Vulpian *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 423.

14. C. Bernard, *Introduction à l'étude de la Médecine expérimental*e, Paris, 1865, p. 180.

15. M. Jeannerod, *Le cerveau machine. Physiologie de la volonté* [The Brain Machine. Physiology of the will], Paris, Fayard, 1983.

16. E. Pflüger, *Die sensorischen Funktionen des Ruckenmarks der Wirbeltiere*, Berlin, Hirschwald, 1853.

some kind of consciousness does exist in the spinal cord, which would explain an action resembling that of a voluntary movement.

This led Vulpian to make an interesting suggestion by proposing that the same spinal centre, responsible for voluntary-like movements produced by external stimulation in spinal animals, could also be stimulated by centrifugal impulses originating from the brain during the performance of voluntary movements when the hemispheres are intact. This hypothesis was evoked later on by Nicolas Francois-Franck (1849-1921) in his lessons at the Collège de France in 1884 and 1885, replacing Etienne-Jules Marey (1830-1904). In 1887, François-Franck agreed with Vulpian on the role of the spinal cord in the execution of voluntary movements, and raised the question of the possible role of these spinal centres in compensating for paralysis in hemiplegic patients.[17] According to François-Franck, this compensation only enabled the performance during locomotion of associated movements on the paralysed side. Interestingly, much later on, Alstermark in the cat and Pierrot-Desseiligny in humans described the role of the propriospinal circuits (local centres in the spinal cord) in reaching movements under the control of the pyramidal tract (see Pierrot-Desseiligny[18]) and provided evidence that they contributed to the recovery of function in hemiplegic patients. This was in agreement with Vulpian's hypothesis.

III. SENSITIVO-MOTOR REACTIONS

For Vulpian, in the spinal animal, the peripheral impulses giving rise to reflexes are not transformed into sensations, although they are at the origin of complex goal-directed movements. Are the sensations giving rise to perception only elaborated in the brain? This question was raised by François Longet (1811-1871). He reported that young dogs and rabbits with ablation of the hemispheres, the striatum, the optic layer, and the upper part of the brain stem until the upper limit of the *pons* perceived painful stimulations and specifically reacted to them. Pinching the ear, the tail or pinching the 5th nerve produced a strong agitation of the animal, which performed several steps and,

17. C. E. François-Franck, *Leçons sur les fonctions motrices du cerveau (réactions volontaires et organiques) et sur l'épilepsie cérébrale*, pref. Professeur Charcot, Paris, Doin, 1887.

18. E. Pierrot-Desseiligny, "Transmission of the cortical command for human voluntary movement through the cervical propriospinal premotoneurons," *Progress in Neurobiology*, 48, 1996, p. 489-517.

emitted repetitive complaints, suggesting a conscious "painful sensation."[19] After a further lesion of the *pons*, these reactions disappeared. Vulpian confirmed the observations by Longet, and agreed with him when he proposed that the *pons* could be the "sensorium commune," where sensory impulsions are transformed into sensations at the origin of complex reactions to painful stimulation.[20] Interestingly, for Müller, the will, source of voluntary movements, was located in the bulbar region and was in connection with the sensorium commune[21] (see Dupont, 2007[22]), a point on which Vulpian disagreed.

When the mesencephalon is intact, as well as the optic layers, mammals and birds without hemispheres showed evidence of sensations related to impulses from the other sense organs, *i.e.* various kinds of tactile, visual, auditory, olfactory and gustatory sensations, as well as pain. For Longet (1860) and Vulpian, these sensations are perceived even though the brain is removed. Vulpian suggests reformulating the well-known sentence of Flourens in the following way: an animal without hemispheric lobes does not stare, nor listen, nor sniff, nor taste, nor touch (this was Flourens's sentence) but it sees, it hears, it feels odors and tastes, and still has tactile sensations (this was Vulpian's complement).[23] By contrast, as Longet said, the cerebral lobes are the receptacle where all sensations take a distinct shape, and become a lasting remembrance; in other words, they serve as the site for the memory, a property through which they give the animal the materials for its judgments (Vulpian,[24] with reference to Longet).

What is the function of the sensations remaining after hemispheric ablation? For Vulpian,[25] when the brain is involved in other actions, *i.e.* a voluntary movement, and the subject is not attentive to these sensations, they provoke sensitivo-motor reactions that protect the body against the various sources of irritation that surround it, as the reflexes do. To give just a few examples: the eye blink reflex in the presence

19. F. A. Longet, *Traité de Physiologie*, Paris, Masson, 1860, p. 211 et p. 213.

20. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 540-541.

21. J. Müller, *op. cit.* , t. 2. p. 92.

22. J.-C. Dupont, "Sources et implications physiologiques du discours psychologique chez Bain," *Revue d'Histoire des Sciences*, 60, 2007, p. 327-356.

23. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 671.

24. *Ibid.*, p. 672.

25. *Ibid.*, p. 674.

of a strong light or when an object approaches the eye, scratching in the presence of skin stimulation, etc. There are also reflex-like actions, related to the animal's orientation such as pursuing a moving target.

IV. AUTOMATIC MOVEMENTS

According to Flourens,[26] one of the characteristic features of the ablation of the cerebral lobes is the "loss of any spontaneous activity as such." In contrast, in the animal with a section isolating the spinal cord from the upper parts of the encephalon, the activities induced by "external irritation" are preserved.

Vulpian was puzzled, however, by the fact that spontaneous movements, centrally organized, could be observed in animals deprived of their cerebral lobes or hemispheres. This included, for example, the respiratory movements or locomotor movements called "automatic movements." For Clarac *et al.* (2000[27]), "In contrast to the emphasis on the sensory control of the motor acts, the concept arose from an 'intrinsic factor' or 'central pattern generator' consisting of neural circuitry within the CNS that is able to shape and pattern motor activity without sensory or descending input." How then is it possible to make a difference between an automatic activity and a spontaneous movement depending on the hemispheres and on a movement depending on the will? For Vulpian, the spontaneous movements depending on the will are "intentional." This is not the case for an automatic activity, such as locomotion, which needs a signal from the brain to start and stop and to be directed towards a goal.

Many automatisms depend on the integrity of the bulbo-spinal level and of the *pons* (respiration, locomotion, and learned components that are also present, especially in humans). The level of the species in the evolutionary scale should be taken into account,[28] and this evolutionary aspect was already stressed by Flourens. In birds, such as the pigeon, when thrown into the air, flying movements which are maintained

26. P. Flourens, *Recherches expérimentales sur les propriétés et les fonctions du système nerveux dans les animaux vertébrés* [Experimental Research on the Properties and Functions of the Nervous System in Vertebrate Animals], Paris, Baillière, 1842, p. 34.

27. A. Prochazka, F. Clarac, G. E. Loeb, J. C. Rothwell and J. R. Wolpaw, "What do reflex and voluntary mean? Modern views on ancient debate," *Experimental Brain Research*, 130, 2000, p. 417-432.

28. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 529-532, p. 581 and p. 684.

in a regular way are seen. Still more complex behaviours are observed after ablations restricted to the cerebral lobes in birds: at times they show a cleaning behaviour, using their bill to clean their feathers; at other times they place their heads under a wing, as they normally do when going to sleep. These last behaviours are not intentional, but resemble natural behaviours specific to the species, and probably belong to instincts.

V. SECONDARY AUTOMATISMS OR HABITS

An interesting question which was raised during the 19th century, and even earlier, was the nature of "habits," also called "secondary automatisms."

William James (1842-1910), in his *Principles of psychology* (1890),[29] devoted a chapter to habits, which he considered an essential brain function: the capacity to modify its own inherited behaviour, on the basis of education. He made a distinction between instincts and habits. For him, habits to which there is an innate tendency are called instincts.[30] Habits are also the transformation by many repetitions of coordinated voluntary movements into skills. "Habit simplifies the movements required to achieve a given goal, makes them more accurate and diminishes fatigue."[31] "Habit diminishes the conscious attention with which our acts are performed."[32] "There are sensations to which we are usually inattentive, but which immediately call for our attention if we are wrong."[33]

For Vulpian,[34] habits belong to the transformation of voluntary movements by repetition. "The habit takes place, as in those complex and fast movements that occur in the handling of some musical instruments, which are at first so laborious, then become so easy." This idea was formulated in a very elegant way by Etienne-Jules Marey (1830-1904) in his book, *La machine animale*,[35] twelve years after Vulpian had formulated the concept:

29. W. J. James, *Principle of Psychology*, New York, Henry Holt and Co, 1890.
30. *Ibid.*, p. 104.
31. *Ibid.*, p. 112.
32. *Ibid.*, p. 115.
33. *Ibid.*, p. 118.
34. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 429.
35. E. J. Marey, *La machine animale* [The animal machine], Paris, Baillière, 1878, p. 28.

> For animal life, a habit seems to extend almost indefinitely the action of the will on the muscles by repeated exercises. The young animal shows by the clumsiness of its movements that it has not yet acquired to master muscular functions yet, while the gymnast and the expert pianist perform wonders of agility, force, precision, the effort of will that they display not seeming to be proportional to the result achieved. Several physiologists think, and we agree with them, that there exists in the brain and in the spinal cord centres of nervous actions which as a consequence of habit acquire given assignments. They succeed in controlling and coordinating given groups of movements without full participation of the part of the brain which governs the reasoning and consciousness of our acts.

Where are the habits organized? The general idea for Vulpian[36] was also that the habits were not organized at the same level as the voluntary movements, "for voluntary movements, the will only acts from a distance […] a given number of intermediary phenomena must necessarily take place in other parts of the brain such as the isthmus of the encephalon, the medulla oblongata, the spinal cord, the nerves." For him, the spinal cord was a possible site where habits are organized by repeated voluntary activities. One important aspect of building new habits is to inhibit some inborn coordination. This was stressed by Ioffe,[37] who showed the contribution of the motor cortex and pyramidal tract in the acquisition of new coordinations and their execution in dogs.

In place of habits, Jackson introduced the item of "secondary automatism" to define learned skill, for example writing.[38] He was a neurologist and much influenced by Herbert Spencer (1820-1903), an English philosopher, in his evolutionary view of the hierarchical organization of the nervous system. He defined three levels: the lower motor centres (spinal cord and brain stem) which are the most organized (most automatic) and the most simple; the middle level, corresponding to the convolutions, the "motor region" of Ferrier which are more complex and less organized (less automatic)[39] and the highest motor centres which are circumvolutions in front of the so-called motor

36. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 703.

37. M. Ioffe, "Pyramidal influences in establishment of new motor coordinations in dogs," *Physiology and Behavior*, 11, 1973, p. 145-153

38. J. Jackson, *Evolution and dissolution of the nervous system*, New York, ed. James Taylor, Basic Books Inc., 1958, vol. 2, p. 59.

39. *Ibid.*, p. 53.

region. They are the most complex and the least automatic, and make up the "organ of mind or physical basis of consciousness." The less automatic centres become more organized (or more automatic) by learning, as occurs when learning to write. Interesting information on the localization of secondary automatism or habits was also provided by the examination of neurological patients. As described by Jackson,[40] patients with lesions of the area of Broca were still able to answer by "yes" or "no" in response to questions in an automatic way. However, they were unable to pronounce "yes" or "no" when asked to do it by a voluntary command. The same dissociation between voluntary and automatic actions was also observed in ideomotor apraxia (see Paillard[41]). This indicates that the central nervous support for the same movements is different when being performed in a voluntary or in an automatic way (secondary automatism, habit).

VI. INSTINCTS

In his book *De l'instinct et de l'intelligence des animaux*, 1851, about instinct and animal intelligence, Flourens raised the question of the definition of instinct with respect to intelligence and with respect to habits and questions about the limits between them. He presented an interesting historical survey on the evolution of concepts from René Descartes (1596-1650), and his well-known theory of the automatism of animals, which he considered to be "animal-machines" (Descartes, 1637[42]). For Flourens (1851), instinct is a species-specific function.[43] Instinct and intelligence are quite opposite. Instincts are innate: the beaver builds its lodge without any learning, driven by an irresistible force. His capacity to build a lodge cannot be used for other tasks. By contrast, intelligence is "elective, conditional, and modifiable, all depends on learning, all is general, *i.e.* what has been learned can be generalized to other tasks." He thus rejects the idea expressed by Etienne Bonnot de Condillac[44] (1720-1780) in his *Traité des animaux*

40. *Ibid.*, p. 134.

41. J. Paillard, "Patterning of skilled movements," in J. Field (ed.), *Handbook of Physiology. Section I: Neurophysiology*, Washington, American Physiological Society, 1960, vol. 3, p. 1679-1708, p. 1692.

42. R. Descartes, *Discours de la méthode pour conduire sa raison et chercher la vérité dans les sciences*, Leyde, Ian Maire, 1637, chap. v, p. 44.

43. P. Flourens, *op. cit.*, 1851, p. 37.

44. E. B. de Condillac, *Traité des animaux*, Paris, de Bure, 1755, t. 2, chap. v, p. 363.

that instincts are not innate capacities with specific functions, but result from habits progressively formed on the basis of repeated reflections.

For Vulpian,[45] instinct is "an innate trend to accomplish given non-reasoned acts, often complex, and often irresistible." Three main classes of instinctive behaviours are defined: individual conservation (for example, feeding, hunting, defensive behaviour, etc.), species conservation (reproduction, etc.) and the relations of animals with other animals (social behaviour). However, he accepts the idea that the performance of the instinctive act also calls, in some cases, for adaptative learned components, such as the adaptation of the construction of the nest to the support context. The late development of comparative ethology with Konrad Lorenz (1903-1989) and Nikolaas Tinbergen (1907-1988) has confirmed the innate origin of most instinctive behaviours, but also the possibility of some learned components as well, included within the genetic framework.[46]

In a first approach, Vulpian claimed that instincts are lost after ablation of the hemispheres.[47] This is illustrated in birds after ablation of the cerebral lobes, concerning the feeding behaviour, as shown by Flourens (1842). This is also the case for instincts related to species reproduction. However, he proposed the possibility that other parts of the brain than those involved in voluntary movements take part in instincts. For Vulpian, the strong suction of the newborn baby seen at birth in mammals and especially in humans at a time when volition is not yet elaborated is in favour of this view.

VII. INVOLUNTARY MOVEMENTS: SECHENOV'S VIEWS

Sechenov is mainly known for his discovery of central inhibition, expressed in his book *The reflexes of the brain.*[48] He was born in 1829, and after an initial training at the Saint Petersburg military engineering College (1843-1848), and a brief period as a military engineer till 1850, he enrolled in medical school at the university of Moscow and graduated

45. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 893.

46. See I. Eibl-Eibesfeldt, *Ethologie, Biologie du comportement*, Paris, Naturalia et Biologia, 1972. French translation from *Grundriss der vergleichenden Verhaltenforschung*, Munich, Piper, 1969 [2nd ed.].

47. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 689.

48. I. M. Sechenov, *Reflexes of the Brain*, Cambridge, MA, The mit Press, 1965.

in 1856 with the intention of becoming a physiologist (see Stuart *et al.*[49]). After his graduation in medicine in 1856, he performed research between 1856 and 1862 in Berlin with Johannes Müller (1801-1858) and Emil du Bois-Reymond (1818-1896), in Leipzig with Felix Hoppe-Seyler, in Heidelberg with Hermann von Helmholtz, and in Vienna with Carl Ludwig. He defended his doctoral dissertation shortly after his return to Moscow (1860). His first academic position was at the Saint Petersburg medico-surgical Academy (1860-1870), starting as assistant until reaching the position of full professor. He had professorial positions successively in Odessa (1971-1976), Saint Petersburg University (1876-1888), and Moscow University (1891-2001). He died in 1905, and was endowed with the unofficial title of "father of Russian physiology" by Ivan Pavlov (1849-1946), the 1904 Nobel Laureate who was his student at the Saint Petersburg medical Institute.

How did Sechenov discover central inhibition? In 1862, he spent six weeks in Paris, in the laboratory of Claude Bernard.[50] In this short period of time he performed his experiments on central inhibition in the frog, published articles in Russian, French and German. He demonstrated that the transection of the encephalon at the level of the optic lobe enhances the intensity of the spinal reflexes induced by a nociceptive stimulation (withdrawal reflex after dipping the hindleg in a dilute acid solution). By contrast, the inhibition of the reaction was produced by the stimulation (crystal of salt, electrical) of the surface of the sectioned brain stem. Roger Smith interestingly analyzed his discovery of a central inhibition in his book on inhibition.[51] At first, Sechenov's findings were the subject of quite a few criticisms.

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49. D. G. Stuart, A. T. Schaefer, J. Massion, B. A. Graham and R. J. Callister, "Pioneers in cns inhibition: 1. Ivan M. Sechenov, the first to clearly demonstrate inhibition arising in the brain," *Brain Research*, 1548, 2014, p. 20-48.

50. J.-C. Dupont, "Qui doit élaborer la psychologie et comment le faire. Psychophysiologie et critique de l'idéalisme en Russie au temps de Setchenov et de Kavelin," *Revue d'Histoire des Sciences humaines*, 21, 2009, p. 29-54; *id.*, "The evolution of physiological psychology in Russia at the time of Sechenov in the international context," in J.-G. Barbara, J.-C. Dupont and I. Sirotkina (eds.), *History of the neurosciences in France and Russia. From Charcot and Sechenov to IBRO*, Paris, Hermann, 2011, p. 23-48.

51. R. Smith, *Inhibition. History and Meaning in the Sciences of Mind and Brain*, Los Angeles, University of California Press, 1992.

Vulpian was one of his contradictors.[52] However, for Roger Smith,[53] the main interest of his finding was, in agreement with Iaroshevskii, "that Sechenov transferred the attention from the inhibition of internal organs to the inhibition of skeletal muscles […] in order to establish a physiological correlate for psychological volition."

This led him to publish in 1863, in Russian, one of his major books *The reflexes of the brain*, initially entitled *Trial to establish the physiological basis of the psychic processes*, a title which was rejected by the tsarist censorship. The processes of reflexes and of central inhibition took a central part in his demonstration which aimed at providing a physiological basis to explain the physiological mechanisms which transform the machine-like character of the reflex response (a given stimulus always produces the same response) into an either enhanced or inhibited response. His book was conceived as a dialog with the reader, using a familiar approach of the problems of the reflex organisation, with examples taken from the daily experience of the ordinary reader. It had a considerable influence in Russia, where his materialist approach was controversial with respect to the conservative approach of most political authorities and of the Orthodox Church. In contrast, the international audience of the book was less important. The first French translation appeared in 1884 and the first English translation only appeared in 1952-1956, edited by the Foreign languages publishing house in Moscow, then in 1965 by the MIT Press. In his postscript, Walter Rosenblith mentioned Sechenov as a forebear of Norbert Wiener, the author of Cybernetics: "Sechenov who wrote – almost a hundred years earlier –: […] to every naturalist the idea of the machine nature of the brain is a godsend."[54]

The spinal reflex of the decapitated frog is given by Sechenov as an example of an involuntary machine-like induced movement in response to a sensory stimulation. A given cutaneous stimulation is always followed by the same reflex movement. If the cutaneous stimulation increases, the movement also increases in amplitude in the same limb or extend to other limbs. The neural circuit of this reflex is given in figure 1.

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52. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 438.

