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Preface

A new yearbook for history and philosophy of biology

The German Society for History and Philosophy of Biology was founded in Jena on June 29, 1991, in the wake of the reunification of Germany. The Society drew members from both the formerly East German and West Germany parts of the country, and from several other European countries and the United States. With a current membership of around 250, the Society has helped to re-establish the history and philosophy of biology as a discipline at German universities and continues to promote the subject as part of higher education. In addition, the Society has supported the establishment of a museum for the history of biology, the Biohistoricum in Neuburg an der Donau, which functions as an archive for books, papers and other materials relevant to the history of biology.

From the start, the Society has been concerned to bring studies in the history and philosophy of biology to a wide audience through its *Jahrbuch für Geschichte und Theorie der Biologie*. Parallel to the *Jahrbuch*, the *Verhandlungen zur Geschichte und Theorie der Biologie* has become the traditional medium of publication for papers delivered during the Society's annual meetings. So far, 9 volumes of the *Jahrbuch* have appeared, under the editorship of Hans-Jörg Rheinberger (1994-1997), Michael Weingarten (1994-2004), Mathias Gutmann (1999-2004), Eve-Marie Engels (1999-2001) and Nicolaas A. Rupke (2003-2004).

From 2005 the *Jahrbuch* will be conducted by a new team of editors. This is an opportunity to implement a number of changes, making the contents more transdisciplinary and international, and increasing the emphasis on comparative biology as a subject of historical and philosophical studies. These changes are reflected in a name change from *Jahrbuch* to *Annals of the History and Philosophy of Biology*. The editors (from different scientific disciplines: botany, zoology, history of science, philosophy) are supported by an international editorial board. Contributions to the *Annals* should engage with the relevant international, scientifc and historiographical discourse.

From 2005 manuscripts should be submitted to the managing editor V. Wissemann. Submissions will be peer reviewed. The preferred language is English. Articles in German should be accompanied by a short (max. 1000 words) summary in English.

The Editors

Vorwort

Am 29. Juni 1991 wurde in Anwesenheit von 60 Personen im Kleinen Hörsaal Zoologie der Friedrich-Schiller-Universität Jena die „Deutsche Gesellschaft für Geschichte und Theorie der Biologie“ (DGGTB) gegründet. Die Gründung stieß nicht nur national, sondern auch international auf ein breites Interesse, und zu den 145 Gründungsmitgliedern zählten Personen aus den Niederlanden, Frankreich, England, Italien, den USA, Österreich, der Schweiz und Liechtenstein.

Neben der weiteren Etablierung und Festigung des Faches Biologiegeschichte, der Erhaltung und Erschließung von Dokumenten zur Geschichte und Theorie der Biologie, der Sicherung von Nachlässen und Institutssammlungen sowie der Gründung eines biologiehistorischen Museums (Biohistoricum in Neuburg an der Donau) sollten von Beginn an auch spezielle biologiehistorische und -theoretische Zeitschriften über die aktuellen Forschungsergebnisse sowie die Arbeit der Gesellschaft informieren. So entschloss man sich, ein Jahrbuch für Geschichte und Theorie der Biologie zu begründen und parallel dazu die wichtigsten Referate der jeweiligen Jahrestagungen im Biologischen Zentralblatt zu veröffentlichen (112. Band, 2. Heft, 1993; 113. Band, 2. Heft, 1994). Für die Drucklegung der Versammlungsbeiträge wurden dann ab 1995 die Verhandlungen zur Geschichte und Theorie der Biologie begründet.

Bisher erschienen vom Jahrbuch neun Bände, die teilweise einen festgelegten Themenschwerpunkt verfolgten, sowie 11 Bände der Verhandlungen. Als Herausgeber – in Verbindung mit der DGGTB – zeichneten bisher verantwortlich: 1/1994 bis 4/1997 Hans-Jörg Rheinberger & Michael Weingarten; 5/1998 Michael Gutmann & Michael Weingarten; 6/1999 bis 8/2001 Michael Weingarten, Michael Gutmann & Eve-Marie Engels; 9/2003 Michael Weingarten, Michael Gutmann & Nicolaas A. Rupke.

Zum 1. Januar 2005 wechselte nun die Herausgeberschaft. Dem Jahrbuch werden vier Herausgeber vorstehen, die aus unterschiedlichen Fachrichtungen (Botanik, Zoologie, Wissenschaftsgeschichte, Philosophie) kommen und die von einem internationalen Editorial Board unterstützt werden. Es ist geplant, pro Jahr mindestens einen Band zu publizieren, wobei das Jahrbuch ebenso für weitergehende Ideen und Arbeiten zur Verfügung steht. Alle Arbeiten werden ein „Peer Review-Verfahren“ durchlaufen.

Die Herausgeber

Zur Argumentations- und Vermittlungsstrategie in Müllers Handbuch der Physiologie des Menschen[1]

Olaf Breidbach

Abstract
Müller´s textbook of human physiology was one of the most important of its kind in the 19th century. Müller not only presented state of the art physiology, but additionally a new approach to physiological experimentation. The aim of physiology was, he maintained, the most complete analysis possible of the physico-chemical reactions that make up the animal body. In practice, he realized, such an analysis was impossible at the time. Yet physiology should adopt just such a reductive approach, describing the various physiological processes as constitutive of the functional morphology of organisms. For this purpose, the nerve-muscle junction proved ideal. Following on from the 18th century study of animal electricity, a particular experimental set-up had become established by the early 19th century, which made possible the examination of reactions at the nerve-muscle junction and thus of the physiology of muscle movement and its control. By this time, the observed reactions were seen as effected life energies or life forces and became burdened by a theoretical overload. Experimental practices showed, however, that these vitalist notions were irrelevant, and as a result the reaction itself became the focus of physiological studies. In the article the strategy by which Müller enfolds his idea of a scientific physiology is reconstructed and the place of experiment and observation in Müllerian physiology is described, outlining physiology as a new prototype of experimental science.

Positionierungen

Johannes Müller (1801-1858) gilt als einer der großen Physiologen des 19. Jahrhunderts. Seine Bedeutung war dabei nicht auf die Disziplin der Physiologie beschränkt. Sein Entwurf einer experimentellen biowissenschaftlichen Forschung wurde zu Beginn des 19. Jahrhunderts für das Konzept einer induktiv analytischen Naturwissenschaft bestimmend.[2] Seine Physiologie wurde im deutschen Sprachraum in neu gegründeten Instituten

[1] Der DFG danke ich für die Förderung im Rahmen des Projektes ‚Empirie versus Spekulation' des SFB 482: ‚Ereignis Weimar-Jena. Kultur um 1800'.

[2] Lenoir, Timothy: Laboratories, medicine and public life in Germany, 1830-1849: ideological roots of the institutional revolution. In: The Laboratory Revolution in Medicine. Hg. A. Cunningham, P. Williams. Cambridge 1992, S. 14-71; Kremer, Richard L.: Building institutes for physiology in Prussia, 1836-1846: contexts, interests and rhetoric. In: The Laboratory Revolution in Medicine. Hg. A. Cunningham, P. Williams. Cambridge 1992, S. 72-109.

Johannes Müller, 1826, Gemälde von J. H. Richter (Bildarchiv Enst-Haeckel-Haus)

verankert, die fast durchweg mit Schülern Müllers besetzt wurden. So wurde sein Wissenschaftskonzept auch strukturell fixiert.[3] Diese Art der wissenschaftlichen Erfolgssicherung ist einzigartig.[4] Sie konsolidierte in ihrem Effekt die Vorreiterstellung der biowissenschaftlichen Forschung im deutschen Sprachraum, die dann ein Physiologe wie James Crichton-Browne (1840-1938) im englischen Sprachraum vor 1900 eher neidvoll kommentierte.[5]

Der in dieser Einwicklung einflussreichste Text war Müllers Lehrbuch zur Physiologie des Menschen, das sein Konzept der Physiologie als einer experimentell geleiteten Erfahrungswissenschaft präsentierte. In diesem Text demonstrierte er, wie die Argumentation des Physiologen durch Experimente zu führen war, was die weitere methodische Vorgehensweise der Physiologen im 19. Jahrhundert bestimmte.[6] In vorliegenden Text wird noch einmal versucht, den Argumentationsaufbau der Arbeit von Müller zu rekonstruieren,[7] um die Bedeutung des Experimentellen in Müllers Darstellung eingehender zu charakterisieren.[8] War in den für Müller charakteristischen Beschreibungen eines Experimentes und seiner Variationen eine eigene Argumentationsebene gefunden, über die sich die Müllersche Physiologie gegen die Argumentationsweise der vormaligen Naturforschung absetzen konnte?[9] Mit seinen Instrumentarien und den

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[3] Koller, Gottfried: Das Leben des Biologen Johannes Müller 1801-1858. Stuttgart 1958, S. 227f.

[4] Mendelsohn, Everett: Revolution und Reduktion. Die Soziologie methodischer und philosophischer Interessen in der Biologie des 19. Jahrhunderts. In: Wissenschaftssoziologie II. Determinanten wissenschaftlicher Entwicklungen. Hg. P. Weingart. Frankfurt a. M. 1974, S. 241-263.

[5] Vgl. Breidbach, Olaf: Die Materialisierung des Ichs – Zur Geschichte der Hirnforschung im 19. und 20. Jahrhundert. Frankfurt a. M. 1997, S. 253ff.

[6] Vgl. http://vlp.mpiwg-berlin.mpg.de/essays/index.html; Dierig, Sven, Kantel, Jörg, Schmidgen, Henning, The Virtual Laboratory for Physiology. A Project in Digitalising the History of Experimentalisation of Nineteenth-Century Life Sciences, ed. Max-Planck-Institut für Wissenschaftsgeschichte. Berlin 2000, Preprint Nr. 140.

[7] Vgl. Stürzbecher, Manfred: Zur Berufung Johannes Müllers an die Berliner Universität. Jahrbuch für die Geschichte Mittel- und Ostdeutschlands 21 (1972), S. 184- 226, auf Arbeiten, die sich explizit mit der Frage einer Rhetorik wissenschaftlicher Texte beschäftigt haben, sei hier nur verwiesen: Finocchiaro, Maurice A.: Galileo and the Art of Reasoning. Rhetorical foundations of Logic and Scientific Method. Dordrecht, Boston & London 1980; Gross, A.G.: The Rhetorics of Science. Cambridge & London 1990; Woolgar, Steve: What is the analysis of scientific rhetoric for? A comment on the possible convergence between rhetorical analysis and social studies of science. Science, Technology & Human Values 14 (1989), S. 47-49.

[8] Vgl. Breidbach, Olaf. Goethes Metamorphosenlehre. Paderborn 2006.

[9] Cantor, Geoffrey. The rhetoric of experiment. In: The Uses of Experiment: Studies in the Natural Sciences. Hg. T. D. P. Gooding, T. Schaffer. Cambridge 1993, S. 159-180.

durch sie bedingten Verfahren, setzt das Experiment Maßstäbe für eine wissenschaftliche Erfahrung.

Dies ist aber keine Erfindung Müllers. Diese Art der Demonstration eigener Aussagen im Kontext einer apparativen Praxis war schon vor 1830 Standart einer Naturforschung.[10] Nach 1830 wird aber die Empirie in neuer Weise thematisch, steht sie doch zumindest im deutschen Sprachraum, in dem auch Müller seine Ausbildung fand, in einem Begründungszusammenhang, der interessanterweise zunächst innerphilosophischen Vorgaben folgt.[11] In dem Jahrzehnt nach 1800 hatte die spekulative Philosophie den Anspruch formuliert, Erfahrung sichern und die Kriterien zur Evaluation des Empirischen vorgeben zu können.[12] Die bisherige Wissenschaftsgeschichte hat diesen Vorlauf, bedingt auch durch die eher selektive Interpretation der Philosophiegeschichte, nur näherungsweise in den Blick genommen. Erfahrung – das zeigt dieser Ausblick – wird nicht erst in der analytischen Konzeption der Naturwissenschaft zum Ansatzpunkt weiter ausgreifender Aussagen der Naturforschung.[13] Das umfangreiche Systematisierungsprogramm von Lorenz Oken, das erst in den 1840er Jahren abgeschlossen war,[14] zeigt dass noch Mitte des 19. Jahrhunderts dieses Programm einer philosophischen Orientierung des Erfahrungswissens innerhalb der Naturforschung selbst Akzeptanz fand.[15] Erfahrung figuriert in diese Theorieentwürfen dabei allerdings allein als Illustration innertheoretisch abgesicherter Aussagezusammenhänge.[16] Bei Müller ist die Erfahrung nun in anderer Weise in den Argumentationsgang eingewoben. Brigitte Lohff hat 1979 diesen Neuansatz Müllers beschrieben;[17] sie skizziert, wie Müller in seiner Darstellung grundsätzliche Aussagen durch Experimente zu erhärten und die so gewonnene Konturierung eines Aussagezusammenhanges dann durch Argumente aus der Literatur zu bekräftigen suchte.[18] Auch in der vorliegenden Arbeit geht es um die Bedeutung der Beschreibung von Erfahrungen im Kontext einer Argumentation. Gezeigt werden kann, dass Müller

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[10] Wiesenfeld, Gerhard: Leerer Raum in Minervas Haus.Experimentelle Naturlehre an der Universität Leiden, 1675-1715. Amsterdam, Berlin & Diepholz 2002.

[11] Breidbach, Olaf: Naturphilosophie und Medizin im 19. Jahrhundert. In: Deutsche Naturphilosophie und Technikverständnis. Hg. K. Pinkau, C. Stahlberg. Stuttgart 1998.

[12] Bach, Thomas, Breidbach, Olaf (Hg.): Naturphilosophie nach Schelling. Stuttgart-Bad Cannstatt. 2004.

[13] Breidbach, Olaf: Schelling und die Erfahrungswissenschaften. Sudhoffs Archiv 2004: Im Druck.

[14] Oken, Lorenz: Allgemeine Naturgeschichte für alle Stände. 6 Bde. Stuttgart 1833-1843.

[15] Breidbach. Olaf, Ghiselin, Michael: Lorenz Oken's Naturphilosophie in Jena, Paris and London. Journal for the History and Philosophy of Life Sciences 24 (2002) 219-247.

[16] Bach, Thomas: „Was ist das Thierreich anders als der anatomirte Mensch…?“ Oken in Göttingen (1805-1807). In Lorenz Oken (1779-1851) Ein politischer Naturphilosoph. Hg. O. Breidbach, H.-J. Fliedner, K. Ries. Weimar 2001: 73-91.

[17] Lohff, Brigitte: Hat die Rhetorik Einfluß auf die Entstehung einer experimentellen Biologie in Deutschland gehabt? Eine Studie zu Johannes Müllers Physiologie. In: Disciplinae Novae. Zur Entstehung neuer Denk- und Arbeitsrichtungen in der Naturwissenschaft. Festschrift zum 90. Geburtstag von Hans Schimak. Hg. J. Scriba. Göttingen 1979, S. 127-146.

[18] Ebda, S. 132; vgl. Dear, P.: Narratives, anecdotes, and experiments: turning experience in science in the seventheenth century. In: The Literature Structure of Scientific Argument. Historical Studies. Hg. P. Dear. Philadelphia 191, S. 135-163.

eine Darstellungsform nutzt, in der die Beschreibungen von Beobachtungen und Experimenten derart eingewoben sind, dass sich der Forscher im Vollzug seines Gedankens durch die im Experiment eröffneten Erkenntnispositionen führen lässt.

Aus der historischen Distanz ist diese sich zusehends etablierende Laborkultur, die einen Maßstab nur in sich selbst – d. h. im Urteil der Vertreter der eigenen Strategie – zu benennen vermochte, zu rekonstruieren. Dabei zeigt sich, dass hierin ein über die Biowissenschaften hinaus verbindlicher methodologischen Kanon für die experimentell arbeitenden Naturwissenschaften fixiert wurde, der sich eben keineswegs bloß im innerwissenschaftlichen Diskurs bestimmte.[19] Die Etablierung von Müller-Schülern in der Universität geht einher mit dem Aufbau physiologischer Laboratorien, die durch den methodischen Ansatz der Müllerschen Physiologie bestimmt sind. Der sich derart etablierende neue Typ eines physiologischen Labors wird – neben den durch Justus von Liebig (1803-1873) bestimmten Einrichtungen – leitend nicht nur für die biowissenschaftliche, sondern auch für die naturwissenschaftliche Forschung insgesamt.

Der Ansatz dieses Erfolges ist schon in der Forschungsstrategie Müllers, seiner Methodik und nicht unbedingt in den mit dieser Strategie erlangten Resultaten begründet. Es war nicht das neue Wissen, sondern die mit dieser Methode gewonnene Disziplin einer Forschung, die den Erfolg dieses neuen Forschungsansatzes konsolidierte. Matthias Jacob Schleiden (1804-1881), ein der Müller-Schule nahestehender Botaniker, konnte dann auch noch 1844 seine Kritiker nicht mit seinen vorhandenen Erfolgen, sondern eben nur mit der Aussicht auf einen Erfolg mundtot machen.[20] Auch Emil H. Du Bois-Reymond (1818-1892), der direkte Nachfolger Müllers auf dem Berliner Lehrstuhl beschwor noch 1872 nicht die neuen Dimensionen, sondern die Limitationen des Wissens, in denen sich die den Müllerschen Prinzipien folgende neue Naturwissenschaft zu bewegen vermochte.[21] Auf einem anderen Blatt steht dann, dass zugleich auch alle anderen Disziplinen in diese Limitationen eingebunden und dem methodologischen Diktat einer den Müllerschen Prinzipien folgenden Naturwissenschaft untergeordnet werden sollten.

Dabei war Müller selbst noch das Kind einer anderen Geisteswelt. Es ist aus der Perspektive eines nur am Progredieren der Forschung interessierten Programms wissen-

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[19] Kremer, Richard L.: Between Wissenschaft and Praxis: Experimental Medicine and the Prussian State, 1807-1848. In: 'Einsamkeit und Freiheit' neu besichtigt – Universitätsreformen und Disziplinenbildung in Preussen als Modell für Wissenschaftspolitik im Europa des 19. Jahrhunderts. Hg. G. Schubring. Stuttgart 1991, S. 155-170; Lenoir, Timothy: Laboratories, Medicine, and Public Life in Germany 1830-1849. Ideological Roots of the Institutional Revolution. In: The Laboratory Revolution in Medicine. Hg. A. Cunningham, P.Williams. Cambridge & New York 1992, S. 14-71.

Kremer, Richard L.: Building Institutes for Physiology in Prussia, 1836-1846. Contexts, Interests and Rhetoric. In: Medicine and the Laboratory in the Nineteenth Century. Hg. P. Williams, A. Cunningham. Cambridge 1996, S. 79-95.

[20] Schleiden, Matthias Jacob: Schelling's und Hegel's Verhältniss zur Naturwissenschaft. Leipzig 1844; Zur Frage der Müller-Schule vgl. Lohff, Brigitte: Gab es eine Johannes-Müller-Schule? In: Wissenschaft und Bildung. (2. Wissenschaftshistorisches Kolloquium der Universität Jena, 1.-8. Oktober 1988, Georgenthal/ Thüringen – Alma mater Jenensis. Studien zur Hochschul- und Wissenschaftsgeschichte, 7). Hg. R. Stolz. Jena 1991, S. 169 –183.

[21] Wollgast, Siegfried: Einleitung. In: Du Bois-Reymond, Emil: Vorträge über Philosophie und Gesellschaft. Hamburg 1974, S. V-LX.

schaftsgeschichtlicher Vereinnahmungen zweifellos als Ironie der Geschichte anzusehen, daß derjenige, der seine Dissertation dem Philosophen Georg Wilhelm Friedrich Hegel (1770-1831) widmete und der noch in Berlin regelmäßig bei Hegels Witwe zum Kaffee erschien, der Begründer der Schule wurde, in der zwei Welten, die einer sich physiologisch verstehenden Naturwissenschaft und die einer spekulativen Philosophie, als definitiv von einander geschieden erachtet wurden.[22] Noch Karl Friedrich Burdach (1776-1847), der Müller noch zur Mitarbeit an dem vierten 1832 erschienenen Band seines Lehrbuchs der Physiologie gewann,[23] entwickelte seine Physiologie in einem naturphilosophischen Rahmen. Für Burdach blieb auch nach 1830 die Analogie die zentrale Methode der physiologischen Analyse. Die Analogie wandelte sich für ihn dabei allerdings zu einem Verfahren, Erfahrungen untereinander in einen Bezug zu bringen,[24] blieb aber dennoch bis in die letzten Jahre der ersten Jahrhunderthälfte in einem naturphilosophisch und damit spekulativ gesicherten Begründungsgefüge verankert.[25] Müller ging in seinem Ansatz über diese, zeitgleich vertretene und um 1830 breit rezipierte Auffassung einer physiologischen Wissenschaft einen wesentlichen Schritt hinaus.[26]

Das Handbuch

Müllers „Physiologie des Menschen“ ist das zentrale Werk der Physiologie des 19. Jahrhunderts.[27] Es ist keineswegs eine bloße Kompilation des Wissensstandes seiner Zeit, vielmehr expliziert dieses Handbuch, in dem von Müller eine Fülle von eigenen Forschungsresultaten eingearbeitet wurden, eine eigene und eben neuartige Methode der Forschung. Neu sind hierbei weder der Gegenstandsbereich noch die Geräte, die Müller nutzte; hierin war er auf dem Stand seiner Zeit, er überblickte und referierte die relevante Literatur des beginnenden 19., aber auch des 18. Jahrhunderts. Innovativ war dabei allerdings seine Art der Gedankenführung, die die Beobachtung und das Experiment in einer neuartigen, in sich konsistenten Weise in einen Argumentationsgang einband.

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[22] Vgl. Gregory, Frederic: Hat Müller die Naturphilosophie wirklich aufgegeben? In: Johannes Müller und die Physiologie. Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 143-154.

[23] Burdach, Karl Friedrich: Die Physiologie als Erfahrungswissenschaft 4. Bd. Leipzig 1832.

[24] Burdach, Karl Friedrich: Die Physiologie als Erfahrungswissenschaft 1. Bd. Leipzig 1826, S. 15.

[25] Burdach, Karl Friedrich: Anthropologie für das gebildete Publikum. 2. Aufl. Stuttgart 1847; vgl. Poggi, Steffano: Neurology and Biology in the Romantik Age in Germany: Carus, Burdach, Gall, von Baer. In: Romanticism in Science. Science in Europe, 1790-1840. Hg. S. Poggi, M. Bossi. Dordrecht 1994, S. 143-160.

[26] Lenoir, Timothy: The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology. Dordrecht, Boston 1982; Lammel, Heinz-Ulrich: Nosologische und therapeutische Konzeptionen in der romantischen Medizin. Husum 1990; wobei hier auch die explizit naturphilosophischen Konzeptionen von einer funktionellen Organisation des Lebendigen keineswegs durchweg als gegen die Empirie und alternativ zu der Entwicklung der Erfahrungswissenschaften zu verorten sind, vgl.: Breidbach, Olaf, Engelhardt, Dietrich v. (Hg.): Hegel und die Lebenswissenschaften. Berlin 2002; Bach, Thomas, Breidbach, Olaf (Hg.): Naturphilosophie nach Schelling. Stuttgart-Bad Cannstatt. Im Druck.

[27] Lohff, Brigitte: Die Suche nach der Wissenschaftlichkeit der Physiologie in der Zeit der Romantik. Stuttgart 1990.

Müllers Physiologie beschreibt Funktionen, hierin war sie durchaus noch klassisch – im Sinne der Physiologie von Albrecht von Haller (1708-1777). Nur waren diese Funktionen nicht mehr nur einfach die Lebensverrichtungen der untersuchten Organe, sondern das Resultat von letztlich chemisch/physikalisch zu beschreibenden Grundreaktionen, der diese Organe konstituierenden Funktionsstände der organisch organisierten Materie. Diese Funktionalität, die derart zu erschließenden Lebensprozesse, waren für Müller in den Blick zu nehmen. Die Naturdinge werden in ihrer sich in ihnen explizierenden Gesetzmäßigkeit untersucht. Damit gab Müller das alte, noch die Naturforschung um 1820 mit bestimmende Konzept auf, die Einzeldingen als Exemplifikationen der Natur, als Mikrokosmos zu begreifen, in dem sich der Makrokosmos der Natur widerspiegelte.[28] Dieser Unterschied erscheint – zugegebenermaßen – zunächst nur als eine Nuance. Die damit formierte Nuancierung hatte aber eine umfassende Konsequenz: Die Natur-Wissenschaft – im Sinne eines Naturphilosophen wie Friedrich Wilhelm Schelling (1775-1854)[29] – wird in Folge dieses Ansatzes zu einer Naturalienwissenschaft, die sich pragmatisch, in Blick auf Anwendungsmöglichkeiten und in Blick auf die Erweiterung und Strukturierung von Kenntnissen (Information) begreift. Diese neue Art der Argumentation sucht die vorliegende Arbeit an Hand der vierten Auflage von Müllers Lehrbuch der Physiologie des Menschen nachzuzeichnen.[30]

1833 erhielt Müller – in der Nachfolge von Karl Asmund Rudolphi (1771-1832) – einen Ruf auf den Lehrstuhl der Universität Berlin.[31] Im selben Jahr erschien der erste Band seines Handbuchs der Physiologie des Menschen, das faktisch eine umfassende Darstellung der Physiologie, ausgehend von einer physiologischen Chemie bis hin zu einer neuen Art der funktionsmorphologischen Betrachtung der Organisation des menschlichen Organismus darstellt. Schon dieser umfassende Aufbau des Lehrbuchs birgt ein Programm, das die Vielfalt der Organismen – mit Einschluss der allerdings nur kursorisch behandelten Pflanzen – in einer neuartigen thematischen Schichtung begreift. Dieser erste Band erfährt bis 1844 drei Neuauflagen. 1837 erscheint der zweite Band seines Lehrbuches in seiner ersten Abteilung mit dem Zusatz des Verfassers: „Zur Förderung der Publication des Handbuchs der Physiologie hat man sich entschlossen, was vom zweiten Band gedruckt jetzt vorliegt, erscheinen zu lassen.“ Komplett lag der zweite Band dann 1840 vor.[32]

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[28] Zum Fortdauern des Mikro-Makrokosmos-Schematismus in der spekulativen Naturforschung vgl. Breidbach. Olaf; Ghiselin, Michael: Lorenz Oken's Naturphilosophie in Jena, Paris and London. Journal for the History and Philosophy of Life Sciences 24 (2002) 219-247.

[29] Vgl. Breidbach, Olaf: Die Naturkonzeption Schellings in seiner frühen Naturphilosophie. Phil. Naturalis 23 (1986) 82-95.

[30] Müller, Johannes: Handbuch der Physiologie des Menschen. Koblenz 1844.

[31] Vgl. Koller, Gottfried: Johannes Müller. Das Leben des Biologen 1801- 1858. Stuttgart 1958, S. 96-123.

[32] Zur Publikationsgeschichte vgl. Lohff, Brigitte: Johannes Müller: Von der Nervenwissenschaft zur Nervenphysiologie. In: Das Gehirn – Organ der Seele? Hg. E. Florey, O. Breidbach. Berlin 1993, S. 39-54.

Der Ansatz

Die Physiologie [so beginnt Müller mit den Prolegomena über die allgemeine Physiologie] ist die Wissenschaft von den Eigenschaften und Erscheinungen der organischen Körper, der Thiere und Pflanzen, und von den Gesetzen, nach welchen ihre Wirkungen erfolgen. Die erste Frage, welche man sich beim Eintritt in diese Wissenschaft zu beantworten hat, ist die nach dem Unterschied der organischen und anorganischen Körper. Sind die Körper, welche die Erscheinungen des Lebens darbieten, in ihrer materiellen Zusammensetzung von den unorganischen Körpern verschieden, deren Eigenschaften die Physik und Chemie untersuchen, oder sind auch die Grundkräfte welche sie bewirken, verschieden, ... oder sind die Grundkräfte des organischen Lebens nur Modifikationen der physischen und chemischen Kräfte?[33]

Die Physiologie fragt also nicht nach den Grundursachen des Lebens, sondern nach den Grundkräften in deren Identifikation sich dessen Erscheinungen erklären. Diese Grundkräfte erfasst sie in der Beschreibung der Gesetzmäßigkeiten der Lebensvorgänge. Dabei skaliert sie die Lebensprozesse gemäß dem ihr zur Verfügung stehenden deskriptiven Ansatz. Sie zergliedert die verschiedenen Reaktionsformen und sucht zunächst nach den Elementen, in denen sich diese Reaktionsabfolge beschreiben lässt. In einem zweiten Schritt wird dann die Ordnung, in die sich diese Elemente einbinden, rekonstruiert. Der physiologische Prozess erscheint damit als eine in einfache experimentell zugängliche Teilschritte zu zergliedernde Reaktionsabfolge der eingangs skizzierten Grundelemente organischer Organisationen. Diese sind nicht mehr die komplexen Lebensvorgänge, die noch Haller beschrieb, sondern chemisch-physikalische Prozesse. Der Physiologe gleicht in seiner Arbeit einem Prosektor, der das ihm zur Verfügung stehende Material auf seine Bestandteile hin zergliedert, diese Elemente dann in ihrem Zusammenhang beschreibt und aus dieser Zusammensetzung ihre Reaktionsformen rekonstruiert. Seine Sektion führt aber über die Gestalten der Organe hinunter auf die sie konstituierenden physiologischen Prozesse. Ein Lebensprozess zeigt sich so als Komposition, die aus dem Zusammenwirken der dergestalt identifizierten Elemente zu begreifen ist.[34]

Dabei ist die hier nach Müller zu vollziehende Sektion derart umfassend. Sie zielt auf die chemischen Elemente, in denen sich die Reaktionsprozesse konstituieren. Die Rekonstruktion der Funktionseinheiten aus den so gewonnenen Darstellungen der Elemente zeigt die prinzipiellen Reaktionsklassen auf, in denen Organik und Anorganik konstituiert sind. Die in diesen organischen Prozessen zu identifizierenden Kräfte benennen demnach nichts anderes als Reaktionstypen. Die Kraft kennzeichnet einen funktionalen Zusammenhang in der Konstitution der den Organismus aufbauenden Elemente und kein in dieser Beschreibung zu fassendes Prinzip. Die Physiologie wird damit zu einer deskriptiven, induktiv verfahrenden Wissenschaft.

Es ist nicht mehr die Metamorphose einer Natur – wie sie noch in Hegels Entwurf einer Stufung der Naturdinge, in der Goetheschen Morphologie oder auch in Burdachs

[33] Müller, Johannes: Handbuch der Physiologie des Menschen. Koblenz 1844, S. 1; im Weiteren wird nach dieser, die definitive Formulierung von Müllers Programm einer Physiologie offerierenden Ausgabe zitiert.

[34] Vgl. Lohff, Brigitte: Johannes Müller und das physiologische Experiment. In: Johannes Müller und die Philosophie. Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 105-123.

Anthropologie beschrieben wurde[35] – in der sich die Natur in ihren Detaillierungen als Natur konstituiert und deren Realisierungen – die Organismen – selbst gegenüber den ihnen als Natur eigenen Ordnungsprinzip ephemer sind. Ganz anders entwirft denn etwa zeitgleich Lorenz Oken (1779-1851) in seiner „Naturgeschichte für alle Stände" eine Systematik des Naturalen, in der der Ordnungszusammenhang der Organismen nur als Explikation eines viel grundsätzlicheren, die Natur in ihre Grundeigenheiten fassenden Prinzips benennt.[36] Müller verweist dabei auf eine auch strukturell konstituierte Naturwissenschaft, mit den Gegenstandsbereichen von Physik und Chemie, vor deren Aussagenspektren er den Gegenstandsbereich seiner Physiologie konturiert. Damit wird die Physiologie eine Disziplin der Sciences. Sie ist Teil der analytisch induktiv vorgehenden Wissenschaftsdisziplinen und demnach nicht mehr Moment einer systematisierend ansetzenden Naturgeschichte.[37]

Stoffe und Zellen

Müller setzte entsprechend mit einer Darstellung der stofflichen Zusammensetzung der organischen Materie an; er beschreibt eine organische Chemie. Er sieht in deren elementarer Zusammensetzung denn auch keine prinzipielle Differenz zwischen Organik und Anorganik. Er konstatiert eine Verschiebung in der Häufigkeit der Elemente, aber eben keine stofflichen Eigenarten. Was er findet, sind komplexere Verbindungen von Kohlenstoff, Sauerstoff, Stickstoff und Wasserstoff.[38]

Der Lebensprozess zeigt sich als ein in seiner Organisationstypik eigenständiger Reaktionsraum. Vor diesem Hintergrund gewinnen denn auch die zellulären Kompartimentierungen des Organismus, die Müller mit direkten Bezug auf Theodor Ambose Hubert Schwanns (1810-1882) Zelltheorie diskutiert,[39] besondere Bedeutung. Diese strukturell hochdifferenzierte Kompartimentierung von Reaktionsräumen ermöglicht eine weitere Stufung der Komplexität organischer Reaktionen, konstituiert aber keineswegs einen prinzipiellen Schnitt zwischen Organik und Anorganik. So hält Müller denn auch für

[35] Breidbach, Olaf: Transformation statt Reihung – Naturdetail und Naturganzes in Goethes Metamorphosenlehre. In: Naturwissenschaften um 1800. Wissenschaftskultur in Jena - Weimar. Hg. O. Breidbach, P. Ziche. Weimar 2001, S. 46-64; hier auch weiterführende Literatur; Breidbach, Olaf: Karl Friedrich Burdach. In: Naturphilosophie nach Schelling. Hg. T. Bach, O. Breidbach. Stuttgart, Bad Cannstatt 2003, im Druck.

[36] Vgl. Bach, Thomas: Was ist das Thierreich anders als der anatomirte Mensch …? Oken in Göttingen (1803-1807). In: Lorenz Oken (1779-1851) ein politischer Naturphilosoph. Hg. O. Breidbach, H.-J. Fliedner, K. Ries. Weimar 2001, S. 73-91; Breidbach. Olaf; Ghiselin, Michael: Lorenz Oken's Naturphilosophie in Jena, Paris and London. Journal for the History and Philosophy of Life Sciences 24 (2002), 219-247.

[37] Lohff, Brigitte: Die Suche nach der Wissenschaftlichkeit der Physiologie in der Zeit der Romantik. Stuttgart & New York 1990.

[38] Müller (1844) 2; Müller fährt fort: „Die Hauptverschiedenheiten in der Zusammensetzung der organischen Materie scheinen von dem Verhältnisse der Mischungsgewichte der Elemente Sauerstoff, Wasserstoff, Kohlenstoff, Stickstoff abzuhängen" (Müller (1844) 5).

[39] Müller (1844) 7.

wahrscheinlich, die Eingeweidewürmer als Zeugnis für die „freiwillige Entstehung lebender Wesen in organischer Materie" deuten zu können.[40]

Dennoch gibt es strukturelle Voraussetzungen für einen organischen Prozess: Ein Organismus ist denn auch, soll sich seine Funktionalität erhalten, nicht ins Unendliche teilbar. Müller benennt Wirkeinheiten in der "Zusammensetzung der organischen Körper".[41] Diese sind nicht einfach auf der zellulären Ebene anzusetzen, vielmehr ist die Funktion eines Organismus an komplexere Wirkeinheiten und deren Abstimmung gebunden. Müller spricht mit explizitem Verweis auf Immanuel Kant (1724-1804) von der „aus ungleichartigen Gliedern eines Ganzen" zusammengesetzten Organisation der Organismen, „die nach dem Gesetze der Zweckmässigkeit" konstituiert seien.[42]

Symmetrien

Diese Zweckmäßigkeit – so Müller – manifestiere sich in der Organisation, und d. h. in der Abstimmung der verschiedenen Teilbereiche der Gewebe. Ganz analog hatte Marie-Francois Xavier Bichat (1771-1802) etwa 40 Jahre zuvor seine Klassifikation der Funktionstypen des Neuronalen auf einer Darstellung der Symmetrie des Nervensystems aufgebaut.[43] Müller greift diesen Gedanken auf, differenziert aber ein entsprechendes Vorgehen, denn schließlich sei die Organisation des Organismus nicht einfach in ein kristallographisches Schema zu bringen.[44] Der Grundunterschied zwischen Organismus und Kristall besteht – ihm zufolge – in der dynamischen Organisation des Organismus. In dieser Dynamik sind die der Physiologie zugänglichen Gesetzmäßigkeiten des Organischen zu beschreiben.

Die Analyse der zweckmäßigen Organisation des Organismus führt Müller zu einem umfassenden Kraftbegriff:

> Allein diese Harmonie der zum Ganzen nothwendigen Glieder besteht doch nicht ohne Einfluss einer Kraft, die auch durch das Ganze hindurch wirkt, und nicht von einzelnen Teilen abhängt, und diese Kraft besteht früher, als die harmonischen Glieder des Ganzen vorhanden sind.[45]

Genau in dieser Hinsicht ist – ganz analog dem Denken des Entwicklungsbiologen Caspar Friedrich Wolff (1734-1794) – der „Embryo von der Kraft des Keimes geschaffen".[46] Schließlich gilt: „Diese vernünftige Schöpfungskraft äussert sich in jedem Thiere nach strengem Gesetz, wie es die Natur jedes Thieres erfordert: sie ist in dem Keim

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40 Müller (1844) 11.

41 Müller (1844) 18.

42 Müller (1844) 18f, vgl. hierzu auch Kant, Immanuel: Kritik der Urteilskraft, In: Kant, Immanuel: Werkausgabe Bd. X. Hg. Wilhelm Weischedel. Frankfurt 1977, S.319-326.

43 Bichat, Xavier: Recherches Physiologiques sur la Vie et la Mort. Paris. 4. Aufl. 1822.

44 Dies versucht später – Fries und Schleiden folgend – Ernst Haeckel in seiner 1866 erschienenen Generellen Morphologie der Organismen. Berlin.

45 Müller (1844) 21.

46 Müller (1844). 21.

schon vorhanden, ehe selbst die späteren Theile des Ganzen gesondert vorhanden sind, und sie ist es, welche die Glieder, die zum Begriff des Ganzen gehören, wirklich erzeugt".[47] Die Typologie einer zweckmäßig organisierten Natur zeigt sich derart in der Analyse der Gesetzmäßigkeiten ihrer Verhältnisbestimmungen. Demnach ist seine Analyse in eine Richtung gewiesen, es gilt nicht mehr einfach, Analogien zwischen verschiedenen Wirkeinheiten aufzuweisen um diese dann als Wirkungen einer Kraft klassifizieren zu können. Vielmehr sind diese Wirkzusammenhänge in ihre Elemente aufzubrechen, um so die Reaktionseinheiten in ihrer Vernetzung darstellen zu können.

Müller diskutiert dabei durchaus die Frage einer möglichen, eben nicht in diesem Sinne physiologisch darzustellenden 'Kraft".[48] Für Müller bleibt dieser Exkurs allerdings Teil einer nüchternen Diskussion um die verschiedenen Vorstellungsansätze, über die die Funktion der organischen Reaktionseinheiten darzustellen ist. Dabei verweist Müller den Physiologen für sein Vorgehen zunächst zurück auf die Chemie. Durch diese Wissenschaft sieht er die Grundelemente, deren Reaktionsvernetzung dann der Physiologe darstellen soll, charakterisiert. Entsprechend beschreibt er die seinerzeit neuen Ergebnisse Liebigs.[49] In diesem Forschungsansatz lässt sich dann das alte, die romantischen Assimilationsvorstellungen bestimmende Paradox auf, dass sich der Organismus von Anorganischem ernährt und nach dem Tode dann auch nur als anorganisch zu analysierendes Stoffgefüge übrig bleibt.[50] Die Frage nach einer teleologischen Organisation des Gesamtnaturzusammenhanges ist damit beantwortet.[51] Müller verweist die entsprechenden Fragen auf die Funktionsmorphologie des Organismus, die in ihrer Genese, und das heißt in der sukzessiven Entfaltung ihrer Reaktionsschichtung zu beschreiben ist.

[47] Müller (1844) 21.

[48] In Haeckels Handexemplar – dies ist im Kontext einer Analyse der Entstehungsgeschichte von dessen Monismus von eigenem Interesse – ist die entsprechende Passage markiert (Müller, Johannes: Handbuch der Physiologie des Menschen. 4. Aufl. Bd. 1, im Bestand des Museums Ernst Haeckel Haus, der Universität Jena): „Man darf daher die organisirende Kraft nicht mit etwas dem Geistesbewusstseyn Analogen, man darf ihre blinde nothwendige Thätigkeit mit keinem Begriff bilden vergleichen. Unsere Begriffe vom organischen Ganzen sind blosse bewusste Vorstellungen. Die organische Kraft dagegen, die Endursache des organischen Wesens, ist eine die Materie zweckmässig verändernde Schöpfungskraft. Organisches Wesen, Organismus ist die factische Einheit von organischer Schöpfungskraft und organischer Materie" (Müller (1844) 23). Müller markiert hier die Position des späteren Haeckelschen Monismus. Müller selbst insistiert aber nicht auf dieser, von Haeckel dann später weiter ausgeformten Idee. Für ihn ist diese Idee einer organischen Kraft, die sich der Rekonstruktion physiologisch aufzuzeichnender Gesetzmäßigkeiten entzieht, schlicht ein Mythos, der nicht begründet werden kann. Interessanterweise führt später dann auch Haeckel in seinen „Welträtseln", seine biowissenschaftlich begründete Weltanschauung in diesem Punkt zum Glauben und begründet so keine übergreifende, eben biowissenschaftlich verankerte Philosophie, sondern – wie er explizit schreibt – eine monistische Religion (Haeckel, Ernst: Die Welträthsel. Bonn 1899, S. 381). Müller spricht in diesem Kontext ganz im Sinne seines, von Haeckel fundamental unterschiedenen Kraftbegriffs von einer „räumlich sich ausbreitenden Kraft oder eines imponderabilen Stoffes" (Müller (1844) 25).

[49] Müller (1844) 36.

[50] Müller (1844) 37.

[51] Lenoir, Timothy: The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology. Dordrecht, Boston 1982.

Damit interessieren dann die Spezifitäten und nicht die qua Analogie herzustellenden potentiellen Gemeinsamkeiten verschiedener organischer Organisationstypen. Während Oken in seiner zwischen 1833 und 1843 erschienen Naturgeschichte noch versucht, Tier und Pflanze als Variationen eines gemeinsamen Organisationsprinzips der Natur darzustellen, sind für Müller die Differenzen in der Organisation dieser Lebewesen interessant.[52] Zeigen sie doch die verschiedenen Modifikationen in der Zuordnung der Reaktionsgrundelemente verschiedener Organismen auf. Müller thematisiert denn auch die differenten Integrationstypen tierischer und pflanzlicher Organisation, benennt – im Rückgriff auf die klassischen phänomenologischen Beschreibungen – Funktionstypen und Organsysteme und überführt so seine generellen Ausführungen in einen spezielleren Teil, der die physiologischen Gesetzmäßigkeiten in den entsprechenden Spezifikationen der differenten Reaktionsordnungen darzustellen sucht.

Vorab steht dabei ein Kapitel, das ausgehend von diesem Verständnis der funktionellen Organisation organischer Systeme die Abgrenzung von Organik und Anorganik näher zu begreifen sucht: In diesem Kapitel behandelt Müller die Elektrizität, die Produktion von Licht und die Wärmeerzeugung.[53]

Argumentationslinien

Wie Müller hier argumentiert, wird in seiner Darstellung der Wärmeproduktion im Organismus deutlich: „Wir werden“, schreibt Müller, “jetzt zur Untersuchung der Ursachen der thierischen Wärmeerzeugung“ kommen. „Hier ist zuvörderst die Verschiedenheit der Temperatur in verschiedenen Theilen von Interesse... Nach der Hypothese von Lavoisier und Laplace ... wird beim Athmen der Sauerstoff der Atmosphäre mit Kohlenstoff des Blutes verbunden“.[54] Er setzt also mit einem Vergleich an, identifiziert – ganz im Sinne der Funktionsmorphologie – die für den Prozess bedeutenden Organe und registriert an diesen eine für die Darstellung des interessierenden Prozesses bedeutende Eigenschaft. Diese interpretiert er dann in Bezug auf die seitens der Chemie erarbeiteten Vorstellungen über die Grundreaktionsformen der Moleküle. D. h. Atmung wird als ein biochemischer Prozeß begriffen, für den ein Organ wie die Lunge den geeigneten Reaktionsraum zur Verfügung stellt.

Die Wärmeproduktion begreift er als Resultat eines in der Lunge zu verortenden Veratmungsprozesses. Wärme wird dann über die Blutkörperchen im Körper verteilt. Er beschreibt die unterschiedliche Wärmekapazität der Blutarten und gewinnt aus diesem Erklärungsschema nun Sekundärfolgerungen, die ihm sein Vorstellungsmodell weiter sichern: „... Aus dieser Ableitung der Wärme vom Athmen lässt sich erklären, warum der Embryo noch keine merkliche eigene Wärme besitzt.“[55] Dabei reduziert er den von ihm studierten Prozess allerdings nicht einfach auf den – kompartimentiert begriffenen –

52 Oken, Lorenz: Allgemeine Naturgeschichte für alle Stände. 6 Bde. Stuttgart 1833-1843.

53 Müller (1844) 72.

54 Müller (1844) 78f.

55 Müller (1844). 81.

Chemismus, sondern sucht auch direkte Wechselwirkungen von chemischen Reaktionen und organischen Strukturen zu fassen.[56]

Ausgehend von diesen Prämissen sucht er nun das Blut als Organ eingehender zu charakterisieren. Er setzt an mit einer mikroskopischen Analyse, beschreibt dann die mechanischen Prinzipien eines hydraulischen Systems, geht von da zu einer chemischen Analyse und beschreibt schließlich organische Eigenschaften des Blutes. In einem zweiten Schritt beschreibt er den Kreislauf. Er geht aus von einer vergleichenden Analyse der Gefäßsysteme im Tierreich, begreift von daher die Erscheinungen des Kreislaufes und führt dann die Beschreibung zum Herz, als Ursache der Bewegung des Blutes, und weiter zu einer eingehenden Analyse des Verhaltens der Gefäßwand – speziell in Blick auf die Aufnahme und Ausscheidung der Stoffe – um schließlich die Lymphe zu thematisieren.[57] Der zweite Teil seiner Physiologie handelt dann „Von den organisch-chemischen Veränderungen in den Säften und den organisierten Teilen“ Als dritter Teil schließt eine Darstellung der Physik der Nerven an. Insoweit findet sich sowohl im Detail der Argumentation wie auch im Gesamtaufbau eine Stufung der Argumentation, ausgehend von der Analyse der stofflichen Grundlagen hin zu Organisationsprinzipien und von dort zu einer Darstellung der entsprechend funktional begriffenen Organsysteme.

Gedankenführung

An seinem Entwurf einer Nervenphysik soll im Weiteren die Gedankenführung in Müllers Handbuch dargelegt werden. Diese wird dabei am Beispiel seiner Reflexlehre eingehender dargestellt. Auch hier setzt Müller mit einer Beschreibung der im Kontext dieser Nervenphysik in ihrem Gesamtzusammenhang zu betrachtenden Funktionselemente an. Darauf folgt in seinem Text eine Analyse der Reaktionstypen, die diese Elemente konstituieren, um schließlich in einer Darstellung der hierbei wirksamen, aus der vergleichenden Analyse der Reaktionstypen erschlossenen Funktionsprinzipien zu münden.

Müller beginnt diesen Teilabschnitt seines Buches mit einer Skizze der vergleichend anatomischen Darstellung des Nervenbaus. Er zitiert dabei u.a. Johann Friedrich Blumenbach (1752-1840), George Cuvier (1769-1832), Felice Fontana (1730-1805), Franz Joseph Gall (1758-1828), und Johann Friedrich Meckel (1781-1833).[58] Er benennt Befunde zur makroskopischen Organisation und zur Stoffzusammensetzung der Nervenfasern, im Vergleich von Wirbeltieren und Wirbellosen; Müller skizziert hierzu den Verlauf dieser Fasern und Befunde über die innere Organisation des Hirngewebes. Er beschreibt mikroskopische, zum Teil mit Kontrastmitteln arbeitende Untersuchungen.[59] Die ent-

[56] Müller (1844). 81.

[57] Müller (1844) 94.

[58] Müller (1844) 513f; vgl. Nyhart, Lynn K.: Biology Takes Form. Animal Morphology and the German Universities, 1800-1900. Chicago & London 1995.

[59] Müller (1844) 529.

sprechenden Befunde zur Organisation der Ganglien und des Nervus sympathicus bei Wirbeltieren werden eingehender skizziert.[60]

Darauf folgen Beschreibungen von Versuchen, die die Reizbarkeit der Nerven demonstrieren. Hierbei zeigt sich im Vergleich der verschiedenen Versuche eine grundsätzliche Gemeinsamkeit in den betrachteten Reaktionen: Die verschiedenen Reizursachen – so führt er aus – „haben gleiche Wirkung, weil das, worauf sie wirken, nur einerlei reizbare Kraft besitzt, und weil die verschiedensten Dinge nur in der gleichen Eigenschaft als Reize einwirken."[61] Damit hat er eine prinzipielle Eigenart der funktionellen Organisation der Nerven benannt. Der Vergleich hat ein Reaktionsprinzip erschlossen. Er sucht dieses Prinzip nun eingehender zu charakterisieren; hierzu werden verschiedene Reiztypen und die Effekte von Reizungen an verschiedenen Orten im Nervengewebe beschrieben. Müller verweist hierbei ausdrücklich auf die Bedeutung von Mikroläsionsstudien, in denen einzelne Gewebebereiche gezielt zerstört werden, worauf der damit induzierte Effekt für die Reizweiterleitung in diesem speziellen Gewebe beschrieben wird.[62] Müllers Prinzip ist demnach kein Reaktionsschema, unter dem die Vielfalt von Teilreaktionen zu verorten ist. Sein Prinzip ist vielmehr die allen Teilreaktionen gemeinsame Grundreaktion, die über eine minutiöse experimentelle Untersuchung möglichst detailliert darzustellen ist.

Besprochen wird der Effekt einer durch Elektrizität produzierende Metallplatten induzierten Erregung. In seiner ausführlichen Darstellung beschreibt Müller dabei nicht einfach ein Experiment und illustriert an diesem eben nicht nur die Möglichkeit über ein entsprechend angesetztes Prinzip verschiedene nervenphysiologische Reaktionen zu klassifizieren. Genau hierin unterscheidet er sich von einem Vorgänger wie Burdach. Auch dieser skizziert eine Fülle von Teilbefunden, offeriert dann aber ein deduktiv erschlossenes Erklärungsschema, über das eine Reaktionsvielfalt zu klassifizieren ist. Für Müller ist es vielmehr wichtig, dass er in seinem Erklärungsansatz die in Rede stehende Erscheinung komplett abbildet. Es geht also nicht allein darum, die physiologischen Erscheinungen zu klassifizieren. Es geht darum, im Vergleich die Grundmechanismen zu entschlüsseln, über die die Vielfalt der Reaktionen als Variationen einer ‚im Prinzip' charakterisierten Reaktionstypik zu entschlüsseln sind. Dabei beschreibt er möglichst einfache Reaktionsmuster, die es insoweit erlauben, auch komplexe Phänomene als Resultat einer Interaktion der eingangs aufgewiesenen Grundelemente zu beschreiben: Die Elemente des Nervengewebes stellen, so schreibt Müller, „hier auf gleiche Art das Elektrometer, wie unter ähnlichen Umständen ein nicht thierisches Elektrometer, z. B. ein magnetischer Multiplikator" dar.[63] Die experimentelle Charakterisierung hat es ihm erlaubt,

[60] Müller (1844) 531f.

[61] Müller (1844) 534; Müller umreißt hierin seine These der spezifischen Sinnesenergie, die in diesem Kontext aber nicht weiter verfolgt wird, siehe hierzu: Poggi, Stefano: Goethe, Müller, Hering und das Problem der Empfindung. In: Johannes Müller und die Philosophie. Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 191-206; Lenoir, Timothy: Helmholtz, Müller und die Erziehung der Sinne. In: Johannes Müller und die Philosophie. Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 207-222.

[62] Müller (1844) 536; ausführlich besprochen werden die Autoren Galvani, Humboldt, Pfaff und Matteucci.

[63] Müller (1844) 540.

die Entsprechungen zwischen einem magnetischen Multiplikator und der Grundreaktionstypik des Nervengewebes eingehender darzustellen. Der Multiplikator gibt ihm das Modell dafür, wie auch die Reaktionsschichtungen im Nervensystem zu verstehen sind. Die Analogie zeigt eine strukturelle Identität, die das Experiment als eine gleichartige Reaktionsschichtung ausweist. Damit wird die Nervenphysiologie zu einer Nervenphysik: „Nachdem nun die allgemeinen und einfachsten Bedingungen, unter welchem durch Galvanismus Muskelcontractionen entstehen auseinandergesetzt worden, muss jetzt von dem Verhalten der thierischen Teile bei der Schliessung, Oeffnung und während des Geschlossenseyns der Kette gehandelt werden“.[64] Müller kann nun alle am Nerv beobachteten oder mit der Nervenfunktion in Zusammenhang gebrachte Erscheinungen – dies gilt seinen Worten zufolge sowohl für Muskelbewegungen, wie auch für den Geschlechtstrieb und für Geistesfunktionen[65] – unter dem gewonnenen Interpretationsansatz verorten.[66] Damit ist eben mehr als eine strukturelle Analogie aufgewiesen. Die verschiedenen zeigen sich vielmehr durch die gleichen Mechanismen konstituiert. Entsprechend sucht Müller, diese verschiedenen Verhaltensweisen des Nervengewebes als Effekte einer von ihm im Experiment eingehender bestimmten Reaktionstypik zu beschreiben, die eben jedem Nervengewebe zu eigen ist. Entsprechend sind denn auch die verschiedenen Leistungen des Nervengewebes als Ausfluss des insoweit charakterisierten Reaktionsprinzips zu fassen.

Damit wird eine Nervenkraft für den Physiologen bestimmbar. [67] Entsprechend sind dann auch – entsprechend den Arbeiten von Francois Magendie (1783-1855) – etwaige durch Narkotika, d.h. durch präzis bestimmbare chemische Substanzen induzierte Fehlfunktionen des Nervengewebes als Beleg dafür zu deuten, dass es Müller möglich war, die Reaktionstypik des Nervengewebes mit seiner Physiologie auch umfassend zu beschreiben. Um die Spezifika der entsprechenden Reaktionen eingehender zu beschreiben, sind dann die Wirkorte dieser Narkotika und primär und sekundär affizierte Organsysteme zu identifizieren. So gelangt Müller zu einer fundierten auf die physikalisch chemischen Grundfunktionen hin zielenden Aussage über die prinzipiellen Funktionsmuster des Nervengewebes.[68]

[64] Müller (1844) 541.

[65] Müller (1844) 544.

[66] Müller bezieht sich hier explizit auf Johann Wilhelm Ritter, dem zufolge ein beständiger Galvanismus den Lebensprozeß im Thierreich begleitet (Müller 1844, S. 543; zu Ritters Position vgl. etwa: Weber, Heiko: J. W. Ritter und J. Webers Zeitschrift „Der Galvanismus“. In: Naturwissenschaften um 1800. Wissenschaftskultur in Jena-Weimar. Hg. O. Breidbach, P. Ziche. Weimar 2001, S. 216-247.). Darauf führt er aus: „Diese Verhältnisse, welche wir in der Ausübung der Muskelbewegung, des Geschlechtstriebs, der Geistesfunctionen täglich kennen lernen, finden auch bei der unmittelbaren Anwendung der Reize auf die Nerven statt“ (Müller (1844) 544).

[67] Müller (1844) 546.

[68] Narkotika werden in der Wirkung analog der Wirkung chemischer Reizmittel beschrieben (Müller (1844) 547) – Örtliche Vergiftung würde nicht durch die Nerven, sondern durch das Blut induziert („Allein es lässt sich auch beweisen, dass die allgemeine Wirkung der Gifte erst wieder vorzugsweise durch die Centralorgane des Nervensystems bedingt ist, welche das vergiftete Blut narkotisiert“ (Müller (1844) 549)).

Müllers Argumentationsstategie

Die Strategie seiner Argumentation, d. h. die Form in der er seine Argumentationslinien darstellt, zeigt sich etwa in der Passage zu „dem wirksamen Prinzip der Nerven“: Müller beginnt mit einer kurzen Skizze der bisherigen Aussagen der Physiologie zu der Frage nach dem Mechanismus der Reizleitung im Nervengewebe. Die Alten, so referiert Müller, fanden die organisierten Teile beseelt,[69] neuere Vorstellungen faßten die Nerven als eine Art elektrischen Apparat. Schließlich vermochte Alessandro Volta (1745-1827) dann die elektrische Natur der galvanischen Erscheinungen zu beweisen. Damit ist das Reaktionsprinzip benannt, das Müller nun in seiner experimentellen Analyse der Reaktionen des Nervensystems zu identifizieren hat, um entsprechend seine These, derart die Reaktionen des Nervensystems in einer Nervenphysik beschreiben zu können, auch zu belegen. Dazu gilt es, die Grundreaktionselemente im Nervensystem zu identifizieren und ihre Funktionalität aufzuweisen. Diese demonstriert Müller nun nicht in einer prinzipiellen Erörterung, sondern in einer minutiösen Darstellung der Befundsituation seiner experimentellen Wissenschaft. Hierzu werden die Teilbefunde in einer Abfolge von sich schlüssig aneinander reihenden Argumenten präsentiert. Es werden also Einzelphänomene beschrieben: „Die Nerven bleiben auch im gänzlich mortificirten Zustande wie alle nassen thierischen Teile, Leiter des Galvanismus, während sie die Fähigkeit, Contractionen der Muskeln zu verursachen, verloren haben.“[70] Dieser Befund zeigt, dass die Nervenfunktionen nicht einfach als Reaktionen von elektrischen Leitern zu verstehen sind. Sie zeigen galvanische Eigenschaften, diese Eigenschaften reichen aber nicht, die Nervenfunktion – Kontraktionen der Muskulatur zu induzieren – zu erklären. Müller führt nun eine Theorie an, die versucht, diese Diskrepanz zwischen der Wirkung eines galvanischen Leiters und eines Nerven zu erklären: Indessen würden auch diese Unterschiede bei der Hypothese von der Identität der Elektricität und Nervenkraft erklärbar seyn, wenn man – wie Fechner (Biot´s, Experimentalphysik, Bd. III) die Nervenfäden als von isolierenden Hüllen umgeben ansieht ...“.[71] Im Anschluss an diese Bemerkung referiert Müller nun neue, eigene Experimente, die das Phänomen der Nervenleitung weiter konturieren: Mit Entdeckung des Elektromagnetismus habe man neue galvanische Instrumente entwickelt, mit denen bewiesen werden sollte, „daß Nadeln, welche man in die Nerven eines lebenden Thieres sticht, magnetisch werden ... Mir hat dies nie gelingen wollen. Dagegen wird die Nadel eines sehr empfindlichen Galvanometers durch Froschschenkelpräparate afficirt, wie die Versuche von Nobili[72] und Matteucci[73] zeigen ...“.[74] Die Nerven produzieren also etwas, was der Reaktion eines galvanischen Elementes entspricht. Müller sucht nun dieses Phänomen eingehender zu charakterisieren, indem er die Experimentalbedingungen, unter denen dieses Phänomen produziert wurde, weiter

69 Müller (1844) 553.

70 Müller (1844) 555.

71 Müller (1844) 555.

72 Leopoldo Nobili (1784-1835).

73 Carlo Matteucci (1811-1868).

74 Müller (1844) 556.

spezifiziert: „Ein durch das Galvanometer nachweisbarer Strom entsteht, wenn der künstliche Querschnitt eines Muskels mit seiner Oberfläche, d. h. seinem Längsschnitt, durch einen Bogen in leitende Verbindung gesetzt wird ... Die Erscheinungen hören auf, sobald in den thierischen Teilen Zersetzung eingetreten ist".[75] Damit hat Müller dargelegt, dass das Muskelgewebe nicht generell, sondern in einer genauer zu definierenden Substruktur, in den durch den Querschnitt freigelegten Teilen, eine entsprechende Reaktion produziert. Diese Erscheinung ist zudem nicht daran gebunden, dass der Muskel im Gesamtgewebe des Tieres verbleibt. Es ist also eine Eigenheit in der funktionellen Organisation des Muskels, die diesen Effekt produziert. Diese ist zudem nur zu messen, wenn das Gewebe seine spezifischen strukturellen Eigenheiten erhält. Löst sich diese Gewebestruktur auf, verschwindet der Effekt. Es sind also nicht einfach die im Muskel zu findenden Einzelelemente, die die entsprechende Reaktion hervorrufen, vielmehr ist die Reaktion Effekt eine ganz bestimmten Struktureigenheit dieses Gewebes, also einer spezifischen Reaktionsschichtung der charakterisierten Grundelemente des tierischen Gewebes. Entsprechend folgert Müller: „Aus diesen Tatsachen ergiebt sich, dass die Röhren der Muskelbündel und der Nervenfäden gegen den Inhalt derselben sich in einer elektrischen Spannung oder Polarität befinden, und dass diese Spannung an ihre lebendige Integrität gebunden ist.“[76] Eine eingehendere Aussage über die spezifische Organisation der Reaktionselemente kann Müller bei dem ihm vorliegenden Stand der experimentellen Analysen nicht ziehen. Entsprechend offen bleibt Müllers Schlussfolgerung: „Bis so weit berechtigen die Elektricitätsphänomene an den Muskeln und Nerven noch nicht zu einer Identificirung des Nervenprincips und der Elektricität.“[77] „Wir müssen daher anerkennen, dass die Identität des Nervenprincips und der Elektrizität nichts weniger als erwiesen ist.“[78] Müller zieht nun keine weitere über den experimentellen Befund hinausgehende Schlussfolgerung. Er verbietet sogar explizit, sich in der weiteren Argumentation von der insoweit dargestellten Faktenlage zu lösen: „Aber wir dürfen auch nicht weiter gehen.“[79] Das heißt also: „Ueber die Natur des Nervenprincips ist man eben so ungewiss, wie über das Licht und die Elekticität“.[80] Zugleich aber ist damit ein Weg für die weitere Analyse gewiesen. Es gilt doch den Phänomenbereich weiter zu charakterisieren: „Aber die Eigenschaften und Bewegungserscheinungen dieser Principien lassen sich gleichwohl mit Erfolg studiren.“[81]

Damit ist in diesem Detail die Argumentationsstrategie Müllers aufgewiesen. Er berührt in seiner Darstellung ein theoretisch brisantes Problem, das er aber nicht theoretisch diskutiert, sondern in einer minutiösen Darstellung seiner Experimentalresultate aus dem Rahmen einer theoretisch geführten Diskussion löst und auch im Argumentations-

75 Müller (1844) 557.
76 Müller (1844) 557.
77 Müller (1844) 557.
78 Müller (1844) 558.
79 Müller (1844) 558.
80 Müller (1844) 558
81 Müller (1844) 558.

gang seiner Gedankenführung an eine Experimentalpraxis bindet.[82] Damit ist die Gedankenführung für diesen Physiologen vereinfacht. Seine Physiologie folgt dem Experiment. Argumente sind für die Physiologie in Experimentalbeschreibungen zu übersetzen. Nur auf deren Grundlage sind die Aussagen der Physiologie zu formulieren. Damit ist sein Begriff eines Reaktionsprinzips dargelegt. Ein Reaktionsprinzip ist die im Experiment offen gelegte Reaktionseinheit, in der sich ein physiologischer Prozess darstellen lässt. Wie die Physik um 1850 nicht nach der Essenz des Lichtes fragt, sondern das Licht nur in seinen Erscheinungen studiert, sei auch eine Physiologie zu konzipieren. Es geht eben auch dieser Physik nicht um eine Wesenscharakteristik des Lichtes, ihre Zielstellung ist zunächst eine Optik, die Darstellung der Gesetzmäßigkeiten des ihr greifbaren Phänomenraums. Und genau in diesem Sinne wird dann auch für Müller die Physiologie zur „Optik". Der Ansatz der Nervenphysiologie als Nervenphysik wird von Müller bis in den Aufbau seiner Argumentation umgesetzt.[83]

Reflex

Dies sei an einem zweiten Beispiel im Aufweis eines, wie Müller noch 1844 schreibt, „der wichtigsten Probleme der Physiologie" dargestellt. Wieder beginnt er mit einer Darstellung des Forschungsstandes der Reflexlehre, in der über eine Analyse der Leitungscharakteristika des Nervengewebes dessen funktionelle Organisation entschlüsselt werden sollte:[84] „Charles Bell", so skizziert er, "hatte zuerst den ingeniösen Gedanken, dass die hinteren, mit einem Ganglion versehenen Wurzeln der Spinalnerven der Empfindung allein, die vorderen Wurzeln der Bewegung vorstehen ... Allein Magendie hat das Verdienst, diesen Gegenstand hinsichts der Rückenmarksnerven in die Experimentalphysiologie eingeführt zu haben."[85] Müller leitet nun seine Argumentation über zu eigenen Experimenten. Allein in solch einer experimentellen Darstellung kann für ihn die Frage nach den physiologischen Grundlagen der Reflexfunktionen entschlüsselt werden: So „... trieb mich die Begierde nach Wahrheit an, eine Reihe neuer Versuche nach einem veränderten Plane an Kaninchen anzustellen."[86]

Problematisch ist, seiner Bewertung zufolge, dass die in der Literatur beschriebenen Präparationen sehr kompliziert waren. Entsprechend undurchsichtig war damit die Interpretation einzelner Versuche, in denen nicht völlig klar wurde, welche Teile des Gewebes in der Versuchsanordnung gereizt wurden und welche genauen Effekte die Reizung

[82] Die damit eingegangenen methodologischen Vorgaben wären vor dem Hintergrund dieser zunächst nur beschreibenden Darlegung des Argumentationsganges von Müller separat zu diskutieren.

[83] Vgl. Mendelsohn, Everett: Physical Models and Physiological Concepts. Explanation in Nineteenth Century Biology. Brit. J. Hist. Sci. 2 (1965) 201-219.

[84] Die Darstellung folgt hier nur der Gedankenführung Müllers zu dem Problemkontext der Reflexlehre sei nur folgende Texte verwiesen, die weiterführende Literatur erschließen Brazier, Mary A. B. A History of Neurophysiology in the 19th Century New York 1988; Clarke, Edwin, Jacyna, L.S.: Ninetheenth-Century Origins of Neuroscientific Concepts. Berkeley 1987.

[85] Müller (1844) 558f.

[86] Müller (1844) 560.

hatte. Es fehlte in diesen Darstellungen ein klares Versuchkonzept. Hier wurde das Rückenmark lebender Säugetiere freigelegt, dabei war das Tier nicht narkotisiert. Dieser Eingriff ist massiv. Das Tier ist schwer verletzt. Die dann an dem noch lebenden Tier studierten Reflexe sind somit nur sehr schwer zu interpretieren, schließlich ist nicht zu erwarten, dass ein derart massiv geschädigtes Tier noch normal reagiert: „... Wie kann man daher in der kurzen Zeit, wo ein Thier nach der Öffnung des Rückenmarks noch lebt, zuverlässig entscheiden, ob das Thier noch Empfindung hat oder nicht?" Müller verwirft denn auch derartig unpräzise Arbeiten.[87] Er experimentiert demgegenüber mit Fröschen, die auch nach Freilegung des Rückenmarks noch „ganz munter" erscheinen. In seinem Text folgt auf diese wertende Darstellung der bisherigen Experimente eine minutiöse Beschreibung der mit seiner Präparation erarbeiteten Befunde. Studiert werden die Effekte auf Reizung verschiedener efferenter und afferenter Nerven. Dabei variiert er die Reizmethode. Er beschreibt die Reaktionen auf verschiedene Arten von mechanischer Reizung.[88] Daraufhin versucht er nun die Nerven mit galvanischen Elementen (einfache Zink. Und Kupferplatten) zu reizen. Dazu schreibt er:

Ich hatte erwartet, wenn auch die hinteren Wurzeln bloss empfindend sind, sie doch fähig wären, das galvanische Fluidum bis zu den Muskeln zu leiten, und es ist sogar unvermeidlich, dass bei heftigem galvanischen Reize einer sehr starken Säule das galvanische Fluidum durch die hinteren Wurzeln so gut, wie durch jede thierische Substanz geleitet wird (...). Allein es ist ganz gewiss, dass der galvanische Reiz eines Plattenpaares durch die hinteren Wurzeln nicht auf die Muskeln wirkt, durch die vorderen sogleich Zuckung erregt.[89]

Diese Versuche wurden wiederholt und – wie er zusätzlich schreibt – seit „lange regelmässig in Vorlesungen gezeigt ... Gleichen Erfolg hatte die Wiederholung durch Thomson, Retzius und Stannius". [90] Beschrieben wird also ein neuer Effekt. Müller verweist nicht auf eine einmalige Beobachtung. Für ihn ist das Kriterium der Wiederholbarkeit in seinem eigenen und im Labor von Kollegen ausschlaggebend dafür, eine Beobachtung als gesichert darstellen zu können.

An diese Beschreibung des im Experiment Aufgewiesenen schließt sich aber nun keineswegs eine umfassende Schlussfolgerung an. Müller offeriert schlicht einen neuen Befund. Auf ihn folgt keine umfassende Kommentierung. Müller setzt vielmehr noch einmal die Experimentalbedingungen auseinander und fasst die Beschreibung der einzelnen Experimente wie folgt zusammen.

Wir sehen daraus [schreibt Müller] dass das Prinzip der Nerven bei diesen Strömungen und Schwingungen die kürzesten Wege nimmt, um von Empfindungsfasern durch das Rückenmark

87 Hier zeigt sich Müllers Bemühen um eine Standardisierung von Versuchsansätzen, in denen Artefakte durch eine sorgfältige Beobachtung von Randbedingungen der Versuche und durch eine entsprechende Wahl von Versuchsobjekten möglichst einzugrenzen sind.

88 Müller (1844) 561.

89 Müller (1844). 561.

90 Müller (1844). 561; das Problem einer Standardisierung von Laborergebnissen in der ersten Hälfte des 19. Jahrhunderts, das insbesondere auch vor dem Hintergrund einer zunächst vornehmlich qualitativen Messung zu diskutieren ist, bleibt im vorlegenden, allein dem Referat Müllers folgen Text unberührt.

auf Bewegungsfasern zu wirken; gleichwie die Elektricität auch den kürzesten Weg von einem zum andern der genäherten Poldräthe nimmt….Richtiger ausgedrückt und in die Sprache der Nervenphysik übersetzt, heisst dies jedoch so, dass bei heftiger Erregung der motorischen Eigenschaft des Rückenmarkes durch einen Empfindungsnerv zunächst nur derjenige Teil des Rückenmarks erregt wird, und wieder Zuckungen erregt, welcher dem Empfindungsnerven den Ursprung giebt, und dass die Erregung anderer Teile des Rückenmarkes und der davon entspringenden motorischen Nerven in dem Maasse abnimmt, als sie sich von der durch den Empfindungsnerven erregten Stelle entfernen. Dasselbe gilt auch von den Hirnnerven.[91]

Die von ihm eingeführte Sprache der Nervenphysik ist die Sprache des Experimentators. Diese Sprache ist die einer minutiösen Darstellung der Regelmäßigkeiten in den einzelnen genau definierten Experimentalszenarien. Sein Experiment führt ihn nicht zu einem abstrakten Modell, sondern zu einer präzisen Beschreibung, in der die Reaktionen auch in ihrer relativen Intensität dargestellt werden. Variationen in den Reaktionen der Gewebe werden auf hinsichtlich Reizort und Reizart präzise beschriebene Eingriffe bezogen. Damit kommt Müller – wie auch schon in der vorab skizzierten – Darstellung der am Muskel zu studierenden galvanischen Effekte zu detaillierten Aussagen über die Effekte von Variationen in der im Experiment aufgebauten Reaktionsschichtung. Die Gesetzmäßigkeiten, die er aufweist, benennen die Regularitäten, die sich in diesen Variationen darstellen lassen. Damit identifiziert Müller die physiologisch effektiven Momente im Reaktionsgefüge der ihn interessierenden Gewebe.

Die Grenzen der Physik

Wieweit trägt aber nun eine entsprechende Argumentationsstrategie? Wie eingangs referiert, sind für Müller auch Geistesfunktionen physiologisch darstellbar. Sind also auch die Seelentätigkeiten – um Müllers Begriff zu nutzen – derart in einer physikalisch-chemisch zu explizierenden Darstellungsschichtung zu orten? Es mag dabei schon überraschen, dass 40 Jahre nach dem vermeintlichen Ende der Suche vom Seelenorgan überhaupt ein solcher Begriff wieder auftaucht.[92] So schreibt Müller Von den Kräften des Gehirns und von den Seelenthätigkeiten im Allgemeinen:

„Das Gehirn der Thiere vergrößert sich von den Fischen bis zum Menschen, nach der Entwicklung der intellektuellen Fähigkeiten mehr und mehr.“[93] Dabei verweist Müller auf Unterschiede in verschiedenen Hirnteilen, stellt die sich damit andeutende Frage nach der „Kraft der verschiedenen Hirntheile“ aber zunächst zurück, um „das Verhältniss der Seelenthätigkeit zu dem Gehirn überhaupt“ zu betrachten.[94] Er spricht nun ex-

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91 Müller (1844) 619.

92 Hagner, Michael: Das Ende vom Seelenorgan: Über Einige Beziehungen von Philosophie und Anatomie im frühen 19. Jahrhundert, In: Das Gehirn – Organ der Seele? Hg. E. Florey, O. Breidbach. Berlin 1993, S. 9-22.

93 Müller (1844) 708f.; vgl. hierzu die in romantischer Tradition stehende Arbeit Carus, Carl Gustav: Vergleichende Psychologie oder Geschichte der Seele in der Reihenfolge der Thierwelt. Wien 1866.

94 Müller (1844) 709.

plizit vom Sitz der Seelenfunktion, bemüht Analogien um diese Verortung des Seelischen, und greift auch hier auf Beobachtungsdaten zurück, wenn er schreibt: „Der Lungenkranke verliert nichts von seinen Seelenkräften trotz der gänzlichen Zerstörung seiner Lungen“[95], ganz anders sei es eben beim Hirn. Dabei gilt, „dass der Sitz der Seelenwirkungen im Hirn und in keinem andern Theile ist, dass die Nerven diese Wirkungen anregen und vermöge ihrer Kräfte ausführen ... ist damit nur bewiesen, dass die Seele durch die Organisation des Gehirns wirkt und thätig ist; es ist aber nicht damit behauptet, dass ihr Wesen bloss seinen Sitz im Gehirn hat. Es könnte wohl sein, dass die Seele nur in einem Organe von einer bestimmten Structur wirken und Wirkungen empfangen könnte, und doch vielleicht allgemeiner im Organismus verbreitet wäre“.[96] Müller bleibt auch hier abwägend. Auch in seiner Analyse des Seelenvermögens bleibt er deskriptiv.

Nur in einem Punkt zeigt sich die Dimension dieser Ordnung des Denkens, in der dann auch das Motiv seiner Strukturierung deutlich wird. Das Kantsche Reich der Zwecke, das sich im Organismus expliziert, ist für Müller nicht einfach eine Formel zu einer optimalen Beschreibung des Organischen.[97] Die Struktur, die sich hier greifen lässt, hat für Johannes Müller auch noch 1844 eine eigene Dignität:

Wenn es einen wahren Grund für die Ansicht giebt, dass das psychische Leben auch nur eine Art der Manifestation des Lebensprincipes der thierischen Wesen ist, so ist es der, dass beiderlei Wirkungen der Ausdruck der Vernunft sein können.[98]

Die Vernunft des Physiologen

Hier gerät die Physiologie an ihre Grenzen. Die Gesetzmäßigkeit erweist sich als die Explikation von etwas, was nur in seinen Phänomenen, aber nicht an sich darstellbar ist.[99] Die Methodik der Physiologie erlaubt es, Phänomene der Lebensprozesse darzustellen; sie charakterisiert Wirkungen, aber sie charakterisiert nicht, was das Leben ist. Dieser Verzicht auf einen umfassenden Erklärungsansatz macht diese Physiologie frei für die Neustrukturierung der von ihr erfassten Phänomene. Deren Ordnungszusammenhang bezieht sich für diese Physiologie nicht mehr auf eine vor und außer dem Experiment (im Sinne eines Kantschen Apriori) stehenden Theorie. Diese Physiologie operiert pragmatisch und erschließt sich in ihrer Operation den Datenbestand, über den sie ihre Aussagen gewinnt. Dies ist anders als das Ordnungsdenken einer zeitgleichen Naturgeschichte, die etwa bei Lorenz Oken oder Burdach die Einzelaussagen der Physiologie in einen

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95 Müller (1844) 710.

96 Müller (1844) 713; „Ob das Lebensprincip und das psychische Prinzip von dem Gehirn aus in einem latenten Zustande, auf den Wegen der Nerven zum Samen oder Keime gelanget ... oder ... alles dieses ist nicht zu beantworten“ (Müller (1844) 715).

97 Vgl. Rheinberger, Hans-Jörg: Zum Organismusbild der Physiologie im 19. Jahrhundert: Johannes Müller, Ernst Brücke, Claude Bernard. Med. Hist. J. 22 (1987) 342-351

98 Müller (1844). 717.

99 Vgl. Baatz, U.: Die Sinne und die Wissenschaften. Zur Erkenntnistheorie bei Johannes Müller und Ernst Mach. In: Johannes Müller und die Physiologie. Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 255-274.

übergreifenden, in dieser Disziplin nicht mehr zu begründenden Ordnungszusammenhang stellt.

Gerade in dieser Abgrenzung zu den zeitgleichen naturphilosophisch begründeten Ordnungsmustern der Naturforschung gewinnt Müllers Konzeption einer Physiologie für das 19. Jahrhundert ihre über seine Disziplin hinaus wirkende Bedeutung. Die von Müller minutiös entfaltete Strategie, das Experiment selbst als Maßstab der erfahrungswissenschaftlichen Argumentation zu nehmen, befreit ihn von der naturphilosophischen Reflexion, unter der die Befunde der Erfahrungswissenschaften – wie etwa bei Oken – zwar systematisierbar waren, zugleich aber immer Prinzipien in der Argumentation mitgeführt wurden, die in der Erfahrungswissenschaft selbst nicht zu thematisieren waren, sondern dieser schlicht vorausgesetzt wurden. Erst der konsequente Verzicht auf eine derartige fundierte Prinzipienlehre erlaubt es, im Experiment Erfahrungsräume zu explorieren und nicht nur immer nach dem zu suchen, was schon vorab bestimmt worden war. Damit fundierte Müller eine moderne, experimentell ausgerichtete Physiologie. Diese Strategie führte aber dann dort an Grenzen, wo in den experimentell einzuholenden Erfahrungsraum die interessierenden Phänomene nur in Teilbereichen abbildbar waren. Akzeptabel waren dieser Physiologie nur die Beobachtungszusammenhänge, die sie in dem ihr verfügbaren Instrumentarium experimentell nachstellen konnte. Die Einschränkung auf eine sich derart in einer auch apparativ konsolidierten Praxis bewegenden Physiologie markierte zugleich aber auch die Stärke ihres Vorgehens. War es so doch möglich, die Analyse von Lebensfunktionen auf breiter Front in ein Format zu bringen, in dem nun auch die innerhalb der Methodik zu beantwortenden Fragen zu formulieren waren.[100] Dies ist bekannt, dieser methodische Pragmatismus kennzeichnet für die Wis-

[100] Vgl. Breidbach, Olaf: Die Materialisierung des Ichs. Frankfurt 1997. Problematisch wurde dieses pragmatische Vorgehen, in dem die grundsätzlichen Fragen der Physiologie, wie sie noch Burdach in seiner Anthropologie formuliert, auf die experimentell lösbare Problemansätze zurückgefahren werden, dann, wenn solch ein pragmatischer Ansatz ontologisiert wurde. Das heißt, wenn die Problemansätze von vornherein auf eine Fragestellung reduziert wurden, die mit den Methoden der Physiologie einholbar war; und damit alternative Beschreibungsansätze, wie der einer nicht physiologischen Psychologie grundsätzlich abgelehnt wurden. In diesem Moment hatte sich der pragmatische Ansatz einer bei Müller noch explorativ zu verstehenden experimentellen Physiologie dogmatisiert. Damit wurde diese Physiologie zu einer Weltanschauung. Inwieweit dies auch die Konsequenz einer innerdisziplinären und nicht allein nur einer umfassender zu zeichnenden wissenschaftskulturellen Entwicklung war, muss im Rahmen dieses Aufsatzes offen bleiben. Mit der entsprechenden Dogmatisierung der empirischen Methode, wie sie nach 1870 sowohl Emil Du Bois-Reymond wie auch Ernst Haeckel (1834-1919) in explizitem, aber keineswegs unproblematischen Bezug auf Müller zelebrierten, wurde der explorative Charakter des experimentellen Argumentierens verlassen, das für Johannes Müller charakteristisch war. Du Bois-Reymond formulierte in seinem Ignorabimus ein weltanschauliches Programm, das in der physiologischen Methodik ansetzend, doch weit über den in ihr formulierten Rahmen hinausweist. Haeckel nahm diesen ‚Ball' dann, zwar inhaltlich in eine andere Richtung gelenkt, aber formal durchaus auf einer vergleichbaren Ebene des Argumentierens, in seinem monistischen Programm auf. Das Resultat – Haeckels „Welträthsel" – brach dann auch gänzlich aus dem Müllerschen Wissenschaftsverständnis heraus und endete letztlich denn auch im Postulat einer Monistischen Religion. Die Akkuratesse der Müllerschen Argumentation wurde in solch einer weltanschaulichen Vereinnahmung verlassen. Gesichert wurde ein entsprechendes Weltbild nicht mehr in der Analyse der Fakten, Sicherheit gab vielmehr der weltanschauliche Konsens innerhalb der physiologischen Schulen. Eine in dieser Perspektive akzentuierte Geschichte der durch Müller fundierten Physiologie wäre aber noch zu schreiben.

senschaftsgeschichte eine der entscheidenden Innovationen des Müllerschen Ansatzes.[101] Die Bedeutung solch eines methodisch geführten Pragmatismus wurde denn auch schon zum Kernansatz des Du Bois-Reymondschen Postulats einer im 19. Jahrhundert gewonnenen Neukonturierung des naturwissenschaftlichen Denkens.[102] Für die Geschichte der der Physiologie des 19. Jahrhunderts hatte diese Bedeutung der Methodik auch zur Konsequenz, das die Geschichte dieses Faches über weite Phasen eben auch als eine Instrumentalgeschichte zu schreiben ist.[103] Dabei konstituiert diese Methodik – wie die vorliegende Studie zeigt – nicht nur eine apparative, sondern auch eine argumentative Praxis. Über die sich im Horizont des experimentell Darstellbaren bewegende sichert sich diese Physiologie gegenüber dem theoretisch weit ausgreifenden, naturphilosophisch bestimmten Erklärungsansatz der Naturgeschichte sensu Oken.

Müller führt seine Auseinandersetzung auch schon in der Anlage seines Argumentationsaufbaus von der theoretisch bestimmten Diskussions- und Darstellungsebene weg. 1800 hatte Schelling in seiner Darstellung eine neue Wissenschaft der Natur eingefordert, die er als Naturwissenschaft bestimmte.[104] Seine Naturwissenschaft suchte im einzelnen Naturphänomen das Ganze der Natur einzubinden. Diesen Ansatz finden wir bei Forschern wie Oken und Burdach aufgenommen.[105] Müller schließt diese umfassende Rückbindung an ein Naturverständnis für die sich in den Einzelheiten bewegende Lehre von den Funktionszuständen der organischen Materie aus. Er bindet seine Argumentation an das Experiment. Dabei illustriert sich in seinen Experimenten nicht einfach eine Idee, die dann in mehreren Vorstellungen detailliert und im Experiment erläutert wird.[106] Seine Argumentation wird vielmehr durch das Experiment geführt und an Hand des Experimentes expliziert. Die Argumentationsstrategie – die Theorie im Experiment aufzuzeigen und auf das im Experiment Explizierbare zu reduzieren – wird dann auch zum Motor einer Gedankenführung. Es wird noch zu analysieren sein, inwieweit sich in dieser Strategie der Pragmatismus eines Ausprobierens selber in den dann im Weiteren nicht mehr eingeholten methodischen Vorgaben einfängt. Auch wird noch zu zeigen sein,

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101 Lohff, Brigitte: Johannes Müller und das physiologische Experiment. In: Johannes Müller und die Philosophie Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 105-124.

102 Vgl. Wollgast, Siegfried: Einleitung des Herausgebers. In: Emil Du Bois-Reymond: Vorträge über Philosophie und Gesellschaft. Berlin 1974, S. V-LX.

103 Vgl. Brain, Robert M.: Representation on the Line: Graphic Recording Instruments and Scientific Modernism. In: From Energy to Information: Representation in Art, Science and Literature. Hg. B. Clarke, L. D. Henderson. Stanford 2002, S. 155-177.

104 Entsprechend differenziert ist denn auch sein naturphilosophischer Ansatz zu rekonstruieren, zur Diskussion dieser Thematik vgl. Breidbach, Olaf: Schelling und die Erfahrungswissenschaften. Sudhoffs Archiv (2004) 88, S. 153-174.

105 Zu dem Gesamtkomplex dieser Auseinandersetzung mit einem umfassenden Naturkonzept vgl. Gregory, Friedrich Nature Lost? Natural Science and the German Theological Traditions of the Nineteenth Century. Cambridge 1992; Poggi, Stefano: Il genio e l'unità della natura. La scienza della Germania romantica (1790-1830). Bologna 2000; Richards, Robert J.: The Romantic Conception of Life. Science and Philosophy in the Age of Goethe. Chicago & London 2002; Bach, Thomas, Breidbach, Olaf (Hg.) Naturphilosophie nach Schelling. Stuttgart _ Bad Cannstatt 2003, im Druck.

106 Vgl. Lohff, Brigitte: Johannes Müller und das physiologische Experiment. In: Johannes Müller und die Philosophie. Hg. M. Hagner, B. Wahrig-Schmidt. Berlin 1992, S. 105-123.

inwieweit diese Art, das Experiment zu nutzen, um 1830 wirklich innovativ ist. Erste Analysen zeigen die Argumentationen Wilhelm Ritters in Teilbereichen in einer ganz ähnlichen Weise ans Experiment gebunden; wobei sich Ritter aber nur in Phasen seiner Argumentation aus seinem ihn insgesamt leitenden theoretischen Grundkontext ausblendet.[107] Damit wird insbesondere die Frage nach der Konsequenz der methodisch am Experiment entlang geführten Argumentationslinie Müllers bedeutsam. In der vorliegenden Studie wurde aufgewiesen, in welcher Konsequenz – sowohl im Detail wie auch in der Gesamtkonzeption seiner Gedankenführung – Müller in seinem Lehrbuch dieser Art des Argumentierens treu bleibt. Dass Müller in dieser Art der Darstellung auf eine explizite theoretische Auseinandersetzung verzichten kann, seine Argumentation vielmehr auf die Möglichkeiten des im Experiment Darstellbaren beschränkt, bildet die Stärke dieser Argumentation, die damit dann allerdings auch an ihre methodischen Einschränkungen gebunden bleibt. Im Vergleich zu dem nahezu zeitgleichen Lehrbuch des Botanikers Schleiden, der ebenfalls eine große Bedeutung für die Konsolidierung des experimentell geleiteten naturwissenschaftlichen Denkansatzes im deutschen Sprachraum gewann, wird dies noch einmal besonders deutlich.[108] Schleiden setzt in seinem Lehrbuch dezidiert wissenschaftstheoretisch an. Seine induktiv analytische Perspektive gewinnt er ausgehend von einem deduktiven Ansatz.[109] Müller ist hier in der Konsequenz seines Vorgehens zumindest für den Deutschen Sprachraum um 1840 singulär. Eine Müller folgende Ideengeschichte der Physiologie würde in dem skizzierten Sinne zu einer Experimentalgeschichte. Dies geschieht damit in einer Phase der Physiologieentwicklung, in der das Experiment im Wesentlichen noch qualitativ beschrieben wird. Nicht erst die Messung, sondern vielmehr diese hier skizzierte konsequente Umschichtung einer Argumentation von der Theorie auf das Experiment verhilft der neuen naturwissenschaftlichen Sichtweise in den Biowissenschaften zum Durchbruch.[110] Das quantifizierende Experiment steht nicht am Beginn der neuen sich induktiv verstehenden Wissenschaftskonzeption. Eine durch das Experiment geführten Wissenschaftskonzeption entwickelt sich in den Biowissenschaften schon vor Etablierung einer quantifizierenden Messung.

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[107] Weber, Heiko: J. W. Ritter und J. Webers Zeitschrift „Der Galvanismus". In: Naturwissenschaften um 1800. Wissenschaftskultur in Jena-Weimar. Hg. O. Breidbach, P. Ziche. Weimar 2001, S. 216-247.

[108] Vgl. Breidbach, Olaf: Zur Anwendung der Friesschen Philosophie in der Botanik Schleidens. In: Jakob Friedrich Fries. Philosoph, Naturwissenschaftler und Mathematiker. Studia Philosophica et Historia 25. Hg. W. Hogrebe, K. Herrmann. Frankfurt 1999, S. 221-242; Breidbach, Olaf. Einleitung. In: Schleiden, Matthias Jacob: Grundzüge der Wissenschaftlichen Botanik. Hildesheim 1998, S. 1*-27*.