53. R. Smith, *op. cit.*, p. 104.

54. W. Rosenblith, "Postcript," in I. Sechenov, *Reflexes of the Brain*, Cambridge, MA, The mit Press, 1965, p. 145.

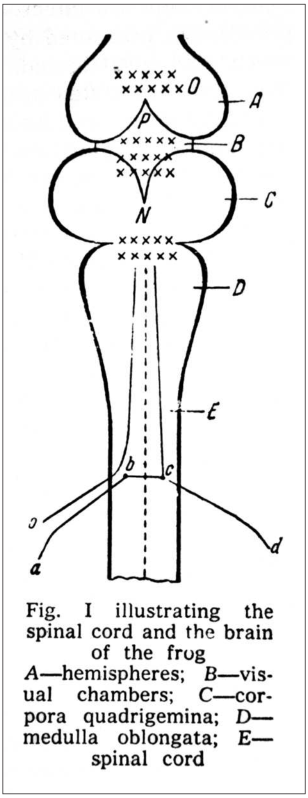

Figure 1. Schema of the central organization of reflexes, and their inhibition and augmentation (from the MIT Press, 1965).

When the brain is intact, there are two additional superimposed central mechanisms, one inhibitory, the other excitatory, which are put into action by the sensory afferents or by the influence of the hemispheres and act on the spinal level (fig. 1).[55]

To illustrate the inhibitory mechanism, Sechenov takes the example of the "start" response of a man to a sound. When repeated, the reflex response is progressively reduced and often disappears. For Sechenov, this is due to the intervention of two superimposed mechanisms: a first one is the involuntary machine-like reflex response and a second one an "appendage" which is the superimposed central inhibition induced by the same stimulus (fig. 1, *P*). In the case of responses to sound, the "start" response is followed by an additional inhibitory response which increases with repetition.

Alternatively the reflex response can be exaggerated as for example in presence of fright: in an intact frog, a slight touch of the skin induces a reaction of fright with generalized movements. Fright, in Sechenov's opinion, is an instinctive reaction "in which there is no room for reason or for will"[56] aimed at preserving the integrity of the organism. These reactions are suppressed after a hemispheric ablation. To explain the exaggerated response, Sechenov proposes also the superposition of two mechanisms: the light spinal reflex response to skin stimulation and the intensive movements in response to the perception of fright, probably through the centre *N* located at the bulbar level (this centre is responsible for locomotor movements) (fig. 1, *N*). "Thus the reader

55. *P*, inhibitory centre; *N*, excitatory centre; *A*, hemispheres.
56. I. M. Sechenov, *Reflexes of the Brain*, *op. cit.*, p. 25.

is now confronted with a case where a psychical phenomenon (fright) becomes part of machine-like processes."[57]

Numerous reflex movements where sensual enjoyment constitutes the psychical element should also be included into the category of involuntary movements, being due to the predominant activity of the reflex-intensifying mechanism.

Sechenov also includes among involuntary movements many "vegetative" reflexes, which are inborn, such as deglutition, but also locomotion, which is at least partly learned. "In some cases, walking can be an involuntary movement. Since it belongs to the category of movements which are acquired by habit and learning, *i.e*. movements which develop under mental influence, it can be assumed that all such movements may become involuntary, provided, of course, the mind is (at least in relation to these acts), in a state similar to that observed in somnambulists and intoxicated people."[58]

VIII. CRITICS BY VULPIAN OF THE LOCALISATION OF SECHENOV'S INHIBITORY CENTRE

According to Sechenov, the inhibitory centre was located at the level of the optic lobe, mesencephalon, pontine area and the upper part of the medulla oblongata, but not at the level of the spinal cord. His view encountered several criticisms. One was from Vulpian,[59] based on his observations on the spinal reflexes of the frog. These included (1) decapitated frogs showing increased reflex activity; (2) when further sectioning the spinal cord caudally, a short distance rostral to the lumbar enlargement, there was a further exaggeration of the hind limb reflexes (no quantitative data were reported, however); (3) this was shown with the hind limb reflexes induced by anal stimulation, and comparing two frogs, one decapitated, and the other with the above lower section of the spinal cord; (4) when a weak anal stimulation was applied, the former frog displayed a slight "start" of the hind limbs, whereas the latter showed an immediate full flexion of both hind limbs, with the heels reaching an area just above the excited area of the anal region, and then stretching backwards as if to push

57. *Ibid.*, p. 20.
58. *Ibid.*, p. 37.
59. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 438-439.

at the irritating agent, which was repeated two or three times; and (5) when a decapitated frog underwent a spinal hemi-section a few millimetres below the emergence of the brachial nerves, the result was an increase of the ipsilateral hind limb reflex action. In short, Vulpian showed more carefully than Herzen (1839-1906) did that a modulating inhibitory influence on spinal reflexes was due to the whole length of the spinal cord, and certainly not restricted solely to supraspinal effects as proposed by Sechenov in his 1863 articles. It can be noted, however, that Vulpian did not review the subsequent work of Sechenov in which he also found spinal inhibition and a clear role for sensory input in evoking central inhibition.[60]

IX. COMPARISONS BETWEEN THE VIEWS OF VULPIAN AND SECHENOV ON INVOLUNTARY MOVEMENTS

When comparing the concepts on involuntary movements of Vulpian to those of Sechenov, there are many common views, with nevertheless several divergent views which should be stressed. Concerning the common views, the fact that the spinal reflexes in the decapitated frog exert an efficient protection of the body against nociceptive agents, just as a voluntary movement would do is noted both by Vulpian and by Sechenov who comments in the following way "the apparent rationality of a movement (from the point of view of preserving the body) does not exclude the mechanical nature of its origin."[61] Both accept the idea that besides the simple reflexes, the involuntary movements include more complex reflexes extending over several body segments. They accept the idea that besides inborn automatisms, there are other aspects of involuntary movements such as learned habits (or secondary automatisms) being acquired by many repetitions of voluntary movements, performed once learned, without the full control of the will, and thus belonging to the category of unvoluntary movements. Both also agree on the fact that instinct represents a special category of movements, which are at least partly independent of the hemisphere, and thus represent also a kind of involuntary movements.

They diverge on two aspects. For Vulpian, in the absence of the hemispheres, there occur sensitivo-motor reactions to nociceptive

60. Sechenov, 1865; Sechenov and Paschutin, 1865; Sechenov, 1868: see complete references in D. G. Stuart *et al.* (2014), *op. cit.*

61. I. M. Sechenov, *Reflexes of the Brain*, *op. cit.*, p. 34.

stimulations, which appear to be conscious, indicating therefore a kind of primitive consciousness organized at the brain stem level. Sechenov does not mention this.

The second difference, which is a major one, is the discovery by Sechenov of central inhibition. There are two superimposed reflexes in each reflex action: a basic reflex, which is stable, a given stimulus always producing the same response, and a superimposed excitatory or inhibitory reflex, which enhances or reduces the basic one, as a function of psychical factors.

X. VOLUNTARY MOVEMENTS: COMPARISON BETWEEN VULPIAN AND SECHENOV

Generally speaking, Vulpian's approach of the will is related to the positivism of Auguste Comte (1796-1957). His guideline is that the "physiology should guide philosophy, the latter should follow it nearly step by step for fear of completely going astray."[62] His materialist views were at least partly those of Alexander Bain (1818-1903), who proposed that physiological and psychological processes were linked.

For Vulpian, as for Flourens, voluntary movements depend on the integrity of the hemispheres. They both insist on the fact that intentional spontaneous activities are characteristic of the intact hemispheres.

In his definition of voluntary movements, Vulpian refers to serial processes preceding the decision to move[63]: "The animal will have perceptions, and taking the opportunity of a present perception or of old perceptions extracted from memory and fixed by attention, it will be able to perform a kind of internal deliberation or judgement usually followed by a decision; and then completely voluntary movements may occur." In this definition, perception, actual or memorized, is critical for determining the following phases of the process.

Voluntary movements, for Vulpian, only originate from the hemispheres. According to him, "volitions" are never "primitive." They are able to generate an action only if preceded by an idea which arises and sustains the action.[64] As an example, the sight of an object could be at the origin of the idea to touch or to grasp it. Vulpian quoted

62. A. Vulpian, *Leçons sur la physiologie générale et comparée du système nerveux*, *op. cit.*, p. 429.

63. *Ibid.*, p. 6.

64. *Ibid.*, p. 704.

these causes as "excito-volitional." These causes are ideas, more or less complex, with desire, passion. "According to this point of view, which is the only true one, volitions, as several modern physiologists do admit, should and must be considered as phenomena of cerebral reflex actions."

Where is the brain support of this process? "Most phenomena of understanding arise from a similar mechanism. According to Vulpian, "the elements of the grey cortical substance of the brain are not spontaneously active. […] There is a need for an initial excitation; this excitation is usually determined by actual sensations which generate ideas. Once activated, the substance has the marvellous faculty to evoke the ideas previously formed therein with their reciprocal relations and then all processes of cerebral innervations may be developed, activated, oriented by innate dispositions, and by habits imprinted by education."[65]

Vulpian raises the question of cerebral localisations, referring first to the theories of Gall and Spurzheim,[66] concerning a classification of functions and their localization in distinct regions of the brain. He criticizes this theory which, according to him, has no physiological basis. He followed Flourens in his conclusion against localisation, based on his experiments with pigeons. With progressive lesion of the cerebral lobes, not one but all faculties were abolished at once. "The faculty to perceive, to judge, to want one thing is located in the same site as the one to perceive, judge, want another, and consequently this faculty, essentially a single one, resides in the same organ."[67] Vulpian adds that there is no evidence for localisation in patients. He nevertheless mentions the observations by Paul Broca (1824-1880) in 1861 on aphasia indicating the localisation of language in the third frontal circonvolution on the left side.[68] After the publication of Vulpian's book in 1866, the discovery by Fritsch and Hitzig (1870) of the excitability of the cerebral cortex reactivated the controversy between

65. *Ibid.*, p. 705.

66. F. J. Gall and J. C. Spurzheim, *Recherches sur le Système nerveux en général, et sur celui du cerveau en particulier ; mémoire présenté à l'Institut de France, le 14 mars 1808, suivi d'observations sur le rapport qui a été fait à cette compagnie par ses commissaires*, Paris, F. Schoell et H. Nicole, 1809.

67. P. Flourens, *op. cit.*, p. 102.

68. P. Broca, "Remarques sur le siège de la faculté du langage articulé suivies d'une observation d'aphémie (perte de la parole)," *Bulletins de la Société d'Anatomie*, 6, Paris, 1861, p. 330-357 et p. 398-407.

localisationists and antilocalisationists. It became very acute in Paris[69] especially between Charcot, who defended the cortical localisations of movements based on clinical observations, and Brown-Séquard, who attributed the paralysis after cortical lesions to an inhibition exerted by the cortical areas irritated around the lesions, on the bulbo-spinal centers.[70] Quoted by Charcot in his foreword to the lessons by François-Franck,[71] Vulpian recognized at that time that cortical regions are related to given body parts either for the movement or for the sensitivity as shown by the stimulation of the areas or their lesions. He agreed that the paralysis after a localized cortical lesion was an important sign of the localization of lesions. However, he still disagreed with the theory of "psychomotor centres." For him, the motor areas identified by the limb movements produced by their stimulation or by the limb paralysis after their lesion are due "not to exciting or eliminating the voluntary center of that limb, but by exciting or interrupting for a great deal at that level the communication between that limb and the whole cortical grey matter."

It is interesting to compare the views of Sechenov[72] to those of Vulpian, concerning voluntary movements. Both agree that the voluntary movement is the final stage of the "phenomena of cerebral reflex action."[73] For Sechenov,[74] "all conscious movements resulting from (psychical acts) are "reflex movements in the strict sense of the term" (Sechenov).[75] They are initiated by an external stimulation, usually an image, actual or memorized.

For Sechenov, at the origin of the psychical act there is a representation of the external world (an idea) based on the acquisition since childhood of multiple representations which are formed independently from the will, as the result of a repetition of chains of involuntary reflexes based on visual, tactile, auditory, proprioceptive, etc. inputs.

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69. See F. Clarac, J. Massion and A. Smith, "Duchenne, Charcot and Babinski, three neurologists of La Salpetrière Hospital, and their contribution to concepts of the central organization of motor synergy," *Journal of Physiology*, 103, Paris, 2009, p. 361-376.

70. See C. E. François-Franck, *Leçons sur les fonctions motrices du cerveau (réactions volontaires et organiques) et sur l'épilepsie cérébrale*, pref. Professeur Charcot, Paris, Doin, 1887.

71. *Ibid.*, foreword by Charcot, chap. I-II, p. 371.

72. I. M. Sechenov, *Reflexes of the Brain*, *op. cit.*

73. *Ibid.*, p. 705.

74. *Ibid.*, p. 80.

75. *Ibid.* p. 80.

He insists on the process of the construction of multiple representations by the repetition of sequences of reflexes.

Memory is the site where the partial representation of images, sound, and movements are stored after many repetitions, and recomposed without external substrate. Thinking is the faculty of recomposing the images and sounds in the absence of external substrate. The association of these concrete partial representations of objects lead to a subjective consciousness after learning. For Sechenov,[76] "thought is the first two thirds of a psychical reflex," the last third being the voluntary movement.

The voluntary movement can be reduced or prevented by inhibitory reflexes, as was already described for the involuntary movement. As a consequence, many psychic reflexes are not concluded by a voluntary movement even if the desire which includes affectivity is present. In this situation the process of thinking lasts as long as the voluntary movement is prevented. The prevention of the voluntary movement is influenced by fear and also by education and the high moral values associated with it. At the opposite, the movement can be intensified only within certain limits by emotional factors.

As a conclusion, Sechenov claims: "given the same internal and external conditions the activity of man will be similar. The choice of one of the many possible ends of the same psychical reflex is absolutely impossible. Its apparent possibility is merely a delusion of self-consciousness"[77] confirming thus his opinion on the machine-like origin of the psychical reflexes.

CONCLUSION

During the 19th century, scientists in France and in most European countries, following the ideas coming from the 18th century philosophers and from new concepts developed with the different revolutions, were very dogmatic, denying all religious influences but following Auguste Comte's positivism. There were even naïve explanations according to which the progress of science would cover all necessities and bring happiness to humanity. Such basic ideas have surely influenced Vulpian and Sechenov giving them a common scientific background.

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76. *Ibid.*, p. 86.
77. *Ibid.*, p. 105.

However, for both scientists, a great difficulty arose when dealing with the brain and the notion of voluntary movements, automatic movements being organized at a sub-cortical level. Both agree that the voluntary movement is the final stage of the "phenomena of cerebral reflex action." For Sechenov, the voluntary movement can be reduced or prevented by inhibitory reflexes, as involuntary movements are. Vulpian was not so affirmative but considered the brain as a whole following Flourens in his conclusion against localisation.

V

The electrodermal activity: looking for early French-Russian contributions

Henrique Sequeira, Pascal Deren and Bernard Maitte

INTRODUCTION

The electrodermal activity (EDA) corresponds to electrical variations recorded at skin level and related to the sweat glands functioning. Even if EDA is currently used as one of most important neurophysiological indices of brain functions, the very early recordings were carried out at the end of the 19th century by Féré and Tarchanoff, respectively a French and a Russian researchers. Conducted independently, their experiments revealed the strong scientific interactions through European physiological institutions, particularly between France and Russia, and the high level of engaged efforts to find reliable physiological indicators of cerebral functioning. Before considering the French-Russian contribution to the discovery of the EDA, we will recall, below, some characteristics underlying this physiological activity.

I. THE ELECTRODERMAL ACTIVITY AND ITS VERY EARLY ROOTS

1. The electrodermal activity

The EDA is the electrical expression of the activity developed by the eccrine sweat glands, mainly located in deep layers of palmar and plantar skin. These glands are under the exclusive control of the sympathetic branch of the autonomic nervous system. Through the sympathetic control, sweat production and consequent EDA variations express activations produced by brain areas. EDA thus reveals high functions subtended by the brain functioning, a sort of body window allowing access to the mind space.

In experimental practice, EDA is usually recorded, thanks to different techniques, like dermal conductance or potential, and using electrodes placed in contact with the skin of index and middle fingers. Following recommendations of Fowles *et al.*,[1] the EDA is generally recorded as cutaneous conductance variations and expressed in terms of electrodermal responses, known as skin conductance responses (SCRs) (fig. 1a). The amplitude of SCRs is strongly sensitive to activations generated by the central nervous system and transmitted to eccrine sweat glands by sympathetic fibers. Consequently, SCRs characteristics reveal mind functions requiring central activations like attention, action, memory or emotion. In this frame, SCRs are now recognized as a good marker of the cerebral impact of emotion on the body. Indeed, during emotional stimulation, SCRs amplitude increases with the subjective assessment of the emotional activation of the stimulus, regardless of emotional valence.[2] Therefore, SCRs constitute a reliable and robust index of the somato-visceral impact of brain activations engaged in the processing of emotional information.[3]

In a more general way, the EDA is considered as a very useful physiological activity, highly sensitive to any content with a specific signification for an individual. For this reason, that measure constitutes one of the best physiological *windows* of brain activity, subtending such information. Indeed, for more than a century, the EDA has been used as a tentative neural index of cognitive and emotional functions in healthy and pathological groups. Thus, the generalized utilization of the EDA among modern neuromarkers in the field of Neuroscience greatly increased the researcher's interest to elucidate and understand early steps of electrodermal recordings.

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1. D. C. Fowles, M. J. Christie, R. Edelberg, W. W. Grings, D. T. Lykken and P. H. Venables, "Publication recommendations for electrodermal measurements," *Psychophysiology*, 18, 1981, p. 232-239.

2. P. J. Lang, M. K. Greenwald, M. M. Bradley and A. O. Hamm, "Looking at pictures: Affective, facial, visceral and behavioural reactions," *Psychophysiology*, 30, 1993, p. 261-273; M. M. Bradley and P. J. Lang, "Measuring emotion: behavior, feeling, and physiology," in R. D. Lane and L. Nadel (eds.), *Cognitive Neuroscience of Emotion*, Oxford, Oxford University Press, 2000, p. 242-276.

3. H. Sequeira, P. Hot, L. Silvert and S. Delplanque, "Electrical autonomic correlates of emotion," *International Journal of Psychophysiology*, 71, 2009, p. 50-56.

2. Very early steps: Before Féré and Tarchanoff contributions

The very early history of electrodermal activity begins probably with experiments performed by Du Bois-Reymond,[4] in Germany; participants put either hands or feet into a zinc sulfate solution; this author observed an electrical current going from the limb at rest to the other one which was voluntary contracted. He explained this phenomenon as corresponding to muscle action potentials. Three decades later, Hermann and Luchsinger, carried out a first experiment in which they showed a link between the sweat gland activity and the existence of a current flow, recorded through the skin cat's paw.[5] In Paris, one year later, Vigouroux observed that different doses of anesthesia modified the psychological reactivity and induced skin current variations in hysterical patients.[6] This author explained such variations as being due to the vascular conductivity. Also in humans, Hermann[7] described skin currents, particularly intense in areas able to easily produce sweating, like palms and fingers, when participants execute movements similar to those executed by subjects of du Bois-Reymond, in 1849. On the basis of this experimental paradigm, Hermann, for the first time, proposed the implication of sweat glands to explain variations of skin currents. In fact, such currents were classically considered as the result of muscular or vascular activities, which could be modulated by the autonomic control of blood flow. Thus, the Hermann's proposition about the potential role of sweat glands was an essential step in the way to link these glands to electrodermal phenomena; however, at this stage proofs linking sweat glands to the EDA and the EDA to brain functions remained to be found. In this frame, experimental

4. E. du Bois-Reymond, *Untersuchungen über thierische Elecktrizität*, Berlin, Reimer, 1849.

5. L. Hermannand and B. Luchsinger, "Ueber die Secretionsströme der Haut bei der Katze," *Pfluger's Archiv*, 19, 1878, p. 300-319.