[109] Schleiden selbst tituliert sein Lehrbuch im Untertitel als Einführung in eine induktive Botanik, diese induktive Botanik arbeitet mit dem Mikroskop und setzt die Detailsicht, die in diesem Sinne rein deskriptiv zu verstehende Analysis gegen die spekulativ ansetzende Naturgeschichte. So führt denn auch diese induktive Botanik nach Schleiden das Mikroskop im Wappen. Die eingehendere wissenschaftshistorische Analyse muss diesen Begriffsgebrauch selbst wieder eingehender verorten. Von diesem Problem sehe ich in der vorliegenden Studie zunächst aber ab.

[110] Vgl. Hentschel, Klaus: Historiographische Anmerkungen zum Verhältnis von Experiment, Instrumentation und Theorie. In: Instrument – Experiment. Historische Studien. Hg. C. Meinel. Berlin 2000, S. 13-51.

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Charles Darwin's Moral Sense – on Darwin's Ethics of Non-Violence*

Eve-Marie Engels

Abstract
The overall aim of this article is to redress some of the deeply rooted and widely held prejudices against Charles Darwin's ethics and social theory. The topic is in particular Darwin's consideration of ethics as laid out most prominently in his book on the descent of man. By allowing Darwin to 'speak for himself', I hope to deprive simplified biologistic interpretations of their basis. Moreover, my aim is to present some other, surprising results. In contrast to widely held expectations connected with Darwin, I want to show first of all, that his ethics is not primarily a biological ethics, that it, secondly, does not constitute an evolutionary ethics, and that thirdly, it does not constitute what is called 'Social Darwinism'. The title of my article intends to emphasize how important Darwin thought it was for us to cultivate a moral sense and to refrain from violence in order to develop and preserve this moral sense. Although for explaining the possibility of natural selection among organisms Darwin drew on the population principle put forth by the British national economist Thomas Robert Malthus and adopted the idea of a "struggle for life" as the motor of natural selection, Darwin did not adopt the economist's theologico-metaphysical premises. For Malthus nature and its laws – in this case the population principle – are invested with a normative status whose recognition and description are at the same time the formulation of a norm prescribed by God. Darwin divorces Malthus' principle from its theologian and normative framework. This law serves him exclusively as a means for explaining the descent of species, not as a moral or ethical rule. This fact is significant for an adequate assessment of Darwin's ethics.

Introduction – the goals of this contribution

Since the appearance of Charles Darwin's work On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life (1st ed. 1859) and his Descent of Man, and Selection in Relation to Sex, published twelve years later (1st ed. 1871),[1] many different, in part contradictory expectations, apprehensions and prejudices have been associated with his name. What filled the one with hope, gen-

* This translation is a slightly modified version of my article entitled "Charles Darwins moralischer Sinn – Zu Darwins Ethik der Gewaltlosigkeit" in Julia Dietrich, Uta Müller-Koch (eds.): Ethik und Ästhetik der Gewalt. Paderborn: Mentis 2005. I thank Dr. Susan Nurmi-Schomers for her excellent translation.

[1] For the sake of simplicity, the following abbreviations will be used when Darwin's works are mentioned: Origin of Species, Descent of Man and, for The Expression of the Emotions in Man and Animals (1st ed. 1872) after the first complete citation, Expression.

erated worries and fears on the part of the other. Many believed to have finally found the key to the understanding of progress, for in their view, Darwin had revealed the mechanisms by which increasingly complex, higher organisms had evolved from lower forms of life until at last the highest form of life, the human being, came into existence. At the same time, a recipe for the improvement of humanity in the present and future seemed to have been found, for the conscious, targeted imitation of these mechanisms was expected to effect progress in the realm of ethics, politics and society. Darwin's theory has been seized upon in attempts to provide a foundation and justification for diverse political agendas and world views and prevailed upon to legitimize diametrically opposed political, socio-theoretical and ethical positions. Motivated by central concepts in his writings such as "natural selection", "struggle for life" and "survival of the fittest" – the latter term having been borrowed from Spencer – a certain pattern for interpreting Darwin's theory emerged which culminated in the neologism "Social Darwinism", connecting Darwin's name inextricably with a very questionable reading of his thought. The concept "Social Darwinism" is generally understood as signifying the demand for a rigorous application of the principle of the "survival of the fittest" in society and politics, which, taken to the extreme, is concomitant with an idolization of unscrupulous exertion of force. German translations of such concepts, e.g. "Kampf ums Dasein" for "struggle for life", have played a part in ossifying this notion and equating it with Darwinism per se. Even today, Darwin's tenets are not infrequently associated with such interpretations of his central concepts without those who cite him having any knowledge of his writings. In doing so they draw less on Darwin's theory than on their own image of him – partly generated in dependence on their own theoretical and practical goals.

The topic of my contribution is in particular Darwin's consideration of ethics as laid out most prominently in his book on the descent of man. By allowing Darwin to 'speak for himself', I hope to deprive simplified biologistic interpretations of their foundation. Moreover, my aim is to present some other, surprising results. In contrast to widely held expectations connected with Darwin, I intend to show first of all, that his ethics is not primarily a biological ethics, that it, secondly, does not constitute an evolutionary ethics, and that thirdly, it does not constitute what is called 'Social Darwinism'. The title of my article intends to emphasize how important Darwin thought it was for us to cultivate a moral sense and to refrain from violence in order to develop and preserve it.

One extreme interpretation of Darwin should not be replaced by another, however. Darwin may have encouraged certain Socio-Darwinist readings of his tenets through his choice of terminology and through isolated remarks in his works. In these we may find the expression of widely held 19th-century notions, they may constitute naively adopted positions from the documented research findings of others or reflect Darwin's personal opinions. In any case, it would be misguided to portray them as the essence of Darwin's ethics. My view on this issue is in part supported by discussions of Darwin's ethics from the 19th and early 20th century – i.e. discussions conducted in the wake of the formulation of his ideas – which have no widespread circulation today.

Darwin's elucidations on the development of social instincts and moral virtues, the role which affect and understanding play for moral action, etc. raise numerous questions which call for discussion in light of current research in sociobiology as well. The problem of moral relativism – raised, perhaps, by Darwin's theory – already disturbed numerous contemporaries. This is not the place to address such questions, however, as they extend beyond the scope of the collection of essays at hand. For similar reasons, only short mention can be made of Darwin's own transformation from a young physico-theologian to a critic of the notion of godly design in nature and the role which the problem of theodicy played for this development. Biographic particulars explain the existence of various "thought-styles" in Darwin's works and make seeming or real contradictions and inconsistencies in his line of argumentation comprehensible. It is also not possible to take into account the intensive and extensive reception of Darwin's theory during his lifetime, which Darwin responded to in successive editions of his works.[2]

In the first section of my article which now follows, I will begin with a short sketch of Darwin's theory of the evolution of species, this being indispensible for an understanding of his reflections on human morality. It will be necessary to illuminate Darwin's notion of nature – which separates him from the still prevalent physico-theology of his time –, the implications of Darwin's departure from this position for his ethics, as well as the various meanings which Darwin ascribed to the term "struggle for existence". In the second section, Darwin's ethics will be presented in the framework of his work on the descent of man. In the third section, the most important results of the investigation will be summed up and made fruitful for a short reflection on the general relationship between biology and ethics.

1. Darwin's "theory of descent with modification through variation and natural selection" – mechanisms of the formation of adaptations and of species

The first edition of Darwin's Origin of Species, which came out on November 24, 1859 numbering 1,250 copies, was sold out almost overnight, and in 1860, a second edition of 3,000 copies appeared. By 1872, Darwin had published six editions of this work. In 1876, a reprint of the sixth edition with minor additions and a few corrections came out. In his autobiography he can proudly announce that by the year 1876, 16,000 copies had been sold in England and that the book had been translated into almost every European language. As he reports, every year or two a catalogue or a bibliography on "Darwinism" was published in Germany (Darwin 1969, pg. 122f). In his autobiography, Darwin refers to this work as without a doubt the most important he ever wrote. Irrespective of numerous additions and corrections in the later editions, he maintains that it remained essentially unaltered.

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[2] Thus I cite my earlier studies (cf. bibliography) and my forthcoming Darwin monography, which contains an extensive chapter on Darwin's ethics. An expansive selective bibliography on Darwin's reception in the 19th century is to be found in my collection of essays (Engels 1995a, pp. 395-414). For an analysis of Darwin's theory by using the terminology of Ludwik Fleck's philosophy of science ("thought-style", "thought-collective" etc.) as a tool cf. Engels 1995b.

Darwin's rejection of the argument from design

Darwin's goal was to show that species emerged as the result of gradual transformations from other species. In propagating this theory, he hoped to overthrow the dogma of a separate creation for every individual species and in doing so to place biology on an empirical foundation, thus bringing to an end the "theological age" of natural history. As Darwin observed in Notebook N after reading a review of Auguste Comte's three-stage law, the zoology of his day was "purely theological" (Darwin in Barrett et al. 1987, pg. 566f).[3] By empiricizing biology, an explanation for the development of purposeful structures in animate nature, i.e. the adaptation of organisms to their life conditions, was to be provided.

In explaining the origin of the species and their adaptation to changing life conditions, Darwin dispenses with the notion of animate nature as being invested with immanent purposefulness guided by a final cause as well as with the premise of a godly intelligence, a designer, as the 'architect' of animate nature, this being the idea which representatives of Christian physico-theology (among them John Ray, William Derham and William Paley) subscribed to. They viewed purposefulness in nature, the functionality of inner organic structures and the adaptation of organisms to their environment, as the expression of godly design and creation. They strived to prove the existence of such harmony in every corner, taking signs of it as an indication, if not to say, proof of God's omnipotence, wisdom and benevolence (argument from design). The most eminent representative of physico-theology in the British academic realm and the foremost advocate of the argument from design put forth by this group was William Paley (1743-1805), whose works Evidences of Christianity and Moral Philosophy were on Charles Darwin's obligatory reading list during his study of theology at Cambridge; the knowledge of which was necessary to pass the exams for the Bachelor of Arts degree. From Darwin's autobiography we can infer that he read Paley's Evidences of Christianity (1794) as well as another work by the same author, Natural Theology; or Evidences of the Existence and Attributes of the Deity, collected from the Appearances of Nature (1802) attentively and enthusiastically (Darwin 1969, S. 59).

Paley's Natural Theology was the standard work on natural theology in the 18th and 19th centuries. In illustrating his theories, Paley drew many examples from anatomy and attempted to trace God's benevolence in every detail of the human body. His model for godly design was the clock, whose existence and functionality was dependent upon a clockmaker. As we read,

> "[t]here cannot be design without a designer; contrivance, without a contriver; order, without choice; arrangement, without any thing capable of arranging; subserviency and relation to a purpose, without that which could intend a purpose; means suitable to an end, and executing their office in accomplishing that end, without the end ever having been contemplated, or the means accommodated to it." (Paley 1802, S. 3)

[3] I am citing the edition of Charles Darwin's Notebooks 1836-1844 edited by Barrett et al. (1987). Apart from Darwin's works, these posthumously published Notebooks are just as indispensible for a comprehensive understanding of Darwin's thought as is Darwin's correspondence and his marginalia (Di Gregorio, Gill 1990).

As Paley contends, the "marks of design" are "too strong to be gotten over. Design must have had a designer. That designer must have been a person, That person is GOD." (Paley 1802, pg. 111).

In his Dialogues concerning Natural Religion, which appeared in 1779, not quite three years after his death, David Hume had already critically addressed the argument from design. According to J. C. A. Gaskin, the editor of Hume's Dialogues, the tenets of William Paley's Evidences of Christianity (1794) and his Natural Theology (1802) had already "been refuted by Hume in the Dialogues (1779) and elsewhere before they were even written" (Gaskin 1993, pg. IX).

Darwin's revolutionary and provocative theoretical achievement consists in his having replaced these physico-theological explanations by the illustration of purely natural processes and laws, explaining not only the emergence of purposefulness in animate nature, but also the evolution of new species on this basis. In doing so he orients himself towards already existent, observable parameters and conceives of the mechanisms which guide the development of species and adaptations using plant and animal breeding as his model, bringing into play the concepts "individual variation", "selection" and "inheritance". As he argues, breeders select those organisms of a species or race which are in possession of certain traits, these being important for the breeding purpose in question. They then see that these organisms reproduce in a controlled manner. In the course of a process of inheritance which extends over several generations, the traits selected by the breeders gradually begin to dominate or take on special characteristics in keeping with the breeder's intentions. According to Darwin, a mechanism analogous to that of artificial selection was to be found in nature, but here the individual traits served a purpose for the organisms themselves and their survival in a certain environment. Darwin drew conclusions on the basis of the observation that members of any one species always demonstrated individual differences or variations and thus also differing degrees of adaptation to the environmental conditions which they were subjected to. Those organisms which were better adapted in terms of what was necessary to survive, i.e. which were better equipped to adapt than were other members of the species, had greater chances of survival and could thus – statistically speaking – reproduce more successfully. In other words, a natural selection of those members who were better adjusted or better equipped to survive took place. In the course of a long process of gradual accumulation, their features became dominant traits in following generations through inheritance. In this way, new species ultimately evolved. In Darwin's theory, natural selection not only explained the extinction of certain species; what is more important, it fulfilled a constructive function which helped to explain the evolution of new species, with the biblical notion of godly creation being replaced by a scientific, i.e. biological explanation.

To explain such processes, an empirically verifiable mechanism immanent to nature had to be made out, however, which took the place of the breeder and performed the function of a motor of natural selection. Darwin called this mechanism "struggle for life" or "struggle for existence", and in doing so, he described a natural process which evidenced a mathematically formulated disproportion between the increase in means of subsistence and the tendential rate of reproduction of organisms. As he argued, the increase in food supply could not keep up with the rate of reproduction. Since the number of members in any one species remained more or less stable, Darwin posited that a mechanism in nature existed which guaranteed such stability. He termed it the "struggle for existence". In putting forth this notion, Darwin drew on the population principle put forth by the British national economist Thomas Robert Malthus, applying it like Malthus, who was referring to Benjamin Franklin, beyond human beings also to the entire realm of flora and fauna. According to Darwin, the mechanism manifested itself here "with manifold force" because plants and animals – unlike human beings – have no artificial food production nor do they practice self-restraint in denying themselves marital relations.

"Hence, as more individuals are produced than can possibly survive, there must in every case be a struggle for existence, either one individual with another of the same species, or with the individuals of distinct species, or with the physical conditions of life. It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms; for in this case there can be no artificial increase of food, and no prudential restraint from marriage." (Darwin 1859, pg. 63).

In this struggle for existence, Darwin argued, those members of a species who were better adapted to their life conditions were more likely to survive. In the course of time, hereditary varieties or races, subspecies and ultimately new species issued from individual variations. In keeping with his model of continuity, Darwin assumed that no sharp distinctions between individual variations, varieties, subspecies or species existed, however. He even believed it was possible, on the basis of his theory of the formation of new species through the accumulation of small, gradual changes, to corroborate the old principle of natural philosophy, namely that nature does not evidence any discontinuities – "natura non facit saltum" (Darwin 1859, pp. 194 and 471).

Although Darwin relies on Malthus in theorizing in this way, he does not adopt the economist's theologico-metaphysical premises. This fact is significant for an adequate assessment of Darwin's ethics. For this reason I will touch upon Malthus' population principle at the end of this section.

According to Darwin, individual variations of organisms are also subject to conditions dictated by the laws of nature, but he refers to them as "spontaneous" and "due to chance". In arguing in this way, Darwin intends to point out our far-reaching ignorance of the laws of variability. As he contends, the term "chance" is a completely incorrect concept which merely expresses our lack of knowledge concerning the causes of individual variations (Darwin 1859, pg. 131). In taking this view, he can do no more than formulate suppositions, however.[4] Although he looks upon natural selection as the most

[4] As Darwin is looked upon from a present-day perspective as one who critiqued and overcame Lamarck, it should be mentioned that Darwin's theory involving the assumption of the inheritance of individually acquired traits contains a Lamarckian element although he rejects Lamarck's notion that new organs are created by the efforts of "inner feeling" on the part of the organisms in question as well as his concept of progress. The laws of heredity laid down by Gregor Mendel in 1866 were ignored for decades and had to be rediscovered at the beginning of the 20th century. Classical and molecular genetics did not begin to develop

important motor which effects the transformation of species, he also takes other factors into consideration such as the inheritance of individually acquired traits, i.e. the inherited effects of the use and disuse of organs, the direct effects of external conditions on organisms, variations "which seem to us in our ignorance to arise spontaneously" and the laws of correlation and variation independent of purposefulness and thus also of natural selection (Darwin 1859 and later editions, chap. 5; Darwin 1876, chap. XV). Thus in Darwin's eye, evolution is the result of a complex dynamics of external life conditions and the internal structure of organisms which are subject to natural laws of various kinds (the law of natural selection, inheritance, variation, etc.), even though the effect of these is not always known or not yet known in individual cases. Darwin does not allow ignorance of the causes of variation to open the door to notions of godly intervention into nature, however. As his correspondence shows, he expressly rejects the idea that variation is pointed in a certain direction.

Darwin is undecided as regards the possibility of evolutionary progress. This becomes clear in the very first edition of his Origin of Species when he points out the difficulties connected with defining the concept of progress and different degrees of organization. On the one hand, he divorces himself from Lamarck's notions of this kind very early on. "Heaven forfend me from Lamarck nonsense of a 'tendency to progression' 'adaptations from the slow willing of animals'" (sic!) he writes in a letter to Joseph Dalton Hooker on 11 January 1844 (CCD 3, pg. 2). On the other hand, we find numerous indications of an assumption that organisms develop into higher-order organisms or even reach perfection in the course of evolution. In this vein, he writes in his concluding remarks that all "corporeal and mental endowments will tend to progress towards perfection" because "natural selection works solely by and for the good of each being" (Darwin 1859, pg.489). At the same time, Darwin unequivocally emphasizes that as yet, no natural scientist had been able to provide a satisfactory definition as to what is to be understood by "progress of organization" (Darwin 1876, pg. 103). This indecisiveness is an example for the co-existence of various thought-styles in Darwin's theory, to which the implicit belief in the validity of a theology of creation initially belonged. As Darwin later concedes: "I was not, however, able to annul the influence of my former belief, then almost universal, that each species had been purposely created; and this led to my tacit assumption that every detail of structure, excepting the rudiments, was of some special, though unrecognized, service." (Darwin 1877, pg. 65). Darwin hopes to have "done good service in aiding to overthrow the dogma of separate creations." (Darwin ibid.).

Apart from artificial and natural selection, Darwin speaks of sexual selection, by which he means the selection of individuals of one sex by individuals of the other. Darwin explains the development of different races among human beings and animals with the help of this principle. The questions which such a theory raises, among them the question as to what relation exists between natural and sexual selection, cannot be discussed within the framework of this article, however.

until well into the 20th century. For this reason Darwin had no notion of the laws of variation and inheritance.

Thus the emergence of purposefulness in nature is viewed by Darwin as the result of a blind natural process, this bringing forth traits which prove to be useful for the individual and his survival after the fact. According to this view, evolutionary change, i.e. the transformation of species, constitutes a process dictated by chance. In other words, the individual variation of organisms which are involved in natural selection does not occur as the result of any purpose to be fulfilled or a certain telos of evolution in general. Even if one assumes there is such a thing as evolutionary progress, it is to be interpreted as the result of blind laws of nature and not systematic design, he posits. In response to objections raised against his terminology to the effect that he spoke of natural selection as one might of an "active power or Deity" or that he personified the concept of nature, Darwin emphasizes the metaphorical nature of such expressions used for the sake of simplicity, rejecting such objections as being superficial. As he explains, what he means by "nature" is merely the overall effect and result of many laws of nature, and for him "laws" constitute series of events as we ascertain them (Darwin 1876, S. 66).

This understanding of nature and its laws constitutes a rejection of the widely held physico-theological understanding of nature as entertained, for one, by the famous philosopher of science, natural scientist and theologian William Whewell in his work On Astronomy and General Physics considered with Reference to Natural Theology (1833). In this investigation, one of the Bridgewater Treatises entitled "On the Power Wisdom and Goodness of God as Manifested in the Creation", Whewell's intention is not only to prove that God is the creator, preserver and regent of the world and the laws of nature, but also the author of all moral laws. "The Creator of the Physical World is the Governor of the Moral World … the Author of the Laws of Nature is also the Author of the Law of Duty;…" Whewell writes. (Whewell 1834, pg. 254f)

Although Darwin critically distances himself from natural theology, he has an appreciation for the meticulousness and detailed knowledge evidenced by its representatives. Numerous friends and mentors of Darwin were natural scientists and men of religion at the same time. "Science, in a sense, was religion," Browne observes (Browne 1996, pg. 129). From the 17th century on, there were any number of excellent scientists and philosophers of science who defended natural theology, among them Francis Bacon (1561-1626), Isaac Newton (1642-1727), Joseph Priestley (1733-1804), William Whewell (1794-1866) and others. After all, the painstaking observations and research of natural scientists was not impaired, but rather promoted by the interests of natural theology, it guiding their insights. Thus in his Descent of Man as well as his Expression, Darwin cites the works of Charles Bell even though he does not share the metaphysical framework of his research.

This also holds true for Darwin's relationship to the national economist and clergyman Thomas Robert Malthus. As we can read in Darwin's correspondence, his autobiography and his Notebooks, he was influenced to a substantial degree by his reading of Malthus' Essay on the Principle of Population in 1838. This principle says that the increase in means of subsistence (food) cannot keep up with the increase in the propagation of the population if it is not inhibited in some way. Whereas the increase in means of subsistence follows an arithmetic series (1,2,3,4,5,6 etc.), the rate of propagation increases geometrically (1,2,4,8,16,32 etc.) when unchecked, Malthus posits. According to him, the population would double every twenty-five years, and this would lead to a gross misproportion between available food and the number of persons in need of it. As he argues, this situation has not come about yet, however; the rate of growth in population has stayed more or less within limits which correspond to the available means of subsistence. Malthus attributes this to the constant effect of a law of nature which he sees as acting with more or less force on every society, inhibiting the growth of its population. As he contends, in the case of human beings this occurs in one of two ways, through "preventive checks" and "positive checks" (Malthus 1989 I, chap. 2). Checks of the first kind are inherent to man on the basis of his exceptional mental capacities, he contends, they enabling him to take into account the remote consequences of his actions. Such "preventive checks" include late marriage and sexual abstinence. Wars, excesses, epidemics and high rates of child mortality constitute "positive checks", Mathus argues.

Malthus views this principle of population as a natural law established by God which can be recognized with the help of revealed religion and experience of nature. Because from a physico-theological perspective, laws of nature are an expression of the will of God, nature becomes a framework of orientation for human action. Whoever resists its laws disrespects the will of God and violates his commands. Thus from this standpoint, poverty laws are an ill because they counteract the goal of controlling population growth and thus the promotion of human happiness. And as is contended, we as individuals are obligated to keep in check our benevolence towards the poor, it being no coincidence that the "great author of nature" (Malthus 1989 II, pg. 213) created us to be dominated by the passion of self-love, causing us to privilege self-interest to a high degree over benevolence towards others and in doing so impelling us to take that course of action which is essential for the preservation of the human species. Each individual primarily strives for his own safety and happiness as well as that of his closest relatives, Malthus contends. Because benevolence – were it to constitute a large and constant source of our actions – would require complete knowledge of causes and their effects, it can only be an attribute of God, not of man. In such a short-sighted being as man it would only lead to grave errors and would transform cultivated society "into a dreary scene of want and confusion." So Malthus prophesizes (Malthus 1989 II, pg. 214). Like William Paley, who Malthus makes positive reference to in various respects while nevertheless criticizing him in some places, the latter views the assurance of the greatest happiness of the greatest number of people as a moral and ethical goal which can best be realized by striving for one's own happiness. And Paley in turn cites Malthus' population principle.[5] Nature and its laws – in this case the population principle – are invested with a normative status whose recognition and description are at the same time the formulation of a norm prescribed by God. So the argument goes.

[5] The position that pursuing one's own happiness promotes the general good is reminiscent of Mandeville and his fable of bees. Malthus explicitly distances himself from this position, however, and contradicts those who interpret him as intending, in any way, to legitimize Mandeville's moral system. As he posits, the system is absolutely false and in fact completely contrary to the proper definition of virtue. "The great art of Dr. Mandeville consisted in misnomers." (Malthus 1989 II, pg. 214, fn. 19) To be sure, a more exacting investigation would be required to discern whether the differences between Mandeville and Malthus are actually as great as Malthus claims them to be.

Darwin adopts Malthus' population principle, but he applies it in a different context and divorces it from its theologian framework. This law serves him exclusively as a means for explaining the possibility of natural selection among organisms. Darwin has to be able to name an instance which operates in analogy to the human breeder, fulfilling his function under natural conditions. This instance is the struggle-for-existence mechanism, which is created by the mathematically definable disproportion between the arithmetic increase of means of subsistence and the tendentially geometric reproduction rate of organisms.

Implications of a rejection of physico-theology for Darwin's ethics

Darwin's rejection of the premises of psychico-theology has far-reaching implications for his ethics. For the very reason that Darwin no longer shares such theologico-metaphysical tenets, i.e. because for him the laws of nature are not an expression of the will of God but rather constitute regular successions of events, they fulfill no normative function. One reason why Darwin does not propagate a normative "evolutionary ethics" is that in his eye, nature and its laws are not the expression of godly design. Ironically, the physico-theologicans subscribed to a normative ethical naturalism – in keeping with the notion that the laws of nature are the laws of God –, whereas Darwin rejects this notion of natural teleology. An important implication of Darwin's theory is its very normative abstinence. It would be an expression of a naturalistic fallacy to derive normative consequences for human action from this theory and to develop a normative ethics based on its premises. In his noteworthy essay entitled "Evolution and Ethics" (1893), Darwin's co-advocate and friend Thomas Henry Huxley makes express reference to the "fallacy" of the "so-called 'ethics of evolution'" (Huxley 1989, pg. 138). Huxley sees this fallacy as consisting in the assumption that human beings, as moral beings, must orient themselves to the natural process of the struggle for existence and the concomitant principle of the survival of the fittest in order to promote the perfection of mankind.

The equivocality of the concept "struggle for existence"

A further aspect of the struggle for existence is relevant for a proper understanding of Darwin's ethics: Darwin expressly states that he uses this expression in a "large and metaphorical sense". To be sure, it refers to the literal struggle for food and existence among living organisms in times of dearth, but it also has additional meanings, such as dependency of living organisms on one another, the survival of the individual and – even more important for Darwin – procreation. Even a plant at the edge of the desert fights for its life to keep from drying out, he argues. "I use for convenience' sake the general term of Struggle for Existence." (Darwin 1876, pg. 52).

Furthermore it is necessary to observe, says Darwin, that the struggle for existence takes on many difference appearances and involves various coping strategies. Which ones are realized depends on the conditions of existence for the organisms in question as well as special, species-specific traits. As Darwin argues, the struggle for existence can take on the form of violent conflict between individuals of a species or between members of different species. It can also involve a struggle for external conditions of existence, however, in which there is no direct confrontation between the members of a species; in this case, differing abilities to adapt decide the success of reproduction (differential reproduction). The struggle for existence can even take on the form of cooperation between individual members of a species or of different species (for ex. in the case of symbiosis).

Thus in his Origin of Species, Darwin uses the expression "struggle of existence" in at least five senses of the word, namely to designate 1) competition between individual members of the same species (intraspecific "struggle"); 2) competition between individual members of different species (interspecific "struggle"); 3) an organism's struggle for existence involving specific environmental conditions which it is subjected to (aridity, cold, wetness etc.; 4) procreation; and 5) the dependency of organisms on one another. Thus first of all, the situation described by Malthus, namely that the increase in life-sustaining resources cannot keep up with the growth in population, does not necessarily mean that a violent intra- or interspecific struggle takes place but rather allows for various different coping strategies, one of these being cooperation. Secondly, it is not specific for all forms of the struggle for life, as the example of the plant at the edge of the desert fighting against aridity to preserve its existence shows. In this case, the struggle for existence is to be attributed to the life of the plant under certain specific geographic conditions.

2. Charles Darwin's ethics – mechanisms and limits of natural selection in the context of moral and cultural development

In his work Descent of Man, which appeared twelve years after Origin of the Species, Darwin pursues various goals. For one he aims to investigate whether his general theory on the origin of species is applicable to mankind. Secondly, the question as to the origin of man's "moral sense or conscience" is to be treated exclusively from the perspective of natural history, this constituting an endeavor which in his eye had never been undertaken before. In this context, natural history does not mean a mere description of nature, but rather the observation of human beings from the perspective of their origin of non-human ancestors. Darwin asks himself "how far the study of the lower animals throws light on one of the highest psychical faculties of man." (Darwin 1877, pg. 102).

Darwin did not wait to consider the applicability of his theories for human beings until after they were completely formulated and published in Origin of Species. Beginning in the very early stages of theory formation, man – along with his cognitive, social and moral capabilities and dispositions – constituted one of Darwin's primary objects of investigation, as his posthumously published notebook entries concerning "metaphysical enquiries" from the years 1837-1839 illustrate (Barrett et al 1987). One also finds observations on the evolutionary origin of mankind in those writings which were published before 1871, in particular towards the end of Origin of Species, where Darwin sees a potential for what he viewed to be much more meaningful fields of research in the "distant future" which would devote themselves to man and his intellectual capacities and which, as he says, would place psychology on a new theoretical foundation (Darwin 1859, pg. 488).

The reason for delaying the publication of his reflections on the origin of man was Darwin's fear of further prejudices against his theory. According to him, man also owes his existence to blind mechanisms of evolution and not to any godly act of separate creation. Thus in nature he forms no exception. A second source of provocation which seemed no less threatening to many of Darwin's contemporaries was the assumption that mankind had "apelike progenitors". For Darwin, there was even no "fundamental difference" between man and the other living organisms in terms of "mental faculties", or "mental powers" (Darwin 1877, pg. 69f). No matter how large the difference may be, it is certainly "one of degree and not of kind", Darwin writes. (Darwin 1877, pg. 130). For this reason, Darwin assigns mankind a place in the animal kingdom (Darwin 1877, S. 152). At the end of Descent of Man he repeats once again his formerly made observation that – irrespective of all noble qualities – "man still bears in his bodily frame the indelible stamp of his lowly origin." (Darwin 1877, vol. II, pg. 644).

Darwin's reflections on ethics are not all 'of one cast', however. This holds for the sources he cites as well as for the systematics he constructs, in which components of various theoretical frameworks find their place. His work Descent of Man contains a rich store of annotations which cite many diverse sources from philosophy, cultural history, anthropology, zoology (among other things Brehm's Life of Animals) etc..

In Descent of Man, Darwin turns to basic assumptions put forth by English and Scottish philosophers (Hume, Smith, Mackintosh, Bain) and reflects on them in the light of his own theory. Whereas they presupposed the existence of a moral sense as inherent to man, Darwin expands the theoretical framework and looks for the evolutionary roots of our moral faculty in the natural history of man, a history which, as he says, connects us with other living organisms. In his Enquiry concerning the Principles of Morals (1751) David Hume remarks in a footnote that "it is needless to push our researches so far as to ask, why we have humanity or a fellow-feeling with others." As he argues, it suffices that we experience this as a principle of human nature. Even if it were possible to trace this experience back to a general principle, this was not the topic which he, Hume, was addressing (Hume 1777, pg. 219f). Hume's remark reflects scientific knowledge of his time, before Darwin's theory on the descent of man was formulated, but it also shows Hume's willingness to dispense with a theologico-metaphysical explanation. As is revealed in his posthumously published Dialogues concerning Natural Religion (1779), one of the repudiations of the natural theological argument from design is based on the notion of self-organizational powers in nature. In entertaining such an idea, Hume was ahead of his times. This is the reason why Darwin's grandfather, Erasmus Darwin, who paved the way for Darwin's theory of evolution, could call upon Hume's Dialogue for support of his views. A naturalistic, biological explanation of the roots of human sympathy in the place of a natural theological one would have incited protest on the part of Hume's contemporaries.

Darwin's program now consists in tracking down the evolutionary roots of our moral faculty. For this reasons, Harald Höffding places Darwin the "moral philosopher" in the tradition of a philosophy of moral sense, claiming that Darwin had provided it with a biological foundation (Höffding 1910, pg. 460). Following Alfred Russell Wallace, Darwin sees the particular advantage of man's intellectual and linguistic capacities in his ability to remain in a harmonious relationship to the changing universe without undergoing bodily change (Darwin 1877, pg. 132).[6] Darwin operates on the assumption that the intellectual faculty of man evolved as a result of natural selection (Darwin 1877, chap. III, IV), some aspects of which, he argues, developed primarily or even exclusively for the advantage of the community, being only indirectly advantageous for the individual (Darwin 1877, pg. 67; cf. S. 133). The variability of man's intellectual faculties and his linguistic flexibility enable man to devise diverse techniques for adapting to changing life conditions, Darwin argues. This is how he succeeded in becoming the "most dominant animal that has ever appeared on this earth" (Darwin 1877, pg. 52). Applying this formulation of the idea of "free intelligence", Darwin distinguishes the flexibility of man's cognitive performance from the automatism of instincts (Darwin 1877, pg. 72). In the course of evolution, he says, the achievements of free intelligence and the role which experience plays as opposed to that played by the instincts become increasingly significant factors. And they are also the necessary precondition for the development of a moral sense, as will be elucidated in the following.

The point of departure for Darwin's reflections on the morality of mankind is his assumption that primitive man, the early, human progenitors of civilized man, possessed well-developed social instincts like those already to be found in many animals, including the "apelike progenitors" of primitive and modern man. Because man descended from non-human beings who were already invested with social instincts, we do not come into this world as tabula rasa, he argues, but rather with an evolutionary heritage of social instincts. An important element of such social instincts is sympathy for members of the same community or tribe. Darwin explains the emergence of these instincts in terms of this theory of natural selection, ascribing to them a function necessary for preserving the community. In his view, such social instincts include parental love, love of one's offspring, sociability, faithfulness, willingness to help etc.. Darwin explains the emergence of complex instincts as resulting from the natural selection of variations on simple forms of instinctive actions, positing that such variations occur as the result of unknown causes which effect the organization of the brain. For want of a better explanation, he refers to such variations as occurring "spontaneously". (Darwin 1877, pg. 72).

For Darwin, sympathy forms the basis, "the foundation-stone", of all social instincts (Darwin 1877, pg. 103). As he contends, the "instinct of sympathy" is the root of our "moral sense or conscience" because our moral sense, like the instinct of sympathy, is directed towards the good of the community, not towards egoistic striving for our own happiness. As he posits, the radius of social instincts originally only extended to the members of the same community or tribe, not to all members of the species. Initially, man was not interested in preserving the species as in preserving his own community

[6] In contrast to his earlier work, in his later studies Wallace upholds the view that man and his mental faculties cannot be explained by the theory of natural selection; for a more detailed analysis of this, cf.. Engels 1989, pp. 405-407. Darwin was very disappointed and wrote to Wallace: "I hope you have not murdered too completely your own and my child." (ML II, pg. 39).

and tribe. Cooperation among members of the same community and tribe ensured survival in confrontation with nature and foreign groups, thus becoming a strategy for the struggle for existence.

Compared to our early apelike and human progenitors, our instincts are however reduced in several ways, concerning the quantity, the specialization and the strength of instincts. The condition for the development of genuine morality is this reduction of instincts along with the evolution of reason, judgment and language. Nevertheless the social instincts still give the impulse to our social and moral actions. They however have to be oriented by reason (cf. Engels 2006).

Thus although the "first foundation or origin" of morality (Darwin 1877, pg. 637) lies in social instincts and these constitute the roots of our "moral sense", they alone do not suffice to explain the phenomenon of morality. As Darwin argues, genuine morality consists in the "moral sense or conscience", in a "sense of right and wrong", this being something only man possesses. Darwin begins the fourth chapter of his Descent of Man by expressing his "complete" agreement with those who view the moral sense or conscience as by far the most significance difference between man and animal. In doing so he cites James Mackintosh's survey and discussion of the issue entitled Dissertation on Ethical Philosophy (1837) as well as Kant's Critique of Practical Reason, quoting a passage on duty (Kant 1788, pg. 86). Here, in the First Book of Part One, Third Main Section, Kant raises the question as to the origin of duty. As Darwin observes, this question had been treated by numerous authors before, but never from the perspective of natural history.

This does not mean that Darwin reduces man's moral sense to social instincts, however. On the contrary, for him, man's moral sense constitutes a qualitatively new capacity not found in the social instincts, - one which had so far been found to exist exclusively in man. As he argues, genuine morality does not mean blindly following instincts; it involves consciously made judgments and actions in accordance with principles like Kant's law of morality and the Golden Rule. This presupposes that an organism's intellectual faculties such as memory, anticipation, imagination etc. have reached a certain level of development which, according to scientific insights gained so far, man alone possessed. For Darwin, "a moral being is one who is capable of comparing his past and future actions or motives, and of approving or disapproving of them. We have no reason to suppose that any of the lower animals have this capacity; therefore, when a Newfoundland dog drags a child out of water, or a monkey faces danger to rescue its comrade, or takes charge of an orphan monkey, we do not call its conduct moral. But in the case of man, who alone can with certainty be ranked as a moral being, actions of a certain class are called moral, whether performed deliberately, after a struggle with opposing motives, or impulsively through instinct, or from the effects of slowly-gained habit." (Darwin 1877, pg. 115f).

Due to the fact that man is equipped with certain intellectual faculties, human social action of the kind described above can be evaluated as moral irrespective of what occasioned the individual action in question. What is decisive is whether the action was carried out by a being capable of morality or not.

In defending this presupposition of a qualitative difference between man and other living organisms, Darwin breaks with his gradualistic understanding of evolution, but does not divorce man from his evolutionary past completely. Our evolutionary heritage does not consist merely in social instincts which provide the "foundation-stone" for morality; it also asserts itself in the battle between our virtues and our "lower", often stronger impulses, he argues. Francis Galton, who is cited by Darwin, explains the imperfection of human nature, the discrepancy between our recognition of what moral action is and our natural inclinations as a kind of faulty adaptation to our new life conditions during the rapid rise from a "Barbaric" state. In arguing in this way, he reinterprets the notion of original sin by premising it on a theory of evolution (Galton 1865, pg. 327).

Operating on the assumption that animals, the apelike ancestors of man and primitive man possess social instincts, Darwin reconstructs the putative process by which man's moral sense developed. He deems it improbable that the formation of social virtues could be explained by the mechanism of selection. The mechanism which is called upon to explain the origin of social instincts does not appear to be applicable when it comes to the development of man's moral sense. Darwin grounds his scepticism on the fact that statistically speaking, individuals who possess the virtue of self-sacrifice, for example, lose their lives at an early age more frequently than do egoistic members of the species, thus failing to pass this disposition on to future generations. For this reason he presupposed that individuals develop social virtues through experience of the necessity of mutual assistance, reciprocal recognition and sanction, praise and blame, the pressure of public opinion and religion, with the most reliable gauge for moral action ultimately being provided by an individual's own habitualized convictions controlled by reason.[7] But Darwin also explicitly points out the possibility of reducing social virtues to self-interest (Darwin 1877, pg. 125). In his concluding remarks he points to one aspect of the emergence of social virtues for which, as he sees it, natural selection played a marginal role in comparison to certain other factors. He writes:

"Important as the struggle for existence has been and even still is, yet as far as the highest part of man's nature is concerned there are other agencies more important. For the moral qualities are advanced, either directly or indirectly, much more through the effects of habit, the reasoning powers, instruction, religion, etc., than through Natural Selection; though to this latter agency may be safely attributed the social instincts, which afforded the basis for the development of the moral sense." (Darwin 1877, pg.. 643)

Whereas Darwin holds an ambivalent position towards progress in terms of evolution, he goes on the assumption that there is moral progress in cultural development, evaluating this possibility optimistically. For him, moral progress consists in overcoming the instinctive dispositions of primitive man and "savages",– which limit benevolence and social action to members of one's own social community – and extending social behavior to members of other races as well as to helpless, diseased and weak human beings and ultimately also to animals. Darwin sees social action which limits sympathy to members

[7] To explain social dispositions, Darwin turns to a Lamarckian model of explanation, namely the assumption that individually acquired, habitualized traits were inherited. (Darwin 1977, pg. 137).

of one's own community as characteristic of the lowest stage of moral development. To explain moral progress in the course of cultural history, Darwin adopts a group-selectionist approach. Those tribes whose members provided mutual support were more capable of surviving than were internally antagonistic groups, whom they ultimately gained dominance over. This was a process which repeated itself many times in the course of history, Darwin says (Darwin 1877, pg. 137). In this way, small tribes joined forces to form larger communities so that the social instincts, including sympathy, gradually expanded and became more complex, ultimately extending to unfamiliar members of the community until the moral qualities which were required if social communities were to function well gradually extended all over the world; Darwin seems convinced that this process will continue into the future. This presupposes that the social instincts which were originally only directed towards the well-being of an individual's own community weaken, however, placing human action to a stronger degree under the control of intellectual faculties. As elucidated above, mental freedom is for Darwin a prerequisite of morality.

For Darwin, "disinterested love for all living creatures" is "the most noble attribute of man" (Darwin 1877, pg. 130), and in his optimistic vision of a far-off future, he assumes that "virtue will be triumphant." (Darwin ebd.). Time and again he emphasizes the anticipated triumph of altruistic sympathy over instincts exclusively directed towards the individual's own community.[8]

If we have finally succeeded in reaching a cultural or civilized state we cannot, Darwin says, neglect the weak and the helpless without it leading to a deterioration of the most noble part of human nature (Darwin 1877, pg. 139). Although in Darwin's view moral progress, as it manifests itself under the conditions of civilization, involving, among other things, support of the diseased and the weak, can have negative consequences for the human race, ethical considerations prevent us from withdrawing our support for the needy. According to Darwin, intentional neglect of the diseased and the weak for the benefit of the human race would be accompanied by a bestialization of mankind and a deterioration of our moral sense. Our social virtues, he argues, have formed in the course of a long and difficult developmental process and they require ongoing cultivation if we want to avoid endangering the degree of moral progress we have taken such trouble to achieve so far. Darwin's high esteem of social virtues expresses itself in his humanitarian sensibility, which was a tradition in the Darwin family.

On the other hand, Darwin sees a danger in allowing reproduction of the diseased and the weak as this could lead to a biological degeneration of the human species if such individuals were to pass on their weak constitution to future generations. With some reluctance, Darwin advocates the protection and reproduction of the diseased and the weak through the establishment of appropriate institutions (homes etc.), through poor laws and medical progress, pointing out that in savage societies, bodily and mentally handicapped individuals were soon excluded from the community, leaving those who survived to enjoy very good health (Darwin 1877, pg. 139). But since the neglect of the

[8] According to Wallace, Darwin's observations on the future of mankind made in his last conversations with him were of a more pessimistic vein (Wallace 1894, S. 10).

diseased and the weak would lead to a deterioration of our social virtues, Darwin argues that "we must therefore bear the undoubtedly bad effects of the weak surviving and propagating their kind" (Darwin ebd.). Darwin's hope is that they would obligate themselves to voluntarily abstain from marrying and producing offspring.

Darwin's observations of this kind concerning the disadvantages of civilization must not be understood in the sense of a wholesale criticism of civilization, however. Darwin's position on civilization is actually quite ambivalent. As he contends, civilized life has its advantages, bringing about an improvement in its members' bodily constitution through better nutrition and protection against live-threatening situations. Nevertheless, these and similar passages invite misuse for ideological and political agendas. Although Darwin does not derive any ethical demands from his theory, his remarks contain obvious value judgments on what constitutes life quality and what is worthy of life. One preliminary conclusion one might draw is that Darwin did not postulate exclusion by force of certain individuals and groups of individuals. As Darwin argues, the mechanisms of selection which were practiced at the beginning of man's evolution in primitive societies must not be applied any more, since they would dull our moral sense. Possible signs of degeneration were for him the price we had to pay for morality, and ways for preventing it should be found which did not involve the elimination of weak members of society.

In arguing in this way, Darwin plays off ethical arguments against the notion that the evolutionary mechanism of the survival of the fittest should be made the gauge for human action. Darwin's moral and ethical value judgments are grounded in his faithfulness towards certain traditions and his orientation towards concepts of philosophical ethics. According to Darwin, moral and cultural progress has detached itself to a considerable degree from the mechanism of natural selection under the conditions of civilization, now being effected in other ways. Thus "great lawgivers, the founders of beneficent religions, great philosophers and discoverers in science, aid the progress of mankind in a far higher degree by their works than by leaving a numerous progeny." (Darwin 1877, pg. 141). In taking this view, Darwin touches upon the issue of how culturally relevant information is passed on to future generations on the basis of linguistically mediated experience, which occurs independent of heredity.

Darwin speculates that natural selection plays only a marginal role in the qualitative increase of morality and the quantitative increase in the number of human beings who engage in moral action, assessing its positive influence to be negligible in this sense. In his eye, moral progress more likely occurs as a result of factors already described – through social learning, reflection, experience and religion. Thus in the area of morality as it exists under civilized conditions, Darwin seems to view natural selection as a mechanism which acts negatively, but not as an instance which fulfills a constructive function as a motor for moral progress.

In the first section of this article I pointed out that the struggle for existence as Darwin sees it can take on the form of cooperation between individuals belonging to the same or different species (the latter case constituting a form of symbiosis, for example). In the second section I showed what great significance Darwin ascribed to man's social virtues. This aspect of Darwin's ethics was already underscored by the contemporaneous reception of his work and proved to serve as a foundation for the practical reconcilability of Christian ethics and Darwin's "new ethics" (Everett 1878). In the reception of Darwin's thought in Russia, the aspect of cooperation was emphasized most prominently by K. F. Kessler and Petr Kropotkin. They cited Darwin in placing the "law of mutual aid in the animal and human realm" alongside the law of mutual confrontation. With his 1909 article entitled "Unvermeidlicher Daseinskampf oder notwendige Harmonie? Darwin und Kropotkin" ("Unavoidable struggle for existence or necessary harmony? Darwin and Kropotkin!"), von Unruh called attention to this important aspect of Darwin's reception.[9]

In an early observation made in a letter to Wilhelm Preyer on March 29, 1869, Darwin expressed apprehensions concerning the equivocality of the expression "struggle for existence", speculating that the German expression "Kampf" etc. did not quite express the same notion (Darwin in Preyer 1891, pg. 362):

> "About the term 'Struggle for Existence', I have always felt some doubts, but was unable to draw any distinct-line between the two ideas therein included. I suspect that the German term, Kampf etc., does not – give quite the same idea. The words 'struggle for existence' express, I think, exactly what – concurrency does. It is correct – to say in English that two men struggle for existence, who may be hunting for the same food during a famine, and likewise when a single man is hunting for food; or again it may be said that a man struggles for existence against – the waves of the sea when shipwrecked."[10]

The notion "concurrency" reflects the ambiguity which has given rise to so many different interpretations, for it has several meanings. The Oxford New English Dictionary of 1893 mentions four meaning: "1. A running together in place or time; meeting, combination…2. Accordance in operation or opinion; cooperation; consent; = CONCURRENCE 3, 4… 3. Pursuit of the same object with another; competition, rivalry … 4. The quality or fact of being concurrent in jurisdiction; joint right or authority." (A New English Dict. 1893, pp. 778f).

Thus "concurrency" can mean "cooperation" or "competition", depending on the circumstances under which someone has to struggle for existence. Harmony and unavoidable struggle for existence can go hand in hand.

3. Conclusion

In light of the previous results I will now underpin the thesis formulated in the introduction, namely that Darwin's ethics constitutes neither a primarily biological-scientific, an evolutionary, or a Social-Darwinistic ethics.

1) As was illustrated, Darwin bases his concrete ethical argumentation primarily on approaches and discussions of philosophical ethics in the tradition of English and Scottish moral philosophy as well as on the cultural and religious tradition in which he grew

[9] Cf. Kropotkin's writings from the years 1902 and 1923 for this line of reception. Concerning various interpretations of the concept "struggle for existence" cf. the informative studies of Daniel Todes (1989 and 1995).

[10] I would like to thank the Cambridge University Library for sending me a copy of the English original.

up and which he felt an affinity for despite his rejection of the physico-theological argument from design, rather than on scientific approaches and theories. To be sure, in Darwin's eye the conditions under which our moral faculties emerge are rooted in the natural history of mankind. And yet this history supplies neither every necessary nor every sufficient condition for the emergence, manifestation and realization of morality which for him constituted the specifically human aspect not to be found among animals. Darwin answers the question as to whether and to what degree an investigation of the natural history of animals can illuminate the origin of the highest faculty of man, i.e. his capacity to develop a moral sense, by maintaining that in the course of his evolution, man inherited social instincts from animals which constituted the necessary condition for the formation of his moral sense. Any human being who did not carry any traces of such instincts would be an "unnatural monster", Darwin says. (Darwin 1877, pg. 116). Despite this heritage, which derives from natural history, – one which connects us with other living organisms –, the moral faculty in a genuine sense of the word is an exclusive trait of man. Although Darwin posits that there are only gradual differences in the cognitive faculties of animals and man, these differences become qualitative differences in the context of his reflections on ethics. The compelling question as to whether we are dealing with a real or merely seeming inconsistency is investigated in another study. (cf. Engels 2006). Man's moral sense distinguishes him from all other animals and invests him with a special status. The fact that in Darwin's view, moral progress also expresses itself in moral consideration of animals, makes his position interesting from the perspective of present-day animal ethics. Adopting its terminology one can say that in Darwin's eye, only human beings can be "moral agents", but animals are also to be recognized as "moral patients".

2) Darwin does not advocate an evolutionary ethics in a descriptive-explanatory or normative sense, as one might possibly expect.[11] As I showed, he neither claims to provide a sufficient description and explanation of human morality by means of the mechanisms of evolution identified by him and drawn from his knowledge of the course of evolution, nor does he derive any normative conclusions for morality and ethics on this basis. Darwin's ethics does contain certain evolutionary elements, however. For one, he offers an explanation of our social instincts based on the theory of natural selection, and secondly, he puts forth the assumption that moral progress emerged in the course of human development through group selection insofar as groups made up of virtuous and cooperative individuals asserted themselves over others.

3) Darwin does not subscribe to Social Darwinism. The widely held view that the expression "struggle for life" which he used in his writings primarily or even exclusively refers to the exertion of force by one against the other or the egoistic assertion of individual interests is misguided. On the contrary, Darwin advocates the view that mutual support, cooperation among members of the same community to ensure survival in the face of threats from nature and foreign groups, can constitute a foundation in the struggle for existence. Cooperation does not stand in the way of the struggle for existence. On the contrary, it promotes the survival of individuals and species. Moreover, for Darwin

[11] Nicola Erny's line of argumentation goes in this direction (Erny 2003).

moral progress consists in extending social behavior to members of other tribes, nations and races as well as to helpless, diseased and weak human beings and ultimately also to animals. In this respect Darwin does not establish any socio-political program based on the principle of the "survival of the fittest" (cf. Peters 1972, Engels 1995b); he does not raise any normative Social-Darwinist claims. It is significant that authors like Alexander Tille[12] and the physician Wilhelm Schallmayer from Munich criticized Darwin's adherence to Christian, humanitarian ideals (Tille 1894; Tille 1895), writing

"[i]nstead of investigating the question as to how culture and civilization hindered the elevation of the human race in all aspects, Darwin limited himself to the study of how what are now praised as the virtues of sympathy, altruism and truthfulness evolved. This is, to be sure, interesting as well, but compared with that practical, fundamental question, it only has very theoretical value …" (Tille 1894, pg. 312).

In a competition held in 1900, which addressed the question "What do we learn from the theories of descent in relation to inner-political developments and the legislature of States?", written up by Ernst Haeckel (Jena), the national economist Johannes Conrad (Halle) and the paleontologist Eberhard Fraas (Stuttgart), and financed by Alfred Krupp (Schallmayer 1903), who donated 30,000 Marks prize money, the physician Wilhelm Schallmayer won the first prize (Schallmayer 1903). In an article he raises the following objection to Darwin and other eminent representatives of the theory of evolution such as Alfred Russell Wallace, Thomas H. Huxley and John Hutton Balfour:

"Ch. Darwin did indeed recognize the severe disruptures which natural selection has suffered from our conditions of culture and in private he made some quite grim remarks about their consequences. But he did not make any efforts to demand that these conditions be changed to good purpose. Wallace, Huxley, Balfour and others recognize the ill as well, but they abhore all notions of socially controlled racial selection and hope for what are in part quite questionable countereffects in the future." (Schallmayer 1902, pg. 271f).

In light of this distancing from Darwin, the latter cannot be made responsible for the dissemination of positions like those exemplarily advocated by Tille and Schallmayer. For this reason as well, the term "Social Darwinism" is misleading.[13]

One must concede, however, that despite his basic humanitarian convictions, Darwin's observations are ambivalent in many respects, leaving great latitude for (mis-) interpretation. There are many reasons for this ambivalence. Darwin lived in a time of upheaval in the biological sciences, an upheaval which he himself contributed to as one of its most eminent protagonists. His thought is shaped by different, in part contradictory thought-styles. Moreover, the age-related ignorance of the laws of heredity, the appealing metaphoric quality of his style, the pitfalls of the translations of his works, the enticements of evolutionary anthropology and the complexity of his theories, which

[12] Concerning the life and work of Alexander Tilles cf. Schungel 1980.

[13] Concerning the wide range of political interpretations of Darwinism, cf. the informative survey on various positions in Bayertz 1998.

drew on several different, individual theories contributed to such equivocality (Engels 2000, pp. 121-137).

Even where Darwin's ethical reflections go beyond the level of description and explanation and the attempt is made to pass judgments, for example in his discussion on the great significance of social virtues and moral progress in cultural history, the endeavor is not made from a meta-ethical standpoint nor does it constitute an attempt to derive norms and values from natural history. Instead, his reflections are primarily informed by his rootedness in a certain philosophical tradition.

What role can biology or a certain biological theory play for meta-ethics and normative ethics, however? And what role does ethics play for biology? This article does not focus on such questions, but I would like to provide a short answer to them in these closing remarks. Biological theories – in connection with other natural, social and human scientific theories – can provide us with insights on the natural foundations and scope of our actions. They could help us to realize the goals set by our moral sense by making the conditions under which we take action transparent. Only in this way will it become possible to take these conditions into account when it comes to our behavior and actions, allowing us to influence such conditions if necessary. It is reassuring to know that nature has equipped us with a heritage of sympathy and compassion and that with the help of our "free intelligence", we have the capacity to make use of it properly. The biological sciences cannot formulate the criteria for appropriateness alone, however. For this, ethics is needed. In our context, ethics is also relevant in another respect, namely insofar as it offers a direct instrument of reflection when it comes to formulating biological concepts and applying them. Criteria for the "degeneration" of the human species, for illness and weakness, are not purely biological in nature, but rather are based on certain values and norms. It is important that an ethics in the biological sciences make this fact explicit and reflects it critically. (Engels 2005).

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In the wake of the "Darwin Correspondence". 40.000 letters to Ernst Haeckel listed and available for study

- Short communication -

Uwe Hoßfeld & Olaf Breidbach

The development of the theory of evolution has attracted a great deal of interest, especially after the centennials of the publication of Darwin´s "Origin" in 1959 and of his death in 1982, from both historians and biologists. It has been shown that studying communication other than through books and papers (correspondence, lectures, etc.) is important for a proper understanding of the science, politics and culture of the 19th and 20th centuries. The Darwin correspondence project in Cambridge, UK (transcription of 14000 letters from 2000 correspondents)[1] has shown how important the correspondence is for a proper understanding of the development of Darwin´s ideas. Having started their search for letters in 1974, the Darwin project published the first volume of the *Correspondence*, and the *Calendar*, in 1985. The project will be finished with 9 volumes of letters in the next "Darwin year" in 2009.[2]

Now – in the wake of the Darwin Correspondence – a catalogue listing around 39000 letters from 9312 correspondents in 72 countries sent to Ernst Haeckel, the "German Darwin", has been published. This book[3] provides an overview of the names of the correspondents, where and when the letter was written, and its kind and length. Letters as a form of scientific communication had a special importance for Haeckel – as for many of his contemporaries. His scientific and private correspondence thus documents the extraordinary multi-fariousness of his connections to naturalists, artists, philosophers etc. interested in evolutionary theory and philosophy. His correspondence can be divided into 4 different categories: 31474 letters <u>to</u> Haeckel from non-family members, 1891 letters <u>from</u> Haeckel to non-family members, 3891 letters from his family, and 1825 letters from Haeckel to members of his family. We still find new letters. 800 newly found letters will be catalogued during the next month. Totally we have 88% letters to

[1] Junker, T. & M. Richmond (eds.) *Charles Darwins Briefwechsel mit deutschen Naturforschern* (Basilisken-Presse, Marburg, 1996).

[2] Burkhardt, F. & S. Smith (eds.) *A Calendar of the correspondence of Charles Darwin, 1821-1882* (Garland Publishing, New York and London, 1985); Burkhardt, F. et al. *The correspondence of Charles Darwin.* (CUP, Cambridge, 1985).

[3] Hoßfeld, U. & O. Breidbach *Haeckel Korrespondenz: Übersicht über den Briefbestand des Ernst-Haeckel-Archivs* (VWB-Verlag, Berlin, 2005).

Haeckel and 12% letters from Haeckel. By far the greatest number of letters – quantitatively and qualitatively – are those that discuss zoological, philosophical and more private problems. The letters listed include originals, copies, and published letters in 7 languages; also drafts of letters and descriptions of letters found in library and auction catalogues or in other archives and museums. The catalogue also gives an overview of the networks to which Haeckel belonged. It also facilitates the uncovering of new aspects of the multifaceted German zoologist and of the comparative reception of Darwinism during the 19th[4] century, or the Darwinian heritage[5] worldwide. The reception of Haeckel`s ideas has come to the forefront only in the last seven years.[6] Most importantly, the correspondence, because of the richness of information it contains, ranks as a unique source for understanding the biological revolution in which Haeckel – maybe even more than the great Englishman – was the central figure.

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[5] Kohn, D. (ed.) *The Darwinian heritage* (PUP, Princeton, 1985).

[6] Pennisi (1997); Kemp (1998); Richardson, M. K. et al. (1998); Hanken & Richardson (1998); Behe, M. J. et al. (1998); Bell (2000); Richardson & Keuck (2001); Uglow (2002); Hoßfeld et al. (2003); Pennisi (2003).

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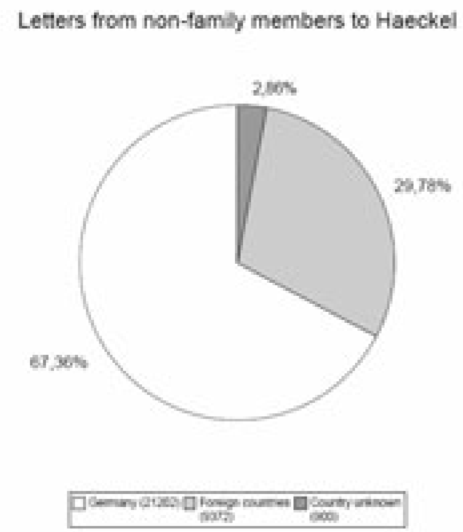
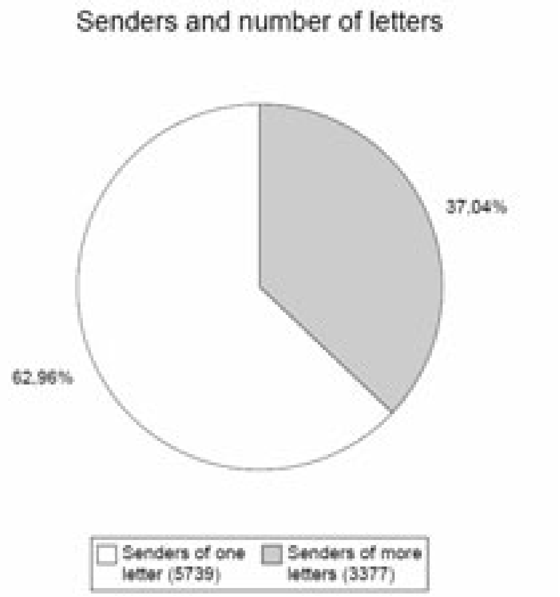
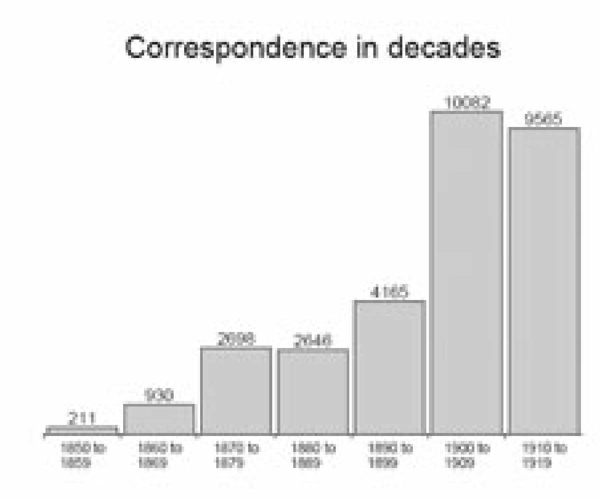
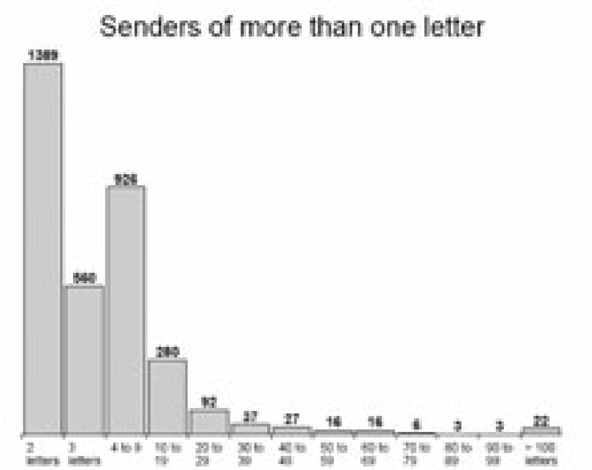


COUNTRY	NUMBER OF LETTERS
Italy	1545
Austria	1297
UK	1277
Switzerland	973
USA	908
France	650
Netherlands	366
Poland	346
Czech Republic	194
Hungary	141
Sri Lanka	140
Romania	139
Belgium	123
Russia	105
others	1170







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Predator-driven macroevolution in flyingfishes inferred from behavioural studies: historical controversies and a hypothesis

Ulrich Kutschera

Abstract
Flyingfishes (Exocoetidae) are unique oceanic animals that use their tail and their large, wing-like pectoral fins to launch themselves out of the water and glide through the air. Independent observations document that flyingfishes use their gliding ability to escape from aquatic predators such as dolphins (marine mammals). The fossil record of flyingfishes is very poor. Nevertheless, the evolution of gliding among flyingfishes and their allies (Beloniformes) was analysed and reconstructed by the ethologist Konrad Lorenz (1903 – 1989) and other zoologists. In this article I review the comparative method in evolutionary biology, describe historical controversies concerning the biology and systematics of flyingfishes and present a hypothesis on the phylogenetic development of gliding among these marine vertebrates. This integrative model is based on behavioural studies and has been corroborated by molecular data (evolutionary trees derived from DNA sequences).

Introduction

Since the publication of Darwin´s classical book (1872, 1st ed. 1859), evolutionary biology has relied primarily upon comparative studies of extant organisms (animals, plants), supplemented whenever possible by information obtained from the fossil record. This interaction between neontological and palaeontological research has greatly enriched our knowledge of the evolutionary history (phylogeny) of a variety of macro-organisms, notably hard-shelled marine invertebrates (molluscs etc.) and vertebrates, for which thousands of well-preserved fossils have been described. Such comparative studies have become considerably more significant with the development of molecular methods for reconstructing DNA-sequence-based phylogenies and with the increased rigour with which the comparative method has been applied. Charles Darwin used a strictly comparative approach when he remarked that "in searching for the gradations through which an organ in any species has been perfected, we ought to look exclusively to its lineal progenitors; but this is scarcely ever possible, and we are forced to look to other species and genera of the same group, that is to the collateral descendants from the same parent-form" (Darwin 1872, p. 182).

Since Darwin's time, the comparative method has been improved and refined so considerably that evolutionary patterns (phylogenies) and adaptations by natural selection have been studied and elucidated in many groups of organisms.

In this review of the phylogenetic development of certain marine vertebrates (Beloniform fishes) I first summarize the power of the comparative method and outline the history of a branch of ichthyological research with reference to the work of the pioneers in this field of evolutionary inquiry. In the second part I develop a hypothesis that explains the evolution of gliding in flyingfishes (Fig. 1) that is based on comparative behavioural studies carried out in the field and on recent molecular data.

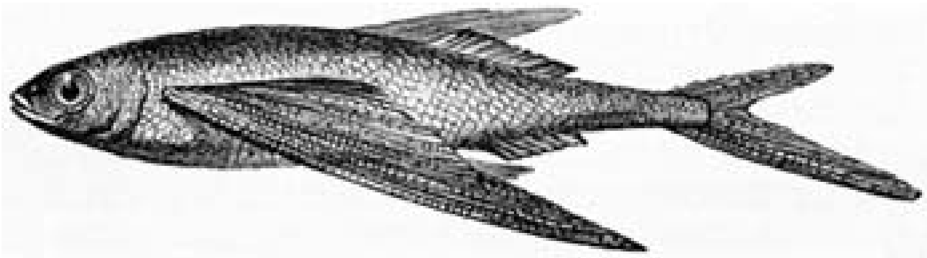

Fig. 1: Lateral view of a gliding flyingfish (Exocoetus volitans). The enlarged pectoral fins and the asymmetrical tail lobes, with the lower larger than the upper, are apparent. Since exocoetids feed mainly on plankton, their mouth is very small (Adapted from Matzdorff 1910).

Historical science and the comparative method

It has long been known that not all scientific hypotheses and theories can be tested in the laboratory using experimental methods. Historical hypotheses are common in fields such as astronomy, astrophysics, planetary science, geology, archaeology, and evolutionary biology. Nevertheless, many experimentalists regard historical sciences as inferior on the grounds that its hypotheses can not be verified unequivocally. The considerable number of chemists and physicists who have repeatedly attacked the scientific status of the Synthetic Theory of Biological Evolution provides proof for this conclusion (Cleland 2001, Kutschera and Niklas 2004). The most severe recent attack on the significance of the historical sciences comes from Henry Gee, one of the former Editors of the journal *Nature*. This prominent person expressed his attitude in a popular book in the following words: "(Historical hypotheses) can never be tested by experiment, and so they are unscientific...No science can ever be historical" (Gee 2000, p. 5 – 8). In two essays, the philosopher C. R. Cleland (2001, 2002) concluded that, although there are fundamental methodological differences between historical and experimental research, there is no evidence for the contention that historical science is epistemically inferior to laboratory tests.

Evolutionary biology shares with geology and other classical historical sciences the task of interpreting properties of extant systems that can not be understood today without understanding their past. In contrast to the phenomena analysed by the geologist (for instance, the hypothesis of continental drift), living organisms such as the famous finches in the Galapagos Islands are distinct from the inorganic world: they have become adapted to their environment via the process of natural selection (Endler 1986, Futuyma 1998, Junker and Hoßfeld 2001, Kutschera 2001, 2003, Mayr 1963, 2001, Bell 1997, Niklas 1997). Comparisons among groups of extant organisms (species) are the most commonly used technique for examining how living systems are adapted to their specific environments. These uses of what is today called "the comparative method" provided the empirical basis for many arguments in Darwin´s *The Origin of Species* (1872) and thousands of related publications that followed.

Abstract

As a young man, Ernst Haeckel harbored a conventional set of Evangelical beliefs, mostly structured by the theology of Schleiermacher. But the conversion to Darwinian theory and the sudden death of his young wife shifted his ideas to the heterodox mode, more in line with Goethe and Spinoza. Haeckel's battles with the religiously minded became more intense after 1880, with attack and counterattack. He particularly engaged Erich Wasmann, a Jesuit entomologist who had become an evolutionist, and the Keplerbund, an organization of Protestant thinkers who opposed evolutionary theory and accused him of deliberate fraud. In these struggles, Haeckel defined and deepened the opposition between traditional religion and evolutionary theory, and the fight continues today. Harvey and Pagel (1991) have pointed out that it is the second nature of biologists to think comparatively because comparisons establish the generality of evolutionary phenomena. For example, we cannot physically re-run the evolutionary sequences that resulted in the phylogenetic development of brooding behaviour in leeches and other phenomena (Kutschera and Wirtz, 2001). However, it is possible to reconstruct the origin and development of this behaviour through strict use of the classical comparative method, combined with an analysis of DNA-sequences and the resulting molecular phylogenies (Borda and Siddall 2004, Kutschera 2004). Because leeches are soft-bodied worms (annelids), the fossil record of this group of invertebrates is very poor. Likewise, the number of fish-like vertebrates that display a morphology similar to that of extant members of the flyingfishes (Exocoetidae) (Fig. 1) is rather limited. The geologist Othenio Abel (1875 – 1946), founder of a branch of the natural sciences that he called Palaeobiology (a term that is still in use today), published a monograph on fossil flyingfishes (Abel 1906). One representative specimen, the flyingfish *Thoracopterus niederristi* from the Triassic, is depicted in Fig. 2 A. The reconstruction of this vertebrate (Fig. 2 B) clearly shows all the basic features of extant exocoetids: exceptionally large, winglike pectoral fins, enlarged pelvic fins, a small

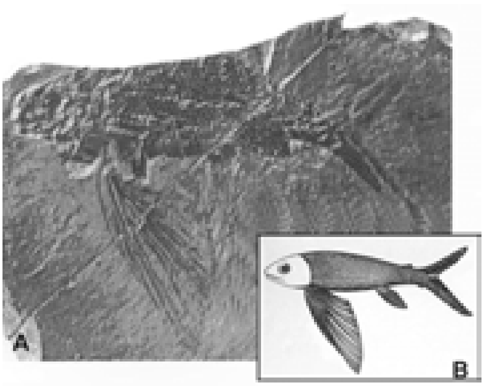

Fig. 2: The fossil flyingfish Thoracopterus niederristi (Triassic) from the Raibler Schichten (Austria). Original specimen (A) and reconstruction of the animal (B) (Adapted from Abel 1906).

mouth and an elongated lower lobe of the tail (Abel 1926). However, to my knowledge, no intermediate fossil species have ever been found so that a historical reconstruction of the evolution of gliding can only be achieved through the comparative method (Harvey and Pagel 1991).

Flyingfish flight and the aeroplane theory: a historical controversy

The ability of aquatic vertebrates to glide above the surface of the water has evolved in several groups of the bony fishes (Osteichthyes). However, we shall discuss here only the most successful of these, the oceanic flyingfishes comprising the family Exocoetidae (Fig. 1) and related taxa. In one of the first scientific publications on this subject, Möbius (1878) summarized his observations, which have formed the groundwork for many subsequent articles on gliding fishes, as follows: "They are more frequently observed in rough weather, and in a disturbed sea than during calms; they dart out of the water…. and they rise without regard to the direction of the wind or waves. The fins are kept quietly distended without any motion, except an occasional vibration caused by the air, whenever the surface of the wing is parallel with the current of the wind. Their flight is rapid, but gradually decreasing in velocity, greatly exceeding that of a ship going ten miles an hour, and a distance of 500 feet. Generally it is longer when the fishes fly against, rather than with, or at an angle to, the wind. Any vertical or horizontal deviation from the straight course, when flying with or against the wind, is not caused at the will of the fish, but by currents of air…. in a rough sea, when flying against the course of the waves; they then frequently overtop each wave, being carried over it by the pressure of the disturbed air. They….fall on board vessels. This never happens from the lee side, but during a breeze only, and from the weather side. During the night they frequently fly against the weatherboard, where they are caught by the current of air and carried upwards to the height of 20 feet above the surface of the water, whilst under ordinary circumstances they keep close to it" (Möbius 1878, p. 344 – 346, translated by the author). The above description is fairly representative of the so-called "aeroplane theory". There are, however, several variants to it, the most notable being the addition of the use of the tail by later writers, both as a propeller in water, and also as an explanation of the loud buzzing sound always heard when the fish fly near or over a boat.

Despite this early exact description of gliding in flyingfishes, a controversy emerged among naturalists as to whether or not these animals flap their wings during flight. Dunford (1906) summarized both concepts as follows: "1. Flying-fish do fly, moving their wings with extreme rapidity. I have carefully and frequently watched them and there can be no doubt whatsoever about it. 2. Flying-fish do not flap their wings, but use them as aeroplanes, like swallows when in skimming or sailing flight. I have carefully and frequently watched them, and there can be no doubt whatsoever about it".

Somewhat similar remarks will be heard in any ordinary group of ship passengers watching the fish. Some will insist that they see the wings flapping, and some will say that they are quite still. It should be noted that Darwin (1872) obviously referred to hypothesis (1.) when he remarked that: "… it is conceivable that flying-fish, which glide far through the air, slightly rising and turning by the aid of their fluttering fins, might have been modified into perfectly winged animals" (Darwin 1872, p. 177).

Among the majority of scientists, the "wing-flapping-hypothesis" (1.) was abandoned around the year 1920, due to careful observations by independent investigators (Hankin 1920, Abel 1911, 1926). Hence, the aeroplane theory (2.) was accepted by most of the workers in this field and the competing concept 1 was no longer discussed. However, about sixty years ago, a field naturalist re-vitalized the wing-flapping hypothesis based on observations during a trip taken on Pacific waters. In his report, Troxell (1937) presented a list of seven points in apparent support of a flapping flight in exocoetids. Breder (1937) discounted these claims and summarized the evidence in support of the aeroplane theory. The pectoral muscles of these motorless gliders are small and in no way adequate to the demands of wing-flapping exertion. There is nothing like a sternum-like structure for the necessary attachment of a corresponding (non-existent) muscle mass, as in bats, birds or pterodactyls. Moreover, the fins are not articulated, and the apparent movement of the "fish-wings" are probably a reaction to forces from the beating tail (Rayner 1986).

In a classical paper, Breder (1930) pointed out that power is applied by flyingfishes only as long as they are in contact with water: "After the forepart of the body has been thrust out of the water by rapid swimming and the pectoral fins are spread, very effective power is supplied by the long lower caudal lobe, the only part submerged, combining the advantages of the slight resistance to motion in air with the strong reactive effect of motion in water. As soon as the tail leaves the water it immediately stops oscillating, and the fish becomes a glider. Up to this time they (the animals) may be considered as a pusher type of plane" (Breder 1930, p. 115 – 116).

This careful description of the flight among flyingfishes of the family Exocoetidae (Fig. 1) has been corroborated by many biologists and can be considered a brief summary of the tenets of the aeroplane theory of gliding. Breder (1930) used the distribution of wing area to classify flyingfishes into two distinct aerodynamic designs. The monoplane type (*Exocoetus* and related taxa, Fig. 1) has a single set of long narrow main wings (pectoral fins) and the biplane type (*Cypselurus* etc.) has under wings (pelvic fins) staggered far back from the main wings. These aerodynamic designs have implications for the maximum distance travelled in gliding and the evolution of flight performance in these aquatic vertebrates (see Fig. 8).

The gliding of members of the family Exocoetidae was studied extensively during the period around 1900 to ca. 1930, as possible analogues to airplanes (Adams 1906, Hoernes 1913, Abel 1926). Descriptions of flights by these animals were considered living model systems for airplanes, because the design of *Exocoetus* was regarded as perfectly in accord with the aerodynamics of gliders. As Breder (1930) pointed out, through modification of paired fins, members of the Exocoetidae have evolved aerodynamic lifting surfaces that enable them to glide one metre above the water for a distance of more than 100 m. The design of the out-stretched pectoral fins was likened by several naturalists to the swept-back wings of hirundine birds such as swallows.

Flyingfishes: why do they leave the water?

It has long been known that there are fishes that can move about on land, sometimes far away from the water. The best-known of these amphibious fishes are the mudskippers (*Periophthalmus* sp.), which dig burrows in the soft, muddy substrate of mangrove swamps of tropical Africa (Keenleyside 1979). Nevertheless, the popular expression "like a fish out of the water" conveys the general inability of fish to survive in the absence of their aquatic environment. This is to a large extent due to the fact that the majority of fishes are unable to exchange gases effectively in air. Sayer and Davenport (1991) have summarized the selective forces that may have caused this step in the evolution of certain members of the bony fishes (Osteichthyes). Extant amphibious fishes leave the water for a number of reasons associated with the degradation of their aquatic habitat, or certain biotic factors. In open aquatic systems, such as large freshwater bodies or coastal waters, the dominant selective forces are possibly the interaction between predation, competition and food availability (Sayer and Davenport 1991).

The question of why flyingfishes glide for 200 m and more through the air, using their tail and the large, wing-like pectoral fins to keep them above the water, has long been a matter of debate. Do they fly to escape large predators, like dolphinfishes and dolphins (marine mammals), or is it an energy-saving mechanism? Adams (1906) was one of the first naturalists to provide evidence for the hypothesis that members of the Exocoetidae fly to evade attacks from predators below. Based on numerous opportunities to watch flyingfishes in various parts of the world, he summarized his observations as follows: "One theory is that they keep up the flight by going against the wind, soaring like sea-birds; but as a fact, the fish will start off in all directions from the bows of a vessel, or when chased out of the water by enemies – as often in a calm as in rough weather, against, across, or before the wind, and, …, will often change the direction of their flight, which is done by touching the water with the lower tip of the vibrating tail. I once spent the greater part of a distinctly warm afternoon, in a dead calm in the Gulf of Aden, watching schools of the Sailors´ Dolphins bounding out of the water, chasing the flyingfishes as greyhound course hares" (Adams 1906, p. 147).

In numerous subsequent reports it has been documented that sometimes a tuna, dolphin or shark can be seen as a fleeting shadow just below the surface following the flight path of flyingfishes. The lateral line is placed along the ventral surface allowing the flyingfish to detect a predator striking from below, and especially adapted eyes enable them to see in both air and water. In addition, it is well known that flyingfishes, which feed mainly on plankton, serve as food for many aquatic predators, including other (larger) fishes, especially tunas, marlin and dolphinfish as well as dolphins, birds, squids and porpoises. This is in accordance with the observation that flyingfishes are a dominant food source found in the stomachs of dolphins (Collette and Parin 1998).

Rayner (1986) pointed out that the periodic flights of exocoetids could be part of an energy-saving strategy similar to that used by penguins and some marine mammals which repeatedly jump out of the water when travelling over long distances. Moreover, Rayner (1986) proposed that an analysis of the biochemical properties of the caudal musculature would be useful in order to verify this hypothesis. Davenport (1992) provided evidence indicating that it is improbable that exocoetids use their flights as part of an energy-saving strategy. This conclusion is based on a comparative analysis of red versus white muscle tissue in exocoetids compared with other marine vertebrates. Davenport (1992) suggested that acceleration to take-off speed in the Exocoetidae requires use of anaerobic white muscles via the inefficient biochemical pathway of glycolysis.

Today, humans are the top-predators in the biosphere. It is not surprising that there are commercial fisheries for flyingfishes in many tropical countries. Adams (1906) commented on this issue as follows: "It is truly amazing to contemplate the countless millions of these fish in tropical waters. Often for weeks together one may every few minutes see startled shoals scatter from the ship´s bows. I have watched for hours the sea thick with myriads of juveniles from a couple of inches in length. These do not fly, but flap on the surface; the flight begins when the fish are about three or four inches long, and increases in length as their size increases. The adults come on board chiefly at night, and mostly in rough weather. … They are often collected and fried for breakfast. The flesh is very white and firm, but somewhat dry, and the bones are particularly hard. Fishermen bring them for sale to ships in the Japanese Ports" (Adams 1906, p. 148).

Since that time several sophisticated techniques have been developed to catch large numbers of flyingfishes, including gill-netting (Japan, Vietnam, Barbados), dip-netting of spawning swarms (Indonesia, India) and attraction to artificial light and dip-netting at night (Pacific islands) (Collette and Parin 1998). During the period from 1983 to 1989, annual global catches of flyingfishes were around 36 000 to 49 000 tonnes (FAO 1991). These data documented that members of the Exocoetidae are an important resource in some tropical areas of the world that support a major commercial food industry.

Systematics of flyingfishes: a matter of debate

The best known gliding fishes are the oceanic Exocoetidae, surface-dwelling (epipelagic) animals which are common throughout tropical and sub-tropical seas. However, in European marine coastal waters a taxonomically unrelated species is known, the flying gurnard (*Dactylopterus volitans*) (Fig. 3 A, B). Chen et al. (2003) have recently shown that the Dactylopteridae can be added to the Smegmamorpha, but no close relationship to the needlefishes (Beloniformes, relatives of the exocoetids) was apparent in these molecular phylogenies. The question whether or not *Dactylopterus* is capable of gliding short distances above the surface of the water is still unanswered. According to Klausewitz (1960), Nelson (1976) and Müller (1983) the flying gurnard can glide, but Lorenz (1965) and Rayner (1986) concluded that is now believed that these reports of flight in *Dactylopterus* are mistaken. In a recent monograph on marine fishes this controversial point is summarized as follows:

"Although these benthic fishes (the Dactylopteridae) are often called 'flying gurnards', they cannot fly or glide out of the water" (Smith and Heemstra 1986, p. 490)

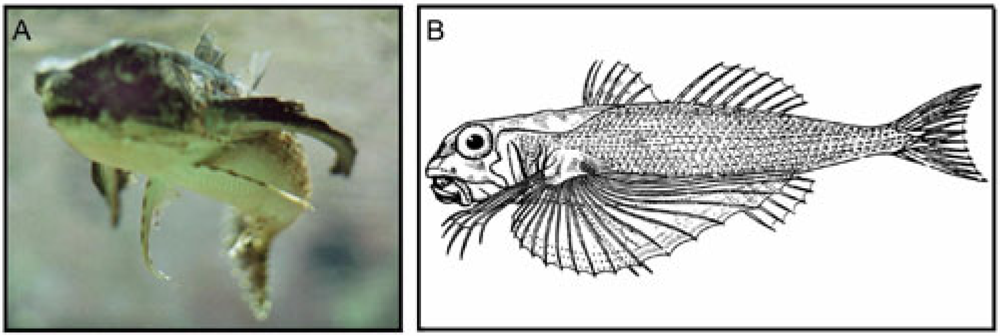

Fig. 3: Adult individual of the European marine species "flying gurnard" (Dactylopterus volitans). This fish can glide under water using the enlarged pectoral fins, but appears to be unable to leave the water for true flights. Original photograph (A), schematic view of the animal (B).

A number of tropical freshwater fishes perform short flapping flight, at least in captivity (aquaria). For instance, the freshwater hatchet fishes of South America (Gasteropelecidae), small animals up to 7 cm in length, make brief jumps out of the water (members of the genera *Thoracocharax*, *Gasteropelecus*, *Carnegiella* and others). According to Rayner (1986) these vertebrates are the only fish which actively flap their "wings" (i.e. the extended pectoral fins) in air to obtain thrust (Fig. 4 A). Wing beat rates of up to 80 Hz have been recorded, which results in a buzzing sound during the jump of the fish. Since the large pectoral fin muscles (that are absent in members of the Exocoetidae) are "white" and contain almost no mitochondria (Fig. 4 B) the flapping jumps must be sustained via anaerobic metabolism (glycolysis) and can only be of short duration (Rayner 1986). According to Klausewitz (1960) there are reports indicating that under natural conditions *Thoracocharax* jumps out of the water in response to predatory attacks, but more field observations are necessary to corroborate this hypothesis (Keenleyside 1979, Rayner 1986).
The taxonomy of the marine flyingfishes, which are easily recognized by their huge pectoral fins (Fig. 1), is confusing and still a matter of debate. In his classical monograph on the "Fishes of the World", Nelson (1976) grouped the Flyingfishes and Halfbeaks together (one family, Exocoetidae), which comprised the subfamilies Exocoetinae and Hemirhamphinae. The families Belonidae (Needlefishes) and Scomberesocidae (Sauries) were regarded as close relatives of the exocoetids (see Fig. 8). Ten years later, the halfbeaks were elevated to the rank of a family (Hemiramphidae), so that the Exocoetidae (flyingfishes) no longer included the subfamily Hemirhamphinae sensu Nelson (1976). It is interesting to note that on one page of this monograph the halfbeak Oxyporhamphus micropterus is described as a "shortwing flyingfish", but this species is not regarded as a member of the Exocoetidae (flyingfishes) (Smith and Heemstra 1986, p. 391). In a careful analysis, Dasilao et al. (1997) concluded that *Oxyporhamphus* is a member of Exocoetidae, with which it shares a total of 10 derived osteological/myological conditions.

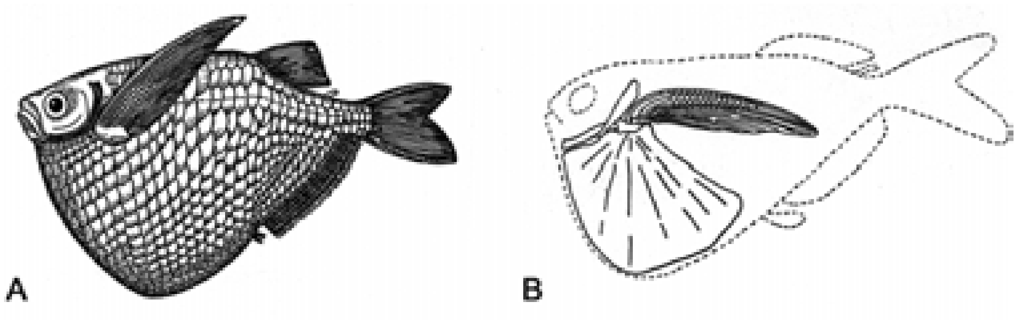

Fig. 4: A South American hatchet fish (Thoracocharax) that is able to perform flapping flight by rapidly beating its enlarged pectoral fins (A). The fish has a highly compressed body with large pectoral fin muscles (B) (Adapted from Klausewitz 1960).

Since that time, a consensus emerged among ichthyologists that can be summarized as follows. The order Beloniformes (also called "flyingfishes and their allies") comprises five closely related families: the needlefishes (Belonidae), easily identified by their elongated upper/lower jaws and a long body; the halfbeaks (Hemiramphidae), fishes that are characterized by a long lower jaw in juveniles of all genera (and adults of most species) and short or moderately long pectoral fins; flyingfishes (Exocoetidae), unique aquatic vertebrates that use their tail and their large, wing-like pectoral fins to launch themselves out of surface waters and glide through the air; sauries (Scomberesocidae), oceanic fishes that live near the surface of the water, and ricefishes (Adrianichthyidae), a group that is not discussed in this article (Collette et al. 1984, Smith and Heemstra 1986, Collette and Parin 1998, Lovejoy 2004). The phylogenetic development of gliding in the Beloniformes has been investigated by numerous biologists. This topic is discussed in the next section.

Evolutionary ethology: the observations of Konrad Lorenz

Generations of naturalists have observed and described the flight among members of the family Exocoetidae (see Fish 1990 and references cited therein). The zoologist Konrad Lorenz (1903 – 1989) was one of the first to speculate on the phylogenetic development of gliding in the Beloniformes. His key publication, published in an obscure journal in German (Lorenz 1963), has never been cited in any of the reviews and original papers dealing with this subject (see, for instance, Fish 1990, Davenport 1992, Dasilao and Sasaki 1998, Lovejoy 2000, Lovejoy et al. 2004). It is likely that these authors were unaware of Lorenz´ work, therefore making it worthwhile to briefly recapitulate the basic observations and conclusions of this eminent scientist.

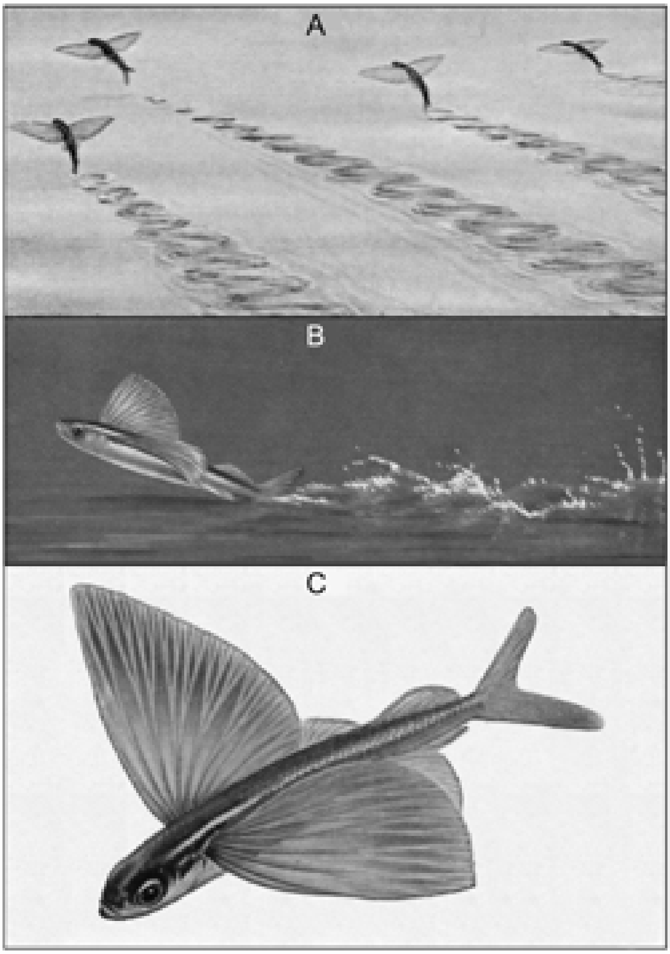

Fig. 5: Tracks of oceanic flyingfishes. The animals are using their tail to accelerate before leaping (A). Functioning like an outboard motor, the enlarged lower caudal lobe vibrates in a rapid side-to-side motion, generating a forward momentum (B). The California Flyingfish (Cypselurus californicus), an example of the four-winged group within the Exocoetidae, in flight (C) (Adapted from Lorenz 1963).