6. R. Vigouroux, "Sur le rôle de la résistance électrique des tissus dans l'électrodiagnostic," *Comptes rendus des séances hebdomadaires de la Société de Biologie*, 31, 1879, p. 336-339.

7. L. Hermann, "Neue Untersuchungen über Hautströme," *Pfluger's Archiv*, 27, 1882, p. 280-288.

arguments brought by Féré[8] and Tarchanoff[9] will constitute a determinant contribution.

II. FÉRÉ'S CONTRIBUTION: LOOKING FOR CLINICAL SOLUTIONS (LA SALPÊTRIÈRE, PARIS)

In Paris, the medical environment sustained since Cabanis (1757-1808) and afterwards through Pinel (1745-1826), developed close links between emotional shocks and mental disorders. However, after 1870's, new theoretical and therapeutic trends appear at the hospital "La Salpêtrière," in Paris. Indeed, at that period, Charcot (1825-1893) became the new director of the clinical service at la Salpêtrière (1862) and Féré (1852-1907) was engaged in the same service between 1881 and 1887 and presented this medical doctorate in 1882. During this period, Charcot's greatly influenced Féré's scientific orientations to the psychological field, such influence mostly corresponding to the idea that the mental pathology was favored by emotional vulnerability. Concomitantly, there were significant scientific progresses related to the physiology of emotions. Firstly, there was an increasing transfer of electrical phenomena knowledge from the physics to the physiology, giving rise to a new discipline, the electrophysiology, which became an essential tool for early physiological and psychophysiological explorations. Secondly, at the same time, Bernard (1813-1878) was looking for *physiology of emotions*, through researches about the *autonomic nervous system.*

1. Charcot's influence at La Salpêtrière

The Charcot's influence at La Salpêtrière was marked by a positivist attitude by which the Psychology was considered as a branch of Physiology. In this context, the Charcot's research program, mainly focusing on hysteria, was based on two main orientations. The first postulates that emotions could induce organic lesions and, by this way,

8. C. Féré, "Note sur les modifications de la tension électrique dans le corps humain," *Comptes rendus des séances hebdomadaires de la Société de Biologie*, 5, Société de Biologie, January 14th, 1888a, p. 28-33; *id.*, "Note sur des modifications de la résistance électrique sous l'influence des excitations sensorielles et des émotions," *Comptes rendus des séances hebdomadaires de la Société de Biologie*, 5, 1888b, p. 217-219.

9. J. Tarchanoff, "Décharges électriques dans la peau de l'homme sous l'influence de l'excitation des organes des sens et de différentes formes d'activité psychique," *Comptes rendus des séances hebdomadaires de la Société de Biologie*, 41, 1889, p. 447-451.

generate mental disorders like hysteria. Consequently, it was necessary to find objective markers revealing the mental impact of emotions and, finally, to be able to classify different types of patients. In coherence with this orientation, Charcot asked Féré to evaluate available knowledge about emotions and to quantify the impact of physical and emotional stimulations in the genesis of mental pathology, particularly hysteria. The second idea was based on the belief that electrophysiological techniques could be used as interesting means for diagnosis and therapeutics. Thus, both ideas will orient Féré's work and future issues about the discovery of skin electrical variations to different kind of stimulations.

2. Féré's approach: clinical aims and emotion

The Féré's approach was also guided by ideas that can be assimilated to methodological limitations. Indeed, he considered that there was no need of statistical procedures because clinical facts present a high degree of variability and are inherent to individuals and specific circumstances. In addition, Féré did not believe on the possibility and interest to transfer data from normal subjects to patients. In spite of these conceptions, Féré's environment presented a rich variety of technical tools to go further on physiological exploration, like dynamometer, pneumograph, tensiometer, plethysmograph, etc. In particular, Féré could use until fifteen techniques to identify physiological markers of emotions.

3. Féré's contribution… about electrical skin variations and consequent debate

The first contribution of Féré linking emotions and body electrical activity was published in 1888[10]. In this pioneer publication, he wrote: "[…] local modifications of tension under the influence of peripheral excitations or emotions […] this supposes that the organism can produce electrical activity."[11] This publication will originate a sustained debate about the electrical nature of recordings carried out at skin surface. Vigouroux, in a paper published one month later (Société de Biologie, February 11th, 1888) assumed a clear opposition to the explanation based in the electrical nature of recordings carried

10. C. Féré, "Note sur des modifications de la tension électrique dans le corps humain," *op. cit.*

11. *Ibid.*

out at skin surface: "[…] the existence of electrical charges at the skin surface remains to be demonstrated […]."[12] In the same publication issue, d'Arsonval (Société de Biologie, February 11th, 1888), on the contrary and in spite of some critics, confirms the reality of electrical skin variations linked to sensory stimulations.[13] In early March of the same year[14], Féré expressed, for the first time, the link between emotion and electrical skin variations: "[…] several kind of sensory excitations (visual, auditive, taste, olfaction) […] and emotion […] induce a rapid increase of current conductance (at the skin level) […]," even if such link is not specific to emotion.[15] At this stage, both Féré and d'Arsonval tried to explain respectively new observed facts: "[…] the organism is able to produce electrical activity" and "[…] excitation modifies the hygrometric state of the skin." Thus begins the promising relationships between emotion, sweating and electrical skin variations.

III. TARCHANOFF'S CONTRIBUTION: LOOKING FOR PHYSIOLOGICAL MECHANISMS (SAINT PETERSBURG, RUSSIA)

Tarchanoff, was born in Tbilissi (Georgia) and appears as a highly colored personage. For instance, his name appears in the scientific literature written as thirteen different forms. He expressed an extended scientific curiosity, developed rich relationships with most representative European laboratories in Physiology and was affiliated with the French *Société de Biologie.*

1. Tarchanoff's implication in physiology

Tarchanoff integrated the Russian team of Sechenov, recognized as an expert in the field of electrophysiology, and presented his medical doctorate in 1871. Some years later (1877), Tarchanoff became the successor of Cyon at the Chair of Physiology of the University

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12. R. Vigouroux, "L'électricité du corps humain," *Comptes rendus des séances hebdomadaires de la Société de Biologie*, 5, 1888, p. 138-142.

13. A. d'Arsonval, "Remarques à propos de la production d'électricité chez l'homme," *Comptes rendus des séances hebdomadaires de la Société de Biologie*, 5, 1888, p. 142-144.

14. C. Féré, "Note sur des modifications de la résistance électrique sous l'influence des excitations sensorielles et des émotions," *op. cit*.

15. *Ibid*.

of Saint Petersburg. During next years, he travelled through Europe and visited France, Germany, and Italy. In particular, he met Du Bois-Reymond and Hermann in Berlin and Bernard in Paris and developed local scientific collaborations, exploring several aspects of the animal physiology related to batrachians, birds, etc. In next years (1880-1886) his scientific interests became more focused on human physiology (heart rate, etc.) and psychology.

2. Tarchanoff: data from Saint Petersburg

In 1889, appeared the first publication in « Messager » (Saint Petersburg) in which the main discussed topic was similar to that previously published by Féré, in 1888.[16] A little later in the same year, Tarchanoff presented at the Société de Biologie (June 29th, 1889) a communication entitled: "Décharges électriques de la peau de l'homme sous l'influence de l'excitation des organes des sens et de différentes formes d'activité psychique." This title strongly matches with that of Féré, presented at the same society on March 3rd, 1888. Finally, a longer version of the Tarchanoff's work was published in Pflüger's Archiv, in 1990.[17]

It should be emphasized that Féré and Tarchanoff's publications differ significantly in terms of experimental approach. Contrary to Féré, Tarchanoff detailed experimental steps, describing carefully the material used, like the galvanometer or recording electrodes. In the same vein, he chose sensory (hot or cold water, pain, audition, cry, taste, olfaction…) and mental (calculations, fear, joy …) stimulations, applied during experiments. In addition, given references are mainly related to German authors (*e.g.* Hermann).

In conclusion, Féré and Tarchanoff followed opposite ways to analyze and interpret similar physiological variations: Féré, influenced by the French school, assumed a clinical approach and explore individual observations; Tarchanoff, influenced by German psychophysiology, adopted a scientific approach, based on experimental steps.

16. J. Tarchanoff, "O gal'vanicheskikh yavleniyakh v kozhe cheloveka pri razdrazhenii organov chuvstv i razlichnykh formakh psikhicheskoy deyatel'nosti" [About galvanic phenomena in human skin during stimulation of the senses, and various forms of mental activity], *Vestnik klinicheskoy i sudebnoy psikhiatrii i nevropatologii*, 7, 1, 1889, p. 73-81.

17. *Id.*, "Sur les manifestations galvaniques de la peau de l'homme par stimulations des organes des sens et différentes formes d'activité psychique," *Pfluger's Archiv*, 46, 1890, p. 46-55.

Despite such differences, both authors were able to build a common and an inseparable scientific heritage, based on relationships between nervous system activity, sweat glands and associated skin electrical variations.

IV. INSEPARABLE HERITAGE: FÉRÉ VS TARCHANOFF

Some years later, Féré synthetized his work in a book entitled "Pathology of Emotions."[18] Curiously, the recording of electrical variations of the skin appeared as a method among others and no reference was given to the Tarchanoff's work. Moreover, since that date, no more work was carried out by Féré on the electrophysiology of the skin and emotion. This could be explained by the fact that Féré had no specific competencies in electrophysiology. In addition, the installation of Janet as official successor of Charcot at La Salpêtrière and the new position of Féré at Bicêtre hospital (1887), certainly contributed to reinforce the clinical interests of Féré. In this context, emotional states were analyzed inside the frame of social consequences of health and electrical variations of skin henceforth considered as having a potential interest for clinical purposes.

Following the Tarchanoff's communication of 1889, in which the Féré's work was not cited, there was no reaction from the scientific community. This communication closes the cycle of the methodological discovery of the EDA and Tarchanoff followed other scientific orientations. Thus, except a communication at the International Congress of Medicine (1894, Roma), related to music influences on skin electrical variations, Tarchanoff pursued known research activities in several topics (*e.g.* « […] hallucinations […] in frogs […] ») until 1900. In particular, since 1896, at Saint Petersburg, his new scientific interest was focused on physiological effects of X-ray, becoming a recognized specialist in this field. Finally, although Tarchanoff initiated a major technical knowledge for the future of the electrodermal recordings, this contribution was just a step in his scientific career.

18. C. Féré, *La pathologie des émotions*, Paris, G. Baillière, 1892.

1. Rebirth of electrical variations of skin… as the Psychogalvanic reflex

The period 1904-1907 open new avenues for the knowledge and applications of electrical variations of the skin. This is due to the contribution of several authors: Mueller, Veraguth, Jung and Peterson. More precisely, the rebirth of the interest for cutaneous electrical signals corresponds to the work of Veraguth about the Psychogalvanic reflex, published in 1909 as "Das Psychogalvanische Reflexphenomen" whereas Féré and Tarchanoff dead in next year.[19] At the same time, the publication of major works on "galvanometer and emotions"[20] initiates a new era for the international career of electrodermal signals as innovative tools for the exploration of emotions and the unconscious.

2. Exploration of electrical variations of skin along the 20th century

The 20th century, through an important contribution of animal and human experimental research, will be a long and rich period in the comprehension of electrodermal signals. The diversity of methods to record electrical skin variations led to a confused way to name these variations. Thus, as previously indicate, the "Psychogalvanic reflex," appeared with Peterson and Jung (1907)[21] and Veraguth (1909)[22] and transformed as "galvanic skin reflex" (GSR), remained in most databases until recently. Meanwhile, and waiting for an international nomenclature, electrical skin variations had been identified as: "skin potential reflex," "neuro-galvanic response," "skin potential response," "skin resistance response," "skin conductance response," "electrodermal response."[23] Finally, in 1981, was adopted the term "Electrodermal Activity"(EDA) as an international standard designation to identify spontaneous and induced electrical skin activity, recorded thanks

19. O. Veraguth, *Das Psychogalvanischen Reflexphenomen*, Berlin, Karger, 1909.

20. F. Peterson, "The galvanometer as a measurer of emotions," *British Medical Journal*, 2, 1907, p. 804-806; F. Peterson and C. G. Jung, "Psychophysical investigations with the galvanometer and pneumograph in normal and insane individuals," *Brain*, 30, 1907, p. 153-218.

21. F. Peterson and C. G. Jung, "Psychophysical investigations with the galvanometer and pneumograph in normal and insane individuals," *op. cit.*

22. O. Veraguth, *op. cit.*

23. C. A. Ruckmick, "Terminology in psychogalvanic reflex," *Psychological Review*, 40, 1933, p. 97-98.

the use of several techniques able to measure potential, resistance, or conductance electrical variations of the skin.[24]

3. Féré's methodological heritage: Skin conductance technique

The Féré's contribution for EDA recordings originates the current exosomatic techniques (skin conductance, SC; skin resistance, SR; skin impedance, SI; skin admittance, SA). These techniques allow to record electric variations of the skin induced by an external voltage applied to the skin surface. Following Ohm's law, a constant voltage (0.5 V to 1 V) applied to the skin allows to record variations in the intensity of the external current which depend on skin resistance. The SC is the most used method for recording electrodermal measures (fig. 1a, 1c): the skin conductance level (SCL), a tonic measure of sympathetic traffic; the skin conductance responses (SCRs), phasic measures of sympathetic discharges following discrete stimulations. Considering several physical principles and advantages and disadvantages of the various recording techniques, the SC became the international recognized method for recording EDA.[25]

4. Tarchanoff's methodological heritage: Skin potential technique

The Tarchanoff's contribution for EDA recordings originates the current skin potential (SP) technique which allows to record potential differences between two skin sites, an active and an inactive ones (fig. 1a, 1b); this is an endosomatic method because, in contrast with exosomatic techniques, does not require an external voltage applied to the skin. The SP allows the acquisition of two measures: skin potential level (SPL) and skin potential responses (SPRs). Successfully recorded in animal exploration of central mechanisms of EDA,[26] SP recordings present several disadvantages linked to amplifier requirements and skin sites properties (electrolytes, skin temperature, etc.). Consequently, despite their technical simplicity, SP recordings are rarely used, excepting in studies with specific methodological purposes.

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24. D. C. Fowles *et al.*, *op. cit.*

25. *Ibid.*; W. Boucsein, *Electrodermal Activity*, New York, Springer, 2012; H. Sequeira, P. Hot, L. Silvert and S. Delplanque, "Electrical autonomic correlates of emotion," *op. cit.*

26. *E.g.* H. Sequeira, S. Ba-M'Hamed and J.-C. Roy, "Fronto-parietal control of electrodermal activity in the cat," *Journal of the Autonomic Nervous System*, 53, 1995, p. 103-114.

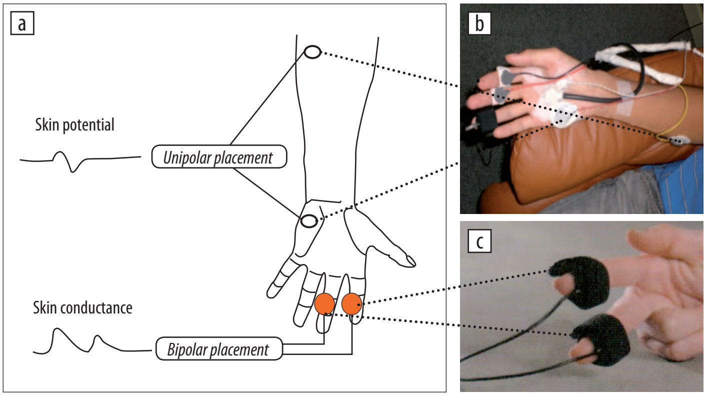

Figure 1. Electrodermal recordings as skin potential (*a*, *b*) and skin conductance (*a*, *c*) techniques, taking origin respectively in Tarchanoff's and Féré's approaches. *a* illustrates unipolar and bipolar placements, respectively for skin potential (SP) and skin conductance (SC) recordings; note also the typical electrodermal responses, recorded by both techniques. In *b* and *c*, broken lines indicate standard positions of electrodes, in real experimental conditions.
Iconographic adaptation from experimental apparatus of Kobayashi, Mandai and Sequeira at Ashikaga Institute of Technology (Japan), 2008.

V. ELECTRODERMAL HERITAGE: ONE HUNDRED YEARS LATER…

Along the last century, EDA became a central physiological tool to explore most psychophysiological topics. Since 1980's, new approaches and disciplines reinforced its interest and enlarged its use in basic and clinical research.

Besides Féré's and Tarchanoff's contributions, the identification and applications of EDA, are a typical result of interdisciplinary exchanges. In fact, in early days of EDA, three main influences can be considered: experimental rulers, initiated in German laboratories; clinical applications, emerging from French positions to deal with mental health and, finally, American psychologists looking for new methods. These influences are also the expression of theoretical or technical information coming from physics, physiology, mental medicine, psychology and even spiritis.[27]

27. P. Deren, *Aux sources d'un marqueur neurophysiologique des émotions : l'activité électrodermale*, Thèse de Doctorat d'Histoire des Sciences et Epistémologie (PhD), 2011, Université de Lille 1, 298 p.

More precisely, EDA became a privileged link between physiology and psychology interrogations, especially oriented to the comprehension of emotional behaviors. This corresponds to the need to explain normal emotional processes in normal individuals and the role of EDA as a potential indicator of emotional vulnerability in mental disorders.[28]

In more recent years, appeared new trends aiming to clarify the EDA's as a potential marker of cognitive and affective processes in different subfields of Neurosciences. The first important point is related to the discovery of neural mechanisms subtending EDA central control. In this frame, animal research developed at the University of Lille represents a strong and significant input revealing main neural structures implicated in the excitatory and inhibitory command of electrodermal responses.[29] Secondly, many studies tried to establish the capacity of EDA to reveal the impact of arousal and valence dimensions of emotional stimuli; several researches confirm the fact that EDA is a robust index of emotional arousal[30] whereas fails to discriminate negative from positive valence. Finally, other works used the EDA as a promising marker of cognitive or emotional processes such as decision making,[31] anticipatory behaviors,[32] subliminal

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28. Y. Miossec, M. C. Catteau, E. Freixa i Baqué and H. Sequeira-Martinho, "Activité électrodermale bilatérale et asymétrie hémisphérique chez les malades psychiatriques," *Psychologie Médicale*, 18, 1986, p. 1227-1230.