The Austrian biologist Konrad Lorenz is regarded as the founder of modern ethology, the systematic study of animal behaviour by means of the comparative method. His insights, concepts and hypotheses contributed to our understanding of how behavioural patterns evolved. Lorenz is also known for his work on the roots of aggression in animals and humans (Jahn 1998). The popular essay discussed here (Lorenz 1963) is to a large extent based on the work of earlier naturalists and on an article published three years earlier on the systematics and biology of flyingfishes (Klausewitz 1960). It should be noted that Lorenz (1963) did not include any references or the source of his figures. However, his illustration on the title page, reproduced here in modified form (Fig. 5), is a copy from earlier work on the Exocoetidae, as reviewed in Fish (1990).

In his review article, Lorenz (1963) described his own observations of flyingfishes and their allies as follows. Flyingfishes carry out a form of powered gliding. The caudal muscles beat the tail at a rate of 50 – 70 beats/s, which propels the fish out of the water (Fig. 5 A, B, 6 B, 7 A). As soon as the body is free of the water surface, the broad pectoral fins open at a maximal angle and an airborne glide begins (Fig. 5 C, 6 B, 7 B). Additional power can be derived during the glide by sculling the water with the enlarged lower lobe of the tail, which results in speeds of up to 70 km/h. Exocoetids do not flap their wing-like fins, but these organs can be used to steer and turn away from surface obstacles such as large rocks or boats.

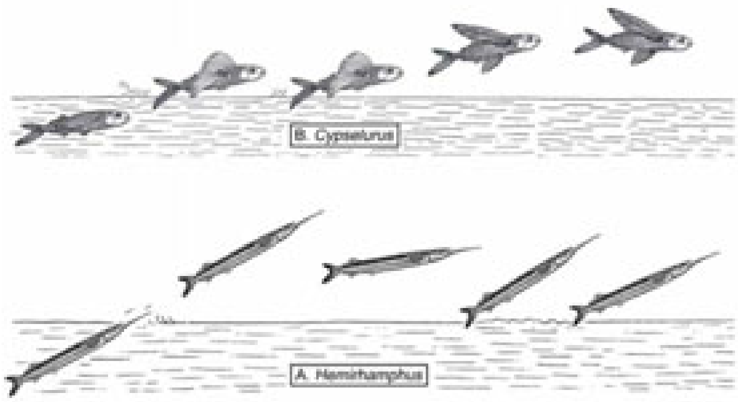

Fig. 6: Behaviour of the halfbeak Hemirhamphus (A) and the four-winged flyingfish Cypselurus (B) in response to attacks from aquatic predators. The halfbeak simply leaps from the water. The three successive stages in a flight by a cypselurine gliding fish can be summarized as follows (B). The fish approaches the water surface with paired fins folded (1.), pectoral fins spread as the animal breaks through the surface and the tail continues to oscillate in the water as the fish taxis along the surface (2.), pelvic and pectoral fins spread as the fish becomes fully airborne (3.) (Adapted from Klausewitz 1960).

Lorenz (1963) argued that the evolution of flight in Beloniform fishes can be reconstructed based on behavioural studies of extant species. He observed in aquaria that some fish species that inhabit the upper ten cm of the water (region just below the surface) have a forked caudal fin with a significantly enlarged lower lobe. These fish species (members of the genera *Pelecus* and *Alburnus*) occasionally "walk on the surface of the water" in response to attacks from predators. Halfbeaks (*Hemirhamphus*, *Oxyporhamphus*) are intermediate forms that display predator-driven jumps out of the water that are reminiscent of the flights of the exocoetids (Fig. 6 A, B). Hence, the evolution of flight in the Beloniformes originated with now extinct species that temporarily "walked out of the water" to escape predators. Lorenz (1963) did not distinguish between exocoetids that have two versus four "wings" (Fig. 8) However, he pointed out that the surface of the water, viewed from below, looks like a mirror: the aquatic prey organism, driven out of the liquid medium, becomes invisible to the predator.

Fig. 7: Oscillatory side-to-side movement of the tail of a flyingfish, viewed from above (A). A four-winged (biplane-type) cypselurid fish, front view in flight (B) (Adapted from Breder 1930).

The evolution of gliding in Beloniform fishes: a synthesis

Darwin (1872) proposed that the phylogenetic development of novel body plans is driven by the same mechanisms that cause the origin of new varieties and species. This classical concept of "phylogenetic gradualism" (Gould 2002) has developed into a basic tenet of the modern theory of biological evolution: large phenotypic changes (origin of higher taxa) are brought about by successive microevolutionary processes. Although exceptions to this rule exist, there is consensus among the majority of biologists that macroevolution (phylogenetic development above the species level) is the product of numerous microevolutionary steps (Mayr 1963, 2001; Futuyma 1998; Zimmer 1998, Carroll 2000, 2001; Simons 2002, Kutschera and Niklas 2004, 2005).

The predator-driven development of gliding in Beloniform fishes discussed here is an example of a macroevolutionary trend. It is obvious that the beating tail of the exocoetids, which propels the fish clear of the water, and the enlarged pectoral fins are organs that have undergone an "intensification and/or change in function" (Mayr 1963): the tail acts as a "motor", the fins are "wings", the flyingfish displays the aerodynamic properties of an aeroplane or a hirundine bird (swallow).

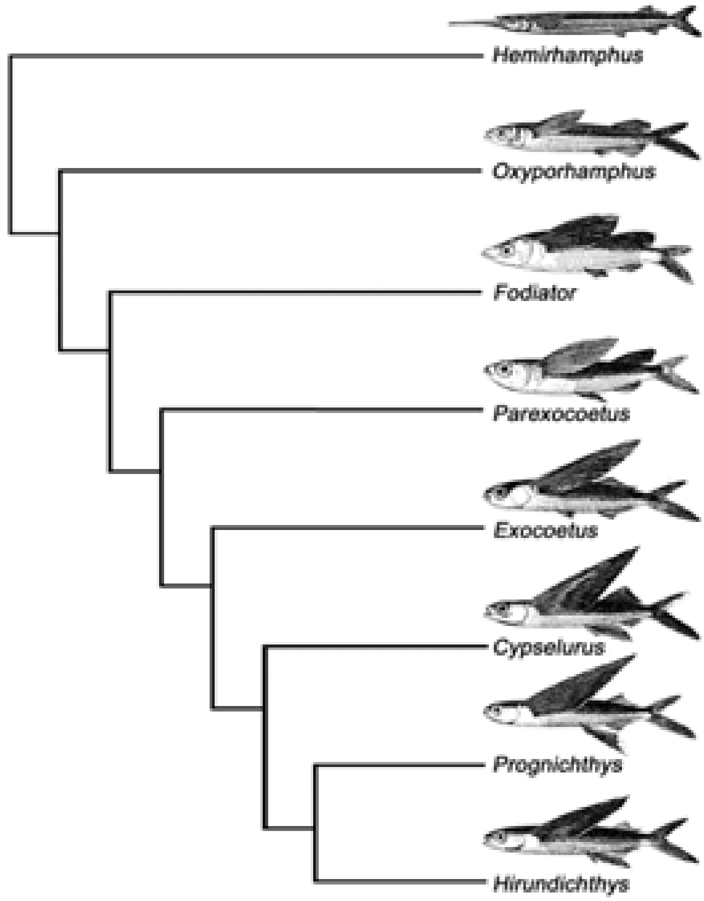

Fig. 8: Cladogram of halfbeaks (Hemirhamphidae) and flyingfishes (Exocoetidae). According to this scheme, the shortwing flyingfish Oxyporhamphus is a member of the Exocoetidae. Three four-winged (biplane-type) exocoetids are depicted in the lower part of the cladogram (Cypselurus, Prognichtys, Hirundichthys) (Adapted from Dasilao and Sasaki, 1998).

A hypothesis for this macroevolutionary trend in the Beloniformes, based on observations of extant "model organisms" that represent various stages in phylogeny, is depicted in Figure 9. This scheme is an expanded and modified version of the "historical reconstruction" presented by Lorenz (1963), with reference to Klausewitz (1960). The data of Dasilao and Sasaki (1998) (Fig. 8) are largely in accordance with the phylogenetic hypothesis discussed here.

Members of the fish family Cyprinidae (minnows or carps) that inhabit the upper region of the waters (*Pelecus*, *Alburnus* and others), are able to "walk on the surface" to escape predatory attacks (Fig. 9 A). These "walking fish" may represent the ancestral stage in this evolutionary trend. Halfbeaks, represented by members of the genus *Hemirhamphus*, are prone to leap out of the water; they usually perform a short "walk on the surface" before they temporarily leave the liquid medium (Fig. 9 B). According to Lorenz (1963), the hemirhamphid *Oxyporhamphus* represents an intermediate form between a typical halfbeak and a true flyingfish. This "shortwing flyingfish" (Smith and Heemstra 1986) has an elongated lower jaw only as a juvenile (i.e., it recapitulates the halfbeak stage during ontogeny), a deeply forked caudal fin (lower lobe longer than upper), and longer wing-like pectoral fins than other typical halfbeaks (Fig. 9 C). Dasilao et al. (1997) have provided evidence that, based on morphological data, the halfbeak Oxyporhamphus should be considered a basal flyingfish, as suggested by Lorenz (1963). However, molecular data presented by Lovejoy et al. (2004) place *Oxyporhamphus* within the *Hemirhamphus* clade. These contradictory results indicate that the "shortwing-halfbeak" *Oxyporhamphus* is an extant intermediate form between the Hemiramphidae and the Exocoetidae.

Breder (1930) was the first to distinguish between two categories of flyingfishes (Exocoetidae), "two-wingers" (*Fodiator*, *Parexocoetus*, *Exocoetus* etc.) in which the enlarged pectoral fins make up most of the lifting surfaces, and "four-wingers" (*Cypselurus*, *Prognichthys*, *Hirundichthys* etc.) in which both pectoral and pelvic fins are hypertrophied (Fig. 8). According to Collette and Parin (1998) two-winged exocoetids may glide for a distance of 25 m, whereas four-winged species may achieve 200 m or more with the extra lift generated by the enlarged pelvic fins. However, both types of exocoetids use their hypertrophied lower portion of the asymmetrical tail fin to provide the impetus for the free flight (Fig. 7 A). A number of studies have shown that "two-wingers" like *Parexocoetus* (Fig. 9 D), along with *Exocoetus* and *Fodiator*, are the least sophisticated gliders. These "primitive" flyingfishes are at the base of the exocoetid tree, as studied by cladistic methods (Fig. 8; Dasilao and Sasaki 1998).

It is obvious that the more sophisticated "biplane gliders" (*Cypselurus* and related taxa) (Fig. 9 E) evolved from more basal "two-wingers"; these "living airplanes" represent the extant peak in Exocoetid evolution. They build up speed by taxiing like aircraft and resemble herring-like swallows. The subtropical flyingfish *Hirundichthys* depicted in Fig. 8 is a bird-like vertebrate, with black fins that look like the wings of some Aves that glide over large distances (Smith and Heemstra 1986).

Lovejoy et al. (2004) reconstructed the phylogeny of 54 species of Beloniform fishes, using fragments of two mitochondrial and two nuclear genes. These molecular data generally confirm the concept depicted here (Fig. 9), with the exception that the intermediate form *Oxyporhamphus* occurs deeply within the *Hemirhamphus* clade.

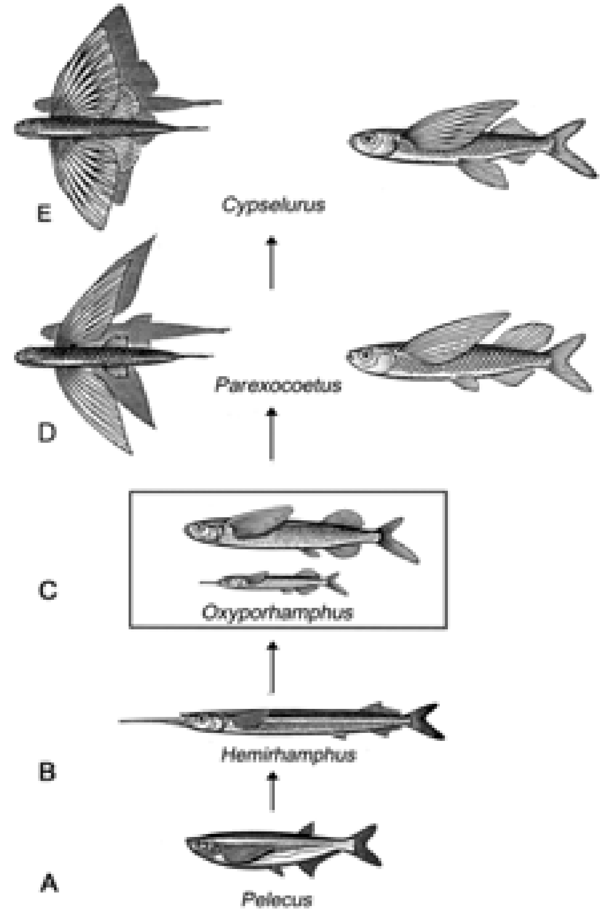

Fig. 9: Predator-driven evolution of gliding in Beloniform fishes, based on behavioural studies of extant species. Cyprinid (Pelecus) that occasionally leaps at the surface (A), halfbeak (Hemirhamphus) that jumps out of the water (B). The short-wing flyingfish (Oxyporhamphus), depicted as adult and juvenile individuum, represents an intermediate form that recapitulates the halfbeak-stage during ontogenesis (C). Monoplane-type flyingfish (Parexocoetus) that has a single set of long pectoral fins (wings) (D) and biplane-type (Cypselurus) that has under wings (pelvic fins) staggered far back from the main wings (E) (Adapted from Lorenz 1963).

In addition, the phylogenetic trees reconstructed on the basis of DNA-sequence data shed light on the ontogenetic recapitulation of the "halfbeak stage" in the "shortwing flyingfish" *Oxyporhamphus* and related taxa (Lovejoy 2000, Lovejoy et al. 2004; for historical accounts, see Gould 1977 and Levit et al. 2004). This topic is beyond the scope of the present article.

In conclusion, the results summarized here show that the evolutionary history of Beloniform fishes can be reconstructed without fossil data. Based on behavioural studies, molecular data and the strict use of the comparative method, the phylogenetic development of gliding in the exocoetids has now been elucidated: the macroevolutionary trend depicted here (Fig. 9) was driven by predatory attacks from below. In marine exocoetids, this selection pressure must have been severe, so that novel bird-like body plans evolved in this unique group of epipelagic fishes.

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Spontaneous versus Equivocal Generation in Early Modern Science

Peter McLaughlin

Abstract
*A distinction not often heeded by historians of science between spontaneous generation and so-called equivocal generation (*generatio aequivoca*) is essential to an understanding of 17th and 18th century theories of generation. Equivocal generation was almost universally rejected, but some sort of spontaneous generation at least of the first life forms whether in one step or in a series of stages is an almost inevitable part of any materialist system. And thus many scientists in the early modern period believed that spontaneous generation could be unequivocal. I shall offer some clarification of the distinction using a number of examples from the 17th and 18th centuries.*

As recent scholarship has pointed out, the final demise of spontaneous generation has been celebrated at least three times in the modern age: once each in the latter 17th, 18th, and 19th centuries (Mendelsohn 1976, Farley 1977). Each time the size of the organisms sought for decreased – first insects, then infusoria, then bacteria. And each time the purported refutation followed upon a revival of the traditional materialist theory of generation: pangenesis, according to which each part of the body contributes a representative particle to the germ. Since pangenesis theories tend to view even normal sexual generation as a sort of spontaneous assembling of preselected particles, they are quite congenial to ideas of spontaneous generation. Francesco Redi's *Experiments on the Generation of Insects* (1668) has often been taken as the decisive refutation of traditional theories of spontaneous generation. Reflecting on Redi's famous experiments on putrefying flesh, John Ray summed up in 1691:

> My Observation and Affirmation is, that there is no such Thing in Nature, as Æquivocal or Spontaneous Generation, but that all Animals, as well small as great, not excluding the vilest and most contemptible Insect, are generated by Animal Parents of the same *Species* with themselves. (Ray 1691, 221)

And in the *Lexicon Technicum* (1704) of John Harris, a reliable source for Newtonian and Lockean orthodoxy, the entry for "equivocal generation" maintains that "the Learned World begins now to be satisfied, that there is nothing like this in Nature." However, numerous thinkers in the mid 17th century (as well as in the mid 18th century) explicitly advocate some form of spontaneous generation; and they also sometimes specifically deny that this kind of generation is in any way equivocal. Thus there seems to be more to the story than meets the eye in Harris' *Lexicon*.

Since the beginnings of modern science in the early 17th century there have been two major breeding grounds for theories about spontaneous generation: the first is the question of the generation in the *present* of small organisms especially insects and intestinal worms; the second is the question of the *historical origin* of life itself including larger animals. Both sorts of questions are related at various levels, but it has been possible for scientists to agree on the answer in one area and to disagree in the other. I shall not give a chronicle of various theories of spontaneous generation; rather I shall use a few examples to illustrate the conceptual problems with which such theories were dealing.

First of all I shall introduce a conceptual distinction between spontaneous generation and equivocal generation (*generatio aequivoca*). This distinction is, I think, essential to an understanding of 17th and 18th century theories of generation. Secondly, I shall illustrate the importance of this distinction on the example of the intestinal worms of the domesticated pig as the problem presented itself to one important biologist, J.F. Blumenbach, near the end of the 18th century. Thirdly, I shall present and analyze the kind of explanation of the historical origin of life proposed by G.-L.L. de Buffon in the middle of the 18th century in order to point out the philosophical presuppositions of this sort of theory.

Spontaneous but not equivocal

The two terms *spontaneous* generation and *equivocal* generation are often used synonymously by historians and were also often used synonymously by scientists in the 17th and 18th centuries – at least by those who *rejected* spontaneous generation. On the other hand, a significant number of those who advocate some form of spontaneous generation explicitly reject what they call "equivocal" generation. In fact aside from the Aristotelians and some eclectics such as Kenelm Digby in the mid seventeenth century one can scarcely find a serious scientist who favored equivocal generation, although many favored spontaneous generation. So what is the difference?

Spontaneous generation is relatively straightforward: In the Aristotelian and Christian tradition generation was as a rule sexual and living creatures had parents of the same species. This is the position expressed above by John Ray. Generation without parents would be irregular, exceptional, and fortuitous – but this had not always been objectionable. In the Aristotelian tradition rules have exceptions. The world is complex and multifarious but not necessarily deterministic. Thus in an exceptional, parentless case when an organism arises out of organic matter (heterogenesis) or when anything organic arises out of anorganic matter (abiogenesis), we can say that generation is spontaneous or fortuitous. Whenever an organism arises without parents we have spontaneous generation. But why, one might ask, does Ray insist that the parents be of the same species?

Equivocal generation is somewhat more complicated. It was considered a form of accidental, non-lawlike generation. In logic an argument is equivocal if the meaning of a term is changed in the course of the argument. Equivocation in generation on the other hand occurs when the species of progeny is changed in generation. That is, whenever parents and progeny, generator and generated don't belong to the same species of thing, the progeny are equivocally generated. For instance, if something organic arises from something non-organic, it is specifically different from that from which it was generated. But also if an animal of one species gives birth to an animal of another species, we also have a case of equivocal generation. Thus if an elephant were to give birth to a hippopotamus or to anything that is not an elephant, generation would certainly be equivocal, although it is hard to view it as spontaneous in any usual sense. This means that any kind of degeneration, transformation or evolution which continues beyond the boundaries of the species would be a form of equivocal generation, generation that is not species-true. This sort of generation was universally rejected in the 18th century. The question is thus: can there be spontaneous generation that is not equivocal?

Many scientists of the 17th and 18th century believed that spontaneous generation could be unequivocal. For instance, if the same solutions, the same ingredients, always give rise to the same species of infusoria, and different solutions give rise to different kinds of infusoria, there seems to be nothing equivocal about this sort of generation. If there is a lawlike connection between the ingredients mixed and the species of organisms produced, then one could maintain (and many did) that there is nothing accidental or equivocal about this. Far from being fortuitous, such generation is merely an instance of matter in motion following its necessary laws, of particles combining into those structures into which they can be organized. In the words of Descartes' errant disciple Regius: "Formatio illa non est fortuita sed fit ex certis et necessariis legibus motus" (Regius 1654, 224). The early atomists Pierre Gassendi, Nathaniel Highmore and Walter Charleton also insisted that spontaneous generation was not accidental, but rather completely determined. Highmore (1651, 83–84) criticized "equivocal generations" opposing them to the „regular disposure" of atoms in the germ. Charleton maintained,

> that those insects or spontaneous Animals have their *causes certain*, and by reason of that energie once conferred upon their Efficients, must arise to animation in such and such a *Figure*, according to the magnitude, number, situation, complexion, quiet, motion or in a word *Temperament* of those particles, out of which their bodies are amassed; and according to the activity of that domestick *Heat*, which ferments and actuate the matter (1652, 54–55).[1]

Perhaps the most prominent advocate of spontaneous generation in the 18th century, John Turberville Needham, maintained that the "vegetative force" which he introduced to explain generation (including spontaneous generation) was constant for each species and thus *prevented* generation from being equivocal:

> God may have established Forces in Nature, subsisting Forces by which such Principles may in certain Circumstances, be invariably united, without any Danger of deviating, so as to render Generation equivocal (Needham 1748, 626).

Spontaneous (or parentless) generation can thus be considered to be just as lawlike and unequivocal as sexual generation.

[1] Gassendi (1658, 805) distinguished between animals whose seed is found in the parents and those whose seed "is hidden in foreign and so to speak unexpected material"; the second kind are wrongly called equivocal "because only the external and apparent cause is taken into account, not the internal hidden one."

In order to clarify the problem of a spontaneous but not equivocal form of generation, we need to introduce a few basics about normal sexual generation as this was conceived in the early modern period. The mechanistic theories of generation of the 17th and 18th centuries share a common basic principle that goes back to Descartes which could be so formulated: The entire heterogeneity of the body of an organism is completely represented materially in the germ.[2] The germ contains at least a sufficient cause of the body if not a small model or miniature; in any case it contains a system that is *alive* with the same kind of life as the later organism. The central question of the theory of the organism was to explain the production of this germ. The two relevant alternatives in the 18th century were the (deistic) theory of pre-existing germs and the (materialistic) theory of pangenesis, which was later replaced by the adoption of various chemical or vital forces. Preexistence theories stipulated that all organisms arise out of germs that God created at the beginning of the world. Within the school there were various positions about how these germs were stored for future use – encased within one another or spread with the winds. But there was no need or indeed room for the subsequent new production of germs. Parentless generation is divine; subsequent generation is the unfolding of preexisting structures that merely pass through one or both of the parents (Roger 1997, McLaughlin 1989). In pangenesis, on the other hand, germs produce bodies, and bodies (by pre-selecting particles) produce germs according to laws of nature – as a rule inside one of the parents, but sometimes outside the parents.

Spontaneous Generation of Worms

Let me now take an example from the first area of spontaneous generation, the generation of insects and intestinal worms in order to illustrate where the distinction between spontaneous and equivocal can lead us. I shall present a problem analyzed by the German physiologist and natural historian, Johann Friedrich Blumenbach, around 1790. We can accept the facts and their interpretation as he presents them, since the purpose of the example is to illustrate how to deal *conceptually* with the problem.

Blumenbach (1789; 1806, ch. 5) ascertains two facts which present him with an interesting problem.

> *Fact 1*. Domesticated pigs constitute a race descended from wild pigs within human history. Although the two races have certain morphological differences, there is no question that they belong to the same species.
>
> *Fact 2*. Domestic pigs have intestinal worms of a species not found anywhere else. Wild pigs do not have this species of intestinal worms. The worms of domesticated pigs cannot survive in wild pigs.

From these facts he derives a problem.

[2] Descartes [1648, 277] wrote: "If one knew exactly in detail all the parts of the seed of a particular species of animal, for instance Man, one could deduce from that alone for reasons entirely mathematical and certain, the whole figure and conformation of each of its parts, just as the other way around knowing some particulars of this conformation one can deduce from this what the seed is."

> *Problem:* Where do the worms in domestic pigs come from? How can a new *race* of pigs contain a new *species* of intestinal worms?

There seem to be two possible solutions to this problem.

> *First:* The species of intestinal worms in domesticated pigs could be descended from one of the species found in wild pigs (just like the domesticated race has branched off from the wild type). This would imply that at some point in time the species boundary was crossed in generation, that is, that equivocal generation had occurred and that an individual of one species of intestinal worms generated an individual of a different species of worms. In this case generation would be equivocal but not spontaneous.
>
> The *second* possibility is that the new species of intestinal worms in the domestic race of pigs arose spontaneously as soon as the domesticated pigs had changed enough so that their intestines presented a significantly different material environment, which had not before existed. Thus a combination of matter which had not been viable earlier could now be viable under the new circumstances. In this case generation is spontaneous but not equivocal.

In his discussion of the problem Blumenbach makes it clear that he prefers the second to the first alternative, thus committing himself to spontaneous generation in order to avoid transformation of forms that crosses the species boundary, i.e. equivocal generation.

The Origin of Life

Let us now turn to the question of the spontaneous first origins of life. Some sort of spontaneous generation of the first life forms whether in one step or in a series of stages is an almost inevitable part of any materialist system. In the mid 18th century the static deistic systems of the 17th century, in which God created the world and all the organisms in one act and then retired to observe his work, gave way to materialist theories of the origin of the earth which had the planets explode out of the sun and cool down, or had evenly distributed particles gravitate into central bodies and heat up. For such theories the question of the origin of life was significantly different from the question of the first origin of the planets or of matter itself: it was a physical question, not a metaphysical one.

The most famous (today) of these new systems is probably Immanuel Kant's *Theory of the Heavens* (1755). Kant's book concludes with a speculation entitled "On the inhabitants of the celestial bodies," in which he maintains that it would be "absurd" to deny that most of the planets are inhabited at some time or other, but he says nothing at all about how he thinks they come to be inhabited. He sticks to generalities.

The trouble (for the historian) with most writers of the period is similar: They say enough, that one has reason to suspect that they *must* have believed in some form of spontaneous generation even of elephants and whales – if they were to be at all consistent. However, they avoid making explicit and unequivocal statements to this effect. Nonetheless, at least one major figure in the Enlightenment had no qualms about making explicit the logical consequences of materialism and affirming them. Georges-Louis Leclerc de Buffon published the table below (Table 1) in 1775 in the second supplement volume of his *Histoire naturelle*.

The title of the table reads "Beginning, end, and duration of the existence of organized nature on each planet." On the left all the known planets and their moons are listed. Then each row gives four numbers for each planet. Column 1 indicates the dates for the beginning of life on each planet – counting from the year in which the planets were formed. Column 2 gives the dates at which life ceases on each planet. Columns 3 and 4 give the durations of life in the past and the future (that is: before and after 1775). Take the second row for instance: on the Moon the first life forms arose 7,890 years after the formation of the Moon, they all died out 72,514 years after the formation of the Moon, so that life lasted a total of 64,624 years, and there is now no more life on the Moon.

This is certainly an interesting, if somewhat wild, speculation. But how does Buffon think he knows all this? He is not merely maintaining, as does Kant, that there is probably life on other planets. He gives exact figures where and when; and (though this cannot be seen from the table) he is not speaking merely of the origin of some life form or other, but of the origin of *particular species* of organisms, which he can name. (The first organisms are huge aquatic animal, i.e. sea monsters.)

The table stands at the end of two full volumes of reported experiments, observations, and calculations which are supposed to prove the conclusions drawn in the table. Buffon's empirical base for these assertions consists almost entirely of a series of experiments performed in his iron foundry, in which he had metal and stone balls of various sizes and compositions heated up as high as the ovens would go: He then measured the cooling rates of these spheres and extrapolated the results to the larger spheres of the planets. From this data he concludes, for instance, that in 13,576 B.C. sea monsters arose on Jupiter's third moon.

The question that needs to be asked is: under what assumptions, philosophical, physical, historical, etc. is it reasonable or perhaps even compelling to believe this. I shall present some assumptions necessary to make the argument plausible that can be found explicated elsewhere in various other writings of Buffon.

Assumption 1: All planets were thrown out of the sun at the same time.

Plausible implications

1) All planets have basically the same physical composition and have been cooling down for the same amount of time.

2) The surface temperature on different planets is thus a function of the size of the planet, with minor adjustments for differences in the distance from the Sun and from other large planets.

3) Empirically based inferences about the material composition of the planet Earth, its age, and the date at which its surface temperature fell below the boiling point of water can be generalized to the other planets.

Time elapsed since the Formation of the Planets .... 74,832 years

BEGINNING, END & DURATION *of the Existence of* ORGANIZED NATURE *on each* Planet

	BEGINNING OF LIFE		END OF LIFE	ABSOLUTE DURATION OF LIFE	DURATION OF LIFE FROM TODAY ONWARD
		number of years after the formation of the planets	number of years after the formation of the planets		
5th	satellite of Jupiter	5,161	47,550[a]	42,389 yrs.	0 yrs.
	THE MOON	7,890	72,514	64,624	0
	MARS	13,685	70,326[b]	56,641	0
4th	satellite of Saturn	18,399	76,525	58,126	1,693yrs.
4th	satellite of Jupiter	23,730	98,696	74,966	23,864
	MERCURY	26,053	187,765	161,712	112,933
	THE EARTH	35,983	168,123	132,140	93,291
3rd	satellite of Saturn	37,672	156,658	118,986	81,826
2nd	satellite of Saturn	40,373	167,928	127,555[c]	93,096
1st	satellite of Saturn	42,021	174,784	132,763	99,952
	VENUS	44,067	228,540	184,473	153,708
	Ring of Saturn	56,396	177,568	121,172	102,736
3rd	satellite of Jupiter	59,483	247,401	187,918	172,569
	SATURN	62,906	262,020	199,114	187,188
2nd	satellite of Jupiter	64,496	271,098	206,602	196,266
1st	satellite of Jupiter	74,724	311,973	237,249	237,141
	JUPITER	115,623	483,121	367,498	

Table 1 (Buffon 1775, 514)

[a] in the original: 47,558
[b] in the original: 60,326
[c] in the original: 127,655

Thus, Buffon's table gives his best estimate of the dates at which the surface temperatures of the various celestial bodies dropped down to a temperature where life is possible.

Let us accept Buffon's estimates of the dates at which the physical conditions of life become available on various planets. But we still need some further assumptions before we can deduce the existence of life from its mere physical possibility.

Assumption 2: Reductionism or mechanistic determinism: all bodies or systems are unequivocally determined by the intrinsic properties of their parts and by the laws governing the motions and interactions of the parts.

> *Plausible implications*
>
> 1) Under the same conditions the same kinds of particles will or will tend to combine into and determine the same kinds of material systems.
>
> 2) Buffon concludes: as soon as the temperature drops a certain amount below the boiling point of water organic molecules arise. Once organic molecules have arisen they begin to combine due to mechanical and chemical forces. All possible combinations are attempted; all stable combinations become fixed. Some of these are not only stable but also viable: these are organisms. All species of organisms that are viable and reproducible under these physical conditions must exist. Each of the planets has in principle the same spectrum of animals and plants as does the Earth.

As Buffon puts it: Given the same matter, "the same temperature supports, produces everywhere the same beings" (Buffon 1775, 510).[3]

We can check the correctness of this interpretation of the constraints on theory formation by a thought experiment. What would happen if some catastrophe occurred and wiped out all the inhabitants of a planet but afterwards allowed the physical conditions to return to normal? Buffon seems to be committed to the proposition that all the species wiped out in the conflagration would return again very soon. And in fact he plays through this experiment and accepts exactly this conclusion, merely assuming the organisms would be somewhat smaller due to the cooling of the earth and that the larger species might not make it the second time around (1777, 363–367). This very same thought experiment was carried out explicitly by Blumenbach with the same results: the same vital force working on the same materials would produce the same spectrum of organic forms. Moreover, Blumenbach even takes such a catastrophe or "total revolution" actually to have occurred in a preadamite period.

> On the whole the creator surely let the same forces of nature act to bring about the new organic realms as fulfilled this intention in the previous world. Except that in the production of the new species the formative drive (*Bildungstrieb*) had to take a direction more or less deviating from the previous one due to matter's being differently modified by such a total revolution. (1806, 19–20)

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[3] Buffon does not take up the question of whether the same species on different planets might look somewhat different given that the gravitational fields are significantly different.

Thus can one also explain the difference between current and fossil forms of the same species without equivocal generation.

Conclusion

In the Aristotelian tradition sexual generation was the rule and spontaneous or equivocal generation the exception. There was no need to distinguish various forms of unusual or exceptional generation that were not "for the sake of" the species form. But the connection in this tradition between being parentless and being irregular was purely contingent. The later mechanistic and materialistic approaches of the 17th century dissolved this contingent connection by applying the traditional concepts in situations and to objects for which they were not originally intended. In a Cartesian material world that is causally closed, things that are not law governed do not occur. In a world that arises out of matter in motion, the first parent-organisms also arise out of matter in motion. If generation without parents is equivocal or non-lawlike, it doesn't occur; and if it occurs it cannot be equivocal. Even parentless generation must be law-governed and species specific. Whether it actually occurs in the present is a genuinely empirical question. The first origin of living creatures must, however, have been without parents, and if it was a natural phenomenon, then it can also be explained by natural laws. Thus the view that at least the first origin of life was spontaneous but not equivocal is a integral metaphysical and methodological assumption of early modern mechanistic materialism.

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Ernst Haeckel and the Struggles over Evolution and Religion

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Abstract
As a young man, Ernst Haeckel harbored a conventional set of Evangelical beliefs, mostly structured by the theology of Schleiermacher. But the conversion to Darwinian theory and the sudden death of his young wife shifted his ideas to the heterodox mode, more in line with Goethe and Spinoza. Haeckel's battles with the religiously minded became more intense after 1880, with attack and counterattack. He particularly engaged Erich Wasmann, a Jesuit entomologist who had become an evolutionist, and the Keplerbund, an organization of Protestant thinkers who opposed evolutionary theory and accused him of deliberate fraud. In these struggles, Haeckel defined and deepened the opposition between traditional religion and evolutionary theory, and the fight continues today.

If religion means a commitment to a set of theological propositions regarding the nature of God, the soul, and an afterlife, Ernst Haeckel (1834-1919) was never a religious enthusiast. The influence of the great religious thinker Friedrich Daniel Schleiermacher (1768-1834) on his family kept religious observance decorous and commitment vague.[2] The theologian had maintained that true religion lay deep in the heart, where the inner person experienced a feeling of absolute dependence. Dogmatic tenets, he argued, served merely as inadequate symbols of this fundamental experience. Religious feeling, according to Schleiermacher's *Über die Religion* (On religion, 1799), might best be cultivated by seeking after truth, experiencing beauty, and contemplating nature.[3] Haeckel practiced this kind of Schleiermachian religion all of his life.

Haeckel's association with the Evangelical Church, even as a youth, had been conventional. The death of his first wife severed the loose threads still holding him to formal observance. The power of that death, his obsession with a life that might have been, and the dark feeling of love forever lost drove him to find a more enduring and rational sub-

[1]This article is based on my forthcoming book, *The Tragic Sense of Life: Ernst Haeckel and the Struggle over Evolutionary Thought in Germany*.
[2]Wilhelm Bölsche, who interviewed Haeckel's aunt Bertha Seth (sister of his mother), describes the impact of the Schleiermachian view on the family in his *Ernst Haeckel: Ein Lebensbild* (Berlin: Georg Bondi, 1909), pp. 10-11.
[3]I have discussed Schliermarcher's religious ideas in *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe* (Chicago: University of Chicago Press, 2002), pp. 94-105.

stitute for orthodox religion in Goethean nature and Darwinian evolution. The passions that had bound him to one individual and her lingering shadow became transformed into acid recriminations against any individual or institution promoting what he saw, through Darwinian eyes, as cynical superstition.[4] The antagonism between conservative religion and evolutionary theory, brought to incandescence at the turn of the century and burning still brightly in our own time, can be attributed, in large part, to Haeckel's fierce broadsides launched against orthodoxy in his popular books and lectures. These attacks and reactions to them were brought to a new level of intensity during the period from 1880 to his death in 1919.

Figure 1: Haeckel in Ceylon, 1881 1882 (courtesy of Haeckel Haus, Jena).

"Science Has Nothing to Do with Christ"—Darwin

On April 21, 1882, Haeckel finally reached his home in Jena after a six-month research trip to India and Ceylon, where his sensitivity to religious superstition had been brought to a higher pitch (fig. 1). Upon his return, he immediately learned that his friend and mentor, Charles Darwin (1809-1882), had died three days before, on April 19. Later, that October, Haeckel traveled to Eisenach, a morning's train ride away, to attend the fifty-fifth annual meeting of the Society of German Natural Scientists and Physicians, during which he would celebrate his friend's great contributions to science. The plenary lecture that Haeckel gave sang a hymn to Darwin's genius and to the extraordinary impact of his theory on all realms of human thought, emancipating that thought for a rational approach to life.[5] Haeckel argued that the Englishman followed upon the path first hacked through the jungle of religiously overgrown biology by the likes of Lessing, Herder, Goethe, and Kant. Indeed, Darwin had solved the great problem posed by Kant, namely "how a purposively directed form of organization can arise without the aid of a purposively effective cause."[6] In his encomium, Haeckel, like the devil, could appeal even to scripture—or at least to one who

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[4]I have discussed the impact of the death of Haeckel's first wife on his science and on his rejection of orthodox religion in "The Aesthetic and Morphological Foundations of Ernst Haeckel's Evolutionary Project," in Mary Kemperink and Patrick Dassen (eds.), *The Many Faces of Evolution in Europe, 1860-1914* (Amsterdam: Peeters, 2005).

[5]Ernst Haeckel, "Ueber die Naturanschauung von Darwin, Göthe und Lamarck," *Tageblatt der 55. Versammlung Deutscher Naturforscher und Aerzte in Eisenach, von 18. bis 22. September 1882* (Eisenach: Hofbuchdruckerei von H. Kahle, 1882), pp. 81-91.

[6]Ibid., p. 82.

translated scripture in the very city of Eisenach: just as Martin Luther, who "with a mighty hand tore asunder the web of lies by the world-dominating Papacy, so in our day, Charles Darwin, with comparable over-powering might, has destroyed the ruling, error-doctrines of the mystical creation dogma and through his reform of developmental theory has elevated the whole sensibility, thought, and will of mankind onto a higher plane."[7]

Haeckel certainly advanced no new ideas in his lecture—something his close friend Hermann Allmers (1821-1902) observed after reading the text[8]—but he did eloquently reinforce four points: that Darwin fulfilled the promise of higher German thought—especially that of Goethe; that the evolutionary theories of Goethe, Lamarck, and Darwin were as vital to modern culture and as substantial as the locomotive and the steamship, the telegraph and the photograph—and the thousand indispensable discoveries of physics and chemistry; that Darwinism yielded an ethics and social philosophy which balanced altruism against egoism; and, in summary, that Darwinian theory and its spread represented the triumph of reason over the benighted minions of the anti-progressive and the superstitious, particularly as shrouded in the black robes of the Catholic Church. In Haeckel's analysis, then, Darwinism was thoroughly modern, liberal, and decidedly opposed to religious dogmatism. To drive his message home, Haeckel read to the audience a letter Darwin had sent to a student of Haeckel, a young Russian nobleman who had confessed to the renowned scientist his bothersome doubts about evolutionary theory in relation to revelation. The letter read:

Dear Sir:

I am much engaged, an old man, and out of health, and I cannot spare time to answer your questions fully,--nor indeed can they be answered. Science has nothing to do with Christ, except in so far as the habit of scientific research makes a man cautious in admitting evidence. For myself, I do not believe that there ever has been any revelation. As for a future life, every man must judge for himself between conflicting vague probabilities.

Wishing you happiness, I remain, dear Sir, Yours Faithfully,

Charles Darwin[9]

What Darwinism offered instead of traditional orthodoxy, Haeckel contended, was Goethe's religion: a "monistic religion of humanity grounded in pantheism."[10] This declaration of rationalistic faith would hardly be the recipe to satisfy those who yet hungered after the old-time convictions.

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[7]Ibid., p. 81.

[8]Hermann Allmers to Ernst Haeckel (January, 1883), in *Haeckel und Allmers: Die Geschichte einer Freundschaft in Briefen der Freunde*, ed. Rudolph Koop (Bremen: Arthur Geist Verlag, 1941), pp. 149-50.

[9]Haeckel, "Ueber die Naturanschauung von Darwin, Göthe und Lamarck," p. 89. Haeckel translated the letter into German. A copy of the original, which I have used here, is held in the Manuscript Room of Cambridge University Library. The letter was addressed to Nicolai Alexandrovitch Mengden.

[10]Haeckel, "Ueber die Naturanschauung von Darwin, Göthe und Lamarck," p. 89.

For the assembled at Eisenach—and for those many others who read the published text of Haeckel's lecture—the recitation of Darwin's letter functioned as a kind of anti-Bridgewater treatise; it drove a wedge into the soft wood of compatibility between science and traditional religion, utterly splitting the two. The lecture revealed that an aggressive, preacher-baiting German was not the only evolutionary enemy of faith but that the very founder of the theory had also utterly rejected the ancient beliefs. Several English authorities complained that Haeckel had committed a great indiscretion in communicating Darwin's private letter even before the earth had settled around his grave.[11] But indiscrete or not, the message could hardly be planner: Darwinian theory was decidedly opposed to that old-time religion. And as Haeckel discovered during the next three decades (and as we are still quite aware), that old-time religious was decidedly opposed to modern Darwinian theory.

Monistic Religion

Haeckel had, over the course of a quarter of a century, expressed his own religious views both negatively and positively. The negative critique attacked orthodox religion, dismissing its belief in an anthropomorphic Deity and deriding its view of an immaterial human soul. Haeckel was an equal opportunity basher of all orthodox doctrines—that of Christianity, Judaism, Muslimism, and the faiths of the East. Yet he still thought of himself as a religious person; though his was the religion of Spinoza and Goethe. He took opportunity to synthesize his negative and positive critiques when invited to Altenburg (thirty miles south of Leipzig) to help celebrate the seventy-fifth anniversary of the *Naturforschende Gesellschaft des Osterlandes* (The Natural Research Society of the Eastern Region). At the meeting on October 9, 1892, Haeckel was preceded by a speaker who said something rather irritating about the relationship of science and religion. Haeckel tossed aside his prepared text and gave a lecture extemporaneously, which he wrote down the next day from memory, augmenting where necessary. The lecture was published in the popular press and as a small monograph, *Der Monismus als Band zwischen Religion und Wissenschaft* (Monism as the bond between religion and science)—a book that would reach a seventeenth edition just after Haeckel's death. It became the foundation for the even more successful *Die Welträthsel* (The world puzzle), which would be published in 1899.

In his small tract, Haeckel argued for a unity of the world, in which homogeneous atoms of matter expressed various properties through the fundamental powers of attraction and repulsion. These atoms propagated their effects through vibrations set up in an ocean of ether. From the inorganic, through the simplest organisms, right up to man, no unbridgeable barriers arose; rather a continuous, law-governed unity ran through the whole. Even what might be called man's soul—his central nervous system—appeared

[11]Haeckel mentioned to Allmers the unfavorable response coming from England at the publication of Darwin's letter. See Ernst Haeckel to Hermann Allmers (26 December 1882), in *Ernst Haeckel: Sein Leben, Denken und Wirken*, ed. Victor Franz, 2 vols. (Jena: Wilhelm Gronau, 1943-1944), 2: 81. Edward Aveling, consort of Karl Marx's daughter and translator of *Das Kapital* into English, wrote Haeckel to describe the cowardly reaction of the British press to Haeckel's exposition of the letter. See Edward B. Aveling to Ernst Haeckel (6 October 1882), in Ernst Haeckel, *Die Naturanschauung von Darwin, Goethe und Lamarck* (Jena: Gustav Fischer), pp. 62-64.

over the course of ages by slow increments out of antecedents in the lower animals. Though Haeckel's enemies thought this cosmology to be the sheerest materialism, he yet maintained his was a strict monism: all matter had its mental side, just as all examples of mind displayed a material face. This meant that the elements of perception and thought could be traced right down to the simplest organisms—every one-celled protist could thus boast of a "soul"—after a manner of speaking. This sort of conception gave the comparative psychologist, according to Haeckel, permission to discover the antecedents of human cognitive ability in animal life. The great unity pervading the universe, a universe governed by ineluctable law, could be understood materially as nature in her organized diversity and spiritually as God; or as Spinzoa expressed it: *deus sive natura*.

While Haeckel wished to whisk away all anthropomorphisms from religion, he thought something was yet worth preserving from the old dispensation. This was the ethical core of traditional orthodoxy, especially of Christianity:

> Doubtless, human culture today owes the greater part of its perfection to the spread and ennobling [effect] of Christian ethics, despite its higher worth often in a regrettable way being injured by its connection with untenable myths and so-called "revelation."[12]

Haeckel's tract had an immediate and, for the author, a surprising outcome: he was sued. This occurred because of a note that he appended to his discussion of anti-Darwinian scientists. He mentioned, as he had often before, Louis Agassiz (1807-1873) and Rudolf Virchow (1821-1902) as objectors to descent theory. He added that more recently, his former student and assistant Otto Hamann (1857-1928) had taken a reactionary turn in his book *Entwicklungslehre und Darwinismus* (Evolutionary theory and Darwinism, 1892). Hamann went from being an enthusiastic supporter of Darwinian evolutionary theory during his years with Haeckel to rejecting it for a more distinctively teleological and ultimately religious conception in his new publication.

In his book, Hamann variously argued: that the paleontological evidence indicated gaps in the fossil record;[13] that von Baer had shown long ago that embryos were of consistent type, not passing from one type to another;[14] and that the gap between the mental abilities of men and animals was absolute.[15] He maintained, in opposition to "Darwinian dogmatism," that one had to explain the goal-striving character [*Zielstrebigkeit*] of life as based on "inner causes" that produced macro-mutations responsive to altered environments. The great harmony in the natural system of coordinated adaptations discovered by the naturalist was "the same as that unity and harmony which men prior to all scientific research feel and have sensed—a unity and limitlessness that goes by the name of God."[16]

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[12]Ernst Haeckel, *Der Monismus als Band zwischen Religion und Wissenschaft, Glaubensbekenntniss eines Naturforschers* (Bonn: Emil Strauss, 1892), p. 29.
[13]Otto Hamann, *Entwicklungslehre und Darwinismus. Eine kritische Darstellung der modernen Entwicklungslehre* (Jena: Hermann Constenoble, 1892), pp, 7-20.
[14]Ibid., pp. 21-26.
[15]Ibid. p. 120.
[16]Ibid., p. 288.

Haeckel felt the sting of this apostasy. The argument of Hamann's volume, he remonstrated, was the very opposite of science; rather it was "from the beginning to the end a great lie."[17] Haeckel attributed the reversal in his one-time student's attitude not to the discovery of new truths about the failure of Darwinism but to his own failure to receive an academic appointment. Hamann had implored his former teacher to recommend him for a vacant chair in zoology at Jena. Haeckel did put him on a list of candidates submitted to the faculty senate, but did not place his former student among the top contenders. Hence, as Haeckel charged in his *Monismus*, Hamann took his revenge by going over to the dark side. Yet, all that would be needed to bring him running back, Haeckel supposed, would be "the jingle of coins."[18]

Hamann sued Haeckel because of this characterization, contending loss of income and slander. He requested the court grant him a total of 7500 marks, 6000 for reduced income and 1500 as punishment for the libel. Haeckel countersued, and the case was heard in the Schöffengericht (a lower court) in Jena. During the process, it came out that Hamann had misrepresented himself as a professor at Göttingen, whereas he was only a Privatdozent there, though professor in the Royal Library in Berlin. Haeckel put in evidence a series of obsequious letters from Hamman, in which the supplicant referred to his former teacher as a god whom he revered. The court concluded that Haeckel did slightly slander Hamann and fined him 200 marks; the judge also levied a fine of 30 marks against Hamann. Both were enjoined not to speak of the conflict again, and Haeckel complied by expunging his remarks from subsequent editions of his *Monismus*. Most on-lookers thought that Haeckel had won the moral victory, or so an anonymous account of the case reported.[19] This trial is probably the source of the rumor, one still bubbling around in the heads of many creationists, that Haeckel had been brought before a "university court" by five of his colleagues where he was judged guilty of having committed scientific fraud. Though Jena had a student *Kerker*, a jail, a university court is an unknown entity and any talk of one could come only from brains on the boil.[20]

Erich Wasmann, a Jesuit Evolutionist

The Challenge of the Catholic Church

Ever since his medical school days in Bavaria, Haeckel had been both attracted and repelled by the Catholic Church, especially by its black-robed combat troops, the Jesuits. While in Rome, unlike Goethe who rather enjoyed the pomp of Papal celebrations, Haeckel felt his north-German sensibilities continually assaulted. Protestant liberals like Haeckel, on due reflection, came to perceived the wars against Austria and France not only as political-social conflicts but also as struggles against an alien religious force. Intel-

[17]Haeckel, *Der Monismus als Band zwischen Religion und Wissenschaft*, pp. 42-43.
[18]Ibid., p. 43.
[19]Anonymous, *Der Ausgang des Prozesses Haeckel-Hamann* (Magdeburg: Listner & Drews, 1893).
[20]This mythical story can be found on a large number of creationist websites. The words "Haeckel" and "university court" in any search engine will dump the sites on to a waiting computer.

lectual and cultural threats from the Church were codified for liberals in the series of condemnations listed in Pope Pius IX's *Syllabus Errorum* (1864), his brief of particulars brought against the modern world. Condemned were such heretical tenets as pantheistic naturalism, the autonomy and sufficiency of reason to discover the truth, freedom of individuals to embrace any religion, civil control of education, and unbridled speech. The declaration by the Vatican Council (1870) of papal infallibility only heightened the cultural clash between the Vatican and liberal movements all over Europe—including those within the Catholic Church itself. Otto von Bismarck (1815-1898), the Chancelor of the German Empire, recognized that the negative reaction of liberals made it opportune to curb the growing power of the Catholic Center Party. He promoted what Virchow called a *Kulturkampf*—a cultural battle—but one fought with the force not of persuasion but of legislation. At Bismarck's instigation, the Reichstag passed a series of laws, the so-called May Laws of 1872-1875, that restricted the civil activities of the Catholic clergy, especially in performing state-recognized marriages and in education. In 1872, the Jesuits, the perceived sinister agents of Pius IX, were expelled from Germany; and the next year all religious orders, except those directly concerned with care of the sick, had to disband. The suppression of the Catholic Church in Germany by the liberal-dominated Reichstag ran against the principles of those same liberals, who often acted out of religious intolerance and prejudice, and, as Gorden Craig has suggested, not a little out of the economic advantages accruing to those of a more materialistic taste.[21] Even among individuals differing on many other issues—Haeckel and Virchow, for instance—the exclusion of the Jesuits and the restrictions on the Catholic clergy found favor. By the end of the 1870s, however, the political situation began to flex as Bismarck's worries turned from Catholics to the growing socialist movements. In 1878, a new Pope, Leo XIII, ascended to the chair of Peter. Leo sought accommodation with the German government; and with a lessening of tensions, the legal and extra-legal opposition to the Catholic Church began to ease. The old *Kulturkampf* abated, but a new one, more personal, was turned against its original author as the young emperor William II (1888-1918) strove to take a greater hand in the social and foreign affairs of his government. Quickly relations with his aged Chancellor deteriorated, until the exit became clearly marked and the door opened. Bismarck departed in 1890. Thereafter the Social Democrats and the Center Party continued to gain seats in the Reichstag, as a more accommodating head of state took command.[22]

The new political dispensation drove Haeckel further into a conservative and anti-religious mode. In a move that angered many of his colleagues at Jena, he and several other professors, students, and town's people met Bismarck and invited him to visit Jena to be honored for his creation of and service to the Empire. With this as something of a fait accompli, Haeckel then informed Archduke Carl Alexander of Saxe-Weimar-Eisenach (1818-1901), officially rector of the university, of the personal invitation. The archduke made the invitation official and Bismarck accepted it. At the end of July, 1892,

[21]Gordon Graig, *Germany, 1866-1945* (Oxford: Oxford University Press, 1980), 78-79.
[22]See James Scheehan, *German Liberalism in the Nineteenth Century* (Chicago: University of Chicago Press, 1978), p. 223.

the old Chancellor addressed a cheering throng of students and townspeople gathered in the market place. Since he had already received honors from various law and medical faculties throughout the Empire, his benefactor devised a new degree to be conferred on the Chancellor—the degree of doctor of phylogeny, honoris causa! The degree, of course, suggested more about the turn of the new government—with rumors spreading that the king might convert to Catholicism—than about any contributions Bismarck might have made to this special branch of biology.[23] Through the next decade, the political and social situation, from the old liberal point of view, continued to deteriorate. In 1903, the newly elected pope, taking the ominous name of Pius X, cast a lengthening shadow up from the south. The threat of Catholic revanchism brought an invitation from friends in Berlin for Haeckel to sally forth and to take up arms against the newly resurgent Church. The invitation,

> especially mentioned that the continually growing reaction in the leading circles, the over weaning confidence of an intolerant orthodoxy, the shift in balance toward ultramontane Papism, and the consequent threat to German spiritual freedom in our universities and schools—that all of this made an energetic defense a pressing necessity.[24]

Haeckel accepted the invitation and, in 1905, gave three lectures in the great hall of the Sing Akademie in Berlin to over two thousand enthusiastic auditors on each of the succeeding days. He rehearsed, in a minor key, the indictment against old enemies, especially those who either rejected or hesitated to endorse evolutionary theory, but orchestrated a thundering denunciation of a new and quite unexpected foe. This was a group most conspicuously represented by an entomologist, a man who was chiefly responsible for bringing the old bear out of his cave.[25] This individual argued strongly for evolutionary theory, grounding his defense in extremely compelling empirical evidence; and he had just written a scientifically exemplary study, *Die moderne Biologie und die Entwickelungstheorie* (Modern biology and evolutionary theory, 1904). But the scientist was also a Jesuit priest, Father Erich Wasmann (1859-1931). For the Jesuits to endorse evolution meant that subtle chicanery had to be afoot. Haeckel declared Wasmann's book "a masterpiece of Jesuitical confusion and sophistry."[26] Wasmann bears some extended consideration not only because of the vehemence of Haeckel's reaction but also because of this Jesuit's scientific acumen, which has preserved his name in the reference lists of modern ento-

[23]See the brief account of Haeckel's involvement in the invitation to Bismarck by Else von Volkmann, granddaughter of Haeckel, in her "Ernst Haeckel veranlasste die Einladung Bismarck's," in *Ernst Haeckel, Sein Leben, Denken und Wirken*, 1: 82-86; see also Haeckel's account of the invitation, in ibid., 2: 119-22.
[24]Ernst Haeckel, *Der Kampf um den Entwickelungs-Gedanken: Drei Vorträge, gehalten am 14, 16, und 19 April 1905 im Salle der Sing-Akademie an Berlin* (Berlin: Georg Reimer, 1905), p. 7.
[25]Haeckel mentioned to his biographer, Wilhelm Bölsche, that it was Wasmann who provoked what he thought would be his last public lectures. See Ernst Haeckel to Wilhelm Bölsche (3 April 1905), in *Ernst Haeckel-Wilhelm Bölsche, Briefwechsel 1887-1919* (Ernst-Haeckel-Haus-Studien, vol. 6/1), ed. Rosemarie Nöthlich (Berlin: Verlag für Wissenschaft und Bildung, 2002), p. 173.
[26]Haeckel, *Kampf um den Entwickelungs-Gedanken*, p. 32.

mological studies, and especially because he provides a telling case of an individual whose scientific observations trumped his initial dogmatic convictions.[27]

The Guests of Ants—Evidence for Evolution

Since his days in the Jesuit seminary in the Netherlands, Wasmann had been an enthusiastic collector of bugs (not unlike the Cambridge student Charles Darwin). Because of a recurring lung infection, the young seminarian could not go to the missions or teach in a Jesuit school after finishing the philosophy curriculum. Instead he was allowed to engage in private theological study and to continue exercising an obvious talent for entomological research. His interest in this latter quickly turned to ants and a class of beetles that lives symbiotically in ant nests, the so-called "myrmecophile" or "guest of ants." In the short period from 1884 to 1890, Wasmann had over sixty publications on ants, termites, and their guests. His meticulous study of slave-making behavior in ants of the new and old worlds culminated in a work that secured his reputation as a leading authority in entomology: *Die zusammengesetzten Nester und gemischten Kolonien der Ameisen* (The commonly established nests and mixed colonies of ants, 1891). He concluded that work with a consideration of its bearing on evolutionary theory. He argued that slave-making ants in the Americas and Europe, which displayed common instincts, had either to have been created originally with these behavioral traits or to have evolved in the two, widely separated locations in a strictly parallel fashion, which on Darwinian grounds seemed quite improbable. One had to acknowledge, therefore, that a higher intelligence had established internal laws of development and instilled their causal processes in the hereditary structure of these organisms.[28] Wasmann's anti-evolutionary convictions, however, became muted after deeper study of those odd beetles that came to live in ant nests. Indeed, through empirical evidence supplied by the guests of ants, he dramatically altered his original attitude toward evolution.

In a series of articles first appearing in *Biologisches Zentralblatt* and in *Stimmen aus Maria-Laach*,[29] and then summarized in *Moderne Biologie und die Entwicklungstheorie*, Wasmann presented extensive and quite detailed empirical evidence for evolutionary transitions in the myrmecophile.[30] He distinguished three kinds inquilines, or ant-guests, according to

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[27]Of the hundreds of authors cited by Edward O. Wilson in his *Insect Societies* (Cambridge: Harvard University Press, 1971), Wasmann has about the eighth largest number of citations, some fourteen (p. 521). Abigail Lustig has written an illuminating essay on Wasmann and colleagues. See her "Ants and the Nature of Nature in Auguste Forel, Erich Wasmann, and William Morton Wheeler," in *The Moral Authority of Nature,* eds. Lorraine Daston and Fernando Vidal (Chicago: University of Chicago Press, 2004): 282-307. Lustig also has published a comparison of the intellectual styles of Haeckel and Wasmann. See her "Erich Wasmann, Ernst Haeckel and the Limits of Science," *Theory in Biosciences* 121 (2002): 252-59.

[28]Erich Wasmann, *Die zusammengesetzten Nester und gemischten Kolonien der Ameisen* (Münster i.W.: Aschendorff'schen Buchdruckerei, 1891), pp. 252-53.

[29]See Erich Wasmann, "Gibt es tatsächlich Arten, die heute noch in der Stammesentwicklung begriffen sind?" *Biologisches Zentralblatt*, 21 (1901): 685-711, 737-52; "Konstanztheorie oder Deszendenztheorie?" *Stimmen aus Maria-Laach* 56 (1903): 29-44, 149-63, 544-63.

[30]Erich Wasmann, *Die moderne Biologie und die Entwicklungstheorie*, 2nd ed. (Freiburg im Breisgau: Herdersche Verlagshandlung, 1904), pp. 210-45. The third edition (1906) was also published in English translation: Erich Wasmann*, Modern Biology and the Theory of Evolution,* trans. A. M. Buchanan (St. Louis, Mo.: B. Herder, 1914).

their morphology and behavior: the aggressive type (*Trutztypus*), the symphilic type, and the mimetic type. Aggressive, tank-like beetles could be found in the genus *Dinarda.* These species displayed heavily armored, compact individuals that were impervious to ant attacks. Wasmann examined four species that were distributed over north central Europe and showed that they varied in color and size depending on the color and size of the species of ants with which they lived. The similarity of color made the beetles less conspicuous in the nests; and appropriate size made them less vulnerable to attacks on their appendages. Wasmann asserted that "we have here, therefore, a case in which we can explain effortlessly and completely satisfactorily, by the simplest natural causes, the differentiation of similar species of the same genus from a common progenitor."[31] He further argued that the genus *Chitosa*, which inhabited southern Europe, had to be related to *Dinarda* through a common ancestor. Thus, he concluded, evolutionary adaptations had been acquired in the descent of species. Moreover, inquilines found in termite nests in India suggested that beetle species in the genus *Doryloxenus*, typical of the myrmecophile dwelling with African wandering ants (*Dorylus*), had come to live with termites, quite different insects; moreover, one could trace alterations in the species of this genus as they evolved more effective adaptations for protecting themselves against termite attacks.

Wasmann drew further evidence of evolutionary transformation in the symphilic group of myrmecophile, those that secreted a sweet exudate and were fed by the ants in return. He showed that species of the *Lomechusini* varied in features dependent on the species of ant with which they lived. The most startling evidence he produced, however, was within the mimetic group. These were beetles that had evolved to look like ants. Wasmann showed that myrmecophile of quite different genera that yet inhabited nests of the same species of ant had converged in their morphologies (see fig. 2).

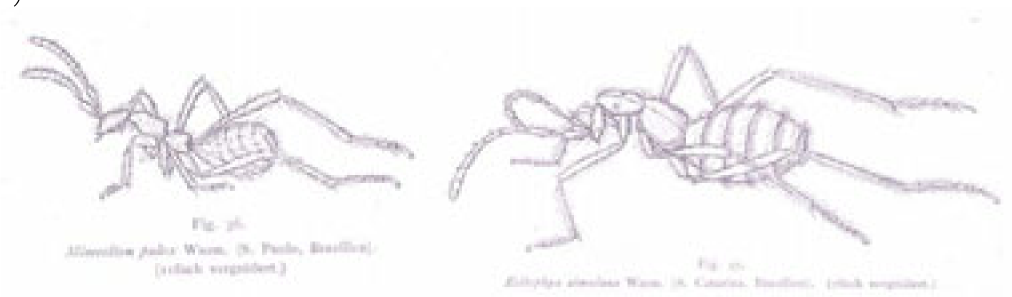

Figure 2: Two species of mimetic myrmecophile, beetles that have evolved to look like ants (from Wasmann's Moderne Biologie und die Entwickelungstheorie).

[31]Erich Wasmann, "Gibt es tatsächlich Arten, die heute noch in der Stammesentwicklung begriffen sind?" *Biologisches Zentralblatt*, 21 (1901): 685-711, 737-52; citation on pp. 694-95.

On the basis of such evidence, Wasmann affirmed that "we ought calmly accept the evolutionary doctrine insofar as it is scientifically founded on a definite class of structures with a sufficient degree of probability."[32]

While Wasmann thought his inquilines—and also various ant species—offered compelling empirical evidence for descent with modification, he would still not yield to Darwinian theory. He argued that several considerations precluded natural selection as the primary agent of change. First, selection could only eliminate possibilities once they arose, not create them initially—a common enough objection (and a common enough misunderstanding of Darwin's device). Second, he argued that most variations were neutral, so that selection would have no purchase on them. Third, though species of the *Lomechusini* evolve because the ants, as it were, selected those with the sweetest liquor—what Wasmann called "amical selection"—the beetles yet ate ant pupa and thus were positively harmful to the ant community, something natural selection should have prevented.[33] Finally, a gradual change, as Darwin would have it, in these inquiline species ought to take hundreds of thousands of years, exhausting, as Wasmann estimated, the geological time available.[34] Instead of Darwinian evolution, Wasmann proposed a theory of evolution that seems to have been a hybrid of ideas drawn from Hugo De Vries (1848-1935) and Hans Driesch (1867-1941). Like De Vries, he argued that alterations in species would come as macro mutations; and like Driesch, he held that *Anlagen*—dispositions—in the hereditary structure of organisms would respond to external causal relationships in a teleologically directed way.

Wasmann maintained that the marshaled evidence suggested that certain natural *Ur-species*, coming from the hand of the Creator, formed the base of the stem-trees whose branches held the derived species of plants and animals. Since we had no evidence of spontaneous generation, we had to assume a divine act as the source of the several types of life. Wasmann regarded it an open question as to the number of original types—perhaps only a few, perhaps more. But one type, he vigorously insisted, was unique, namely the human.

Wasmann rejected the possibility that human beings might have arisen out of the stock of lower animals.[35] Human intellect simply bore no relationship to what passed as animal intellect—an argument that Wasmann retained from his earliest considerations of the question. He continued to reject Haeckel's monistic metaphysics as the proper foundation for understanding human beings or animals. While he allowed that man's body might have been prepared by an evolutionary process prior to the reception of the soul,

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[32]Wasmann, *Moderne Biologie und die Entwicklungstheorie,* p. 219.

[33]While E. O. Wilson cites Wasmann's work throughout his *Insect Societies*, he obviously did not penetrate Wasmann's German very deeply. Wilson believes that Wasmann did not recognize that symphilic beetles often preyed on ant pupa (p. 390), something that Wasmann, in fact, emphasized as part of his argument against natural selection.

[34]We now know that beetles were diversely proliferating during the Permian, 300 million years ago; and fossil ants of more than 90 million years old have recently been discovered. It is reasonable to suppose the symbiosis between the two has existed for many millions of years. See Grimaldi, D.A., Agosti, D., and Carpenter, J.M., "New and Rediscovered Primitive Ants (Hymenoptera: Formicidae) in Createous Amber from New Jersey, and their Phylogenetic Relationships." *American. Museum Novitates,* no. 3208 (1997): 1-43.

[35]Wasmann, *Moderne Biologie und die Entwicklungstheorie,* pp. 273-304.

the leading contenders for this kind of pre-adaptation—Neanderthal man and Dubois's Java man—were, he thought, both unlikely candidates as proto-humans. Neanderthals, as Virchow suggested, were quite within the range of human variation—so they were real human beings; and Dubois's discovery appeared to be only that of a giant ape unrelated to the human stock.

The Confrontation between Wasmann and the Monists

In his Berlin lectures, Haeckel took delight in referring to Wasmann as the "Darwinian Jesuit," an ironically intended designation that yet begrudgingly suggested some respect for this Jesuit's accomplishments in entomology.[36] But he simply derided Wasmann's rejection of a thorough-going evolutionism in the case of human beings: "If Wasmann assumes this introduction of the soul for the development of the type, then he must postulate in the phylogeny of the anthropoid apes an historical moment in which God descends and injects his spirit into this hitherto spiritually bereft ape soul."[37] Haeckel thought the whole assumption absurd, but not innocent of political consequence. He suspected that the conservative Prussian government would seek a union of "crown and altar" not for reasons of religious conviction but for reasons of practical advantage. He was convinced that this would be no even match; under the banner of reconciliation, the crown would become "the footstool of the altar," as the Church bent the state to its own purposes.[38]

When Wasmann read of Haeckel's attack in the several newspapers that described the lectures, he penned a long open letter to his nemesis, which appeared on page one of the morning edition of the *Kölnische Volkszeitung* (2 May 1905).[39] He complained that Haeckel too easily identified evolutionary theory with monism, and thus misleadingly suggested that the Jesuits and the Church had come over to the Darwinian side. Wasmann rejected Haeckel's assumption of only one meaning for evolution, and he protested that his own theistic version had no official sanction from the Church or the Jesuits. About this second point, Wasmann would eventually be proved mistaken: his view of evolution came to be widely accepted by the Catholic Church as a way of accommodating this latest scientific, though dangerous, advance. Under Wasmann's orchestration, the Vatican could at last admit the world actually moved.

The drama of the evolution-religion conflict and a sense of its high-culture entertainment value brought Wasmann, amidst a flurry of newspaper interpretations of the debate, an invitation in 1906 to reply to Haeckel at the Sing Akademie. He declined the offer, but a short time later did accept a comparable invitation issued by a group of prominent scientists in Berlin. Initially he was to have addressed a meeting of the entomological society, but Ludwig Plate (1862-1937), a member of the inviting committee

[36]Haeckel, *Der Kampf um den Entwickelungs-Gedanken*, p. 75.
[37]Ibid., p. 83.
[38]Ibid., p. 84.
[39]Erich Wasmann, "Offener Brief an Hrn. Professor Haeckel (Jena)," *Kölnische Volkszeitung* 46, no. 358 (2 May 1905): 1-2.

and an associate of Haeckel, insisted that the meeting be open to the public.[40] Wasmann agreed and he further allowed that after his three public lectures, his opponents could present their objections and he would respond. Initially some twenty-five critics requested time, but Wasmann left it up to the committee to pare down the list to something manageable.

Figure 3: Erich Wasmann, S.J., about 1900 (courtesy of Maastricht Natural History Museum).

On February 13, 14, and 17, 1907, Wasmann lectured in the Sing Akademie each day to over one thousand people, who paid one mark for each occasion (two for reserved seating). He took as his subjects: the general theory of evolution and its support drawn from entomology; varieties of evolutionary theory—theistic and monistic (atheistic); and the problem of human evolution.[41] At 8:30 on the evening of February 18, with the audience swelling to some two thousand men and women, eleven opponents confronted Wasmann in the auditorium of the Zoological Gardens. His objectors were allotted varying amounts of time, with Plate, the principal organizer, receiving the longest period at half of an hour. Wasmann was granted thirty minutes to answer his eleven critics (fig. 3). He mounted the podium at 11:30 p.m., with the full complement of the audience still in their seats. He focused his response on Plate's objections, and brought in others as time permitted. He asserted that he would surrender to the idea of spontaneous generation if the scientific evidence demonstrated the likelihood, but he could not allow the creation of matter and its laws to be proper scientific subjects. These latter problems lay in the province of metaphysics, about which he would nonetheless be happy to argue. His own position on the purely scientific issues, he said, were close to that of Hans Driesch: one had to postulate, internal vital laws to devise adequate explanations of species descent. Though Plate and others continued to attribute an interventionist theology to Wasmann, he claimed that his science did not require that—though he was philosophically committed to the belief that God had created matter and its laws, which laws might, he allowed, eventually include those governing spontaneous generation. And while the evolution of man's body from lower creatures had yet to be shown, he also allowed that as a possibility. But, he maintained, it was the natural science of psychology that absolutely distinguished human mentality from animal cognition, and

[40]Wasmann had already crossed pens with Plate in the pages of the *Biogisches Zentralblatt* (1901), where he defended evolutionary descent in the guests of ants but not on the monist's terms. See Wasmann, "Gibt es tatsächlich Arten, die heute noch in der Stammesentwicklung begriffen sind?"

[41]Several accounts of Wasmann's lectures and the ensuing debate are extant. I have relied on the book-length descriptions given by Wasmann himself and his principal opponent, Ludwig Plate. See Erich Wasmann, *Der Kampf um das Entwicklungsproblem in Berlin* (Freiburg im Breisgau: Herdersche Verlagshandlung, 1907); and Ludwig Plate, *Ultramontane Weltanschauung und moderne Lebenskunde, Orthodoxie und Monisms* (Jena: Gustav Fischer, 1907). Wasmann's book was also published in English as *The Berlin Discussion of the Problem of Evolution*, authorized translation (St. Louis, Mo.: Herder Book Co., 1909).

therefore a gradual transition in mind from animals to man was precluded by science itself.

Wasmann's opponents shelled him not only with intellectual objections but also lobbed the occasional invective designed to dismember less substantial egos—Plate concluded that "Father Wasmann is not a genuine research scientist (*Naturforscher*), not a true scholar"; the anthropologist Hans Friedenthal (1870-1943) referred to Wasmann as a "dilettante" in the area of human evolution."[42] Yet Wasmann met the over-wrought responses with a calm professionalism made piquant with a "dry sense of humor" (as the *Berliner Morgenpost* characterized his lectures).[43] The *Deutsche Tageszeitung* judged that with the exception of Plate, Wasmann's opponents "seemed almost like pygmies."[44] After midnight, at the conclusion of the reply to his critics, Wasmann, according to the *Kölnische Volkszeitung,* received from the audience a "thunderous ovation."[45] It seems clear that if he did not always convince his auditors—some five hundred articles in the various German papers reported a variety of judgments—he at least charmed them. But from our historical perspective, he did more than that. He showed that evolutionary theory at the turn of the century still had not achieved consensus, though was rapidly approaching fundamental agreement among professionals of every philosophical conviction. And his subtle arguments demonstrated that no necessary antagonism had to exist between evolutionary theory and a liberal, philosophically acute brand of theology. Not all objectors from the side of religion showed themselves as high-minded as Wasmann. Certainly Arnold Brass of the Protestant Keplerbund did not.

The Keplerbund vs. the Monistenbund

Haeckel's book *Die Welträthsel* set off a swarming and stinging reaction from the many quarters that had already been aroused by Haeckel's frequent attacks on religion. While the book seemed, especially to the young, like a flaming torch lighting the way to liberation from the crushing hands of orthodox science and religion, others thought it an incendiary faggot set at the base of Christian civilization. Many of those for whom it illuminated the path to freedom joined the Monistenbund, originally a union of scientists and dedicated citizens who subscribed to Haeckel's program of monistic philosophy. Haeckel had harbored the idea of such an organization for several years. While attending the International Free-Thinkers Conference in Rome in 1904, where he was celebrated as the anti-pope, he thought it might then spontaneously form. When that failed, he took practical steps to bring it into existence.[46] The planning began in the wake of his Berlin lectures against Wasmann, and the initial meeting took place on January 11, 1906, in

[42]Plate, "Ultramontane Weltanschauung," p. 77, 93.
[43][Anonymous], "Pater Wasmanns Berliner Vorträge," *Berliner Morgenpost* (14 February 1907).
[44]*Deutsche Tageszeitung* (19 February 1907), as quoted by Wasmann in *Kampf um das Entwicklungsproblem in Berlin,* p. 148.
[45][Anonymous], "Pater Wasmann," *Kölnische Volkszeitung* (morning edition) no. 149 (20 February 1907), p. 2.
[46]Ernst Haeckel to Wilhelm Bölsche (15 October 1905), in *Ernst Haeckel-Wilhelm Bölsche: Briefwechsel*, pp. 180-81.

Figure 4: Eberhard Dennert, founder of the Keplerbund, about 1900 (from Dennert's Bible und Naturwissenschaft).

Jena. The first president selected was the radical Protestant pastor, Albert Kalthoff (1850-1906), though Haeckel quickly importuned the noted naturalist August Forel (1848-1931) to assume leadership.[47] Eventually the Nobel Prize winner Wilhelm Ostwald (1853-1932) would occupy the chair (1911), presiding over an organization that would grow to some six thousand members before disbanding in 1933 rather than be taken over by the Nazis. While the league was initially guided by Haeckel's declarations of monistic philosophy—especially its anti-dualism, anti-clericalism, and notions of scientific management of the state—it became a more heterogeneous alliance, embodying, as one of its early presidents maintained, the principles of the Enlightenment further elevated through modern science. It continued to stress scientific epistemology, world peace, international cooperation, and eugenic principles of forming a healthy society. While some of its members—Wilhelm Schallmayer (1857-1919), for instance—would preach race hygiene, others, like Magnus Hirschfeld (1868-1935), would preach tolerance for homosexuals. After the Great War, the Monistenbund became decidedly more pacifistic and socialistic. The society spread to most of the European countries, as well as America, where the journal *The Monist*, edited by Paul Carus (1852-1919), published Haeckel and many other like-minded philosophers and scientists.[48]

In 1907, the year after the founding of the Monistenbund, Eberhard Dennert (1862-1942), a botanist and teacher in the Evangelical Pädagogium in Bad Godesberg, called into existence "the Keplerbund for the Advance of Natural Knowledge" (fig. 4). This was an organization of Protestant scientists and laymen dedicated, as their initial call declared, to the conviction that:

> Truth encompasses the harmony of natural scientific facts with philosophical knowledge and religious experience. Accordingly, the Keplerbund is expressly distinguished from the materialistic dogma of biased Monism and struggles against the thoroughly atheistic propaganda of this latter, which falsely claims to be grounded on natural science.[49]

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[47]Heiko Weber, "Der Monismus als Theorie einer einheitlichen Weltanschauung am Beispiel der Positionen von Ernst Haeckel und August Forel," in *Monismus um 1900: Wissenschaftskultur und Weltanschauung*, ed. Paul Ziche (Berlin: Verlag für Wissenschaft und Bildung, 2000), 81-127.

[48]See Niles Holt, "Monists & Nazis: A Question of Scientific Responsibility," *Hastings Center Report* 5 (1975): 37-43. See also Richard Weikart, "Evolutionäre Aufklärung? Zur Geschichte des Monistenbundes," in *Wissenschaft, Politik und Öffentlichkeit*, eds. Mitchell Ash and Christian Stifter (Vienna: Universitätsverlag, 2002), pp. 131-48. For a contrasting picture of the Monist League, see Daniel Gasman, *The Scientific Origins of National Socialism* (New York: Science History Publications, 1971), especially pp. 31-54.

[49]Eberhard Dennert, *Die Naturwissenschaft und der Kamp um die Weltanschauung*, Schriften des Keplerbundes, Heft 1 (Godesberg b. Bonn: Naturwissenschaftlicher Verlag, 1910): 29.

The founder of the bund, Dennert, had trained in the Realeschule at Lippstadt under the Darwinian enthusiast Hermann Müller (1829-1883), who was the brother of the more famous Fritz Müller (1822-1897). The school master sent his best pupils to Jena. Dennert went to Marburg, where under the strongly anti-Darwinian Albert Wigand (1821-1886), he cultivated a distaste for evolutionary doctrine.

Dennert reacted like a tightly wound spring to Haeckel's *Welträthsel*, immediately firing off a broadside: *Die Wahrheit über Ernst Haeckel und seine "Welträtsel"* (The truth about Ernst Haeckel and his "Riddle of the Universe," 1901), one of the over ninety books and pamphlets venting his religious enthusiasms. [50] Under the flapping spread of his many tracts he sought the reconciliation of religion and science by draining the blood from one and emasculating the other. Religion, he asserted, was not a matter of understanding, of intellectual demonstration, but a matter of feeling. He thought it manifest from his own surveys of the faith of past scientists that "natural scientific research [*Naturforschung*] does not exclude simple Biblical faith, and that religious belief and religious life do not draw their proof from the intellect, but entirely from other factors. These factors [feelings of the heart] are available to every person."[51] In contrast to religious faith, science did require the most rigid intellectual demonstration: only unequivocal fact and theory strictly derived from fact could be admitted into its domain. But Darwinism, with its atheistic implications, froze the heart and supplied no set of demonstrated facts from which to launch its speculations. Thus, as a second requirement for reconciliation, Darwinian evolution had to be rejected. Typical of Dennert's effort was the often reprinted tract *Vom Sterbelager des Darwinismus* (On the deathbed of Darwinism, 1902), which cursorily and loosely examined the work of several biologists (e.g., Albert von Kölliker [1817-1905], Oskar Hertwig [1849-1922], Gustav Theodor Eimer [1843-1898]) who had alternative evolutionary proposals. The argument seems to be that all of these different variations on evolutionary theory somehow prove Darwin and Haeckel's version to be moribund. The heterogeneity of proposals concerning evolution and the ultimately inadequate efforts to substantiate it suggested to Dennert that the very doctrine of descent itself must also be quite doubtful. At least we could have no "clear and exact demonstration of evolutionary theory [*Entwicklungslehre*]," and thus the mode of its occurrence would of necessity remain forever hidden.[52]

Dennert found a particularly aggressive and paranoid ally in another hapless naturalist, Arnold Brass (b. 1854). Brass had failed to start his academic career in a way that would lead to a professorship: he wanted to work at the Naples Zoological Station, but was not chosen; at Marburg, his application for recognition of his habilitation was re-

[50]Eberhard Dennert, *Die Wahrheit über Ernst Haeckel und seine "Welträtsel, nach dem Urteil seiner Fachgenossen*, 2nd ed. (Halle: C. Ed. Müller's Verlagsbuchhandlung, 1905). The book is mostly a compilation of the positions of the various objectors to Haeckel, beginning with Ludwig Rütimeyer's charge of fraud.

[51]Eberhard Dennert, *Bibel und Naturwissenschaft* (Halle: Richard Mühlmann's Verlag, 1911), pp. 312-20.

[52]Eberhard Dennert, *Vom Sterbelager des Darwinismus*, neue Folge (Halle: Richard Mühlmann's Verlag, 1905), p. 6. Dennert rather liked Kropotkin's emphasis on cooperation in nature but thought it militated against the Russian's retention of Darwinian selection theory (pp. 123-34). But in sum, he thought transformation might occur, but we would never have any proof of it nor could we ever discover its mode. If we yet postulated it, we would have to assume internal driving forces (*Triebkräften*) as responsible (p. 6).

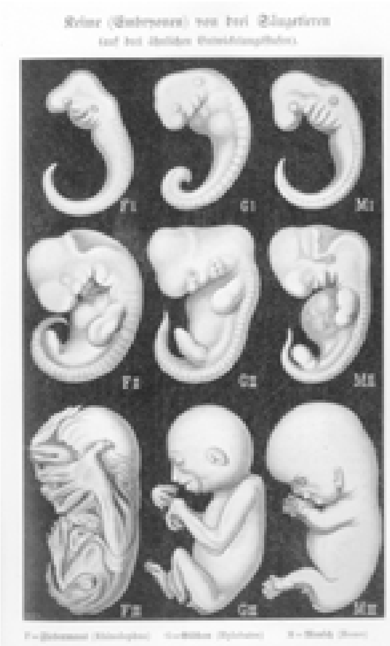

Figure 5: Vertebrates (bat, gibbon, human) at three stages of development (from Haeckel's Menschen-Problem).

jected. He had to fall back on itinerate work in zoology, usually producing drawings for various books and articles in anatomy. After the turn of the century, as he reflected on the derailment of his academic career two decades before, Brass began to suspect the conspiratorial hand of Ernst Haeckel.[53] Haeckel would later deny any such connivance, since he barely knew the man. In 1906, Brass published a tract that came to the defense of Dennert, who had been dismissed by Plate and Haeckel as an inept Christian apologist. In the booklet, *Ernst Haeckel als Biologe und die Wahrheit* (Ernst Haeckel as biologist and the truth, 1906), Brass remained fairly polite, actually rather sycophantic. He acknowledged Haeckel's "genius" and command of vast areas of zoology—far superior to Darwin's in this respect. But he thought himself able to meet the Jena lion on common ground. He expended most of his effort in the book describing the presumed deficiencies of Darwinian theory and arguing for the compatibility of reliable science with evangelical theology. After this publication, he began to lecture on Haeckel's monism, for which he received some financial support from the Keplerbund.[54] In these lectures, his opposition to monism in general and Haeckel in particular grew in stridency.

On April 10, 1908, Brass delivered a lecture in Berlin to a meeting of the Christian-Social Party at which he claimed that Haeckel had illustrated a recent talk in an "erroneous" fashion.[55] As reported in the Berlin *Staatsbürgerzeitung,* Brass asserted that in arguing

[53]Naively Brass let slip out his various failures to obtain desired academic positions, and increasingly detected Haeckel as the culprit. See Arnold Brass, *Ernst Haeckel als Biologe und die Wahrheit* (Halle: Richard Mühlmann's Verlag, 1906), pp. 10-11. See also the second edition of Brass's *Affen-Problem* (1909) as quoted by Reinhard Gursch, *Die Illustrationen Ernst Haeckels zur Abstammungs- und Entwicklungsgeschichte* (Frankfurt a. M.: Verlag Peter Lang, 1981), p. 89: "In 1886, I had submitted a habilitation work on the systematics of the mammals, etc. at Marburg for the first and only time. This audacity had angered Haeckel and others at the time. To exclude the possibility of my again attempting a habilitation in Marburg, Plate, a student of Haeckel, was admitted to the position of docent."

[54]Brass later denied he received any money from the Keplerbund—and maybe he did not. But the business director of the Keplerbund, Wilhelm Teudt, reported that Brass did receive financial guarantees from the society for his lectures in winter of 1807-1808. Haeckel would use this as an indictment. See Wilhelm Teudt, *"Im interesse der Wissenschaft! Haeckel's "Fälschungen" und die 46 Zoologen,"* Schriften des Keplerbundes, Heft 3 (Godesberg bei Bonn: Naturwissenschaftilicher Verlag, 1909), p. 7.

[55]I have reconstructed the course of these debates from two opposing sources, from the account of the Keplerbund's general business manger, Wilhelm Teudt, and from that of the secretary of the Monistenbund, Heinrich Schmidt. Both quote verbatim from newspaper articles and other sources, and both, of course, offer their particular interpretations of the events. See Teudt, *Im Interesse der Wissenschaft*; and Henrich

for the biogenetic law, Haeckel had made a "mistake" (*Missgeschick*) by depicting an ape embryo sporting the head of a human embryo and a human embryo with an ape head. The newspaper reported that "the lecturer could speak here from the most exact personal knowledge, since he himself had presented to Haeckel the correct illustrations."[56] The supposedly "mistaken" illustration was from Haeckel's Jena lecture on the occasion of the two-hundredth anniversary of Linnaeus's birth. The lecture was published as *Das Menschen-Problem und die Herrentiere von Linné* (The problem of man and the anthropoid animals of Linnaeus, 1907), and it had several illustrations appended to it. In the illustration that compared the embryos of a bat, gibbon, and human being, Brass claimed that Haeckel had switched the heads of the gibbon and human being depicted in the second row (fig. 5).[57]

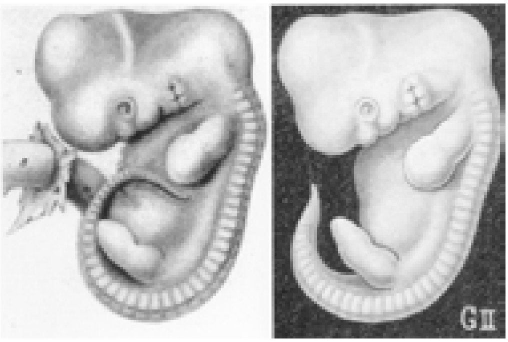

Figure 6: Macaque embryo (from Selenka's Menschenaffen, left) and Haeckel's depiction of a gibbon embryo (from his Menschen-Problem).

When Haeckel learned of Brass lecture, he explosively responded in an open letter to a colleague that the charge was a "barefaced lie" (*freche Lüge*); he did not make the alleged "mistake" and Brass certainly never prepared any illustrations for him. In a fury, he had his lawyer contact several newspapers threatening suit if they perpetuated this "brazen invention."[58] Brass immediately modified his charge in two newspaper articles (*Statsbürgerzeitung* and *Volk*, Berlin, April 25, 1908), now saying that the head of the gibbon in the illustration bore "more than the usual similarity to the human embryo at a similar developmental stage, which I have repeatedly sketched and illustrated from a preparation."[59] Haeckel quickly wrote to the same newspapers saying that he himself had not drawn the illustrations but had a designer do so relying on figures taken from well-known authors: the ape embryo, which he called a "hylobates" (a genus of gibbon), he said he took from Emil Selenka (1842-1902) and the human embryo was based on the work of a couple of authors, including Wilhelm His.[60] A comparison of Selenka's and His's images with those of Haeckel's lecture shows, indeed, a close

Schmidt, *Haeckels Embryonenbilder: Dokumente zum Kampf um die Weltanschauung in der Gegenwart* (Frankfurt a.M.: Neuer Frankfurter Verlag, 1909). In 1900, Schmidt had become Haeckel's assistant and protégé. See Uwe Hossfeld, "Haeckels 'Eckermann': Heinrich Schmidt (1874-1935)," in Matthias Steinbach and Stefan Gerber (eds.), *Klassische Universität und akademische Provinz: Die Universität Jena von der Mitte des 19. bis in die 30er Jahre des 20. Jahrhunderts* (Jena: Bussert & Stadeler, 2005), pp. 270-288.

[56]Schmidt, *Haeckels Embryonenbilder*, p. 8.

[57]Ernst Haeckel, *Das Menschen-Problem und die Herrentiere von Linné: Vortrag, gehalten am 17. Juni 1907 in Volkshause zu Jena* (Frankfurt a. M.: Neuer Frankfurter Verlag, 1907), table 3. This is the same illustration Haeckel had used in his *Der Kampf um den Entwickelungs-Gedanke* two years earlier.

[58]Schmidt, *Haeckels Embryonenbilder*, p. 8; Teudt, *Im Interesse der Wissenschaft,* p. 13.

[59]Ibid., p. 14.

[60]Ibid., pp. 14-15; Schmidt, *Haeckels Embryonenbilder*, p. 9.

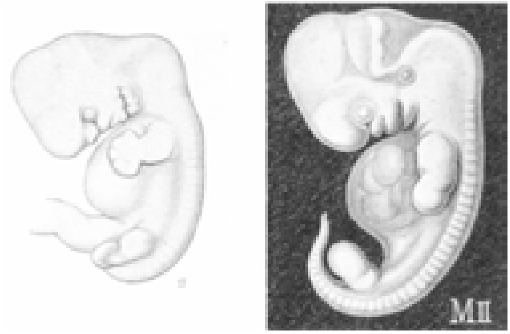

Figure 7: Human embryo (from Hiss Atlas 3: Anatomie menschlicher Embryonen, left) and Haeckel's depiction of the human embryo (from his Menschen-Problem).

similarity (see figs. 6 and 7).[61] It is quite clear that Haeckel did not switch heads of the embryos as Brass had initially charged.