29. V. Bloch, "Le contrôle central de l'activité électrodermale," *Journal de Physiologie*, 57, 1965, supplément 13, p. 1-132; J.-C. Roy, H. Sequeira and D. Delerm, "Electrodermal activity: spinal and reticular mechanisms," in J.-C. Roy *et al.*, *Progress on Electrodermal Activity*, New York, Plenum Press, 1993, p. 73-92; H. Sequeira, S. Ba-M'Hamed and J.-C. Roy, "Fronto-parietal control of electrodermal activity in the cat," *op. cit.*

30. P. J. Lang, M. K. Greenwald, M. M. Bradley and A. O. Hamm, "Looking at pictures: Affective, facial, visceral and behavioural reactions," *op. cit.*; H. Sequeira *et al.*, *op. cit.*; F. D'Hondt, M. Lassonde, O. Collignon, A. S. Dubarry, A. M. Robert, S. Rigoulot, J. Honoré, F. Lepore and H. Sequeira, "Early brain-body impact of emotional arousal," *Frontiers in Human Neuroscience*, 4, 2010, p. 1-10; V. Kosonogov, L. De Zorzi, J. Honoré, E. S. Martínez-Velázquez, L. Delbarre, J.-L. Nandrino, J. M. Martinez-Selva and H. Sequeira, "Facial thermal variations: a new marker of emotional arousal?" submitted for publication, 2016.

31. A. R. Damasio, D. Tranel and H. Damasio, "Individuals with sociopathic behavior caused by frontal damage fail to respond autonomically to social stimuli," *Behavioral and Brain Research*, 41, 1990, p. 81-94.

32. C. Amiez, E. Procyk, J. Honoré, H. Sequeira and J.-P. Joseph, "Reward anticipation, cognition, and electrodermal activity in the conditioned monkey," *Experimental Brain Research*, 149, 2003, p. 267-275.

stimulations,[33] reasoning,[34] cognitive loading,[35] psychosomatic interactions,[36] or sleep exploration.[37]

In conclusion, Féré and Tarchanoff, two discoverers with two different life styles and two methodological and technical approaches, transmitted us a common scientific heritage, the EDA, still alive one hundred years later and promised to an expanding future in Neuropsychology, Psychopathology, Neurology, Chronobiology, Criminology and in Cognitive and Affective Neuroscience. Last but not least, Féré and Tarchanoff represented the early European ideal for the free transfer of knowledge between individuals, institutions and cultures.

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33. L. Silvert, S. Delplanque, H. Bouwalerh, C. Verpoort and H. Sequeira, "Autonomic responding to aversive words without conscious valence discrimination," *International Journal of Psychophysiology*, 53, 2004, p. 135-145.

34. L. Carbonnel, F. Vidal, H. Sequeira and J.-P. Caverni, "A reasoning bias revealed by electrodermal activity," *Psychophysiology*, 43, 2006, p. 387-393.

35. E. Salvia, A. Guillot and C. Collet, "Autonomic nervous system correlates to readiness state and negative outcome during visual discrimination tasks," *International Journal of Psychophysiology*, 84, 2012, p. 211-218.

36. D'Hondt *et al.*, "Early brain-body impact of emotional arousal," *op. cit.*

37. J. Delannoy, O. Mandai, J. Honoré, T. Kobayashi and H. Sequeira, "Diurnal emotional states impact the sleep course," *Plos One*, 2015, *in press.*

VI

Nikolai Timofeev-Resovsky on genes, radiation and society: Russian-French cooperation

Tatiana A. Kursanova

Nikolai Timofeev-Resovsky (1900-1981) is known as one of the founders of radiation genetics, experimental population genetics and microevolution. His works were of special importance to Soviet biology because they were in direct opposition to "Lysenkoism" and confronted the damage it had done. His life passed all the historical events of the twentieth century: the social changes in Russia, the Civil War and the World War II, fascism and the Resistance movement in Europe, the Stalinian camps. He began his higher education in 1916 at the Moscow City University of the People, named after A. Shanyavsky. From 1917 to 1922, he studied at the Moscow State University. From 1920 to 1925, he did research at the Institute of Experimental Biology under the directorship of Nikolai Koltsov. Timofeev-Resovsky started his genetic experiments involving Drosophila in the department headed by Sergei Chetvericov, working on population genetics.

During the years 1923-1925, Oskar Vogt, an eminent neurophysiologist was visiting Moscow. He was director of the Kaiser Wilhelm Institute for Brain Research in Berlin (KWH). In part, his visit was aiming to recruit researchers for his Institute, especially in the relatively new field of genetics. The KWH was involved in establishing a laboratory for brain research in Moscow, which it did: it was the Institute of the Brain (Institut Mozga). Conversely, the Soviet recommended Timofeev-Resovsky to Vogt. So in the summer of 1925, Timofeev-Resovsky left Russia for Germany for an unspecified period of time, which lasted twenty years. In 1929, he became Director of the Department of Experimental Genetics of KWH. The research conducted in the genetic department, along with the permanent seminar on genetics, organized by Timofeev-Resovsky, attracted many young postdoctoral researchers, who would ultimately become prominent scientists, and the department gained the status of an institute. It collaborated with eminent scientists including Max Delbrück, Niels Bohr, Theodosius Dobzhansky, Boris Ephrussi, Frédéric Joliot-Curie, etc. In 1935, Timofeev-Resovsky published the major work *Über die Natur der Genmutation und der Genstruktur* with Max Delbrück and Karl Zimmer; it was considered to be a major advance in understanding the nature of gene mutation and gene structure. The work was a keystone in the formation of molecular genetics.[1]

In collaboration with H. Muller, Timofeev-Resovsky became a co-founder of radiation genetics. Before he departed from Russia in 1920, he began to study induced and spontaneous mutation process and observed X-irradiate Drosophilae under Nikolai Koltsov's supervision. Later, in Germany his experiments with reverse mutations were the first to employ X-ray as a tool for research, aiming to shed light on genes structure. These experiments aroused a vast scientific interest and brought him worldwide fame. In 1932, Timofeev-Resovsky made a plenary lecture report on this topic at the 6th International Congress of Genetics in Ithaca. Meantime in 1932, neutrons were discovered and in 1936 their properties were described. Following the interest in analysis of mutations, in 1936, Timofeev-Resovsky decided to bombard flies with neutrons. He turned to the bureau of the Rockefeller Foundation in Paris, in order to find out if the Foundation was supporting the studies on neutron physics in any European centres and if it was possible during two days to use a source of neutrons to irradiate flies. The foundation representative, H. M. Miller, agreed to clarify whether the laboratory facilities of Frédéric Joliot-Curie were suitable for this purpose.[2] Joliot-Curie persuaded the Society of Physical Chemists to invite Timofeev-Resovsky to give lectures in Paris in the autumn of 1937. Timofeev-Resovsky found the funding for the trip and left for Paris with his wife. With Joliot-Curie, he learned to work with the cascade generator of fast neutrons to induce mutations. In Paris, he published several works in French, one of the most famous being "Le Mécanisme des mutations et la structure du gene."[3]

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1. V. V. Babkov and E. S. Sakanian, *Nikolai Vladimirovich Timofeev-Resovsky*, Moscow, Pamyatniki istoricheskoi mysli, 2002.

2. *Ibid.*, p. 131.

3. N. V. Timofeev-Resovsky, "Le mécanisme des mutations et la structure du gène" [The mechanism of mutations and the structure of the gene], *Congrès du Palais de la Découverte*, Paris, 1937, p. 495-516.

In 1939, this work was slightly revised and was published as a separate brochure.[4] On his return to Germany, being impressed by the works of Joliot, Timofeev-Resovsky succeeded in setting up a small cascade generator of fast neutrons of 300 000 V in Berlin-Buch, which was later used by Timofeev-Resovsky together with other scientists in their pioneer experiments.

Timofeev-Resovsky was conducting his research on a multisubject basis, resting on two sciences – physics and biology. He attended the seminars of Niels Bohr in the Carlsberg Institute in Copenhagen. There, in cooperation with H. Muller he made a report on his view about the nature of gene mutation. In the 1930s, under the influence of Niels Bohr's reports, physicists became interested in biology. This topic was not new to Timofeev-Resovsky, since his teacher and famous Russian zoologist, Nikolai Koltsov, was experimenting on physical-chemical processes occurring in living cells at the beginning of 20th century. In particular, since 1916 he had been developing the hypothetical understanding of the physical-chemical structure of chromosomes, which contain genes with all the genetic information of the organism. By the 1930s, his theoretical works on the genes, from the physical-chemical point of view, were widely recognized.

French geneticist Boris Ephrussi (1901-1979), professor of genetics at the university of Paris, was born in Kishinev in Russia. Before his departure to France, Boris Ephrussi was a student of Nikolai Koltsov at the A. Shanyavsky University. There, he met Timofeev-Resovsky. Ephrussi started his scientific training as a Russian émigré in Paris in 1920. His research helped to transform mammalian, and especially human, genetics. He studied the initiation and regulation of embryological processes by intracellular and extracellular factors. A major trend of his early research concerned the effect of temperature on the development of fertilized sea urchin eggs. Ephrussi's works involved tissue cultures. From his studies on the brachyury mutation in mice, he reach the conclusion that intrinsic factors (*i.e.* genes) play a key role in development. In the next part of his career, Ephrussi added his embryological concerns with a firm conviction that one must understand the role of genes in order to decipher the embryological processes. He moved to Caltech in 1934 and stayed there until 1935 to learn genetics within the intellectual empire of T. H. Morgan. The Rockefeller Foundation supported this move. During this period

4. *Id.*, *Le mécanisme des mutations et la structure du gene*, Paris, Hermann, 1939.

he conducted an important research with George Beadle, who joined him in Paris in the autumn of 1935. From this research on Drosophila eye transplants, he developed the "one gene, one enzyme" hypothesis. During World War II, Ephrussi spent most of his time as a refugee at the Johns Hopkins University. Afterwards he started to work in France on cytoplasmotic genetics. He began to work at the *Institut de Biologie Physicochimique* (the Rothschild Institute) in Paris, and later worked at the CNRS in Gif-sur-Yvette, where he studied the contribution of cytoplasm to the cell phenotype. Timofeev-Resovsky took him to the seminar of Niels Bohr. It was then, according to Timofeev-Resovsky's memoirs, that they had the idea, supported by Niels Bohr, to plan a similar seminar. Ephrussi suggested that the meeting took place in expensive resorts, off-season, when best hotel rooms were not busy and cheap. The symposia would be held in small towns of Western Europe, in autumn and spring, and would last three to five days. Since scientists from all countries were not wealthy, the organizers had to ask financial support at the European representative office of the Rockefeller Foundation in Paris in 1937. The Fund agreed to support the project. Two conferences were held in 1938, but the war made further events impossible. Roughly 18 scientists attended those seminars from many European countries: physicists, physical chemists and biologists. Physicists Pierre Auger, François Perrin, biochemist Louis Rapkine and, certainly, Boris Ephrussi, so-called "French Russian," according to Timofeev-Resovsky, represented France.[5]

Louis Rapkine (1904-1948) (Legion of Honour, 1947) was also Russian-born. His parents moved to Paris escaping from the Kiev pogrom (1905). He was a French biologist, specializing in embryology and enzymology, mostly known for his efforts in saving and restoring the French scientific community during World War II. He worked at the Collège de France, at the Département de biophysique. He became famous for his research on enzyme in cooperation with P. Tripinac (1938). He established the Comité français pour l'accueil et l'organisation du travail des savants étrangers (French committee for the reception and the organisation of the work of foreign scientist) and he helped thirty scientists to emigrate secretly from France (1940). Rapkine's Fund for French scientists was established and given his name (1951), and helped to purchase tools and materials for scientific use. The Fund lived on under the name of the Pasteur

5. *Id.*, *Istorii, rasskazannye im samim*, Moscow, Soglacie, 2000, p. 231.

Foundation (1985) in New York City (a part of the Pasteur Institute network).[6] Jacques Monod (1910-1976), who was awarded the Nobel Prize in physiology or medicine in 1965, with A. Lwoff and F. Jacob, for their discoveries concerning the genetic control of enzyme and virus synthesis, owed Ephrussi the discovery of physiological genetics and Louis Rapkine the concept that only chemical and molecular descriptions could provide a complete interpretation of the function of living organisms. Monod said:

> Influenced by my friendship with L. Rapkine and my admiration for him, whom I visited frequently and at length in his laboratory, I had been tempted, even though I was poorly prepared, to study elementary biochemical mechanisms, that is enzymology. But under the influence of another friend whom I admired, Boris Ephrussi, I was equally tempted by genetics.[7]

According to the point of view of historians of biology, the new ideas of Morgan and his followers met with criticism at the end of the 1920s in France. The very role of chromosomes and inheritance was doubted, and the chromosome theory was still viewed by many French biologists as an artificial structure, which ignored the physiological side of inheritance. Therefore, French biology was closer to a bio-chemical orientation in genetic studies than to a cytological one. After World War II, the French school of biological genetics reached the first place in the world, owing mainly to the efforts of Ephrussi, as well as to the works of A. Lwoff, F. Jacob and Jacques Monod.[8] The main characteristic of the French school was the consideration of the cell as a biological system, where nuclear and cytoplasmic reactions were studied as functionally conjugated processes. Ephrussi, who shared the views of Timofeev-Resovsky, described the essence of this device as follows: "The phrase 'Gene is a base of life' is just a revolutionary slogan, which should not shade the concept of cell as a whole."[9]

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6. D. T. Zallen, "Louis Rapkine and the Restoration of French Science after the Second World War," *French Historical Studies*, 17, 1, 1991, p. 6-37. Also see Louis Rapkine, in *Service des Archives de l'Institut Pasteur*.

7. J. Monod, "From enzymatic adaptation to allosteric transition," in *Nobel lectures, Physiology or Medecine, 1963-1970*, Amsterdam, Elsevier, 1972, p. 190.

8. E. B. Muzrukova, *T. H. Morgan i Genetika*, Moscow, Graal, 2002, p. 208.

9. N. Roll-Hansen, "Drosophila Genetics: A Reductionist Research Program," *Journal of the History of Biology*, 11, 1, 1978, p. 159-210.

The end of World War II and the following entrance of the Soviet troops in Berlin caused safety problems for Timofeev-Resovsky both from German, since he was Russian-born, and from the Soviet side since he stayed in Berlin, worked there during the war and was not going to come back to Russia. The Brain Institute was evacuated from Berlin-Buch to the western zone, to Gettingen. The only part that stayed was the department of bio-physics and genetics of Timofeev-Resovsky. The friends of Timofeev-Resovsky were preparing a working place for him in America and Great Britain. Joliot-Curie was waiting for him in Paris. His friends were supporting the idea of his departure including his pre-war colleagues from the seminars of physics, those from the Collège de France, those from the Paris University and François Perrin and Pierre Auger.[10] Nevertheless, Timofeev-Resovsky postponed his departure arguing that the neutron generator was complex to disassemble, and hoping that it would be easy to make terms with Russian soldiers.

After the fall of Berlin, the Russians arrested Timofeev-Resovsky. However, Colonel General A. Zavenyagin recognized that Timofeev-Resovsky's experience in radiobiology and genetic effects of radiation would be useful to the Soviet atomic bomb project and ordered his release. Timofeev-Resovsky became director of the KWIH facility in Berlin-Buch and was visited by Zavenyagin, and also by Igor Kurchatov, chief scientist of the Soviet atomic bomb project. However, after being denounced by a visiting scientist in an Academy of Sciences delegation from Moscow, Timofeev-Resovsky was secretly re-arrested on 14th September 1945 by another element of the NKVD (different from that under Zavenyagin). Timofeev-Resovsky was sent back to Russia; he got a ten-year sentence. On his way to the prison camp in Karaganda in northern Kazakhstan, one of the most terrible camps in the GULAG, Timofeev-Resovsky went through the Butyrskaya prison, the central transit prison in Moscow. He felt he was slowly dying without any hope to be released. But Joliot-Curie came to save him.

Frédéric Joliot-Curie, a Nobel Laureate in Chemistry (1935) for his research on artificial radiation and synthesis of artificial isotopes, a leader in the French Resistance during the war, Director of the French National Center for Scientific Research and the first French High Commissioner for Atomic Energy, was very thought of in Russia. He visited Moscow in 1947 and pleaded with Lavrenty Beria, the head

10. V. V. Babkov and E. S. Sakanian, *op. cit.*, p. 228.

of the NKVD, that Timofeev-Resovsky be found, released and given serious work. The argument did not fall on deaf ears, as Beria was also in charge of the Soviet atomic bomb project, which was a top priority for Stalin. Eventually, Timofeev-Resovsky was found, his health problems caused by his incarceration in the GULAG treated, and was sent in 1947 to work at the secret Laboratory B in Sungul, in South Ural, where he went on studying defence on radiation.[11]

11. *Ibid.*, p. 355.

VII

La France face à la soviétisation de la science roumaine (1945-1949)

Cristiana Oghina-Pavie

Entre la fin de la deuxième guerre mondiale et le début de la Guerre froide, la Roumanie parcourt un processus rapide et profond de soviétisation. Comme tous les autres domaines de la vie intellectuelle, la science roumaine subit des influences, pressions, épurations et persécutions qui reflètent autant les changements institutionnels internes que la confrontation internationale entre les deux blocs antagonistes. Alliée avec l'Allemagne pendant la guerre, la Roumanie retourne les armes le 23 août 1944. Libéré et occupé par l'Armée rouge, le pays est dirigé jusqu'en mars 1945 par un gouvernement de coalition démocratique, ensuite par le gouvernement Petru Groza, représentant une union de gauche sans appui populaire mais bénéficiant du soutien de l'URSS. L'abdication du roi Michel et la proclamation de la République Populaire de Roumanie le 30 décembre 1947 ouvrent la voie de l'installation effective du régime communiste dans le courant de l'année 1948.

Pendant ces années cruciales (1945-1949), la Roumanie est un terrain de rivalité, puis de confrontation entre la France et l'URSS. Similaire, dans ses grandes étapes, aux processus de soviétisation des autres pays de l'Est, le cas roumain présente une particularité significative. Depuis le XIX^{e} siècle, l'influence française est non seulement présente dans tous les domaines de la vie intellectuelle, mais constitutive de l'identité nationale. Pour reconquérir ses positions à l'Est, la France mise sur une diplomatie culturelle active, qui devient indésirable pour les autorités communistes dès 1947. Pour étudier la place qu'occupent la science et les scientifiques roumains dans la rivalité culturelle entre la France et l'URSS, nous aborderons plus particulièrement l'exemple des sciences de la vie : biologie, agronomie et médecine. Cet exemple est intéressant à plus d'un titre. Les principaux acteurs de la soviétisation de la science en Roumanie sont précisément des médecins et des biologistes. Ils sont les initiateurs de la propagande prosoviétique, occupent des fonctions étatiques importantes et sont les artisans de la nouvelle Académie et de l'introduction du lyssenkisme. En revanche, les relations avec la France, échappant progressivement au contrôle diplomatique, sont poursuivies par les contacts directs entre les partis communistes et ne retrouvent leur véritable portée, sur un plan épistémologique, que dans la période d'abandon progressif du lyssenkisme.

I. LA SCIENCE ET L'INFLUENCE CULTURELLE (1945-1947)

Dès la Libération, la France appuie sa diplomatie culturelle en Roumanie sur l'Institut français des hautes études, créé à Bucarest en 1924, pièce maîtresse de la mission universitaire chargée d'organiser des cours de langue et des conférences, d'animer des bibliothèques dans les principales villes du pays. L'activité de l'Institut, qui n'a pas cessé pendant la guerre et l'occupation allemande, retrouve un nouveau souffle dès novembre 1944. Les conditions sont qualifiées par les représentants français d'« éminemment favorables » à la France[1]. Les accords culturels avec l'Allemagne et l'Italie sont naturellement dénoncés, l'influence anglaise et américaine est minime et le nouveau gouvernement roumain accepte toutes les exonérations et facilités pour l'importation des produits culturels français. Par ailleurs, les occupants soviétiques de la Roumanie sont favorables à la France, dans le contexte de la signature du traité entre la France et l'URSS en décembre 1944. L'Institut retrouve sa fonction d'avant la guerre, celle du « rayonnement » de la culture française, notamment littéraire et artistique.

En octobre-novembre 1944 est créée une Association pour le resserrement des liens avec l'Union soviétique (ARLUS) qui se propose de faire connaître la littérature, la musique et la science soviétiques, par l'intermédiaire d'une revue hebdomadaire, *Veac nou*, de bibliothèques et de cercles d'études. La réunion de constitution se tient à l'université de Bucarest à l'initiative de Constantin I. Parhon (1874-1969) qui en devient le président. Les vingt membres fondateurs sont tous des intellectuels et la plupart sont des professeurs des universités.

1. G. Bowd, *La France et la Roumanie communiste*, Paris, L'Harmattan, 2008, p. 41 *sq.*

Après une rencontre avec le vice-commissaire soviétique A. I. Vychinski, ARLUS rend publique son organigramme. Un mois seulement après sa constitution, l'association comprend douze sections parmi lesquelles se trouvent les sciences et les sciences appliquées[2].

La place occupée par les médecins et biologistes dans les fonctions de responsabilité d'ARLUS est particulièrement remarquable. Le président, C. I. Parhon, professeur de neurologie et psychiatrie, spécialisé en endocrinologie, est membre de l'Académie roumaine depuis 1939, fondateur de plusieurs sociétés savantes de médecine et biologie en Roumanie. Militant de gauche depuis l'entre-deux guerres et pacifiste, il fait partie de ceux qui agissent sans doute par conviction. Deux des vice-présidents sont médecins : Daniel Danielopolu (1884-1955), secrétaire perpétuel de l'Académie roumaine de médecine, ministre de la Santé (décembre 1944-mars 1945) et son successeur au poste ministériel, Dimitrie Bagdasar (1893-1946), neurochirurgien, ayant effectué sa spécialisation à Boston, communiste de longue date. Moins connu sur le plan scientifique, le secrétaire général, Simion Oeriu (1902-1976), biochimiste, devient dès 1945 sous-secrétaire d'État et commissaire général dans la commission de l'armistice. Le président de la sous-section enseignement supérieur d'ARLUS est un sympathisant socialiste, Constantin Motas (1891-1980), zoologiste, docteur de l'université de Grenoble, tandis que le président de la section de sciences appliquées est Traian Savulescu (1889-1963), professeur de physiopathologie végétale à l'Institut agronomique de Bucarest, sans activité politique connue avant 1944. Communistes, socialistes, pacifistes, anti-fascistes ou simplement opportunistes, les premiers adhérents d'ARLUS font tous partie du cercle restreint des proches de C. I. Parhon.

Avec l'aide de la Société pan-soviétique pour les relations culturelles de l'URSS avec l'étranger (voks) et des autorités militaires d'occupation en Roumanie, ARLUS déploie en quelques mois une structure territoriale pyramidale, formée de cercles dans toutes les villes, entreprises et institutions déclarant un an plus tard 140 000 adhérents. Les objectifs plutôt culturels du début se transforment rapidement en une activité

2. Économie, science, littérature et philosophie, sciences appliquées, sciences sociales, armée, transport et télécommunications, enseignement, presse, propagande, art et sport-tourisme. ARLUS, *Activitatea în cifre si imagini*, Bucuresti, Imprimeriile Independenta, 1945 ; Adrian Cioroianu, *Pe umerii lui Marx. O introducere in istoria comunismului românesc*, Bucuresti, Editia a II-a, 2007, p. 107-148.

de propagande intense, dans tous les domaines, y compris scientifiques. L'ARLUS organise le voyage des scientifiques roumains à Moscou, où ils rencontrent Staline, Lyssenko, Lepechinskaïa, Oparine et d'autres scientifiques soviétiques[3]. La revue *Veac nou* comporte des articles sur les thèmes récurrents de la propagande scientifique : les réalisations de la science soviétique dans chaque discipline, le niveau culturel des masses ouvrières, la transformation socialiste de l'agriculture, les libertés et les moyens dont disposent les scientifiques en URSS, etc.

Devant l'ampleur des actions d'ARLUS, la légation de France à Bucarest souhaite faire de l'influence culturelle française un rempart contre l'avancée soviétique en Roumanie :

> On ne peut pas affirmer qu'après la fin de l'oppression allemande la Roumanie ait retrouvé sa pleine liberté. Elle vit maintenant sous une sorte de tutelle et c'est plus que jamais pour elle une occasion de tourner les yeux vers notre pays. En regardant vers nous, elle pense échapper, au moins moralement, aux barrières dont elle se sent entourée[4].

Les milieux universitaires en sont la cible naturelle. En 1946, le nouveau directeur de l'Institut français, Philippe Rebeyrol, en visite à l'université de Iasi, est reçu par les facultés réunies devant lesquelles le recteur rappelle que « la Sorbonne était avant la guerre la seconde des universités roumaines, par le nombre des étudiants originaires de ce pays[5] ». Il insiste auprès de la légation de France à Bucarest

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3. Mitita Constantinescu et Traian Savulescu, *Moscova vazuta de Mihail Sadoveanu*, Bucuresti, Cartea Rusa, 1945 ; D[r] Mihai Mihailide, « Savant şi ostaş devotat al partidului » (2), *Viata medicala*, 51-51, 6 mars 2013, [http://www.viata-medicala.ro/*articleID_6555-dArt.html] (consulté le 25/04/2016).

4. Archives du ministère des Affaires étrangères de France, Centre des Archives diplomatiques de Nantes (CADN), Bucarest Ambassade, 343, Rapport sur l'activité de l'Institut français et de la Mission universitaire française en Roumanie pendant l'année universitaire 1944-1945, le 8 avril 1945.

5. Les professeurs de l'université de Iasi remettent au directeur de l'Institut français un « Appel à la France ». Ils rappellent que plus de 80 % des universitaires de Iasi ont fait leurs études en France et qu'avant la guerre plus de la moitié des livres achetés en Roumanie étaient des livres français. Craignant un déclin de l'influence française, les professeurs demandent « de mettre à notre portée les outils de la connaissance, les livres et les revues. La vie spirituelle que nous y trouverons rallumera le flambeau éteint. Sa lumière et sa chaleur sont nécessaires à la jeunesse. Il y a là une grande œuvre de salut public à entreprendre. Elle serait digne de la France » (CADN, Bucarest Ambassade, 343, le directeur de l'Institut français des hautes études en Roumanie au ministre de France, le 4 décembre 1947).

pour réactiver le système de bourses du gouvernement français pour les roumains désireux d'étudier en France et pour faciliter la venue des livres français. Cependant, le ministre de France en Roumanie, Jean Paul-Boncour, recommande aux services culturels d'agir avec discrétion et prudence pour ménager la susceptibilité soviétique et ne pas faire une concurrence ouverte aux manifestations culturelles de l'ARLUS[6].

Rebeyrol envisage également de procéder à une série de mesures destinées à adapter le fonctionnement de l'institut à la nouvelle situation de la Roumanie. Il s'agit premièrement d'accorder une place nouvelle aux sciences et aux techniques car, traditionnellement cantonné aux disciplines humanistes, l'Institut doit corriger son image élitiste et toucher des catégories sociales plus larges, y compris les étudiants et les milieux populaires[7]. Cette mission est confiée à une jeune agrégée de physique et docteur ès sciences, Marguerite Cordier, nommée à Bucarest en 1946. La Faculté des sciences de Bucarest lui propose d'utiliser les laboratoires universitaires pour ses propres recherches. Elle donne des cours réguliers pour la préparation du certificat français d'études supérieures de sciences et, surtout, des conférences à Bucarest et en province. Deuxièmement, Rebeyrol saisit la nécessité d'inviter en Roumanie des intellectuels et scientifiques qui bénéficient de la confiance des nouveaux dirigeants : « de grands savants, artistes et écrivains français appartenant aux partis de gauche[8] ». Ainsi, il accueille en Roumanie Henri Wallon et Ernest Labrousse en 1947 et organise des cérémonies commémoratives pour Jean Perrin et Paul Langevin auxquelles participent le chef du gouvernement roumain, plusieurs ministres et des académiciens. Cordier donne à Bucarest et dans toutes les villes importantes des conférences sur Paul Langevin et sur l'organisation de la science française et anime à l'Institut un cercle

6. « Tout en prenant de plus en plus conscience de l'échec des efforts de rapprochement intellectuel et moral roumano-soviétique, entrepris, il y a un an, par l'association ARLUS, Moscou peut en effet nourrir encore quelque projet grandiose avec lequel notre groupe universitaire semblerait rivaliser par avance. » (CADN Bucarest Ambassade, 343, le représentant politique français en Roumanie au ministre des Affaires étrangères, le 4 décembre 1945.)

7. A. Godin, *Une passion roumaine. Histoire de l'Institut Français de Hautes Études en Roumanie (1924-1948)*, Paris, Éditions l'Harmattan, 2000, p. 166-167.

8. L'idée est émise lors d'un entretien de Rebeyrol avec Ana Pauker, ministre des Affaires étrangères, Lucretiu Patrascanu, ministre de la Justice et Simon Stoilov, ministre de Roumanie en France. Ce dernier, mathématicien, avait obtenu son doctorat à Paris et enseigné aux universités de Iasi et de Bucarest. Il a combattu dans la Résistance française.

d'études techniques et scientifiques et un cercle d'études médicales. Les représentants français voient la science comme un domaine plus neutre d'un point de vue idéologique et misent sur la réceptivité des universitaires, notamment des médecins de l'Institut de sérums et des vaccins Cantacuzino qui a entretenu d'étroites relations avec l'Institut Pasteur depuis sa fondation en 1921[9].

II. QUAND LE RIDEAU TOMBE (1948)

Le directeur de l'Institut français espère pouvoir conserver des relations cordiales avec l'Institut roumano-soviétique, organisme d'enseignement et de recherche créé en 1947 qui prolonge l'action de propagande d'ARLUS, grâce notamment à la position dirigeante accordée à Stefan S. Nicolau (1896-1967), microbiologiste ayant travaillé à l'Institut Pasteur entre 1920 et 1939 et qui est lié à la France « par une amitié fidèle et dévouée, malgré sa grande prudence politique ». C'est sans compter sur les répercussions immédiates et radicales de la Guerre froide. Le tournant de politique extérieure de la France et la création du Kominform[10] mais aussi la situation interne de la France (l'éloignement des communistes du gouvernement, l'accusation d'espionnage portée à des étudiants roumains, le soutien accordé par le gouvernement français à l'émigration roumaine anti-communiste) ouvrent une période d'opposition ferme à la présence française en Roumanie, dans un climat interne de terreur policière et de radicalisation du régime communiste[11].

L'Institut français des hautes études et les bibliothèques françaises sont obligés, d'abord par prudence, de retirer des livres de ses bibliothèques, la Légation de France rencontre des difficultés à obtenir des visas pour les conférenciers invités. Ce sont les relations directes entre le Parti Communiste Français et le Parti Ouvrier Roumain (PMR) qui permettent d'entretenir en Roumanie une présence culturelle française

9. Fondé par Ioan Cantacuzino (1863-1934), ancien assistant de Metchnikov à l'Institut Pasteur, docteur *honoris causa* de l'université de Lyon (1923).

10. G. H. Soutou, « La perception de la menace soviétique par les décideurs de l'Europe occidentale : le cas de la France », in S. Dockrill, R. Frank, G. H. Soutou, *L'Europe de l'Est et de l'Ouest dans la guerre froide, 1948-1953*, Paris, Presses de l'université de Paris-Sorbonne, 2002, p. 22-23.

11. D. Sandru, *Comunizarea societatii romanesti in anii 1944-1947*, Bucuresti, Editura Enciclopedica, 2007, p. 243-256.

de tendance communiste[12]. Dans le domaine scientifique, deux visites marquent l'année 1948 : celle du biologiste Marcel Prenant, en février, et celle du médecin André Boivin en avril. La première est organisée non pas par les représentants officiels de la France en Roumanie, mais par l'Institut roumain de culture universelle et présente un caractère politique marqué. L'université de Bucarest accorde à Prenant le titre de docteur *honoris causa*, probablement sous la pression des autorités communistes[13] et des articles dans tous les journaux centraux relatent largement les quatre conférences qu'il donne à Bucarest[14]. Quant à André Boivin, qui avait été professeur de chimie médicale à la faculté de médecine et chercheur à l'institut de bactériologie de Bucarest entre 1930 et 1936, il est reçu par ses anciens élèves ou collègues, devenus de hauts dignitaires. Autant Prenant que Boivin acceptent de relayer auprès des autorités roumaines le souhait de la France de continuer à entretenir des relations avec les intellectuels roumains, notamment dans les domaines scientifiques et techniques. Cependant, les mois suivants sont ceux de la rupture : fermeture de l'école primaire et du Lycée français de Bucarest, fermeture de l'Institut français des hautes études et expulsion de son personnel, fermeture des antennes d'enseignement du français par des lecteurs et des bibliothèques françaises et enfin, en novembre 1948, dénonciation de l'accord culturel conclu en 1939 entre la France et la Roumanie, tandis que toute la presse roumaine entreprend une campagne commandée contre « l'asservissement de la culture roumaine à l'impérialisme français ». Les moyens d'action officiels de la France sont désormais extrêmement limités.

La soviétisation de la science est institutionnalisée par la dissolution de l'Académie roumaine et la création de l'Académie de la République Populaire de Roumanie, en juillet 1948. Les instruments principaux

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12. G. Bowd, *op. cit.*, p. 77.

13. « Je tiens du doyen de la Faculté des Sciences que le Conseil de la Faculté n'a pas accepté d'abord de conférer ce doctorat à M. Prenant, arguant que d'autres savants français méritaient avant lui de recevoir cette distinction. Une nouvelle délibération fut nécessaire, à la suite d'un ordre étonnant du parti communiste […]. » (CADN, Bucarest Ambassade, 346, dossier « Conférences en Roumanie du professeur Prenant », directeur de l'Institut français de Bucarest au Ministre des affaires étrangères, février 1948.)

14. Elles concernent l'histochimie, les « théories nouvelles sur l'évolution », la vie de Louis Pasteur et la Résistance française. Prononcée devant des auditoires de plus de 1 000 personnes, cette dernière conférence est une apologie du PCF et une attaque ouverte à l'adresse du gouvernement français. En outre, M. Prenant participe au congrès du PMR.

de cette transformation sont les activistes d'ARLUS déjà présents dans les rangs de l'Académie et occupant des fonctions importantes dans l'État communiste. Il s'agit principalement de C. I. Parhon, président du Présidium de la Grande Assemblée nationale (fonction la plus élevée dans l'État) et de Traian Savulescu, coordinateur des ministères de l'Agriculture et de la Sylviculture et deuxième vice-président du conseil des ministres. Ils conçoivent et imposent le projet de la nouvelle Académie en tant qu'institution de l'État, fondée par décret en juillet 1948. Comme dans les autres pays de l'Est, l'Académie RPR[15] est constituée sur le modèle soviétique comme une instance suprême de la planification de la science, dotée d'un réseau d'instituts de recherche et de stations expérimentales. En tant que nouveau président, Savulescu procède à l'épuration de l'Académie et fixe son programme scientifique. Les vagues d'épuration de l'Académie et des instituts de recherche ne prennent pas en compte le lieu de formation ou bien les liens privilégiés avec un pays occidental. Les biologistes et les médecins exclus, mutés dans une autre institution ou, au contraire, nouvellement nommés, sont indifféremment de formation française, allemande ou américaine. Les évictions concernent les personnalités scientifiques qui ont détenu des postes ministériels avant 1945 et ceux qui ont eu une activité politique[16]. Sont touchées beaucoup plus lourdement les sections littéraires et historiques de l'Académie, ainsi que le personnel enseignant des universités à cause de l'influence possible sur la population estudiantine.

Présenté à l'ouverture de la première session générale, en octobre 1948[17], le programme scientifique de l'Académie RPR est une critique virulente de la « soi-disant liberté académique » de l'ancienne

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15. N. Krementsov, « Lysenkoism in Europe. Export-Import of the Soviet Model », in Michael David-Fox et Gyorgy Peteri (éd.), *Academia Upheaval : Origins, Transfers and Transformations of the Communist Academic Regime in Russia and East-Central Europe*, New York, Garland Publishing Group, p. 179-202.

16. Ainsi, Constantin Motas, membre fondateur d'ARLUS, arrêté en mai 1949 pour avoir été proche du leader du Parti Social-Démocrate (C. Titel Petrescu) est condamné à 20 ans de prison et exclu de l'Académie. Il sera libéré en 1956 et nommé immédiatement directeur de l'Institut de spéléologie. Selon le témoignage de Ştefan Negrea, son élève et collaborateur, la libération de C. Motas eu lieu sous la pression de plusieurs hydrobiologistes européens (Ştefan Negrea, « Historical development of biospeleology in Romania after the death of Emile Racovitza », *Travaux de l'Institut de Spéléologie Emile Racovitza*, Bucarest, xlv-xlvi, 2006-2007, p. 131-167).

17. *Stiinta, literatura, arta si slujitorii lor in Republica Populara Român*a, Monitorul official, 1948.

institution et une apologie de la science utile, organisée, basée sur le marxisme-léninisme et planifiée selon les objectifs économiques et sociaux du gouvernement. La section de géologie, géographie et biologie est ainsi orientée vers la valorisation des sols, l'amélioration des plantes et des animaux et le recensement de la flore et de la faune, tandis que la section médicale doit concentrer ses recherches sur les maladies du travail professionnelles, « sociales » (maladies vénériennes, malaria, tuberculose, typhus), infantiles et endémiques (goitre). Entièrement englobée dans l'État, l'Académie RPR bénéficie dès lors de tout son appareil politique et répressif pour orienter la science dans les directions imposées par l'URSS.

III. INSTITUTIONNALISATION DU LYSSENKISME (1949)

Dès le mois de janvier 1949, la sous-section d'agronomie de l'Académie réunit 300 chercheurs et techniciens des instituts de recherche et des stations expérimentales pour faire connaître « le complexe Dokoutchaev-Williams », concernant les sciences du sol et « la conception mitchuriniste de la biologie ». Sont détaillées les recherches planifiées en Roumanie, en stricte conformité avec les préceptes soviétiques[18]. C'est la première étape, technique, de l'importation du lyssenkisme. Lors du congrès pour la paix et la culture des intellectuels de Roumanie[19] et d'une session spéciale de l'Académie RPR, la portée idéologique du lyssenkisme est proclamée en dehors du cercle d'agronomes et biologistes. En référence directe à la session d'août 1948 de l'Académie des sciences agricoles Lénine, Traian Savulescu détaille toutes les implications théoriques et pratiques du lyssenkisme dans un rapport intitulé « De la pratique d'amélioration des plantes aux principes de biologie générale. Réflexions en marge du rapport de T. D. Lyssenko[20] ». Sans débat, l'Académie RPR adopte ensuite une motion d'hommage à Lyssenko. Intégrée dans une série de mesures

18. Les communications sont réunies sous le titre : « Cuceririle stiintei sovitice pe tarâmul agriculturii în desbateri la Institutul de cercetari agronomice » [Les conquêtes de la science soviétique dans le domaine de l'agriculture en débat à l'Institut de recherches agronomiques], *Studii. Revista de stiinta si filozofie*, an II, 2, avril-juin 1949, p. 64-132.