Brass, nonetheless, quickly escalated in another lecture: "Haeckel has not only falsely represented the developmental condition of the human, ape, and other mammals, in order to be able to sustain his hypothesis, he took from the scientific store of a researcher the figure of a macaque, cut off its tail, and made a gibbon out of it."[62] Haeckel in fact did use a macaque embryo with a shortened tail instead of a gibbon embryo. In the Selenka volume, the illustrations of gibbon embryos immediately follow those of macaques, without, however, any gibbon embryo at the stage which Haeckel needed.[63] The similarity of macaque and human embryos would seem to make Haeckel's case even stronger. But there is no doubt that Haeckel's use of the macaque embryo instead of a gibbon embryo rendered him vulnerable. Brass promised that Haeckel's malfeasance would be extensively demonstrated in a little book he was preparing. Haeckel perceived the forthcoming tract as another repetition of the old charge, a creature he had slain over and over, which was now returning to seek vengeance against an old man.

Brass's book appeared as *Das Affen-Problem* in late 1908.[64] In the tract, he expanded his indictment by enumerating several trivial particulars and at the same time deflated what had been his initial, quite serious charge. The first plate of Haeckel's *Das Menschen-Problem* depicted a representation of four ape skeletons and a human skeleton, assuming poses similar to those in a famous illustration by Thomas Henry Huxley (1825-1895). Brass contended that Haeckel had made the human too stooped, the gorilla too erect, the apes with their feet flat on the ground, and the gorilla displaying his teeth in an all too human grin.[65] Concerning the second plate, which showed embryos of a pig, rabbit, and human being at three very early "sandal" stages, Brass mostly suggested they lacked other surrounding features (e.g., yolk) and that they were too symmetrical.[66] Finally, concerning the third plate of the embryonic stages of the bat, gibbon, and human being,

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[61]For their respective depiction of a macaque embryo and a human embryo, see Emil Selenka, *Menschenaffen (Anthropomorphae): Studien über Entwickelung und Schädelbau*, vol. 5 of *Zur Vergleichenden Keimesgeschichte der Primaten* (Wiesbaden: C. W. Kreidel's Verlag, 1903), p. 357; and Wilhelm His, *Anatomie menschlicher Embryonen*, 3 vols. with 3 atlases (Leipzig: Verlag von F. C. W. Vogel, 1880-1885), III atlas, table 10.
[62]Schmidt, *Haeckels Embryonenbilder*, pp. 9-10; Teudt, *Im Interesse der Wissenschaft*, p. 15.
[63]Selenka, *Menschenaffen*, pp. 353-63.
[64]Arnold Brass*, Das Affen-Problem:Prof. E. Haeckel's Darstellungs- u. Kampfesweise sachlich dargelegt nebst Bemerkungen über Atmungsorgane u. Körperform d. Wirbeltier-Embryonen* (Leipzig: Biologischer Verlag, 1908).
[65]Ibid., p. 8.
[66]Ibid., pp. 8-10.

Brass simply dropped his original charge that Haeckel had swapped the heads of the gibbon and human embryos. He found other falsifications, however: the bat was the common bat (*Vespertilio murinus*) instead of the horseshoe nosed bat (*Rhinolophus*) that Haeckel claimed; the human embryo in MII was represented with forty-six vertebrae instead of the thirty-three to thirty-five normally present; and the so-called gibbon at GIII was really a macaque that had its tail removed.[67]

Haeckel responded to Brass's new charges in the December 29, 1908 number of the *Berliner Volkszeitung* in a long article that recounted the activities of the Keplerbund and its opposition to Darwinian theory and monism. Haeckel acknowledge that like virtually every illustrator he had "schematized" his depictions, removing features inessential to the point of the discussion.[68] I think an impartial judge would recognize that Haeckel's schematizations did not materially alter his essential message, namely, that the embryonic structures of vertebrates at comparable stages were strikingly similar and that the best explanation of the similarity was common descent.

The Response of the 46

The contretemps between Haeckel and the Keplerbund generated a massive reaction from scientists and laymen alike. Hundreds of articles and pamphlets, some calm and reflective, most vituperative and dismissive streamed from the presses. The Keplerbund sought a thorough condemnation of Haeckel and to that end they sent around a letter to many distinguished anatomists and embryologists seeking their support. They did get a response, but not precisely the one they had hoped for. In mid February, the following letter, signed by some of the most distinguished researchers in biology, appeared in a number of German newspapers:

> The undersigned professors of anatomy and zoology, directors of anatomical and zoological institutes and natural history museums, and so on, herewith declare that they certainly [*zwar*] do not approve [*nicht gutheissen*] of the few instances in which Haeckel practiced a kind of schematization but that in the interest of science and the freedom to teach they condemn in the sharpest way the battle that Brass and the Keplerbund have waged against him. They further declare that the developmental concept, as it is expressed in descent theory, can suffer no injury from a few inappropriately repeated embryo illustrations.[69]

The letter was signed by forty-six biologists, including Theodor Boveri, Karl Escherich, Max Fürbringer, Alexander Goette, Richard Hertwig, Karl Kraepelin, Arnold Lang, Ludwig Plate, Karl Rabl, Gustav Schwalbe, and August Weismann. Lest their meaning be unclear about their mild reproof of Haeckel, Karl Rabl (1853-1917), the great Leipzig cytologist, published in the *Frankfurter Zeitung* a clarification of what they meant by "schematization":

[67]Ibid., pp. 15-21.
[68] Teudt, *Im Interesse der Wissenschaft,* p.28; Schmidt, *Haeckels Embryonenbilder*, pp. 16-17.
[69]Ibid., p. 50; Teudt, *Im Interesse der Wissenschaft*, p. 49.

concerning the schematizations that went a bit too far, this is not a question of falsification or betrayal. The mild form in which the objection was clothed has been dictated by the great regard the zoologists and anatomist feel for Haeckel. They know very well how to appreciate how much they owe Haeckel and they know also that the few schemata of lesser value are hardly of consequence, as opposed to the numerous first-rate ones that Haeckel has produced and that have become the common property of science.[70]

Rabl securely situated Haeckel in the minds and sentiments of the significant scientists at the beginning of the twentieth century; and he and the other members of the forty-six provided, I think, a just evaluation of the old warrior's protracted dispute with the Keplerbund.

Conclusion

"Darwin's *Origin of Species* had come into the theological world like a plough into an ant-hill," wrote Andrew Dixon White in 1894. "Everywhere," he remarked, "those thus rudely awakened from their old comfort and repose had swarmed forth angry and confused."[71] None more angry and confused than the theologians and theologians manqué who saw in Haeckel the embodiment of the anti-Christ. From sophisticated German theologians who found his scientific world view an appropriate challenge to Christianity to English preachers who feared "the depth of degradation and despair into which the teachings of Haeckel will plunge mankind," the German Darwinian came to symbolize Evolution Militant.[72] Moreover, the complex relations of religion with political parties and revolutionary social movements, especially the Marxists, made even more hyperbolic the reactions of the lower minded orthodox to a doctrine that seemed to deny the hand of the creator in shaping the living world. To what shoals did that doctrine lead? "Primitive barbarism, Sun worship, Mohammedanism, self-love: these are the awful rapids to which Haeckel would steer the ship of humanity," so warned the preacher of the Hampstead Congregationalist Church.[73]

But was evolutionary theory in necessary conflict with sophisticated theology? I do not think so, and Erich Wasmann's own way of dealing with evolution would suggest this. Today, not many philosophers—or even theologians of cultivated taste—would be

[70]Schmidt, *Haeckels Embryonenbilder*, p. 63.

[71]Andrew Dixon White, *A History of the Warfare of Theology with Science in Christendom,* 2 vols. (New York: George Braziller, [1894] 1955), 1: 70. Michael Ruse delivers a pungent account of the reaction of contemporary religious sects to evolutionary theory in his *The Evolution-Creation Struggle* (Cambridge: Harvard University, 2005). Ronald Numbers provides a scholarly treatment of the American Fundamentalist response to evolution in the early part of the twentieth century in his *The Creationists* (New York: Knopf, 1992).

[72]For examples of calm and sophisticated responses to Haeckel's attacks on religion, see, for example, Friedrich Loofs, "Offener Brief an Herrn Professor Dr. Ernst Haeckel in Jena," *Die Christliche Welt* 13 (1899): 1067-72; and Georg Wobbermin, *Ernst Haeckel im Kampf gegen die christliche Weltanschauung* (Leipzig: J. C. Hinrichs'sche Buchhandlung, 1906). The analytic and reflective consideration was not the strong suite of the English preacher R. F. Horton; see his "Ernst Haeckel's 'Riddle of the Universe,'" *The Christian World Pulpit* 63 (1903): 353-56 (quotation from p. 353).

[73]Ibid., p. 355.

ready to endorse his Thomistic dualism. Yet his readiness to reflect on articulate scientific theory and accept striking empirical evidence indicate the kind of flexible mind that is not saturated with dank ideology—a mind that in a later day might be ready to conceive sensory cognition (which he thought the provenance of animals) and human reason as more dynamically related, one that might interpret the "soul" not as an entity but as an achievement. Wasmann stands as a case of an individual for whom empirical truth triumphed over dogmatism. By contrast, the crude opposition of individuals like Brass would not have stirred Haeckel to wrath, except for that failed academic's mendacity. Wasmann's scientific intelligence and sophisticated acumen created for Haeckel a much more dangerous situation: that Jesuit showed how one could be both an intelligent evolutionist and a sophisticated religious thinker. This was the deeper problem for the Monist position. Of course, it did not take much to discharge Haeckel's long-term suspicion and disdain for the Church of Rome. Even when the more vitriolic and personally damaging dispute with the Keplerbund broke out, he still thought of that group as somehow allied with Wasmann's Jesuits, so intellectually pernicious did he regard the latter. In 1910, Haeckel brought out a small tract entitled *Sandalion: Eine offene Antwort auf die Fälschungs-Anklagen der Jesuiten* (Sandalion: an open answer to the charges of falsification of the Jesuits).[74] "Sandalion" referred to the sandal-shaped embryos of vertebrates. But by "Jesuits" he meant not only the Catholic religious order but also Protestant religious thinkers of a low, Jesuitical type. Protestant Jesuits! He saw those dark shapes looming everywhere. That part of the World-Soul where Haeckel now dwells must be even more chagrined and suspicious of Jesuit intrigue after eavesdropping on the meeting of the Pontifical Academy of Sciences in 1996, where Pope John Paul II declared that "fresh knowledge leads to recognition of the theory of evolution as more than just a hypothesis."[75] The Pope, in stating the Church's position, however, hardly broke new theological ground. He essentially reiterated the resolution that Wassman had worked out a century before.

Haeckel had lost his taste for any orthodox religion after his habilitation work in Italy and Sicily. The wonderful excesses of southern Catholicism should, perhaps, have amused him; instead he took them as a personal affront. The death of his first wife, Anna, not only caused him to abandon formal observance, the soul-searing event turned him against the kind of superstition that would worship such a malevolent being. Yet because of his second wife, his children, and their social life in Jena, Haeckel retained nominal membership in the Evangelical Church. The attacks of the Keplerbund, however, finally

[74]Ernst Haeckel, *Sandalion: Eine offene Antwort auf die Fälschungs-Anklagen der Jesuiten* (Frankfurt a.M.: Neuer Frankfurther Verlag, 1910).

[75]John Tagliabue, "Pope Bolsters Church's Support for Scientific View of Evolution," *NewYork Times* (25 October 1996): A1. This is a report of Pope John Paul II's address to the Pontifical Academy of Sciences. The current Pope, Benedict XVI, may be having second thoughts. His friend, the Cardinal Archbishop of Vienna, Christoph Schönborn, has asserted: "Evolution in the sense of common ancestry might be true, but evolution in the neo-Darwinian sense—an unguided, unplanned process of random variation and natural selection—is not. Any system of thought that denies or seeks to explain away the overwhelming evidence for design in biology is ideology, not science." His essay appeared as an op. ed. in the New York Times: Christoph Schönborn, "Finding Design in Nature," *New York Times* (7 July 2005): A27.

ever, finally drove him out. In December, 1910, he formally declared, in a published account of his religious trajectory, that he had left the Evangelical Church.[76] What undoubtedly surprised those who read the article was that he had still been a member of the Church.

Coda: "The Rape of the Ants"

After his encounter with Haeckel and the Monists, Wasmann continued his research on inquilines and their hosts. His correspondence network of important ant-men—August Forel, William Morton Wheeler (1865-1937), and Hugo von Buttel-Reepen (1860-1933)—continued apace, with the exchange of many ant species among them. Wasmann built up the largest entomological collection of ants in the world, some 3500 different species. He also strove unremittingly against Haeckelian evolutionary theory and its cultural spread, which he believed to be rife during the first decades of the new century. He lectured and wrote on the dangers to German culture of Monistic thought, especially that connection about which Virchow had warned, namely, its alliance with the Social Democratic Party and the Communists. Wasmann thought this danger particularly acute after the Great War, with German institutions and society in shambles and with their need of reconstruction. In a lecture delivered to the Catholic Union in Aachen on January 28, 1921, Wasmann asked, rhetorically, about the direction to take in the wake of the destruction of German cultural and social life.

> Our answer can only be shouted: back to Christianity and away with Haeckelian Monism! For the impregnation of anti-Christian ideas of this neopaganism into our social networks bears the chief responsibility for not only the material collapse of our Fatherland but also its ethical and religious orientation. For that reason we say: Haeckel's Monism is a cultural danger [*Kulturgefähr*].[77]

During Wasmann's last years, he saw the beginning of a transformation in German society, but in a way that confirmed his dark forebodings. Wasmann died in 1931. His ants, however, were fated to have a curious connection with the Nazi regime.[78]

After his death, Wasmann's large collection of books and reprints, along with his ants and beetles, were donated to the Natural History Museum of Maastricht to be used for all researchers. In October of 1942, Dr. Hans Bischoff, curator of the Berlin Zoological Museum, received an order from Heinrich Himmler, head of the *Schutzstaffel* (*SS*) and himself an amateur entomologist. Bischoff was to go to Holland and get Wasmann's ants. He first traveled to the Jesuit house in Limburg looking for the collection. He was told it was transferred to the Natural History Museum in Maastricht. The museum personnel and other citizens learned of Bischoff's mission; and, with the connivance of even the Quisling mayor, they hid the ants in the basement of the city hall. Only temporarily foiled, Bischoff returned to Maastricht the next spring with a contingent of SS troops.

[76]Ernst Haeckel, "Mein Kirchenaustritt," *Das freie Wort* 10 (1910): 714-17.

[77] The lecture is in the Nachlass of Erich Wasmann held in the Natural Museum of Maastricht.

[78]The outline of the following story was told to me by Dr. Fokeline Dingemans of the Natural History Museum of Maastricht. For other details, I have relied on a story, "Ants Rescued by Richmonder," in the *Richmond Times*-Dispatch (10 February 1946). I am grateful to David Leary (University of Richmond) for providing information on John Wendell Bailey.

Quite formally he stated the ants were being repatriated. They were German ants! The burgomaster retorted that Wasmann was born in the Tyrol. They were Italian ants. The Dutch, needless to say, did not win the argument. The ants and Wasmann's book collection were carted off to Berlin. A *Time Magazine* article of 1944, entitled "The Rape of the Ants," stood aghast at the perfidy of the *SS*, who even stooped so low as to steal ants.[79]

After the Normandy Invasion, Colonel John Wendell Bailey (1895-1986), head of typhus control in Europe, made his way to Maastricht in fall of 1945 to examine Wasmann's collection. Bailey was a professor of entomology at the University of Richmond and a former student of Harvard Professor William Morton Wheeler, Wasmann's old friend. When he got to the museum he learned about the fate of the ants. He decided to chance it and traveled the 600 miles to Berlin and the Zoologisches Museum, which lay in rubble. He did manage to locate Bischoff and with some tactful threats discovered that Wasmann's ants and books had been stored in the deep vaults of a bank. The bank lay in ruins, but the vaults were still secure. Miraculously the entire collection of ant species and the library had survived. Since the bank was in the Russian sector, Bailey had to negotiate with a Russian general, whom he befriended with many cartons of American cigarettes and several bottles of whiskey. After the proper papers were signed, Bailey and several G.I.s loaded the ants and books—some 160 insect trays, 150 small boxes, 100 bottles of specimens in alcohol, and 50,000 books and reprints—on two trucks and three jeeps and took them to the American sector. Bailey discovered, however, that some of the insects were missing, which he later found in Himmler's country home in Waischenfeld, just over the Swiss border. Bailey shipped the ants and books back to the Maastricht Natural History Museum, where today they are still used in research.

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[79] "The Rape of the Ants," *Time* 44, no. 21 (20 November 1944), science section.

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Plant systematics in Jena during the early 19th century Fr. S. Voigt's treatment of the "méthode naturelle"

Cum igitur scientia omnis in similium collectione & dissimilium distinctione consistat […] *conatus sum id praestare in universa plantarum historia.*[1]

Nicolas Robin & Frank Hellwig

Abstract:
This paper describes studies on plant affinities at the Jena University during the early 19th century. At the end of the 18th century, the outline of a natural system by A. J. G. K. Batsch (1786) announced the break of botanical research in Jena with the Linnean sexual system. Supported by J. W. von Goethe the concept of a natural classification of plants gained ground notably through the interpretation of A.-L.de Jussieu's natural method by Fr. S. Voigt (1806). In a first part we focus on the first application of a natural method within the framework of the systematic arrangement of the Jena botanical garden. From the implementation of A. J. G. K. Batsch's natural system to the reception A.-L.de Jussieu's "méthode naturelle" we detail the basis of the botanical practice of Fr. S. Voigt (1781-1850), professor of botany and director of the botanical garden since 1807. We analyse his reception of the French science, his interpretation of the weighting of plant characteristics, notably taking into account the publications of A.-L.de Jussieu, P. E. Ventenat and above all of L. Cl. Richard. To conclude on the originality of the development of plant systematics at Jena during the early 19th century, we provide a comprehensive study of the publications of Fr. S. Voigt. We explore his reception of the ideas of A.-L.de Jussieu as well as his own ideas and concepts in the field of plant affinities.We demonstrate how his representation of a natural classification of plants evolved and additionally point out the educational character of his publications.

[1] Caesalpino, A. *De Plantis Libri XVI.* Florentiae, G. Marescottum, 1583: [Vorwort: *Serenissimo Francisco Medici magno aetruriae*, p. 4]. "Since the entire knowledge is therefore based on the collection of similar and on the discrimination of different plants (objects), […], I have tried to achieve this with a complete overview of the history of the plants"

1. Introductory questions concerning the natural method and its applications

Friedrich Sigmund Voigt (1781-1850), professor of botany and director of the botanical garden in Jena since 1807, considered himself the first in Germany[2] to introduce Goethe's idea of the *Metamorphose der Pflanzen* (1790) in his textbooks of botany, and also the first to include the natural families of plants following the "*méthode naturelle*" developed by the French botanist Antoine-Laurent de Jussieu (1748-1836) in his *Genera plantarum* (1789)[3]. These were included in his *Darstellung des natürlichen Pflanzensystems von Jussieu*[4] (1806). A.-L. de Jussieu's natural method had been spread in the German-speaking world first through the translation of the *Genera plantarum* by the Swiss Paul Usteri[5] (1768-1831). The natural method developed by A.-L. de Jussieu as well as the works of Joseph Gaertner[6] on carpology contributed to the basic methodological changes in the the field of botany at the end of the 18th century. James L. Larson wrote that "both men [A.-L. de Jussieu and J. Gaertner] were convinced that the achievement of a natural method would transform botany from a science of memory and nomenclature into a new science […]"[7]. The natural method results in a classification which can be understood as an approximation towards an ultimate goal: the construction of a natural system. This aim had been exposed already by Carl von Linné in the form of his *Fragmenta Methodi naturalis*, published respectively in his *Systema naturae* (1735) and later in his *Fundamenta botanica* (1736)[8]. Augustin-Pyramus de Candolle (1778-1841), author of the remarkable *Theorie élementaire*[9],

[2] Fr. S. Voigt stated this in the introduction of his *Lehrbuch der Botanik* (1827): "Den er [Fr. S. Voigt about himself] möchte wohl in Teutschland so ziemlich der erste gewesen seyn, welcher die Ansicht von der Metamorphose der Pflanzen, die genauere Darstellung des natürlichen Pflanzensystems, […], in ein Lehrbuch der Botanik aufgenommen hat." Voigt, Fr. S. *Lehrbuch der Botanik*. Jena, A. Schmid, 1827: p. iv.

[3] Jussieu, A.-L. *Genera plantarum* […]. Paris, Hérissant & Barrois, 1789. Concerning the natural method developed by A.-L. de Jussieu see Stevens, P. F. *The development of biological systematics* […]. New-York, Columbia University Press, 1994.

[4] Voigt, Fr. S. *Darstellung des natürlichen Pflanzensystems von Jussieu*. Leipzig, 1806.

[5] See Jussieu, A.-L. *Genera plantarum secundum ordines naturales disposita* […] *recudi curavit notisque auxit Paulus Usteri*. Turici Helvetorum, Ziegleri & Filiorum, 1791. The french original was known to Fr. S. Voigt and also used and cited in his textbooks.

[6] See Gaertner, J. *De fructibus et seminibus plantarum*. Stuttgart, Typis Academiae Carolinae, 3. vol.

[7] Larson, J. L. *Interpreting Nature. The science of Living Form from Linnaeus to Kant*. Batltimore & London, The John Hopkins University Press, 1994.

[8] For further ideas on the concept of natural system in the textbooks of C. von Linné see: Müller-Wille, S. *Botanik und weltweiter Handel. Zur begründung eines Natürlichen Systems der Pflanzen durch Carl von Linné (1707-1778)*. Berlin, VWB, 1999. S. Müller-Wille explained: "Tatsächlich versprachen sowohl das *Systema naturae* von 1735 als auch die *Fundamenta botanica* von 1736, in naher Zukunft „Bruchstücke" als eine bloße Liste von Gattungsnamen heraus, die in durchnummerierte „natürliche Ordnungen (ordines naturales)" gegliedert waren-ohne jeden sichtbaren Versuch, diese *natürlichen* Ordnungen in Definitionen zu kennzeichnen." Müller-Wille, S. 1999, p. 81.

[9] De Candolle, A. P. *Théorie élémentaire de la botanique* […].Paris, Déterville, 1813.

presented three categories of natural methods to his readers[10], first the tentative method of the French botanist Pierre Magnol (1638-1715), then the general comparison of plant features as carried out by Michel Adanson[11] (1727-1806) in his "*Familles des Plantes*" (1763), and finally the simultaneous use and weighting of every plant characteristic, as in the works of Bernard and Antoine-Laurent de Jussieu[12]. As early as 1773 the latter presented this idea of weighting and subordination of plant characteristics to the *Académie des Sciences* in a paper on the Ranunculaceae (crowfoot) in which the species were described with the terminology of M. Adanson. Considering this principle of subordination as well as the comparative anatomy of fructifications and seeds he later laid the foundation for his "méthode naturelle" and consequently the physical basis of his natural orders in his *Genera plantarum* (1789). The notion of *Methodus naturalis* is introduced in the introduction of the *Genera plantarum* in the following way:

"Haec dudum quaesita ordinatio, inter omnès longè praecipua, sola verè uniformis ac simplex, affinitatum legibus constanter obtemperans, est *methodus* dicta *naturalis* quae omnigenas connectit plantas vinculo indiviso, & gradatim à simplici ad compositam, à minimâ ad maximam, continuatâ serie procedit, […]"[13]

Few years later A.-L. de Jussieu contrasted in his article *méthode naturelle des végétaux* (1824) available in the *Dictionnaire des sciences naturelles*[14] the concepts of natural methods and systems, the latter being described as systematic tables where the organisms were arranged in a specific order. He aspired to develop a continuous natural classification of the plant kingdom; the idea of continuity is also omnipresent in his treatment of the natural method.

To complete this general introduction we want to give an additional explanation of the difference in the meaning of the word "méthode" in French and "Methode" in German. In a first sense, the German "Methode" and the French "méthode" are the well-known elements of logic applied to natural history. For example P. F. Stevens wonders whether A.-L. de Jussieu's method can be interpreted through René Descartes's *Discours de la méthode* (1637), with the tools of the Cartesian science[15]. However, the French "méthode" has a second meaning, precisely at the time of A.-L. de Jussieu; the word was used in fact to characterize the temporary and imperfect draft of a natural

[10] *Ebenda*: p. 67. Concerning A. P. De Candolle and the notions of method and system see Drouin, J.-M. Classification des sciences et classification des plantes chez Augustin-Pyramus De Candolle, in: *Revue de synthèse,* 1-2, 1994: pp. 149-165.

[11] M. Adanson was certainly disregarded because of his rejection of C. von Linné's binomial nomenclature, but may be regarded as one of the precursors of phyletic systematists in the 20th century. See Stuessy, T. F. *Plant taxonomy*. New York : Columbia Univ. Press, 1990.

[12] Thus A.-P. De Candolle defined the term charakter: „*Un caractère est une des manières d´envisager les organes en général, appliquer à un organe en particulier.* „ See De Candolle, A.-P. 1813, *op. cit.* note 17: p. 150.

[13] De Jussieu, A.-L. 1789, *op. cit.* note 3 : p. xxxv.

[14] De Jussieu, A.-L. [Artikel] Méthode naturelle des Végétaux, in: *Dictionnaire des sciences naturelles*. Strasbourg & Paris, F. G. Levrault, 1824, vol. 30: pp. 426-468.

[15] See Stevens, P. F. 1994. *op. cit.* note 3: pp. 60-62 and Planchen, A. *Classification, evolution and the nature of biology*. Cambridge, Cambridge University Press, 1992: pp. 109-111.

system still under construction[16]. In contrast to this the "Methode" in German can hardly be applied to this temporary representation of a natural classification of plants and consequently Fr. S. Voigt replaced it by the word "System" in his interpretation entitled *Darstellung des natürlichen Pflanzensystems von Jussieu* (1806). In French again A.-L. de Jussieu did not use the word system to name his classification but the expressions *Methodus naturalis* and *Genera plantarum secundum ordines naturales disposita* in the title of his book. Other German botanists were also aware of this distinction between "méthode" and system[17]. The botanist Leo count Henckel von Donnersmarck[18] offered an accurate definition of system and method in a letter to Augustin Nicaise Desvaux (1784-1856), the editor of the *Journal de Botanique*:

"Vous connaissez tout aussi bien que moi la différence qu'admet l'école entre système et méthode. Dans le premier tout part d'une considération fondamentale unique, tandis que dans la seconde, l'édifice repose sur des fondemens souvent assez nombreux."[19]

This letter from L. Henckel von Donnersmarck was a reply to an inappropriate obituary of Fr. S. Voigt (37 years before his death!) by A. N. Desvaux, where he treated German science with much condescencion. He claimed that Fr. S. Voigt's *Darstellung des natürlichen Pflanzensystems von Jussieu*[20] had "avenged" his nation for its lack of philosophical ideas about the plant classification!

"[Fr. S. Voigt] ne marchais pas aveuglément sur les traces de ceux qui l'avaient précédé parmi ses compatriotes. En effet, tous n'avait, pour ainsi dire, jeté qu'un coup d'œil indifférent sur les rapports naturels des végétaux ; séduit par la trompeuse simplicité du système de Linnée [sic.], ils négligeaient toute recherche tentant à établir une classification plus méthodique, mais en apparence plus difficile."[21]

While Fr. S. Voigt did not use the term "méthode" he was aware of the French meaning of the word, as he wrote in his *Lehrbuch der Botanik* (1827):

"Das sogenannte natürliche Pflanzensystem, in Frankreich vorsichtig *méthode naturelle* genannt, sucht nach allen an der Pflanze, innerlich wie äusserlich, aufzufindenden Merkmalen, das Aehnli-

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16 A.-L. de Jussieu wrote: "Utilitas methodi ea est ut genera species similes connectantia, in sectiones notis secundariis designatas convocata, ulterius congregata in classes ex nonnullis sectionibus composites signoque definitas primario ac simpliciori […]"Jussieu, A.-L. 1789, *op. cit.* note 3 : p. xxvij.

17 K. P. Sprengel explained in a translation of the *Théorie élémentaire* of A.-P. De Candolle: "Dann befolgt man eine natürliche Methode, die eben deswegen nicht System genannt werden kann, weil es an der Einheit des Prinzips mangelt." See Sprengel, K. [& De Candolle, A.-P.]. *Grundzüge der wissenschaftlichen Pflanzenkunde zu Vorlesungen*. Leipzig, C. Cnobloch, 1820: p. 106.

18Leo V. F. Henckel von Donnersmarck, count (1785-1861), botanist and collaborator with Georg August Pritzel (1815-1874) to the: *Thesaurus literaturae botanicae* […]. Leipzig, Brockaus, 1851.

19 Leo Henckel von Donnersmarck to Augustin Nicaise Desvaux, Dessau the 25th December 1814, in: *Journal de Botanique appliquée à l'Agriculture, à la Pharmacie et aux Arts*, 4, 1814: pp. 222-224.

20 Voigt, Fr. S. 1806. *Op. cit.* note 4: 24 p. + tab.

21 Desvaux, A. N. Notice biographique sur M. Friedrich Sigmund Voigt, in: *Journal de Botanique*, 1813, 1: pp. 95.

che neben das Aehnliche zu stellen. […]. Das künstliche Pflanzensystem wählt dagegen nur ein einzelnes, sehr beständiges Merkmal, […].”[22]

This comment reminds us of the French definition of system and method given by L. Henckel von Donnersmarck above.

In the first part of this paper we pay attention to the base of Fr. S. Voigt's botanical practice: the botanical garden. We wonder whether this botanical garden reflected, after the appointment of Fr. S. Voigt to the post of director in 1807, his reception of the "French" natural method or still conserved the attempt of a natural system realized by his predecessor A. J. G. K. Batsch. According to J. W. von Goethe's statement[23], the taxonomical arrangement of the garden had not been changed before the 1820s. In fact, the plan of the garden inaugurated in 1794 followed A. J. G. K. Batsch's *Dispositio generum* (1786)[24] and Jena is considered to be one of the first gardens arranged following a natural system[25]. Nevertheless it seems problematic to believe that Fr. S. Voigt, successor to A. J. G. K. Batsch after F. J. Schelver (1778-1832), maintained the original arrangement of 1794 when at the same time he rejected Batsch's natural system, promoting instead the ideas of A.-L. de Jussieu in his botanical teachings and textbooks.

In the course of our discussion we want to analyse Fr. S. Voigt's reception of the "French" natural method. We will explain how Fr. S. Voigt took the main principle of the natural method into account, the concept of the constancy and subordination of the plant features. Fr. S. Voigt tried to implement this evaluation of the plant characteristics on the basis of their relative instability, particularly when he referred to: "*die gradativen Werthe der Charactere*“[26].

Finally we highlight the didactic treatment of the natural method by Fr. S. Voigt. Thereby, follow the question whether the natural method could be a useful way of diffusing plant systematics at the beginning of the 19th century.

2. Fr. S. Voigt and A. J. G. K. Batsch's arrangement of the Jena botanical garden.

Around 1800 the University of Jena was recognized as a privileged forum for debates on scientific theories[27] and especially about the comprehension and visualization of the

[22] Voigt, Fr. S. 1827. *Op. cit.* note 2: p. 193.

[23] Bradish, J. A. von. Goethes Beamtenlaufbahn, vol. 4. New York, 1937 : p. 282. [Oberaufsicht über die unmittelbaren Anstalten für Wissenschaft und Kunst in Weimar und Jena].

[24] Batsch, A. J. G. K. Dispositio generum plantarum ienensium. Jena, Heller, 1786.

[25] See Poliansky, I. 2001 & 2004a and b. Fr. S. Voigt mentioned also Laurentius Heisterus who had arranged the Helmstedt botanical garden according to his *Systema plantarum generale* of 1749. Fr. S. Voigt speculated that Bernard de Jussieu might have been influenced by L. Heisterus's system in his own arrangement of the garden of Trianon. [See Heisterus, L. *Systema plantarum generale ex fructificatione* […]. Helmstedt, Weygand, 1749.]

[26] See Jussieu, A.- L. 1824. *Op. cit.* note 14: p. 447.

[27] Bach, T. & Breidbach, O. Die Lehre im Bereich der „Naturwissenschaften“ an der Universität Jena zwischen 1788 und 1807, in: *N.T.M.*, 9, 2001: pp. 152-176.

natural order of nature. J. W. von Goethe as one of the protagonists in this debate, proposed a revision of the criteria used in classification by Carl von Linné and in that way encouraged the young "Privatdozent" of *Materia medica* A. J. G. K. Batsch in his attempt to construct a natural system, published at first in his *dispositio generum* (1786). J. W. von Goethe reflected in his *Geschichte meiner botanischen Studien*:

"Seine [A. J. G. K. Batsch] Denkweise war meinen Wünschen und Forderungen höchst angemessen, die Ordnung der Pflanzen nach Familien in aufsteigendem, sich nach und nach entwickelndem Fortschritt, war sein Augenmerk. Diese naturgemäße Methode, auf die Linné mit frommen Wünschen hindeutet, bei welcher französische Botaniker theoretisch und praktisch beharrten, sollte nun einen unternehmenden jüngeren Mann zeitlebens beschäftigen, und wie froh war ich, meinen Teil daran aus der ersten Hand zu gewinnen."[28]

In 1788, A. J. G. K. Batsch took up a position as "professor extraordinarius" of medicine and natural history and one year later became "ordinarius". In 1794 he founded the botanical garden in Jena. In the same year he published a new *Dispositio generum plantarum*[29], without quoting his own previous natural orders and classes from 1786. This *Dispositio* was in fact not a natural system but a list of 128 natural families (*familiae seu ordines vegetabilium naturales*[30]). A comparison of the names of families used by A. J. G. K. Batsch and by A.-L. de Jussieu shows that about half of the names of the families listed by A. J. G. K. Batsch are the same as the names used by A.-L. de Jussieu. A. J. G. K. Batschs's 128 natural families were the foundation for the new arrangement of genera in the botanical garden, as illustrated in the plates of Batsch's *Conspectus horti botanici* (1795)[31]. Since half of the names were taken from A.-L.de Jussieu's *Genera plantarum*, the arrangement of the plants in the botanical garden offered the visitors in a way a reflection of the early reception of A.-L. de Jussieu's natural families in Jena.

"Als Pionier der neuen botanischen Taxonomie hatte Antoine-Laurent de Jussieu im Jahre 1789 ein Gesamtsystem auf natürlicher Grundlage erstellt, das von Batsch zum Vorbild für die Anlage des botanischen Gartens in Jena genommen wurde."[32]

I. Poliansky did not agree with this explanation[33]even though he noticed that A. J. G. K. Batsch's families were similar to the natural families of A.-L. de Jussieu[34]. Furthermore,

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[28]Goethe, J. W. von. Geschichte meiner botanischen Studien, in: *Schriften zur Botanik und Wissenschaftslehre.* München, DTV, 1975: p. 55.

[29] Batsch, A. J. G. K. *Disposito generum plantarum europae* […]. Jena, Croeker, 1794: 136 p.

[30] We note the reference to the terms *Ordines vegetabilium naturales* by A. J. G. K. Batsch and the use of the terms *Ordines naturales* by A.-L. de Jussieu (1789).

[31] Batsch, A. J. G. K. *Conspectus Horti botanici ducalis Ienensis* […]. Jena, Ch. G. Goepferdt, 1795.

[32] Wyder, M. *Goethes Naturmodell.* Böhlau, 1998: p. 228.

33 Poliansky, I. Der außerordentliche Garten. Zur Geschichte des Herzoglichen Botanischen Gartens zu Jena, in: Müller, G.; Ries, K. & Ziche, P. (Hrsg.). *Die Universität Jena. Tradition und Innovation um 1800.* Stuttgart, Pallas Athene, 2001: p 206.

[34] "Während Batschs Pflanzenfamilien mit den Jussieuschen weitgehend übereinstimmnten, hat er die Klassen und Ordnungen ganz anders konzipiert." Poliansky, I. *Die Kunst, die Natur vorzustellen.* [Minerva. Jenaer Schriften zur Kunstgeschichte, vol. 14]. Jena & Köln, W. König, 2004a: p. 224.

he rejects[35] the interpretation of J. W. von Goethe's *Schriften zu Naturwissenschaft* by D. Kuhn who wrote that "*Batsch richtete ihn* [the botanical garden] *nach dem natürlichen System Jussieus ein, damals eine bemerkenswerte Neuerung.*"[36] However I. Poliansky assumes that the botanical garden was arranged following A. J. G. K. Batsch's natural system of 1786:

"Während die meisten Botaniker Deutschlands Linné folgten, wurde der Garten nach dem von seinem ersten Direktor A. J. G. K. Batsch (1761-1802) entworfenen "natürlichen" System angelegt" [with note of the author: A. J. G. K. Batsch, Dispositio generum plantarum…. Jenae, 1786][37]

The only piece of evidence of the first arrangement of the botanical garden is a range of plates published in 1795 in A.J.G. K. Batsch's *Conspectus horti botanici*. The problem is that these plates do not show a system but only an arrangement of the families taken from A. J. G. K. Batsch and A. L.de Jussieu and a tentative grouping according to some classes previously described by A. J.G. K. Batsch. We would expect that he used his classification from 1786 as a basis for the arrangement of the families in the garden. Indeed we find in his *Conspectus horti botanici*[38] that according to his natural system from 1786 he started with *Senticosae* in bed 1, corresponding more or less to the actual *Rosoideae*[39] and followed by *Malvaceae* in bed 2. Nevertheless we notice many deviations from his system, for example, the *Siliquosae* (*Brassicaceae*) which follow the *Malvaceae* in his system (1786) are not presented in bed 3 but in bed 15, closer to bed 13 which contained the *Apiaceae* (*Umbellae*). These two families are also much closer to each other in A.-L. de Jussieu's natural system (class 5, n°60 and n°63 respectively). We may conclude that A. J. G. K. Batsch was aware of A.-L. de Jussieu's ideas of a natural system and tried to take it into account when he was planning the botanical garden. This view is supported by the register of his *Dispositio generum plantarum* (1794) which, like A.-L. de Jussieu starts with the cryptogams. A. J. G. K. Batsch had placed cryptogams at the end of his system before. On the basis of this description we assumed that the arrangement of the botanical garden followed A. J.G. K. Batsch's natural groupings combined with a reception of A.-L. de Jussieu's natural families and orders. Futhermore we know that A. J. G. K. Batsch's arrangement of the botanical garden was maintained until 1823[40]. Consequently we cannot accept the previous interpretation of I. Poliansky (see above), especially not in consideration of Fr. S. Voigt's rejection of A. J. G. K. Batsch's natural system. Indeed Fr. S. Voigt worked all his life on the improvement of a natural method, and spread the ideas of French naturalists in Germany. Therefore, it is hard to understand why he would use

[35] See Poliansky, I. Natursystem, Systemästhetik und das Überleben der Physikotheologie. 2004b: p. 129.

[36] Kuhn, D. *Goethe die Schriften zur Narturwissenschaft.* Zweite Abteilung: Ergänzungen und Erläuterungen Band 9A. Weimar, H. Böhlaus Nachfolger, 1977: pp. 329-328.

[37] Poliansky, I. Der außerordentliche Garten […], 2001: p. 204.

[38] Batsch, A. J. G. K. 1795. *Op. cit.* note 31.

[39] The woody groups were planted in special areas of the garden: "Insuper invenies in areis magnis A. B. C. (etiam extra areas in variis horti locis) arbores et frutices plures; locatur in A. Amygdalus, Prunus, Rubus […]." Batsch, A. J. G. K. 1795. *Ebenda.*

[40] See Bradish, J. A. von. 1937. *Op. cit.* note 23.

an outdated system[41] as a basis for the arrangement of plants in the garden of the university. I. Poliansky notes correctly that after the death of A. J. G. K. Batsch J. W. von Goethe continued to defend Batsch's natural system and furthermore "ordered", through the authority of the *Herzogliche Commission,* that the arrangement of the garden be preserved[42]. However, in a French report to the "*Oberbefehlshaber*" Berthier[43] about the scientific institutions at Jena, Weimar and Eisenach in 1806 J. W. von Goethe did not mention the name of A. J. G. K. Batsch when he described the botanical garden and explained that the beds were arranged *autant que possible* according to the order of the natural families[44]. In the same year Fr. S. Voigt, who was not yet appointed director of the botanical garden, wrote:

"Und wirklich hat auch Linné uns 58 Familien aufgestellt, deren bei weitem grösster Theil natürlich ist. Batsch hat sie erweitert und verbessert (Anleit. Z. Kenntn. U. Gesch. D. Pflanzen. Halle, 1787.), aber beide haben nie zur Einheit des Ganzen gelangen mögen, ihre aufgestellten Ordnungen sind Bruchstücke, sie haben selten einen Zusammenhang untereinander Systematische Grundabtheilungen fehlen gänzlich."[45]

Consequently Fr. S. Voigt had to work during the first years of his appointment *autant que possible* in a botanical garden which reflected the ideas of A. J. G. K. Batsch. The latter based his natural system on the exact study of the floral elements, and paid particular attention to the shape of the corolla, rather reminiscent of the approach adopted by Joseph Pitton de Tournefort[46]. A. J. G. K. Batsch ignored the fundamental difference between monocotyledonous and dicotyledonous plants and only in his *Tabula affinitatum* (1802) adopted the class *Cryptogamia* from Linné as his eighth and last class *Cryptogama*

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[41]We provide for example a comment from K. P. Sprengel (1822):„Batsch ist tot, dessen systematische Eintheilung hat die Botaniker gegen sich, sie ist von keinem recipirt und wird auch nicht allgemein recipirt werden. Wenn Sie nur bedenken, dass, um Naturgeschichte zu popularisieren, Lehrer nöthig sind, diese müssen ebenfalls erst von zur Zeit lebenden Lehreren gebildet werden, alle Lebenden hängen aber an Linné und Jussieu, keiner wird künftige Lehrer und Schulmeister nach Batsch unterrichten wollen, da bürge ich dafür." See Sprengel, K. Allgemeinen Encyklopädie der Wissenschaften und Künste, 1822: p. 96 quoted from Middell, K. „Die Bertuchs müssen doch in dieser Welt überall Glück haben" […]. Leipzig, Leipziger Universitätsverlag, 2002: p. 271.

[42] "Bergrath Voigt wird, nachdem er durch die Autorität Herzoglicher Commission berechtiget, den Hofgärtner Wagner in seine Grenzen zurück zuwesen, die botanische Anstalt in die erste, vom Professor Batsch, mit wissenschaftlicher Genehmigung Herzoglicher Commission, bestimmte Ordnung zurückbringen"; J. W. von Goethe to Ch. G. Voigt, 19 April 1815, in: WA IV, 30: p. 185 quoted from Poliansky, I. *Die Kunst, die Natur vorzustellen. Die Ästhetisierung der Pflanzenkunde um 1800.* [Minerva. Jenaer Schriften zur Kunstgeschichte, vol. 14]. Jena & Köln, W. König, 2004a: p. 278.

[43] Probably Louis Alexandre Berthier (1753-1815), marshal of the empire. He took part in the campaigns of Austerlitz, Jena and Friedland, and became Duke of Valengin in 1806, sovereign prince of Neuchâtel in the same year and vice-constable of the empire in 1807.

[44]*Bericht über den Zustand der wissenschaftlichen Einrichtungen in Sachsen-Weimar-Eisenach für den französischen Oberbefehlshaber Berthier.* 1806, in: J. W. von Goethe Akten, GSA: fol. 254. See also Bradish, J. A. von. 1937. *Op. cit.* note 23: p. 282.

[45] Voigt, Fr. S. 1806. *Op. cit.* note 4: p. 24.

[46] Tournefort, J. P. de *Élemens de botanique ou méthode pour connoître les plantes.* Paris, Imprimerie Royale, 1694: 451 p. + ill.

"Gewächse mit ganz besonderen versteckten oder unkenntlichen Befruchtungstheilen"[47]. On the one hand A. J. G. K. Batsch was an actor within and not an outcast from the European scientific community at the end of the 18th century. On the other hand the international botanical community rarely refered to A. J. G. K. Batsch's natural system after 1800. An exception is J. Ch. Graumüller, "Privatdozent" of botany and a colleague of Fr. S. Voigt, who published a *Tabellarische Uebersicht des alten Linneische Pflanzensystems und des verbesserten von Thunberg sowie auch der natürlichen Systeme von Jussieu und Batsch.*[48] (1811). J. Ch. Graumüller did not mention Fr. S. Voigt's interpretation of A.-L. de Jussieu which was presented in a similar form of tables. The botanical gardens were a place of expression, a place of formation for the actors of the botanical science, but also a medium of diffusing knowledge. This is the reason why in order to use the botanical garden for his academic teaching, Fr. S. Voigt tried as soon as possible to introduce his own ideas, most notably in establishing an alpine garden arranged according to A.-L. de Jussieu's natural system[49]. This means, that during the first part of the 19th century at the Jena botanical garden we find some beds previously structured by A. J. G. K. Batsch coexisting with some parts of the garden arranged according to Fr. S. Voigt's improvement of A.-L. de Jussieu's "méthode naturelle".