19. Aimé Césaire participe à ce congrès en tant que délégué de la France. Le congrès décide de créer une association pour la diffusion de la science.

20. T. Savulescu, « De la practica domesticirii plantelor la principii biologice générale. Reflexiuni pe margineau raportului lui T.D. Lyssenko », *Analele Academiei RPR Sectia de Stiinte biologice*, Seria a, t. 2, 1949.

radicales, dont la réforme de l'agriculture à laquelle il est officiellement associé, le lyssenkisme ne rencontre aucune opposition manifeste dans les rangs de l'Académie.

Enfin, en juin-juillet 1949, prenant pour prétexte la publication d'articles en anglais et en français dans la *Revue d'ophtalmologie*, la section médicale, puis l'ensemble de l'Académie condamne le « cosmopolitisme, manifestation idéologique empoisonnée de la classe exploitante[21] ». Affirmant la prééminence d'une science nationale et prolétarienne, Savulescu reprend à cette occasion la formule : « Pourquoi voulez-vous être de petits Français, au lieu d'être vous-mêmes[22] ? » Roland Barthes, chargé d'affaires culturelles à la Légation de France à Bucarest, voit dans cette résolution de l'Académie une mise au pas des scientifiques roumains, effet conjoint des pressions soviétiques et de leur propre volonté de satisfaire et même de devancer les souhaits de l'URSS :

> La campagne très ample menée contre la Revue d'Ophtalmologie et le *cosmopolitisme* atteint ou va atteindre bien de savants roumains […]. Mais le cas de ces savants est à tout prendre moins tragique que celui des intellectuels roumains qui s'asservissent totalement aux impératifs soviétiques et renchérissent sur les mots d'ordre imposés. Tel est le cas de l'auteur de ce second rapport, le Président de l'Académie RPR, Trajan Savulescu : cet universitaire, qui doit toute sa formation à la science française[23], n'a pas hésité à prononcer contre les savants occidentaux un réquisitoire tout aussi indigne que les flagorneries grossières adressées sans fin à la science soviétique[24].

21. *For a Proper Orientation of Scientific Activity in the Rumanian People's Republic*, Resolution of the Presidium of the Academy of the RPR June 28th, 1949. Report of the Section of Medical Science of the Academy of the RPR, Report by Professor Traian Savulescu, President of the Academy of the RPR.

22. La phrase a été prononcée par Vera I. Muhina, sculptrice soviétique, lors d'une conférence organisée à Bucarest par ARLUS en novembre 1946.

23. Sur le point précis de la formation française de Savulescu, Roland Barthes est mal informé. Le président de l'Académie RPR a fait des études en Roumanie et a obtenu son doctorat en systématique végétale en 1916 à Bucarest, où il a enseigné pendant toute sa carrière universitaire.

24. Lettre de Roland Barthes, attaché culturel près de la Légation de France en Roumanie, intitulée « Politisation de la Science en Roumanie », le 21 juillet 1949, n° 665, adressée à la dgrc, mae, 1948-1955, Enseignement, 155 ; A. Guénard, « De la reconstruction à l'éviction. Entre 1944 et 1949, une politique culturelle française en Europe centrale et orientale confrontée à l'organisation du Bloc communiste », *Matériaux pour l'histoire de notre temps*, 36, Paris, 1994, p. 21-27.

L'amertume de Roland Barthes n'est pas partagée par tous les visiteurs français de la Roumanie. Pendant le même été 1949, une délégation de l'Union française universitaire est accueillie par les universitaires et officiels roumains. Jeanne Lévy et Yvette Neefs font dans *La Pensée* un compte rendu enthousiaste, sur la situation de l'enseignement, de la culture et de la science roumaine, y compris des travaux entrepris à l'Institut de recherches agronomiques de Bucarest : « amélioration des plantes d'après les méthodes de Mitchourine et Lyssenko, de vernalisation, contrôle de semences, phytopathologie, microbiologie du sol, engrais[25] ». En effet, le Rideau de fer n'est pas imperméable. Si l'action diplomatique française dans le domaine culturel est mise en échec, les relations entre les partis communistes continuent et permettent les contacts, strictement contrôlés par les autorités roumaines.

CONCLUSION

Dans le domaine des sciences de la vie, l'ancienneté de l'influence française revêt une importance particulière qui se révèle quelques années plus tard. Jusqu'en 1955-1956, les scientifiques roumains publient peu de travaux d'obédience lyssenkiste, en dehors des articles de propagande et des conseils techniques agronomiques. Les publications sont des traductions massives des ouvrages soviétiques, tout comme les cours donnés dans les universités. Les seuls ouvrages originaux publiés sous les auspices de l'Académie RPR concernent des domaines plus « neutres », comme la systématique et la pathologie végétale. Après 1956, l'abandon du lyssenkisme n'est pas aussi radical qu'ailleurs[26] ; il s'effectue très progressivement et tardivement, seulement dans les années 1960. Cette période de longue transition est celle pendant laquelle les références françaises néo-lamarckiennes, faciles à accommoder avec la rhétorique lyssenkiste permettent aux scientifiques roumains d'opérer une synthèse originale entre les préceptes lyssenkistes, la tradition néo-lamarckienne française et la continuité avec la science nationale roumaine d'avant la deuxième guerre mondiale.

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25. J. Levy et Y. Neefs, « Révolution culturelle en Roumanie. Témoignage sur la République Populaire de Roumanie », *La Pensée*, 27, 1949, p. 67-79.

26. W. de Jong-Lambert, « Lyssenkoism in Poland », *Journal of History of Biology*, 45, 2012, p. 502.

VIII

Cold war, french-russian connections and the unexpected meeting of post-stalinist physiology of higher nervous activity with western electroencephalography and clinical neurophysiology

Jean-Gaël Barbara

INTRODUCTION

London, July 1947. In the height of the Cold War, a community of Western scientists sets up an international committee, transformed two years later into the *International Federation of Societies for Electroencephalography and Clinical Neurophysiology* (IFSECN).[1] At that time, the *US Navy* is already funding some of these researchers,[2] when its "Chatter program" on unethical human experimentation on psychoactive drugs is started in the autumn of 1947.[3] The program

1. "Proceedings of the international federation of EEG societies," Paris, September 1-4th, 1949, *Electroencephalography and Clinical Neurophysiology*, 1, 1949, p. 508.

2. M. A. B. Brazier, "Impressions of the second international electroencephalographic congress," Paris, September 1-4th, 1949, *Electroencephalography and Clinical Neurophysiology*, 1, 1949, p. 509-512. Brazier writes: "the preparation of this manuscript was aided by a grant from the us Navy Department onr, no. 5 ori-76 Project 8." The funding of the project no. 5 ori-76 from the Office of National Research concerned at least in part psychophysical investigations on human subjects, some of them with mental illness.

3. *Project MKULTRA, the CIA's program of research in behavioral modification. Joint hearing before the select committee on intelligence and the subcommittee on health and scientific research of the committee on human resources, United States Senate, Ninety-Fifth Congress, August, 1977*, printed for the use of the Select Committee on Intelligence and the Committee on Human Resources, p. 67. "Project chatter was a Navy program that began in the fall of 1947. Responding to reports of 'amazing results' achieved by the Soviets in using 'truth drugs', the program focused on the identification

represents the Cold War American reaction against the rumours of soviet military scientific research on a "truth serum" used by secret services during interrogation procedures.

This whole research domain was indeed also very active in the USSR and it developed within the framework of the physiological school of Ivan Petrovich Pavlov (1849-1936) based on the famous theory of the conditional reflex. Scientific exchanges between Russian and Western scientists on this topic, beginning at the turn of the 20th century, have progressively opened the Pavlovian doctrine to the new scientific achievements of international neurophysiology. Conversely, Western scientists showed regained interest, although from different and heterogeneous points of view, for the Pavlovian concept of conditioning during the same period.[4]

However, the Stalinian regime discontinued such collaborations, in particular by the organization of the so-called "Pavlovian Session," a politically biased meeting ending with the removal, from the Moscow and the world scientific scene, of those Soviet scientists previously inclined to establish connections between the Russian physiology of reflexes and Western neurophysiology.[5]

As soon as 1952 however, Stalin was developing his concept of "peaceful coexistence," where controlled scientific exchanges were promoted between the East and the West. During the same period, the French government elaborated the concept of *détente* which favoured international scientific exchanges with the East.[6]

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and testing of such drugs for use in interrogations and in the recruitment of agents. The research included laboratory experiments on animals and handicapped subjects involving *Anabasis aphylla*, scopolamine, and rhescaline in order to determine their speech-inducing qualities. Overseas experiments were conducted as part of the project. The project expanded substantially during the Korean War, and ended shortly after the war, in 1953."

4. See for example, S. Sarkisov, "Some new developments in the morphophysiology of the cerebral cortex," *British Medical Journal*, 2, 4410, 1945, p. 7-40; E. Adrian, "Centenary of Pavlov's birth," *British Medical Journal*, 2, 4627, 1949, p. 553-557.

5. "Academy of Sciences of the USSR, Academy of Medical Sciences of the USSR," *Scientific session on the physiological teachings of Academician I. P. Pavlov, June 28-July 4 (1950). Inaugural address, reports, resolution*, Moscow, Foreign Languages Publishing House, 1951.

6. M. P. Rey, *La temptation du rapprochement. France et URSS à l'heure de la détente (1964-1974)*, Paris, Publications de la Sorbonne, 1991.

In the fifties, the Western and the Soviet communities of neurophysiologists met and opened discussions on the progressively unifying theme of the neurophysiological study of Pavlovian conditioning.

Our goal here is to summarise briefly some of the stages of these new scientific connections, by stressing mainly the scientific rationale, the setting of international research programs and the role of French-Soviet relations.

These contacts did not only give birth to the important international organization of the Cold War period, the "International Brain Research Organization" (IBRO).[7] They were perhaps more importantly pivotal in the development of the new research field which, among others, made up the movement of the *neuroscience* in the early 1960s: the neurophysiological study of learning.

This case study may help understand how a new science, associated with real but often obscure strategic issues, emerged at the international level in the context of the Cold War. Also, it may show how a new science could create new modes of knowledge production by the cultural hybridization of ideas from initially distinct scientific schools.

I. THE OFFICIAL SOVIET VIEWS ON PAVLOVIAN STUDIES (1950-1955)

Some Soviet scientists, often members of the Communist Party, established scientific contacts with the West in the post-war years during official missions or visits to international scientific congresses.[8] Their objectives were to present the most advanced Soviet researches in the area of brain physiology and morphology. Scientific presentations became however more ideologically radical in tone, after the "Pavlovian Session." Although sometimes alluding to the materialist Marxist doctrine, they referred exclusively to the ideas of the Russian

7. P. Buser, J.-G. Barbara, B. Lichterman and F. Clarac, "The International Brain Research Organization from its conception to adulthood," in J.-G. Barbara, I. Sirotkina and J.-C. Dupont (eds.), *Franco-Russian relations in the Neurosciences*, Paris, Hermann, 2011.

8. For example Semen Aleksandrovich Sarkisov (1895-1971). Sarkisov was a Russian morphologist who studied in Berlin in the laboratory of Oscar Vogt. He was also an early electroencephalographer in the USSR and he had numerous contacts with Great Britain, William Grey-Walter for example. He was an active member of the Soviet Communist Party and a Soviet Red Cross representative in the uk during the Second World War. This position enabled him to promote Soviet propaganda in his own scientific domain. See S. A. Sarkisov, *op. cit.*

physiological schools of Sechenov, Bekhterev, Vvedensky and Pavlov, when strictly scientific questions were concerned.

For example, French neurophysiologist, Michel Jouvet, recalls the scientific exchanges, in 1951, between his university in Lyons and those of Moscow and Leningrad during which he personally met two students of Pavlov, "the ultramarxist and sinister Bykov, decorated with Soviet medals from shoulders to trousers, and the smiling and friendly Asratyan."[9]

Konstantin Mikhailovich Bykov (1886-1959) became a member of the permanent IUPS committee on behalf of Leon Abgarovich Orbeli (1882-1958) ousted by the Stalinian regime after the Pavlovian Session.[10]

Bykov chaired Soviet delegations in the ensuing congresses, a particularly important one at the 19th International Physiological Congress in Montreal in 1953, with D. A. Biryoukov, P. S. Kupalov, L. G. Voronine, V. S. Rusinov and G. D. Smirnov. A session was organised on the "physiological theory of knowledge" stressing the neurophysiological aspects.[11]

In Montreal, Bykov attended the nationalistic presentation of his fellow citizens and colleagues Vladimir Sergeevich Rusinov (1903-1995) and Georgiy D. Smirnov (1914-1973) who gave an account of the Soviet research programs in electroencephalography and neurophysiology to the Western community, in line with the Pavlovian Session resolution. The orthodox Pavlovian views of V. S. Rusinov were clear at the Pavlovian Session when he vigorously challenged the ideas of Ivan Solomonovich Beritashvili (1885-1974) on spontaneous brain rhythms. Rusinov explained these rhythms were supported by a minimal inflow of sensory stimuli, by different types of neuronal connexions, and by a dynamical equilibrium between excitation and inhibition. In his later presentations, Rusinov showed that the demonstration of spontaneous nervous activities dated back to the work of Russian

9. M. Jouvet, interview of Michel Jouvet by S. Mouchet, J.-F. Picard, Montluel, October 31th, 2007 [http://www.histrecmed.fr/index.php?option=com_content&view=article&id=56:jouvet-michel&catid=8:entretiens], (last accessed 26/04/2016). See also, M. Jouvet, "Comment le sommeil fut dissocié en deux états, télencéphalique et rhombencéphalique," in C. Debru, J.-G. Barbara and C. Cherici (eds.), *L'Essor des neurosciences, France 1945-1975*, Paris, Hermann, 2009.

10. IUPS, International Union of Physiological Sciences.

11. The topic was "La théorie physiologique de la connaissance", by H. Piéron, *L'année psychologique*, 53, 1953, p. 727-738.

schools, with that of Sechenov in the first place. Moreover, Rusinov criticized a somewhat idealistic Western view of spontaneous brain activities, in favour of the autonomy of the mind, while opposing his materialistic Marxist view.[12]

Between 1952 and 1958, the Soviet delegations in Western countries were still under strict control by the official orthodox physiological doxa, established during the Pavlovian Session. However, Soviet delegates abandoned the style adopted by Bykov at the 1950 physiological congress in Copenhagen, when he criticized the mass of American scientists presenting low-level research and behaving like joyful tourists.[13] The friendly and nice character of V. Rusinov towards his Western colleagues replaced this attitude.[14]

II. THE FEDERATING TOPIC OF PAVLOVIAN CONDITIONING BETWEEN THE EAST AND THE WEST (1955)

A second meeting was organized in 1955 by French neurophysiologist and epileptologist Henri Gastaut (1915-1995) in Marseilles, where he managed to bring together Western scientists and their orthodox Soviet counterparts, V. S. Rusinov and G. D. Smirnov.

Gastaut was in Montreal in 1953, and he may have had contacts with them at the physiological congress, since he attended the very elitist satellite symposium, organised a few days earlier, in collaboration with UNESCO and CIOMS[15] on the "brain stem activity in relation with electroencephalogram and conscious states."[16]

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12. V. S. Rusinov, G. D. Smirnov, "Quelques données sur l'étude électroencéphalographique de l'activité nerveuse supérieure," in M. M. Fishgold and H. Gastaut (eds.), *Conditionnement et réactivité en électroencéphalographie*, *Electroencephalography and Clinical Neurophysiology*, Supplément 6, 1957, p. 9-23.

13. "Whereas all countries, among them some good-sized ones, sent delegates numbering, as was ours, in tens of participants, there were 400 Americans! One bumped into American 'tourists' literally at every turn," *Science*, 112, 1950, p. 768, quoted in W. Horsely Gantt, "Russian physiology and pathology," in *Soviet Science*, Washington D.C., American Association for the Advancement of Science, 1952, p. 29.

14. D. Albe-Fessard, Autobiography in L. R. Squire (ed.), *The History of Neuroscience in Autobiography*, Washington D.C., Society for Neuroscience, 1996, vol. 1, p. 2-48.

15. Council for International Organizations of Medical Sciences.

16. *Activité du tronc cérébral en relation avec l'électroencéphalogramme et les états de conscience*, this is the famous symposium published under the title *Brain mechanisms and consciousness*, E. A. Adrian, F. Bremer, H. H. Jasper (eds.), Oxford, Blackwell, 1954.

At that time, Gastaut was a key figure in many respects in these international relations between world neurophysiologists. He was an early promoter of IFSECN[17] and, as its General Secretary,[18] he had contacts with CIOMS and UNESCO. Besides, Gastaut had also organised international meetings in Marseilles since 1950. In Marseilles 1955, his goal was to bring together scientists to discuss the "electrical activity of the brain in relation with psychological phenomena."[19]

To his great surprise, most of the discussions dealt with the neurophysiological aspects of Pavlovian conditioning. Rusinov and Smirnov presented the essential role played by Russian schools in these studies, within the general framework of the Pavlovian theory of reflexes. More surprisingly, Rusinov expressed reservations in regard to the exclusive use of electroencephalography in the study of the higher nervous activity, probably because he acknowledged the strong temptation of some Western scientists to correlate electrophysiological measurements with mental states, somewhat considered by them, according to Rusinov, as autonomous, in an idealistic and poorly rigorous perspective.[20]

On the Western side, many scientists followed independent but parallel research paths, leading to similar electrophysiological approaches, in order to find physiological correlates of mental states in freely moving animals using Pavlovian conditioning as a tool.[21]

Such convergence of interests between Western and Eastern research communities, long separated by the Iron Curtain, and more importantly by language barriers,[22] can be obviously explained by the inevitable confrontation of Pavlovian conditioning and modern neurophysiology

17. International Federation of Societies for Electroencephalography and Clinical Neurophysiology.

18. Gastaut was then the international secretary of the *Société Française d'EEG*, the president of which was Alphonse Baudoin (1876-1957).

19. H. Piéron, "L'activité électrique du cerveau en relation avec les phénomènes psychologiques," *L'année psychologique*, 56, 1956, p. 641-651.

20. V. S. Rusinov and G. D. Smirnov, *op. cit*.

21. Herbert Jasper, Michel Jouvet, Raúl Hernández-Peón and Pierre Buser. These scientists had reciprocal influence upon one another in the choice of their methods in the neurophysiological investigation of conscious animals. For example, Jouvet decided to enter such a scientific area when he attended a conference by Herbert Jasper on habituation on conscious apes in the laboratory of Horace Winchell Magoun.

22. Much energy and funding are deployed at that time to provide Western scientists with translations of articles and lists of article titles. Documentation services develop bibliographical procedures to advertise foreign articles, some of them being translated. Conversely, a similar policy is developed in the USSR especially concerning communications by Soviet scientists at international meetings.

which had been going on since the 1940s. Since neurophysiological techniques and methods provided then measurements of brain electrical activities on conscious human subjects and on conscious freely moving animals, a new neurophysiology of learning and more broadly of behaviour was becoming possible.

III. COLLABORATIVE RESEARCH AT AN INTERNATIONAL SCALE (1956-1958)

In the foreword of the Proceedings of the Marseilles meeting, British neurophysiologist William Grey-Walter (1910-1977) observed that a good measure of its success was the desire of participants to perform new experiments and the international research network that emerged for the study of the neurophysiology of Pavlovian conditioning.

In the course of the meeting, Gastaut gave an account of his general ideas concerning this topic, which met with a lively interest among participants. The discussions transcribed highlight the vigorous debates relating in particular to terminological difficulties and some questionable interpretation, often on the table when a new research field is established.

For three years, 1956-1958, Gastaut collaborated with scientists from different countries on the neurophysiology of conditioning in order to improve his models of the formation of temporary connections of the brain based on cortical and sub-cortical physiological mechanisms.

Although Gastaut mostly worked with scientists from Western Europe, Poland, South America, Canada and Japan, he published papers in the USSR. Some Russian scientists, ousted after the Pavlovian Session, like Pyotr Kuzmich Anokhin (1898-1974), decided to work on this project, in particular relating to the involvement of a sub-cortical structure most studied at that time, the "reticular formation."

These Russian scientists seemed to work against the still vivid traditional Pavlovian views held at the meeting when other Soviet scientists attacked Gastaut's model in 1958 and favoured cortical mechanisms exclusively. However, these former Russian innovative researchers observed that Pavlov himself did not, at the end his scientific career, rule out the possible physiological role of deep brain structures during conditioning.

A great deal of reorganisation in the researches devoted to the neurophysiology of conditioning occurred worldwide in the 1956-1958 period. This trend can be in part explained by the official rehabilitation of the scientists condemned at the Pavlovian Session.[23] Previously, as early as in 1949, Nobel Prize winner Sir Edgar Adrian underlined it was becoming evident that a constructive criticism could be confronted with Pavlovian ideas, not only in the West, but also, from 1955 on, on the other side of the Iron Curtain, in order to fight the orthodox Pavlovism supported by the Stalinian regime.

This great international research activity was supported and funded in the midst of the Cold War by the political institution, UNESCO, under the auspices of which the new international organisation, IBRO, was created and given the mission to manage the new scientific relations between the East and the West in the domain of the physiological enquiry of the mind. IBRO was officially created at the Moscow colloquium in 1958 mainly at the initiative of Gastaut and Rusinov with the most influential official supports.[24]

IV. THE CREATION OF A SCIENTIFIC COMMUNITY BETWEEN THE EAST AND THE WEST DURING THE COLD WAR (1958)

The research field of the neurophysiological investigation of the higher nervous activity became an increasingly and highly strategic scientific area during the Cold War. The "Chatter program" of the US Navy stopped in 1953, but it was expanded into other projects relating to the general problem of "mind control," under the common label MKULTRA.[25]

The community of scientists under investigation here represents the group of international leading experts of the 1950s and later, in the scientific study of psychical activities, the investigation of their biophysical properties and sensitivity to psychoactive agents, some of them discovered during the same years.

This field of research of the 1950s, which appears as strategic as that of astronomy at the same period, although on different levels and scales, promoted a new political interest and the consequent support of international scientific relations, in order to ensure peace to some

23. At the Congress of Physiologists, Biochemists, and Pharmacologists, Kiev, 1955.

24. B. L. Lichterman, "The Moscow Colloquium on Electroencephalography of Higher Nervous Activity and Its Impact on International Brain Research," *Journal of the History of the Neurosciences*, 19, 2010, p. 313-332.

25. *Ibid.*, *Project MKULTRA, the CIA's program of research*, 1977.

extent, or at least "peaceful coexistence," when minimal communication was maintained on sensitive issues.

In this context, Ivane Beritashvili organized six conferences in Gagra (Georgia), between 1948 and 1972, under the auspices of the Georgian Academy of Sciences on neurobehavioral sciences with participants from USSR. Especially important was the third Gagra symposium held in 1958, which focussed on the highlight topic of that period on the formation of temporary nervous connections in conditioned reflexes.[26]

A few months later, with great solemnity, Gastaut and Rusinov managed to organise, with H. Jasper and I. Beritashvili, the grand Moscow meeting, from October 6th to October 11th 1958, which served as a creative catalyst of East-West scientific relations for the upcoming years. In many respects, and for the first time, Russian Pavlovism truly confronted Western neurophysiology in the discussions of the meeting.

Such discussions were now possible, although Anokhin, the former dissident of orthodox Pavlovism, condemned Russian neurophysiologists as lagging behind Western advances, which the former orthodox Pavlovian, Rusinov, acknowledged.[27] The attack of Anokhin pointed out the fact that the Pavlovian Session massively slowed down the opening of the field of modern neurophysiology of the higher nervous activity, which used to be an area of excellence of Russian physiology in the 1930s and the 1940s.

Lively and constructive discussions took place with many Russian physiologists somehow sceptical with regard to the neurophysiological model of Gastaut. Asratyan rejected the model, Konorski showed little interest, Sarkisov dealt with terminological issues, Beritashvili expressed serious doubts, whereas Golikov pointed out the fact that Gastaut did not use the ideas of Russian physiologist Oukhtomsky properly. However, the model was also criticised by Gastaut's Canadian colleague, Herbert Jasper, who suggested some degree of modification in accord with some neglected neurophysiological data.

What really mattered however was the possibility of such discussions, besides some persisting terminological difficulties. As Wilder Penfield remarked in 1955, Soviet scientists were well-informed about

26. M. G. Tsagareli, "Ivane Beritashvili: Founder of Physiology and Neuroscience in Georgia," *Journal of the History of the Neurosciences*, 16, 2007, p. 288-306.

27. B. L. Lichterman, *op. cit.*

the novel international researches which they understood in detail and quoted with ease.[28] The strict Pavlovian terminology *stricto sensu* was becoming less attractive, mostly among younger Russian generations, although it persisted during the immediate following years and appeared, to Western scientists, to truly hinder scientific progress. The field of the neurophysiology of conditioning generated much research on both sides of the Iron Curtain with an increasing capacity to engage in dialogue, although the ways of asking questions and the methods still differed.[29]

One of the characteristics of these exchanges was the *chassés-croisés* between the West and the Soviet World.[30] Soviet scientists initially gave an account of Pavlovian conditioning as a *phénoménotechnique* inherent to the Pavlovian theory of reflexes, as most Western scientists appreciated it in the early 1950s. But the findings of the 1930s and 1940s by Pavlov's students, who did not conform to the orthodox Pavlovism of the 1950s, altered the Pavlovian theory on the basis of substantially different experimental protocols of conditioning (Anokhin, Konorski). Yet, in the 1950s, Western scientists finally concluded that Pavlovism was substantially only a method, the underlying theory of which was to be rectified, while stressing the pioneering studies of Anokhin and Konorski. For other research topics as well, for instance regarding to what Pavlov termed the "unconditional centre," Western scientists

28. W. Penfield, "A glimpse of neurophysiology in the Soviet Union," C*anadian Medical Association Journal*, 73, 1955, p. 891-899.

29. H. Jasper, "Discussion", part II, in F. N. Furnes (ed.), *Pavlovian Conference on Higher Nervous Activity*, New York, New York Academy of Sciences, 1961, p. 970. "It is apparent […] that much has been accomplished during the past two years, both in Soviet and American laboratories, working on essentially the same problems from different approaches and with somewhat different methods. It is interesting, as well, to see that our approaches in methods are not as different as they used to be. Monographs such as this [the Pavlovian Conference, New York, 1961] serve to increase the areas of understanding and agreement in this difficult field of research. They also bring out important differences and serve to sharpen our points of view so that we may think more clearly about complicated problems. The perfection of electrophysiological techniques capable of recording the electrical activity of local areas of the brain, and even the discharge patterns of single cells in the brain, throughout the conditioning process, is making it possible to test some of the hypotheses that have been proposed, by Pavlov and others, to explain brain mechanisms underlying complex behavior and learning."

30. See in a more general perspective J.-G. Barbara, "French neurophysiology between East and West: polemics on Pavlovian heritage and reception of Cybernetics," in J.-G. Barbara, I. Sirotkina and J.-C. Dupont (eds.), *op. cit.*

ended up with the idea that new neurophysiological findings were in agreement with the novel theoretical perspectives of the students of Pavlov in the 1930s.[31] It became also possible to reformulate these ideas with contemporary concepts taken from cybernetics, a field previously banished and disqualified by Stalin as a Western and *bourgeois* science.

V. FROM THE NEUROPHYSIOLOGY OF CONDITIONING TO THE NEUROSCIENCE OF LEARNING AND MEMORY

From the 1930s to the 1970s, the neurophysiology of conditioning became a central research topic with different lines of research at the level of synapses, neurones and the whole brain. During this period, some Russian scientists, among them Anokhin and Asratyan, developed active international careers and their works and ideas dominated this field of research worldwide. Other sub-fields expanded in the USSR, such as the Pavlovian olfactory conditioning of honeybee, which progressively became a modern neuroscientific domain of regained interest in recent years.[32]

The studies of single neuronal activities and of the forms of synaptic plasticity have gradually extended the list of the known cellular mechanisms explaining the formation of temporary connections during conditioning. Neurophysiological correlates of the neuronal mechanisms imagined in the 1940s by psychologist Donald Hebb, or those of the early 1950s described by Nobel Prize winner, John Eccles, could explain the reinforcement of synapses during memory and learning mechanisms.

In the 1960s, the scientific movement of neuroscience emerged after the first *Neuroscience Research Program* launched by Francis O. Schmitt. During this meeting, "behavioural states" and "correlates of learning" were discussed in the framework of the Pavlovian theory of learning, with the dominant themes of reinforcement, arousal states, orientation

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31. P. Buser and A. Roger, "Interprétation du conditionnement sur la base des données électroencéphalographiques," in *Premier Congrès International des Sciences Neurologiques, IVe Congrès International d'Électro-encéphalographie et de Neurophysiologie clinique, VIIIe Réunion de la Ligue Internationale contre l'Épilepsie, Réunions plénières, rapports, discussions et documentation*, Bruxelles, Acta Medica Belgia, 1957, p. 417-444.

32. In France, Martin Giurfa from the Centre de recherches sur la cognition animale, CNRS, Université Paul Sabatier, Toulouse, received the 2006 Silver medal of CNRS on this research topic.

reactions, forms of memory, brain rhythms or habituation. Numerous quotations of Russian scientists are indicative of the common ideas and works of the 1950s, where Eastern and Western neurophysiological traditions came closer to one another.

The main problem was no more to decide what proper physiological measurement should serve as a correlate to the Pavlovian concepts, in the line of past discussions on the objectification of Pavlovian *excitation* by the desynchronization of the alpha rhythm observed with electroencephalography. Now, the focus was on the understanding of the neurophysiological cellular mechanisms at stake in conscious and behavioural states.

CONCLUSION

Following the 1958 Moscow colloquium, several smaller meetings took place worldwide, some of them organized by American and Russian scientists: the Macy conferences and the so called "Pavlovian Conference" in 1961, organised jointly by the American and Soviet Academy of Sciences.

However, it is necessary to stress the important part that France played in the initiatives of bringing the East and the West closer, with the consequent development of such scientific subjects as learning and the determinism of behaviour, when neuroscience was emerging.

In order to understand this role better, it is necessary to come back to the important leader of the French school of neurophysiology, namely Alfred Fessard (1900-1982). Fessard was a student of Henri Piéron (1881-1964), but also of the physiologist of labour Henri Laugier (1888-1973), first director in 1939 of the CNRS (Centre National de la Recherche Nationale), and then Assistant Secretary-General of the United Nations in charge of social affairs at the United Nations Economic and Social Council (ECOSOC) from 1946 to 1951.

In 1946, Fessard was sent on mission to the United States to visit the most prestigious laboratories together with Louis Bugnard, with the medical research committee of OSRD.[33] In 1952, Fessard had attempted to resurrect the idea of an "International Brain Institute" previously put forward by French neuropsychiatrist Roger Pluvinage. This idea was based on the early 20th century "Interacademic Brain

33. Office of Scientific Research and Development (OSRD). See J.-F. Picard, *La Fondation Rockefeller et la recherche médicale*, Paris, puf, 1999.

Institute" label given by the Central Commission for Brain Research (1903) to a few excellent institutes.

The project of Fessard was given priority support by UNESCO and discussions with ECOSOC followed. However, it was not until the 1958 Moscow colloquium that Fessard's idea took the form of an organization entirely dedicated to the study of the brain, while the previous view of an international institute was abandoned.

Beyond these political and institutional aspects where France was prominent, our goal here was more generally to show that science can manage to expand truly at the international level in order to create new regimes of knowledge production in a way supported by relations between fundamental scientific domains, relatively autonomous from social contexts, but nevertheless very dependent on them for their effective achievement. This seems clear concerning the emergence of the neuroscientific study of learning and behaviour in the context of the Cold War, which unquestionably served its scientific and institutional development.

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The author wishes to thank Chantal Barbara and Merab G. Tsagareli for helpful comments on the manuscript.

IX

The origin of the cooperation between sweden, the USSR and France on the rhythmical activity of the spinal chord and fictive locomotion

Laurent Reynet

INTRODUCTION

The locomotive apparatus is an essential structure for the survival and reproduction of many vertebrates and invertebrates, and it has been studied since the 17th century.[1] Charles Scott Sherrington (1857-1952) made a considerable contribution to the understanding of locomotion. In 1910, he provided a theory of the alternate excitation of muscles based on the concept of reflex. His contribution left such a trace that the Sherringtonian theory continued to dominate the conception of locomotion until the 1960's. As a founder of modern neurophysiology and as a Nobel Prize laureate, Sherrington's ideas were very influential. After World War II, the reflex concept was still at the centre of the stage in neurophysiology. Sherrington described and coined concepts for several types of reflexes at the beginning of the 20th century. On this basis, he was able to explain more complex movements with a chain of reflexes. For Sherrington, the locomotor movement is mainly an assembling of little parts of movement triggered one after the other. Amongst the several types of reflexes, Sherrington emphasized the importance of the proprioceptive stimulation,[2] while the concept of

1. F. Clarac, "Some historical reflection on the neural control of locomotion," *Brain Research Review*, 57, 2008, p. 13-21.

2. C. S. Sherrington, "Flexion-reflex of the limb, crossed extension reflex, and reflex stepping and standing," *The Journal of Physiology*, 40, 1910, p. 28-121.

"stretch reflex" came only fourteen years later.[3] The authors showed that the stretch of the muscle evoked a reflex contraction localized in the same muscle and an inhibition of the motoneurones linked to the antagonist muscle. They concluded that, in locomotion, the muscles were stretched to an extent that stretch reflexes must operate.[4] The stance phase (where the limb is in contact with the ground) begins when the limb is at the anterior extreme position (AEP), and finishes when the limb is at the posterior extreme position (PEP). Then, the swing phase starts when the limb returns to the AEP. Hereafter, the cycle restarts. At each extreme position, according to Sherrington, a new reflex is triggered.

Meanwhile, Thomas Graham Brown (1882-1965) proposed an alternative theory of locomotion.[5] His peers, however, would not recognize his works when he was still alive. Even if he worked with Sherrington in Liverpool (1910-1913), and became professor at Cardiff in 1924, his scientific life was short. He quit his scientific career to go mountain climbing. However, Graham Brown had the time to document the central origin of the stepping movement in the spinal cord. In his first article on this topic, he reported that Sherrington was aware of the evidence about the central origin of complex movements:

> […] in early 1911, Sherrington and he [Graham Brown] […] demonstrated at a meeting of the Physiological Society a cat in which scratching may be elicited although both hind limbs have been completely de-afferented.[6]

Thus, if the dorsal roots pathways are broken, the first part of the reflex arc cannot convey excitation and so the reflex should be impossible. Graham Brown proposed a theoretical model to explain how a central alternating pattern may control locomotion. Two half-centers were postulated. Each of them had the function to excite flexors or extensors and to inhibit the opposite muscle and the other

3. E. G. T. Liddell and C. S. Sherrington, "Reflexes in response to stretch," *Proceedings of the Royal Society of London*, 96, 1924, p. 212-242.

4. A. Lundberg, *Reflex control of stepping. The Nansen memorian lecture, October 10th 1968*, Oslo, Universitetsforlaget, 1969, p. 8.

5. T. Graham Brown, "The intrinsic factors in the act of progression in mammals," *Proceedings of the Royal Society of London*, 84, 1911, p. 308-319.

6. *Ibid.*, p. 312; quoted from D. G. Stuart and H. Hultborn, "Thomas Graham Brown (1882-1965), Anders Lundberg (1920-), and the neural control of stepping," *Brain Research Review*, 59, 2008, p. 74-95.

half-center.[7] However, his diagram of the half-centres hypothesis was only published once in a German journal in 1916.[8] In 1941, at a Cambridge meeting of the Physiological Society, Graham Brown showed a film of a decerebrate and deafferented cat walking on a treadmill. The scientific community only became aware of the importance of Graham Brown in 1973 when a report of this film was made.[9]

But, how could an idea, which was well accepted later, be ignored for half a century? A first explanation may be found in the ambivalent attitude of Sherrington towards Graham Brown's work.[10] The fact that the experiments showing a central control of movements were controversial because of the uncertainty about the deafferentation is a second explanation. The animals with dorsal root transection always showed some sorts of deficits.[11] Moreover, some advocates of the reflex theory, like James Gray (1891-1975), argued that at least one dorsal root must remain for locomotion to be possible. Later works showed that animals without dorsal root were indeed very lethargic but still capable of locomotion. In the late 1960s, the Sherringtonian conceptions of locomotion were questioned. In fact, the interest in locomotion, as an object of neurophysiological research, was lost for a time. In the 1950s, the most important research was done mainly at the neuronal level with intracellular recordings. Anyway, the study of locomotion restarted, independently, in the 1960s in three laboratories: one in Sweden, one in the USSR and one in France. A similar interest amongst the three groups led to a fruitful cooperation. A conception of an intrinsic control of locomotion against the reflex thesis became dominant at that time. Five to ten years later, a scientific community was formed on this topic, which continued to grow in the subsequent years.

So, how did these three groups return to the study of locomotion and why did they support the idea that the control of locomotion may be explained, for the most part, by an intrinsic generator? One possible

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7. T. Graham Brown, "On the nature of the fundamental activity of the nervous centres, together with an analysis of the conditioning of rhythmic activity in progression, and a theory of evolution of function in the nervous system," *The Journal of Physiology*, 48, 1914, p. 18-46.

8. D. G. Stuart and H. Hultborn, *op. cit.*

9. A. Lundberg and C. G. Phillips, "T. Graham Brown's film on locomotion in the decerebrate cat," *The Journal of Physiology*, 231, 1973, p. 90-91.

10. A. Lundberg, *op. cit.*; D. G. Stuart and H. Hultbor, *op. cit.*

11. S. Grillner in V. Brooks (ed.), *Handbook of Physiology. Section 1: The Nervous System*, Motor Control. Part 2, Washington D.C., American Physiological Society, 1981, vol. 2, p. 1179-1236, 1194.

way of explaining this simultaneous and independent reopening of the study of locomotion is to try to reconstruct the reasoning of each group of scientists from the beginning.

I. THE RETURN TO LOCOMOTION IN SWEDEN

Anders Lundberg (1920-2009) and his group at the University of Göteborg, Sweden, were probably the first to return to locomotion after World War II. This can be understood through the broad connexions between this group and Sherrington's school, particularly with John Carew Eccles (1903-1997). In the 1950s Eccles was studying the stretch reflex focusing from the point of view of the underlying neuronal mechanisms. Eccles and his colleagues were the first to publish in 1952 intracellular recordings of motoneuronal action potentials in the cat spinal cord. This work refined the biophysical model of the functioning of the neurone on the basis of ion-transfer mechanisms. So, the stretch reflex was still central in neurophysiology, but in the 1950s the focus was at cellular level. Around the end of the decade, the main characteristics of the nerve cell were well described until a new turn appeared in the 1960s. Eccles' preface to the proceedings of a symposium entitled *Physiology of spinal neurones* is very clear:

> Gathered for this week of lectures and discussions were representatives from many of the leading schools of research into the properties of nerve cells and of their functional organization in the spinal cord, which has long been regarded as the simplest level of the central nervous system. However, after reading the complexities of neuronal interconnection here described, one may well wonder if this is an illusion! […] I am going to be rash enough to predict that the centre of interest in the nervous system is now moving from the investigation of properties of individual neurones and of the individual synapses to the much wider concepts of the patterns of functional organization, which give ultimate meaning to the individual neuronal and synaptic properties and subsume all these into various levels of organization.[12]

Anders Lundberg actually took this turn, together with Eccles. They worked together in Canberra (1956-1961) on the motoneurones of the spinal cord implicated in the stretch reflex. They found that the spinal cord was more complex than expected. For example, the excitatory

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12. J. C. Eccles, "Preface," *Progress in Brain Research*, 11, 1964, p. vii.

potentials of a Ia fiber were found in more than 400 motoneurones belonging to 22 different hindlimb muscles.[13] Lundberg returned to Sweden to analyze the supraspinal control of segmental reflexes.[14] He was particularly interested in the ascending and descending pathways in relation to the local activity of the motoneurones in the spinal cord. More generally, Lundberg and Engberg were interested in understanding the role of spinal and supraspinal neuronal networks in the control of simple or complex movements, including locomotion.[15]

Lundberg's particular interest for locomotion came from two paths of research as stated in his first review about the problem of locomotion in 1969:

> My interest in stepping has been not only to try to elucidate its mechanisms but also to find a functional meaning for the pattern of *Ia* connections from large spindle afferents to motor nuclei. Some motor nuclei receive effects from a wide receptive field (J. C. Eccles *et al.* 1957; R. M. Eccles R.M., Lundberg, 1958) and not only as originally suggested (Lloyd, 1946) from close synergists and strict antagonists.[16]

This understanding of the complexity of motor units led to fruitful experiments with his student Ingemar Engberg (1935-2005). They recorded the EMG activity in the cat related to pictures decomposing the movement and found that the EMG excitation of the muscles started 5 to 10 milliseconds before the end of the previous part of the step. So, doubts concerning the pertinence of the chain-reflex theory were accumulating:

> […] this burst of activity in its onset neither is a *Ia* stretch reflex nor a reflex response from the pad. We consider it extremely unlikely that it is a spinal

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13. J. C. Eccles, R. M. Eccles and A. Lundberg, "The convergence of monosynaptic excitatory afferents on to many different species of alpha motoneurones," *Journal of the Physiology*, 137, 1957, p. 22-50.

14. B. Alstermark, H. Hultborn, E. Jankowska and L. G. Pettersson, "Anders Lundberg (1920-2009)," *Experimental Brain Research*, 200, 2010, p. 193-195.

15. Jankowska, personal communication.

16. A. Lundberg, *op. cit.*, p. 38; J. C. Eccles, R. M. Eccles and A. Lundberg, "The convergence of monosynaptic excitatory afferents on to many different species of alpha motoneurones," *op. cit.*; R. M. Eccles and A. Lundberg, "Integrative pattern of Ia synaptic actions on motoneurones of hip and knee muscles," *Journal of the Physiology*, 144, 1958, p. 271-298; D. P. C. Lloyd, "Integrative pattern of excitation and inhibition in two-neuron reflex arcs," *Journal of Neurophysiol.*, 9, 1946, p. 439-444.

> reflex from another receptive system in the hindlimb. Likewise we do not believe that it is a spinal reflex from the fellow limb or from forelimbs because identical findings were made during trot and gallop and the position of the forelimbs relative to that of the hindlimbs was not relevant.[17]

Afterwards, Elzbieta Jankowska and collaborators returned to the earlier question. They found that the DOPA elicits a rhythmical pattern in the flexor and extensors motoneurones. Quoting the previous work of Graham Brown the authors translated the "half-centres" hypothesis in a neuronal framework, proposing a new diagram. They linked directly their new data to the problem of stepping:

> The double reciprocal organization of these connections makes them particularly interesting in relation to stepping. Presumably stepping can be evoked through this network not only by reflex action from primary afferents but also by descending volleys and probably also by autochtonous activity in the interneurones.[18]

So, the Swedish group expressed criticisms of the reflex theory and made an alternative proposal.

In 1965, Lundberg went to Leningrad to participate at a workshop where he met Mark Shik who was also working on locomotion. Lundberg learned that many articles of the Moscow team were published in *Biofizika*, one of the few soviet journals translated in English. From that time, the Moscow group gained international audience after being cited by Lundberg.[19] Sten Grillner, one of Lundberg's students, went abroad for a year of postdoctoral research.

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17. I. Engberg and A. Lundberg, "An electromyographic analysis of stepping in the cat," *Experientia*, 18, 1962, p. 174-176.

18. E. Jankowska, M. Jukes, S. Lund and A. Lundberg, "The effect of dopa on the spinal cord. 5. Reciprocal organization of pathways transmitting excitatory action to α-motoneurones of flexors and extensors," *Acta Physiologica Scandinavica*, 70, 1967, p. 369-388.

19. I. Engberg and A. Lundberg, "An electromyographic analysis of muscular activity in the hindlimb of the cat during unrestrained locomotion," *Acta Physiologica Scandinavica*, 75, 1969, p. 614-630.

II. THE INFLUENCE OF NIKOLAI BERNSTEIN ON THE RETURN TO LOCOMOTION IN THE USSR

The Moscow group exerted an important influence on the constitution of the field. They found a simple and useful preparation to carry experiments on locomotion. In the 1960's, this group was mainly composed of Mark Shik, Grigori Orlovsky, Fyodor Severin (1942-1968) and Yuri Arshavsky. The Moscow team in the Institute of Biological Physics started to publish on locomotion in 1965. At that time they worked with a dog walking on a treadmill in the biomechanical tradition starting from Étienne-Jules Marey (1830-1904) and until Nikolai Bernstein (1896-1966).[20] Their main discovery was the "mesencephalic locomotor region" (MLR): a small area situated on both sides of the brainstem. The stimulation of this region can evoke locomotion in a mesencephalic cat.[21] Associated with an experimental protocol where it is possible to make EMG recordings as the cat walks on a treadmill, the MLR had been a key for the study of locomotion. The Moscow team showed that the intensity of the MLR stimulation has an impact on the speed of the step cycle, on the force developed by the limb in the stance phase[22] and finally on the gait. If the stimulation is strong enough, the cat may change its gait from walk to trot and from trot to gallop. So, the higher parts of the nervous system control only one simple feature of the locomotion, namely, the propulsive force developed by the muscle. However, it would not control the whole speed of the stepping cycle.[23] Moreover, the Moscow team reproduced the experimentations with sections of the dorsal roots and showed that it was still possible to elicit locomotion. All these elements found on the control of locomotion were very positive results for the theoretical hypothesis adopted in this laboratory.

The origin and the influences of the works of the Moscow group are very clear. Their framework came from a reformulation

20. Y. I. Arshavsky, Y. M. Kots, G. N Orlovsky, I. M. Rodionov and M. L. Shik, "Investigation of the biomechanical of running by the dog," *Biophysics*, 10, 1965, p. 737-746.

21. M. L. Shik, F. V. Severin and G. N. Orlovsky, "Control of walking and running by means of electrical stimulation of the mid-brain," *Biophysics*, 11, 1966, p. 756-765.

22. F. V. Severin, M. L. Shik, G. N. Orlovsky, "Work of the muscles and single motor neurons during controlled locomotion," *Biophysics*, 12, 1967, p. 762-772.

23. M. L. Shik and G. N. Orlovsky, "Co-ordination of the limbs during running of the dog," *Biophysics*, 10, 1965, p. 1037-1047.

of the Bernsteinian theory of movement by a physicist, Mikhail Lvovitch Tsetlin (1924-1966) and another reformulation from a notable mathematician of the 20th century, Israel Gelfand (1913-2009). In 1958, Gelfand organized biological seminars and he set up two research groups. Mathematicians involved were interested in the problems of the transmission of information and cybernetics. At the same moment, the scientific context changed radically for physiology in the USSR. In May 1962, an All-Union Conference on Philosophic Questions of Higher Nervous Activity and Psychology was held in Moscow. This was the backlash of the so-called "Pavlovian Session" held in Moscow in 1950. Bernstein lost his position in 1951, three years after being awarded the Stalin Prize for science. By 1962, though, Bernstein and all his work had been rehabilitated.

In their early works, the Moscow team tried to understand how the nervous system solves the problem of motor redundancy in the control of movement.[24] They stuck to Bernstein's idea of a system of control made of different levels where the individual elements are not directly controlled:

> The idea of constructing a system with purposive behavior which is not inherent in its elements is particularly important. Typical in this connexion is the situation in which each element receives not complete information on the state of the elements in the system but only a penalty (incentive) the value of which depends on the state of the system as a whole. *In such a system the individualized control of each element in the system is replaced by the existence of interaction between elements*. […] The system consists of several levels. Each level consists of a large number of individual elements. It is assumed that the elements of one level change the system of interaction between the elements of the other level.[25]

It seems that the Moscow team found the part of the brain corresponding to the "regulator" in Bernstein's theory: the MLR. In fact they were actively looking for such a brain region through old literature – Hinsey, Ranson, McNattin in 1930, Waller in 1940

24. N. Bernstein, *The co-ordination and Regulation of movements*, Oxford, Pergamon Press, 1967, p. 130; L. Latash, *Synergy*, New York, Oxford University Press, 2008, p. 35-45.

25. I. I. Piatetskii-Shapiro and M. L. Shik, "Spinal regulation of movement," *Biophysics*, 9, 1964, p. 525-530.

and Grossman in 1958 – and through experiments: "We looked for a region by stimulating which it would be possible to control locomotion in mesencephalic cats – a preparation robbed of voluntary motor activity and incapable of locomotion on exteroceptive stimulation."[26]

So, the discovery of the MLR may be understood as an outcome of hypothesis testing. Three aspects may be dissociated about the origin of these positions. First, the general shift towards the pavlovian physiology and the centrality of the reflex concept. Second, the importance of Nikolai Bernstein, who criticized the importance of the reflex concept and proposed a theory of movement with several levels of control. Third, the work of a group of mathematicians looking for a way to show how a system can reach a goal without direct control of each element.[27]

With the discovery of the MLR, the Moscow group members were considered as specialists in this emerging field. Claude Perret, a French neurophysiologist, also visited this team when he discovered their articles in *Biofizika*. For him, it was natural to cooperate with them, for he was working on the same problem.

III. THE RETURN TO LOCOMOTION IN FRANCE: CLAUDE PERRET

Before his PhD thesis, Claude Perret worked on the motor control of the conditioned cat in the *Laboratory of comparative neurophysiology*, led by Pierre Buser (1921-2014), Paris VI University. After completing this work he looked for a new experimental protocol, which would permit a more precise neurophysiological analysis: the acute cat preparation. Around 1968, when Perret was working under the direction of Arlette Rougeul-Buser (1924-2010), they made a strange and unexpected observation. After a surgery during which all the cortex of the cat was removed, a spontaneous locomotor activity emerged after the anaesthetic effect ceased. Even if this kind of observations had been made several times since the beginning of the 20th century, such a locomotor

26. M. L. Shik, F. V. Severin and G. N. Orlovsky, "Control of walking and running by means of electrical stimulation of the mid-brain," *op. cit.*

27. M. Toom, personal communication. M. Toom emphasized the fact that the persecutions on Jewish people was important to understand the political position in this institute.

activity had never been studied with electrophysiological techniques.[28] This was a good preparation, which could be reproduced and which allowed easy recordings of the electrical activity. With the curare, the deafferentation was realized without any destruction of the spinal cord circuitry. So, the criticisms on the deafferentation by cutting the dorsal roots, previously mentioned, were not significant on this preparation: "The curarization do not suppress all the 'locomotor' activity even if it suppresses the kinetical part of afferents […]; the efferent rhythms persist but are slower, more scarce and less organized […]."[29]

This work, as well as the studies on the rabbit carried out by Denise and Guy Viala, is at the origin of the term "fictive locomotion" proposed by Pierre Buser.[30] "Fictive locomotion" refers to the presence of a rhythmical locomotor activity without movements. Later, Perret performed subsequent studies with Didier Orsal and Jean-Marie Cabelguen. Sten Grillner refined the preparation on other species. He took the spinal cord of a lamprey and put it in a liquid similar to the physiological conditions in the body. Nowadays, many studies are done with this "walking spinal cord" on rat. So in the same manner as Lundberg, Perret studied the spinal activity in relation to an activity broader than the simple reflex. The locomotor behaviour was used to study central rhythms, the gamma loop and also the ascending and descending pathways of the spinal cord. The discovery of the "fictive locomotion" relies on an unexpected observation and a broad interest led into a correct interpretation. Retrospectively, this is not surprising, because one aim of the French school of neurophysiology, led by Alfred Fessard (1900-1982), was to develop research both at integrative and cellular levels.[31]

In 1973, Perret went to the Moscow group laboratory and later to Lundberg's in order to work with Sten Grillner. These cooperations were maintained for several years and were the first impetus for the constitution of a scientific community around the problem of locomotion.

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28. C. Perret, *Analyse des mécanismes d'une activité de type locomoteur chez le chat*, Paris, Thèse de Doctorat d'Etat, université Paris VI, 1973, p. 16-17.

29. *Ibid.*, p. 52; recordings, p. 34-35.

30. J.-M. Cabelguen, personal communication.

31. J.-G. Barbara, E. Broussolle, J. Poirier and F. Clarac, "Figures and institutions of the neurological sciences in Paris from 1800 to 1950," part 2, *Revue Neurologique*, 168, 2012, p. 106-115.

CONCLUSION

Three different groups returned to the study of locomotion, in the late sixties, for three different reasons. Lundberg started from the study of the properties of the motoneurones and of their afferent fibres. He found that the stretch reflex pathway was more complex than expected and went on studying the relationships between the reflex and the ascending and descending pathways in the spinal cord. Finally, he selected locomotion as a good way to inquire into these relationships.

The Moscow team came back to locomotion via an opposite pathway but in a converging direction. They started from a highly theorized ground meeting the former work of Nikolai Bernstein about the integrated question of movement. Their experiments to test whether their theoretical hypothesis had a biological reality were rewarded.

Claude Perret seemed to start from an unexpected experimental finding: he found a way to elicit an obstinate locomotion in a reproducible manner. He then started to study the intrinsic rhythm in the spinal cord with a curarized animal, which would become a widely used preparation called "fictive locomotion."

A common feature, which can be found in the three groups, is the critical attitude towards the reflex concept. Lundberg and Perret saw its limits in their work when the Moscow group started with the critics made by Bernstein from the 1930s.[32] Finally, the return to the study of locomotion seems to be one part of the history of the big turn previously mentioned by Eccles.

Acknowledgement

I would like to thank everyone who answered my questions: Yuri Arshavsky, Pierre Buser, Jean-Marie Cabelguen, Elzbieta Jankowska, Claude Perret, and Andrei Toom (a mathematician, now professor at the Federal University of Pernambuco, Brasil; he was a student of the mathematician and collaborator of the Soviet group of biologists, Ilya Piatetski-Shapiro, 1929-2009). I am grateful to Jean-Gaël Barbara and Jean-Claude Dupont for their kind support and to Erich Pinzón-Fuchs and Silvia De Cesare for their precious help.

32. N. Bernstein, *op. cit.*, p. 115-116.

Index

A

B

C

D

E

F

G

H

I

J

K

L

Y

Z

Liste des auteurs

Étienne Aucouturier : Collège universitaire français de Saint-Pétersbourg et Université Paris 8, France.

Oleg P. Belozerov : Institute for the History of Science and Technology, Russian Academy of Sciences, Moscow, Russia.

Jean-Gaël Barbara : Laboratoire de neurobiologie, CNRS UMR 7102, Université Pierre-et-Marie-Curie & laboratoire Sphere, CNRS UMR 7219, Université Paris Diderot, Paris, France.

Céline Cheric : Université de Picardie – Jules Verne et laboratoire CHSSC, EA 4289, Amiens, France.

Francois Clarac : 248 Le Corbusier ; 280 Bd. Michelet, 13008 Marseille, France.

Pascal Deren : Centre d'histoire des sciences et d'épistémologie de Lille 1, CNRS UMR 8163 et UMR 8524, Université de Lille, France.

Jean-Claude Dupont : Université de Picardie – Jules Verne et laboratoire CHSSC, EA 4289, Amiens, France.

Natalia Fedounina : Université de pédagogie et psychologie de Moscou, Russie.

Eduard I. Kolchinsky : Saint Petersburg Branch of the Institute for the History of Science and Technology, Russian Academy of Sciences, Russia.

Mikhail B. Konashev : Saint Petersburg Branch of the Institute for the History of Science and Technology, Russian Academy of Sciences, Russia.

Tatiana A. Kursanova : Institute for the History of Science and Technology, Russian Academy of Sciences, Moscow, Russia.

Marina Loskutova : National Research University, Higher School of Economics & Saint Petersburg Branch of the Institute for the History of Science and Technology, Russian Academy of Sciences, Russia.

Bernard Maitte : Centre d'histoire des sciences et d'épistémologie de Lille 1, CNRS UMR 8163 et UMR 8524, Université de Lille, France.

Jean Massion : 16 route de Rognes, 13410 Lambesc, France.

Cristiana Oghina-Pavie : Université d'Angers, Laboratoire CERHIO – CNRS UMR 6258, Angers, France.

Anatoli V. Polevoi : Saint Petersburg Branch of the Institute for the History of Science and Technology, Russian Academy of Sciences, Russia.

Igor I. Popov : Saint Petersburg State University, Russia.

Daria Popova : Collège universitaire français de Saint-Pétersbourg, Russie.

Laurent Reynet : Laboratoire Sphere, CNRS UMR 7219, Université Paris Diderot, Paris, France.

Vladimir O. Samoilov : Pavlov Institute of Physiology, Russian Academy of Sciences, Saint Petersburg, Russia.

Stéphane Schmitt : Laboratoire Sphere, CNRS UMR 7219, Université Paris Diderot, Paris, France.

Henrique Sequeira : SCALab, Équipe DEEP, CNRS UMR 9193 et Neurosciences, UFR Biologie, Université de Lille, France.

Stéphane Tirard : Université de Nantes et Centre François Viète d'épistémologie et d'histoire des sciences et des techniques, EA 1161, Nantes, France.

Tatyana I. Ulyankina : Institute for the History of Science and Technology, Russian Academy of Sciences, Moscow, Russia.

Table des matières

II
PHYSIOLOGIE ET MÉDECINE

Mise en pages : Élisabeth Gutton

Achevé d'imprimer