3. The systematic studies of Fr. S. Voigt

3.1. Fr. S. Voigt-reader of the French science

3.1.1. The germination according to J. H. Jaume Saint-Hilaire

Fr. S. Voigt's first analysis of the criteria of a natural classification appeared in 1805 in the ninth volume of the *Magazin der Naturkunde.* At this time Fr. S. Voigt taught physiology of plants as "Privatdozent" at Jena University and unusually for the early nineteenth century he included the study of cryptogams in his lectures [50]. His first article entitled *Ueber den Bau und die Art zu keimen, bei einigen Monokotyledonen*[51] was a simple presentation of observations on the germination of monocotyledonous plants by Jean Henri Jaume Saint-Hilaire (1772-1845). This French author elaborated a comment by Jean-Baptiste Lamarck (1744-1829) about germination and its connections with fertilization[52]. Indeed,

[47] Batsch. A. J. G. K. 1802. *Tabula affinitatum* […]. Weimar, Landes-Industrie-Comptoir, 1802.

[48] Graumüller, J. Ch. *Tabellarische Uebersicht* […]. Eisenberg, J. W. Schöne, 1811.

[49] Jahn, I. Zur Gründungs- und Entwicklungsgeschichte der Jenaer Botanischen Garten (von 1586 bis 1864), in: *Wissenschaftliche Zeitschrift,* 37(1), 1988: p. 23.

[50] Bach, T. & Breidbach, O. 2001, *Op. cit.* note 27.

[51] Voigt, Fr. S. Ueber den Bau und die Art zu keimen, bei einigen Monokotyledonen in: *Voigt´s Magazin der Naturkunde*, 11, 1805: pp. 218-227 + tab. iv.

[52] Jean-Henri Jaume de St-Hillaire was a student of J.-B. Lamarck at the Muséum National d´Histoire Naturelle between 1799 and 1802. See: http://www.Lamarck.net. Considering the development of the seeds he assumed that "ce n'est point l'acte de fécondation qui donne la vie aux plantes". He supposed notably the

in his *Mémoires de physique et d'histoire naturelle*, the latter set out the germination as the first moment of vegetation. In order to explain the germination, he distinguished three main causes:

"1. de l'humidité qui pénètre la semence […]; 2. du contact de l'air qui favorise le déplacement et le mouvement des fluides […]; 3. de l'action d'un certain degré de chaleur nécessaire pour exciter les premiers mouvements organiques du végétal naissant."[53]

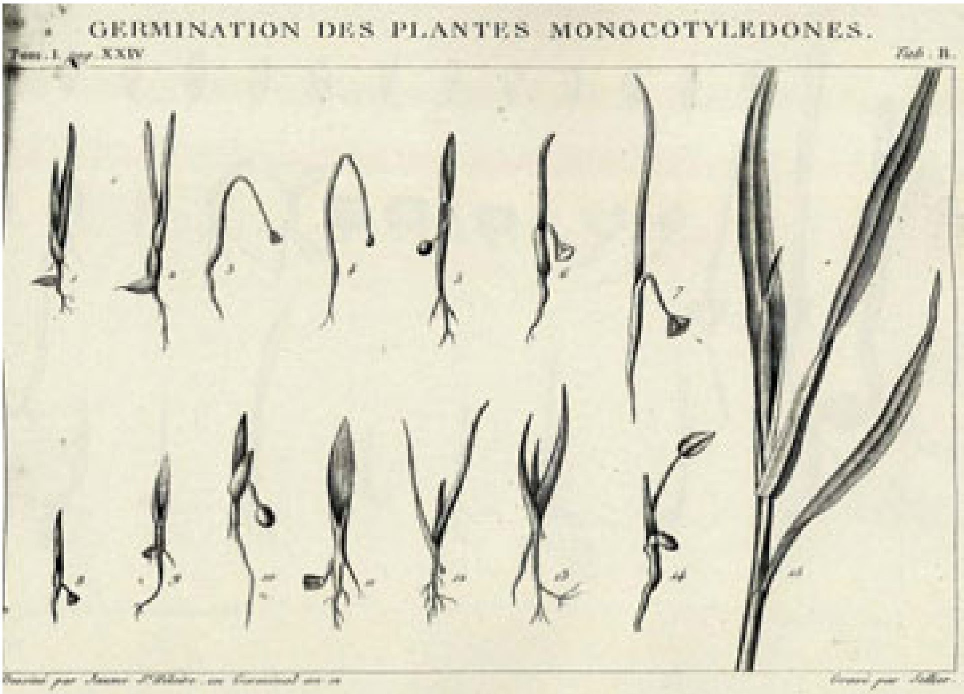

Figure 1. J. H. Jaume Saint-Hilaire, Exposition des familles naturelles et de la germination des plantes, 1805.

influence of a "stimulus de la liqueur ou de la vapeur fécondante" between fertilization and germination. Lamarck, J.-B. *Mémoires de Physiques et d'histoire naturelle* […]. Paris, 1797: p. 285.

[53] Lamarck, J.-B. 1797. *Op. cit. supra*: p. 287.

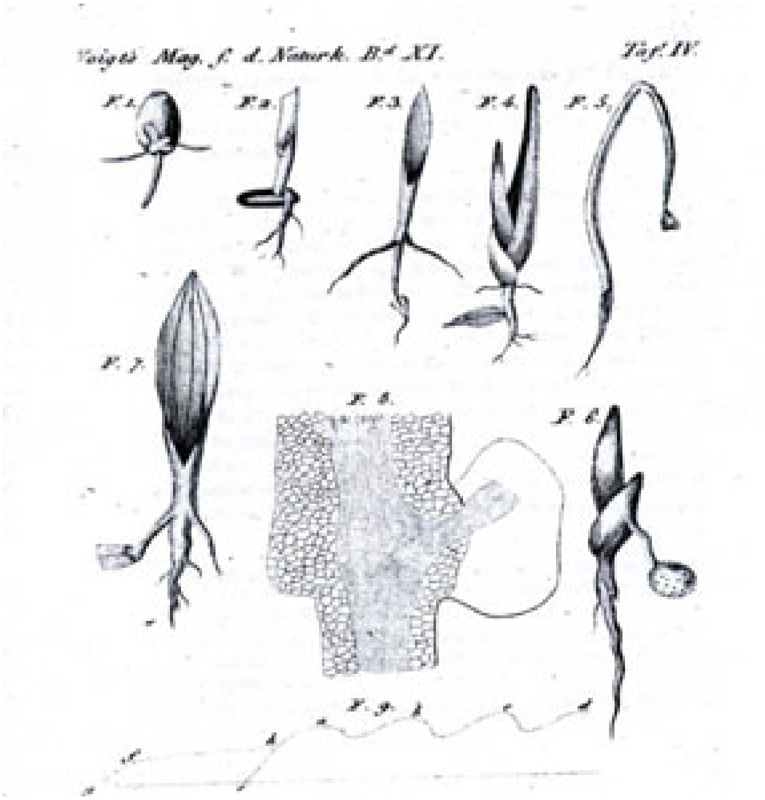

Figure 2. Fr. S. Voigt, Ueber den Bau und die Art zu keimen, bei einigen Monokotyledonen, 1805.

Moreover J. H. Jaume Saint-Hilaire tried to support the distinction between monocotyledons and dicotyledons and also the interpretations of René-Louiche Desfontaines (1750-1833), who had shown in his *Mémoire sur l'organisation des monocotyledons* [*sic*] *ou plantes à une feuille séminale* (1796)[54] that dicotyledonous plants produced not the same stems as the monocotyledonous plants. With this aim J. H. Jaume Saint-Hilaire offered a description of the structure of the seeds, of the phases of the germination and of the orientation of the radicle. He demonstrated for example the influence of structure on the flow of sap. He wrote:

"Cette découverte qui avait échappé à tous les physiologistes m'a paru être en accord avec la germination qui diffère également dans ces deux grandes classes de plantes, formées d´abord par Césalpin, et adopté par l'auteur de la méthode naturelle"[55]

[54] See De Candolle, A.-P. *Mémoires et souvenirs*. Candaux, J.-D. & Drouin, J.-M. ed. Genève, Georg, 2004: p. 87.

[55] Jaume Saint-Hilaire, J. H. *Exposition des familles naturelles et de la germination des plantes.* Paris, Treuttel & Würtz, An XIII.-1805: pp. xxi-xxij.

Interestingly, Fr. S. Voigt published an article which can be regarded as an abstract of the French textbook by J. H. Jaume Saint-Hilaire. However he did not translate the whole analysis, omitting for example the interpretation of the distinction between the characteristics of the monocotyledons and dicotyledons. He dealt with this question in his *Darstellung des natürlichen Pflanzensystems Jussieus* (1806), where he wrote that the last order of the monocotyledons is connected with the first class of the dicotyledons by the similiraties in the shape of their leaves[56]. To illustrate his article from 1805, he selected some relevant figures[57] from Jaume Saint-Hilaire which were partly modified according to his own observations (see Figure 1 and Figure 2). This first article gives evidence of the ability of Fr. S. Voigt to evaluate recent botanical developments, even those published in foreign languages.

3.1.2. The weighting of plant features and the reception of L. Cl. Richard

During his stay in Paris (1809-1810) Fr. S. Voigt met A.-L. de Jussieu, R. L. Desfontaines, professors of botany at the *Muséum national d'Histoire Naturelle* and L. Cl. Richard (1754-1821), professor of botany at the *École de Medicine.* This time in Paris between 1809 and the first months of 1810 was decisive for the rest of his career[58]. A. N. Desvaux wrote:

> "M. Voigt, après avoir passé quelques mois à Paris, convint qu´il avait plus acquis sur la philosophie de la science, que pendant plusieurs années qu´il avait passé à réfléchir sur l´ensemble de la méthode naturelle."[59]

Fr. S. Voigt attended with interest the lectures and the pratical demonstrations of botany provided by L. Cl. Richard. In 1811 Fr. S. Voigt published a translation of L.-Cl. Richard's *Démonstrations botaniques, ou analyse du fruit considéré en général*[60]. This book pays special attention to the consideration of the structure of the fruit, seed and also the radicle after germination. L. Cl. Richard considered this organ to be the main distinction between monocotyledons and dicotyledons, and that A.-L. de Jussieu's "true" moncotyledons did not have a "true" radicle. This was the basis of his new classification, which appeared for the first time in German as a supplement of Voigt's translation of L. Cl. Richard's textbook. This classification included two main groups (*Exembryonatae and Em-*

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[56] He specified his point of view in his *Darstellung des natürlichen Pflanzensystems Jussieus*: "Die Letzte Ordnung der Monocotyledonen schliesst sich unter andern auch durch die Form ihrer Blätter an die erste Klasse der Dicotyledonen an, welche nur aus wenigen Gattungen einer einzigen Familie besteht." Voigt, Fr. S. 1806. *Op. cit.* note 4: p. 21.

[57] Fig. 1 (Fr. S. Voigt) = Fig. 5 tab. A. (J. H. Jaume Saint-Hilaire) Cotyledons and their orientation; Fig. 2 (V) = Fig. 29 tab. A (J) *Oriza sativa,* curve of the root due to the position of the seed during germination; Fig. 3 (V) = Fig. 32 tab. A. (J) *Paspalum stoloniferum*, origin of the lateral roots; Fig. 4 (V) = Fig. 2 tab. B (J) *Cyperaceae* position of the root; Fig. 5 (V) = Fig. 4 tab. B (J) evolution of the husk of the seed; Fig. 7 (V) = Fig. 11 tab. B (J) *Ephémérines,* absence of the husk; Fig. 6 (V) = Fig. 10 tab. B *Anthericum annuum*, continuation of the embryo; Fig. 8 (V) = Fig. 8 tab. B (J) rush of sap from the embryo to the vessel.

[58] Fr. S. Voigt made a second important stay abroad, in London and Cambridge, during the year 1827.

[59] Desvaux, A. N. 1813. *Op. cit.* note 21.

[60] Voigt, Fr. S. 1811. *Analyse der Frucht* […]. Leipzig, C. H. Reklam, 1811.

bryonatae) and four new natural classes ("Exembryonatae: Class I. Anarhizae : „*Pflanzen ohne Wurzel und Embryo*" and Embryonatae: Class II. Endorhizae : „*der Embryo, dessen Radicula ein oder mehrere Radicellarknötchen einschliesst*", Class III. Synorhizae: „*Der Embryo, dessen Wurzel noch mit dem Endosperm zusammenhängt*" & Class IV. Exorhizae: „*Der Embryo, dessen Radicula sich zur wirklichen Wurzel verlängert*".") [61]. L. Cl. Richard certainly used a character more constant than the number of stamens, but his arrangement was still artificial because it referred only to one feature of the plants, consequently Fr. S. Voigt did not apply this classification in his textbooks. Nevertheless, through this translation of the morphological observations of L. Cl. Richard, Fr. S. Voigt asserted his conviction that morphology especially that of fruits and seeds was the key to taxonomy. Throughout this translation he commented on the German reception of A.-L. de Jussieu's natural method:

„Wenn man bedenkt, das hier, nach mehr als zwanzig Jahren, noch nicht einmal Jussieu einen bedeutenden öffentlichen Anhänger zählt, dass seiner und seines herrlichen Systems an den wichtigsten Orten kaum flüchtig erwähnt wird, wie war da zu erwarten, dass man Sachen auffassen sollte, die noch über die Ansichten der gegenwärtigen französischen Botanik weit hinaus liegen ?"[62]

Curiously enough Fr. S. Voigt did not mention his own "*Darstellung des natürlichen Pflanzensystems Jussieus*[63](1806), a series of tables presenting A.-L. de Jussieu's main natural orders, classes and families. There he added several comments on the artificial elements of A.-L. de Jussieu's classification which were still in conformity with C. von Linné's system[64]. Fr. S. Voigt concluded that: "*Die Unterscheidung nach der Zahl ist leichter*"[65], implying of course that the number of stamens was simplest or easiest to see and use, and at the time the most effective character for recognising natural groups. However, he hoped that the fundamental principles underlying the "méthode naturelle" would soon be discovered. Fr. S. Voigt explained in his *Darstellung des natürlichen Pflanzensystems Jussieus*[66](1806) that plants which differ in constant features could not be placed in the same group of species. He noticed further that the features of the embryo[67] afforded the highest classification principle:

"Da der ganze Reichthum der Vegetation nur auf Erzeugung des Embryos berechnet ist, so muss auch bei diesem die Kennzeichen zur obersten Eintheilung zu finden seyn. Wir haben sie mehrmals angegeben, es sind die Cotyledonen." [68]

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[61] *Ebenda*: pp. 170-200.

[62] Voigt, F. S. 1811. *Op. cit. supra*: p. 110.

[63] Voigt, F. S. 1806. *Op. cit.* note 4.

[64] "Gerade da wo Linné am meisten mit Jussieu übereinstimmt, ist er [A.-L. de Jussieu] am weitesten von seiner Regel abgewichen, und hat Ausnahmen (keine Fehler) gemacht, die hoffentlich niemand hier noch unserm Jussieu vorwerfen wird." Voigt, Fr. S. 1806. *Op. cit.* supra: p. 22.

[65] *Ebenda*: p. 23.

[66]Voigt, Fr. S. 1806. *Op. cit.* note 4.

[67] Embryo: term defined by Joseph Gaertner (1732-1791), A.-L. de Jussieu used the term *corculum.*

[68] Voigt, Fr. S. 1806. *op. cit* supra: p. 17.

Fr. S. Voigt also translated a table entitled: "*de la valeur des caractères*" by Pierre Étienne Ventenat[69] (1757-1808), in which, following the ideas of A.-L. de Jussieu, the number and form of the cotyledons is the most important classifying characteristic[70](see figure 3).

Figure 3. *VENTENAT, Tableau de la valeur des caractères [...] in: Fr. S. Voigt, Jussieu's natürliches Pflanzensystem, 1806: p. 19.*

[69] See Ventenat, P. E. *Tableau du règne vegetal selon la méthode de Jussieu.* Paris, J. Drisonier, An VII (1798): 4 vol. Pierre-Etienne Ventenat (1757-1808), librarian of the Pantheon, was a staunch supporter of A.-L. de Jussieu, he was known too for his fine book ordered from the empress Joséphine owner of the "Jardin de la Malmaison": *Le jardin de la Malmaison.* Paris, Crapelet, 1803.

[70] This analyse of the importance of the generic features has been also performed by the Englander John Lindley in his textbook called: *An Introduction to botany*, London, 1832.

P. E. Ventenat presented in this table the main principle of the "méthode naturelle" the subordination of the plant features. A.-L. de Jussieu explained that each characteristic has a relative value and accordingly the value of one variable feature could be equivalent to the sum of the relative values of inconstant characters[71]. In the following years Fr. S. Voigt refined his point of view. The diagnosis of the natural family *Campanulaceae* in his *Lehrbuch der Botanik* (1827) is an example of his procedure. It starts with the features of the embryo, then those of the seed, the fruit, and the stamens etc., and finally the characteristics of the stem. Placing the description of the embryo first shows how much Fr. S. Voigt's ideas had changed and matured since 1806[72]. L. Cl. Richard had explained in his *Démonstrations botaniques* (1808)[73] the constancy of the features of the embryo and especially its position or the orientation of the radicle. L. Cl. Richard converted Fr. S. Voigt to similar ideas, and the latter proposed in his *Lehrbuch der Botanik* (1827) the presence or absence of the embryo as the most important character in classification[74], not simply the number of cotyledons as used by A.-L. de Jussieu. The reception of the idea of the subordination of the plant characteristics and the modifications of this concept by Fr. S. Voigt illustrate his progress of thought, but this example does not give us an insight into his own representation of the affinities of plants and his way of diffusing the natural method to his students.

3.2. Fr. S. Voigt's didactic treatment of plant affinities

In this part we explore chronologically the didactic treatment of plant systematic in his teachings and textbooks. First, we wonder how Fr. S. Voigt taught the Linnean system. We find a response to this question in his botanical dictionaries. In the first edition of his *Handwörterbuch der botanischen Kunstsprache* (1803) the names of classes and families of the Linnean sexual system were included. Furthermore, in his *Übersicht der Naturgeschichte*[75] (1819), a chapter is entitled: "*Fehler und Inconsequenzen des Linneischen Systems, die aber nicht verbessert, sondern nur studiert werden sollen*". Although he removed the Linnaean names in the second edition[76] of his dictionary of 1824 on the pretext that they were well-known by most botanists, he also stressed the preliminary and uncertain state of natural classification and did not introduce the new taxon names of the "méthode naturelle". These first publications highlight the attitude of Fr. S. Voigt towards the Linnean system, which

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[71] See Jussieu, A.-L. 1824. *Op. cit.* note 14: p. 352.

[72] In his *Genera Plantarum* (1789), A.-L. de Jussieu began with the feature of the calyx, the stamen, the corolla […]: "Classis IX. Ordo IV. Campanulaceae: Calyx superus limbo diviso […]. Corolla summo calici inserta […]. Stamina ibidem inserta sub corollâ […]." Jussieu, A.-L. 1789. *op. cit.* note 3 : p. 163.

[73] Richard, L. Cl. *Démonstrations botaniques ou analyse du fruit considéré en général.* Paris, Gabon, 1808: xii + 111 p.

[74] Significance of the features of plants according to Fr. S. Voigt: "1. *Anwesenheit oder Abwesenheit des Embryos. 2. Zahl der Cotyledonen. 3. Lage des Embryo im Saamen, und Anwesenheit oder Fehlen des Eiweißes. 4. Anheftungsart des Saamens in der Früchthülle. 5. Bau der Frucht und ihres Offnens. 6. Verhältniss der Staubfäden zu den Stempeln. 7. zahl des Geschlechtstheile. 8. Die Blumenkrone. 9. Der Blüthenstand. 10. Stellung der Blätter und Aeste Anwesenheit oder Abwesenheit der Stipulae. 11. Der Gesammtbau, ob Bäume, Kräuter etc. 12. Die übrigen Qualitäten, ob es plantae lactescentes, venenatae, aquaticae n. dgl. sind.*" Voigt, Fr. S. 1827. *Op. cit.* note 2: pp. 209-210.

[75] Voigt, Fr. S. *Übersicht der Naturgeschichte zum Gebrauch für höhere Schulen und zum Selbstunterricht.* Jena, 1819.

[76] Voigt, Fr. S. *Wörterbuch der botanischen Kusntsprache.* Jena., A. Schmid, 1824.

remained useful, and the not yet completed "méthode naturelle". Nevertheless he thought that morphological studies were a key to natural classifications.

In his first textbook entitled *System der Botanik* (1808),[77] Fr. S. Voigt introduced, after a precise analysis of plant morphology and physiology, the descriptions of remarkable natural families followed by a short introduction of the "méthode naturelle" developed by Bernard and A.-l. de Jussieu[78]. The basic natural taxonomic category in Fr. S. Voigt's outline of a natural system is the species, which he called *Gattung*[79] following Johann Friedrich Blumenbach's terminology[80]. The next higher groupings are the genera (*Pflanzengeschlecht* in his *System der Botanik* (1808), *Geschlecht* Bl. in his *Lehrbuch der Botanik* (1827)), and these encompass species with similar shapes of flowers and fruits. These features are considered to be of special value because they are more constant than the features of the vegetative parts of the plants which are easily influenced by environmental factors. Fr. S. Voigt was convinced that genera as well as species were natural categories.[81] In this regard he takes a standpoint different from that of J.-H. Jaume Saint-Hilaire, who considered the genera as mere arbitrary divisions of higher taxa that were introduced in order to structure the vegetable kingdom in a better way[82]. If several genera were similar in their general construction and in the most significant parts of fructifications they composed an order or a natural family (*Ordo seu familia naturalis*). Fr. S. Voigt's basic systematic approach is agglomerative rather than divisive. This may reflect his reception of J. W. von Goethe's metamorphosis of plants[83]. To Fr. S. Voigt the interpretation of the metamorphosis of the whole plant kingdom would have been useful also in determining the relative weighting of characteristics, he wrote:

"Daher suchen er [A.-L. de Jussieu] und seine Nachfolger eine gewisse Stufenfolge in dem relativen Werthe der Charactere aufzufinden und zu bestimmen, und würde, wenn er die deutsche Lehre von der Metamorphose damals gekannt hätte, gewiss diese mit Begierde dazu benutzt haben."[84]

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77 Voigt, F. S. *System der Botanik*. Jena, Akademische Buchhandlung, 1808.

78 See the chapter *Uebersicht mekwürdiger Pflanzenfamilien* in: Voigt, Fr. S. 1808. *Op. cit. supra*: pp. 351-384. These were well received, particularly in the *Allgemeine Literatur Zeitung;* see the review about Voigt's *System der Botanik* in: *Algemeine Literatur Zeitung*, 325, 1808: pp. 562-563.

79 "Dass ich mit Blumenbach Species durch Gattung, (und nichr, wie Einige, denen es beliebte, unsere Sprache zu ihrer Bequemlichkeit nach Willkühr zu verbessern, durch Art, und Genus durch Gattung) übersetze, darüber verweise ich diejenigen, welche von der Richtigkeit dieser Übersetzung noch nicht überzeugt seyn sollten, auf das, was *Hr. Hofr. Blumenbach* in der Vorrede zu seinem Handbuch der Naturgeschichte gesagt hat." Voigt, Fr. S. 1808. *Op. cit. supra*: p. 113.

80 Voigt, Fr. S. 1808. *Ebenda*: p. 188.

81 *Ebenda*: p. 190.

82 See Stevens, P. F. 1994. *Op. cit.* note 3: p. 96.

83 See Breidbach, O. Friedrich Siegmund Voigt – Botanik nach Lesart von Goethes Metamorphosenlehre, in: *Goethe-Jahrbuch*, 2004: pp. 238-252 and Hellwig, F. & Robin, N. Fr. S. Voigts Idee der Metamorphose des Pflanzenreichs. In preparation.

84 Voigt, Fr. S. 1827. *Op. cit.* note 2: pp. 208-209.

Perhaps he regarded the idea of metamorphosis as a justifying principle behind the attribution of a specific value to a certain character. Was the metamorphosis to be helpful not only to understand the features of plants but also to understand the affinities of plant groups?

In 1812 Fr. S. Voigt arranged his catalogue of the botanical garden Jena and of the garden *Belvédère* in Weimar[85] according to 106 natural families previously described in the literature, for example by J. H. Jaume Saint-Hilaire[86]. In this catalogue Fr. S. Voigt placed the genera *Chara* or *Potamogeton* within the family *Najades* as Monocotyledons, although A.-L. de Jussieu had defined the *Najades* as Acotyledons[87]. In fact, although Fr. S. Voigt characterized the *Najades* as Monocotyledons, he observed that they were at the boundary to the Acotyledons:

„Es sind die unvollkommensten Gewächse mit deutlichen Blüthentheilen, und oft mit dunkeln, schmalen, pergamentartigen Blättern versehen. Sie gränzen nahe an die Acotyledonen, mit denen sie auch vielfach die Sternartige Stellung ihrer Blätter gemein haben. Viele tragen nur Staubfäden und Stempel, ohne weitere Hüllen. “[88]

This idea derived without doubt from his reading of Jaume Saint-Hilaire who placed his family *fluviales* (*Potamogeton, Chara, Naias* etc.) between the ferns and the *Aroideae*, the latter being placed by Jaume Saint-Hilaire between the Monocotyledons[89].

In 1817 Fr. S. Voigt described 147 natural families in his *Grundzüge einer Naturgeschichte*[90]. The *Najades* were included in a new family called *Saurureae*, but this time included in the Acotyledons. In 1819 he deleted the group *Najades* from his classification probably following the publications of R. Brown[91], in 1827 he put the *Najades* within the order of *Potameae* as *Embryonnatae Monocotyledones*. Fr. S. Voigt's natural families are in general conformity with A.-L. de Jussieu's natural groups but he introduced more hierarchy in their arrangement, in that way emphasizing that some families are subordinated to others. Fr. S. Voigt used the layout of the printed page to state his point, for example:

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85 Voigt, Fr. S. *Catalogus plantarum*. Jena, Goepferdtii, 1812: 78 p.

86 In comparison with the 100 natural families of A.-L. de Jussieu, Fr. S. Voigt introduced 12 new families : *Dicotyledones: Passifloreae, Succulentae, Personatae, Orobranchoidae, Rhinanthoideae, Pyrenaceae, Primulaceae, Drimyrhizeae* and *Monocotyledones : Aloideae, Smilacinae, Alismoideae, Gloriosae*. He transferred 5 families from the Dicotyledons *to* the Monocotyledons: *Laurineae, Polygoneae, Atriplices, Amaranthi, Plantagines*. Nevertheless *he* described again the *Plantagines* as Dicotyledons in his *Lehrbuch der Botanik* (1827). Moreover, Fr. S. Voigt deleted 6 families: *Cannae, Pediculares, Vitices, Sempervivae, Scrophulariae, Lysimachiae*.

87 Jussieu, A.-L. 1789. *Op. cit.* note 3.

88 Voigt, Fr. S. 1808. *Op. cit.* note 77: p. 137.

89 J. H. Jaume Saint-Hilaire proposed a vague definition of the group *Najades:* "*Elles* [Fluviales or Najades] *renferment des monocotylédones et des dicotylédones et nécessite par conséquent une réforme*." Jaume Saint-Hilaire, J. H. 1805. *Op. cit.* note 55: p. 50.

90 Voigt, Fr. S. *Grundzüge einer Naturgeschichte*. Frankfurt a. M., H. L. Brönner, 1817: pp. 636-637.

91 Voigt, Fr. S. l819. *Op. cit.* note 75. See Brown, R. *Prodromus florae novae Hollandiae*. London, Taylor & Johnson, 1810.

A) Apetalae[92]

34. *Aristolochiae.*
35. *Cucurbitaceae,* gurkenartige Gewächse.
    36. *Passifloreae,* Passionsblumen.
37. *Urticeae*, Nesselarten.
    38. *Monimiae.*
39. *Amentaceae*, Kätzchenbäume.

A similar conceptualisation of relationships of families and subfamilies is also revealed in his *System der Botanik* (1808), for example *Apetalae,* 19. *Cucurbitaceae* (*Passifloreae, Asaroideae*). We may consider that this arrangement of plant groupings expresses an idea of affinity between families.

In his *Übersicht der Naturgeschichte* (1819)[93] Fr. S. Voigt attempted to make the natural method easy to learn. He listed 33 natural groupings of local plants[94] (see figure 4), which was done in a similar way by A. J. G. K. Batsch in his *Anleitung zur Kenntniss der Gewächse*[95]. The use of the term *Verein* instead of *Familie* as well as the use of German names for these *Vereine* confirms his intention to facilitate a general understanding of the affinity of plants rather than the details of the natural system which were difficult to apply by his students during their herborisations.

1. Schmetterlingsblumen oder Hülsenfrüchte (Papilionaceae)	17. Rachen-, Quirlblumen (Labiatae)
2. Rosenartige Gewächse (Rosaceae, Pomaceae…)	18. Compositae
3. Malvenartige Gewächse (Malvaceae)	19. Amaranthes (Amaranthii)
4. Mohnartige Gewächse (Papaveraceae)	20. Ballblüthen (Oleaceae)
5. Ranunkelartige Gewächse (Ranunculaceae)	21. Kätzchenbäume, Laubhölzer (Amentaceae)
6. Schooten Gewächse ; Kreuzblumen (Cruciferae)	22. Nesselarten (Urticeae, Scabridae)
7. Nelkenartige Gewächse (Caryophylleae)	22. Kürbißarten (Cucurbitaceae)
8. Saftige, fleischige Gewächse (Succulentae)	23. Dreiknüpfige Gewächse (Tricoccae)
9. Epheuarten (Hederaceae)	24. Lorbeerartige Gewächse (Laurineae)
10. Schirmpflanzen, oder Dolden Gewächse (Umbellifereae)	25. Nadelhölzer, Zapfenbäume (Coniferae)

[92] Voigt, Fr. S. 1817. *Op. cit. supra*: p. 639.

[93] Voigt, F. S. 1819. *Op. cit.* note 75.

[94] *Ebenda*: p. 127. "Um für gegenwärtige Zwecke das wichtigste der Pflanzenwelt im Speziellen zu geben, werden zwei und dreißig bekannte natürliche Vereine ausreichen, die jedoch, sowie hier stehen, noch kein wissenschaftlich natürliches System ausmachen." In fact he described 32 number of *Vereine* because of a printing mistake.

[95] Batsch, A. J. G. K. *Versuch einer Anleitung, zur Kenntniß und Geschichte der Thiere und Mineralien* […], Jena, Akademische Buchhandlung, 1788-1789.

11.Windenarten (Convolvulaceae)	26. Palmen (Palmae)
12. Contorten (Contortae)	27. Gräser (Gramina)
13. Heidartige Gewächse (Ericaceae)	28. Liliengewächse (Liliaceae)
14. Primelartige Gewächse (Primulaceae)	29. Farrenkräuter
15. Tollkräuter (Lurideae)	30. Moose
16. Scharfblättrige Gewächse (Asperifoliae)	31. Algen, Tange (Alga marinae), Flechten
	32. Schwämme und Pilze

Figure 4: The natural associations "Vereine" according to Fr. S. Voigt.

In his *Lehrbuch der Botanik*[96] (1827) Fr. S. Voigt developed a new system of classification, but one still based on the same fundamental principle: the weighting of plant characteristics. There were 203 natural families at this time, but such an increase in the number of natural families is common in the literature of the early 19th century. Thus, in the introduction to his natural system (1830)[97] J. Lindley announced the necessity of creating new natural divisions, while F. G. Bartling[98], who tried to combine A.-L. de Jussieu's natural method and the "*Theorie élémentaire*" of A.-P. de Candolle, proposed in his "*Ordines plantarum*" 60 orders and 170 natural families. K. P. Sprengel confirmed this tendency in an unauthorised German translation of the "*Théorie élementaire*":

„Es kann nicht fehlen, dass, je weitere Forschritte man macht, desto mehr Familien werden entdeckt werden; denn immer wird man bey einzelnen Gruppen solche Auszeichnungen gewahr, die sie von der Familie unterscheiden, zu welcher man sie sonst zu zählen pflegte."[99]

The chronology below (Figure 5) clearly illustrates the development of Fr. S. Voigt's ideas of a natural system and the relations of his developing ideas with his contact to Parisian botanists. We must not forget the influence of A.-P. de Candolle on the work of Fr. S. Voigt. Although the latter mentioned the Genevan professor many times, he did not apply his advanced terminology[100] nor did he mention that plant affinities could be established on the basis of a common symmetrical arrangement of the floral organs, an idea very clear in the Théorie élémentaire[101].

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96 Voigt, Fr. S. 1827, *Op. cit.* note 2.

97 Lindley, J. *An introduction to the natural system of botany.* London, Longman, Rees, Orme, Brown & Green, 1830.

98 Bartling, F. G. *Ordines naturales plantarum.* Gottingae, Dietrich, 1830.

99 Sprengel, K. P. [& De Candolle, A.-P.] 1820. *Op. cit.* note 6: p. 139.

100 "Auch manches bloss von einem einzelnen Autor gebrauchte Wort, wenn es überflüssig schien, ist bei Seite geblieben , wie die kleinlichen Abänderungen ohne allen Werth deren sich Herr Decandolle hie und da bedien, z. B. petiolulatus etc., oder die ganz unschickliche Bezeichnung einer einjährigen Pflanze durch pl. Monocarpa u. s. w". Voigt, Fr. S. 1824. *Op. cit.* note 76: pp. vii-viii.

101 In this way the morphological variation between species may be explained through the modification of this symmetry resulting from the failure, the degeneration and the fusion of organs.

1805: ***Ueber den Bau und die Art zu keimen, bei einigen Monokotledonen.***
Reception of Jaume Saint-Hilaire's observations on the germination.

1806: ***Darstellung des natürlichen Pflanzensystems von Jussieu.***
Reception of A.-L. de Jussieu's "*méthode naturelle*" and transcription of P. E. Ventenat's "*tableau de la valeur des caractères*"; Features of the embryo taken to be most important in classification.

1808: ***System der Botanik.***
Presentation of 24 natural families; Subordination of several natural families; First fusion of J. W. von Goethe's idea of the metamorphosis of plants with the natural method.

1811: ***Analyse der Frucht des Saamenkorns von Louis-Claude Richard.***
For the first time in Germany diffusion of L.-Cl. Richard's outlines of a natural classification.

1812: ***Catalogus plantarum quae in hortis ducalibus botanico Jenensi et Belvederensi coluntur.***
Arrangement of the plants in 106 natural families.

1817: ***Grundzüge einer Naturgeschichte.***
147 recognised natural families, some subordinated to others.

1819: ***Uebersicht der Naturgeschichte.***
Educational presentation of 33 natural "associations".

1824: ***Wörterbuch der botanischen Kunstsprache.***
Deletion of taxonomic names of the Linnean classification;
Reception of A.-P. de Candolle's *glossologie.*

1827: ***Lehrbuch der Botanik.***
Absence or presence of the embryo as well as the features of this organ taken to be the most important taxonomic characteristic.

Figure 5: Fr. S. Voigt's treatment of the natural system.

In fact the study of the reception of the "*Théorie élémentaire*" in Weimar-Jena is quite complex because of the existence of the several German translations, for example the edition of J. J. Römer[102] and the "translation"/ interpretation of K. P. Sprengel criticized by A.-P.de Candolle himself[103].

3.3. Conclusion

To conclude we want to discuss two points of P.-F. Stevens's interpretation of A.-L. de Jussieu's "*Méthode naturelle*" and the "German-speaking world". First he assumes that "*There is nothing in Voigt's text about continuity*". This idea of continuity was like the weighting of plant characters a basis of A.-L. de Jussieu methodological thought. P. F. Stevens refers to Fr. S. Voigt's *Darstellung des natürlichen Pflanzensystems von Jussieu* (1806), which includes indeed no idea about continuity, but two years later he published his first textbook entitled *System der Botanik* (1808) with the aim to introduce the idea of Metamorphosis in the construction of a natural method. Fr. S. Voigt's approach to the structure of the vegetable kingdom is agglomerative, he based his whole approach on the affinities on the concept of the weighting of each feature of plant organs, he links the affinities of plants to their environment, and finally he notices groupings of plants in form of connected families and sub-families, genera and species. Consequently, the idea of continuity has gained ground in the textbooks of Fr. S. Voigt, for exemple he wrote in 1827:

> "Aber auch Monopetalie und Polypetalie sind nur relative, wenn auch beständigere Formen (man denke z. B. an die *Ericaceae*) es müssen demnach die *Nyctagineae, Primulaceae, Phlox* u. s. w. auch mit den Caryophylleis zusammengestellt, und anderseits unter diesen die *Cerea*, nemlich *Cactus*, als abweichende, übrigens dazu gehörige Bildungen, erkannt werden."

Although his publications have seldom been mentioned by the botanical community the evaluation of Fr. S. Voigt's propositions allows us to qualify a second assertion of P. F. Stevens:

> „If these German-speaking naturalists had attempted to understand the ideas of Candolle and Jussieu by reading commentators and translators speaking their own language, they still would not have found a clear idea of Nature."[104]

Fr. S. Voigt was educated in the field of botany by A. J. G. K. Batsch at the Jena botanical garden. However the difference between the natural system developed by A. J. G. K. Batsch and the outlines of Fr. S. Voigt's classification is clear. Nevertheless, because of the active support of J. W. von Goethe, the continuity of the botanical research on natural classification had not been troubled. J. W. von Goethe never contrasted the natural

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[102] Römer, J. J. *Theoretische Anfangsgründe der Botanik* […]. Zürich, Orell, Füssli & Co., 1814-1815: 2 vol.

[103] Sprengel, K. P. [& De Candolle, A.-P.] 1820, *op. cit.* note 6. De Candolle wrote: „Mr Sprengel, qui unissant dans l´ouvrage ses idées aux miennes et dans le titre son nom au mien, en fit un livre vraiment absurde où la fin de chaque chapitre est en opposition avec le commencement…" De Candolle A.-P. *Mémoires et souvenirs (1778-1841).* Candaux, J.-D. & Drouin, J.-M. Hrsg. Genève, Georg, 2004: p. 277 and De Candolle, A.-P. *Organographie végétale.* Paris, Deterville, 1827: p. v. note 1.

[104] Stevens, P. F. 1994. *Op. cit.* note 3: p. 94.

system developed by A. J. G. K. Batsch or A.-L. de Jussieu's "méthode naturelle". On the contrary he sustained the necessity to break with the criteria of classification stated by C. von Linné and naturally supported the thought process of A. J. G. K. Batsch. However, he did not seem to follow unconditionaly all the systematical propositions of the latter uncritically. For example, we find the following comment in his "*naturwissenschaftlichen Schriften*":

"Durch den fördernden Umgang mit Batsch waren mir die Verhältnisse der Pflanzenfamilien nach und nach sehr wichtig geworden, nun kam mir Usteris Ausgabe des Jussieuschen Werks ["Genera plantarum"] gar wohl zustatten; […]. Jedoch konnte mir nicht verborgen bleiben, daß die Betrachtung der Monokotyledonen die schnellste Ansicht gewähre, indem sie wegen Einfalt ihrer Organe die Geheimnisse der Natur offen zur Schau tragen und sowohl vorwärts zu den entwickelten Phanerogamen als rückwärts zu den geheimen Kryptogamen hindeuten."[105]

We have demonstrated in this paper that Fr. S. Voigt's ideas and the evolution of his sketches of a natural classification of plants were quite clear. He always paid significant attention to the French botanists who were developing the natural method. We have mentioned his translation of the L. Cl. Richard but we may state furthermore that Fr. S. Voigt was also one of the german translators of G. Cuvier (1769-1832) [106], who in his zoological publications, himself had underlined the importance of the subordination of the plant features. From his studies of L. Cl. Richard to his translation of G. Cuvier Fr. S. Voigt's purpose was always to understand the affinities of the organisms. Furthermore the works of Fr. S. Voigt come within the framework of the dynamics of research on plant systematics in the Weimar-Jena area around 1800, also sustained by the Weimar publisher *Landes-Industrie-Comptoir* which diffused translations of English textbooks and interpretations on the natural method, for example James Edward Smith's *Grammar of Botany* or John Lindley's *Introduction to Botany*. In the development of Fr. S. Voigt's scientific works we can observe the reception of the French science, the critical evaluation of traditional and new taxonomic concepts.

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[105] Goethe, J. W. von. *Naturwissenschaftliche Schriften*. Leipzig, Insel Verlag, 1916: p. 227.

[106] See Voigt, Fr. S. Das Thierreich, geordnet nach seiner Organisation: als Grundlage der Naturgeschichte der Thiere und Einleitung in die vergleichende Anatomie / vom Baron von Cuvier. Leipzig, Brockhaus, 1831-1843: 4 vol. Cain, J. *op. cit.* note 14, 1957-1958: pp. 185-217. See Robin, N. (2006)

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Neither Creation nor Evolution: the Third Way in Mid-Nineteenth Century Thinking about the Origin of Species[1]

Nicolaas A. Rupke

Abstract

A revision of the standard account of nineteenth-century evolutionary biology is proposed. The debate about the origin of species was not merely one of creation versus evolution. There existed a third view, which equated the problem of how species originated with that of how life had commenced. Major and minor scientists alike postulated that species had appeared not as a result of divine fiat nor by the transmutation of one form of life into another, but by the spontaneous generation of their first seeds, germs or primordial embryos from dead matter, a process also referred to as autochthonous generation. Especially in the German-speaking world, until the appearance of Charles Darwin's On the Origin of Species *(1859), this was the dominant theory of how plants, animals and humans had come into being. Among the leading autochthonists were Hermann Burmeister, Heinrich Bronn and Carl Vogt. Sympathisers included Alexander von Humboldt and Charles Lyell. One of the empirical foundations of the theory was the observed fixity of species. Added to this was the discovery of repeated extinctions and reappearances of life on earth. The existence of geographical provinces of distribution of plant and animal species seemed to confirm autochthonous origins, and Alphonse de Candolle, Joseph Dalton Hooker and other botanists flirted with the theory. Also the existence of human varieties and their segregated distribution across the globe was attributed to the autochthonous generation of each race. Leading biomedical scientists, among whom Johannes Müller and Rudolf Virchow, worked with the notion of spontaneous species generation. Its philosophical roots greatly varied, from the idealism and vitalism of* Naturphilosophie *to vulgar materialism. Vogt and other left-wingers linked their belief in the spontaneous emergence of life following Cuvierian earth catastrophes to their support of the Revolution of 1848. In the* Origin of Species, *Darwin ignored the autochthony view. Following the book's appearance, many autochthonists converted to evolution, and their previous view was written out of the record and "forgotten."*

Tertium datur: Autochthonous Generation

During the middle of the nineteenth century, several eminent biologists and paleontologists rejected both "evolution" and "creation" as modes of the origin of species. They

[1] I warmly thank Peter Bowler, Bill Bynum, Thomas Junker and especially David Livingstone and Jim Moore for encouragement and advice.

regarded the doctrine of special creation as incommensurable with science but at the same time thought that the notion of species transformation/transmutation, going against the observed fixity of species and being allied to the speculations of German *Naturphilosophie*, was poor science, too. If they held neither of these two beliefs about the cause of organic diversity, what then did they believe?

In the literature about the history of Darwinism and of evolutionary biology in general, the debate about the origin of species has traditionally been discussed in terms of the two possibilities of "creation" and "evolution" only, and scientific views on the issue have been measured by a yardstick that ranges from orthodox creationism to radical Darwinism (from the large body of literature see for example Zimmermann 1953, 337-456; Bowler 1984, 109-150; Junker and Hoßfeld 2001, 49-74). This portrayal is incomplete, however. Some of the life scientists, in opposing "evolution" as well as "creation," put forward a third theory of the origin of species. This was referred to as the doctrine of "autochthons" (from a Greek word meaning "sprung from the earth;" Wagner 1845, 408; synonymously used was "Urzeugung der Arten;" here I use "autochthonous generation").

Autochthonous generation postulated not just spontaneous generation of simple forms of life but a kind of spontaneous mega-generation of all species, small and large, simple and complex, primitive and advanced, *Homo sapiens* included; and not only that: autochthonous generation was said to have produced new species as groups of individuals (not as a single representative or a pair of each), and as communities of species (not as one species at a time), in several different, geographically widely separate locations (not in one place), and this process had taken place repeatedly throughout geological history (not once in the distant, primordial past). Autochthonous generation postulated much more than conventional spontaneous generation, being primarily a theory of the origin of species and not an hypothesis about the origin of life – like "normal" spontaneous generation was and today still is. For this reason – and because it was not a view of cranks but was advocated by leading life scientists – the theory must rank as a separate, third category of nineteenth-century thought about organic origins. It was a theory rather than an hypothesis, in the sense that it was a proposition based on a broad basis of sound inferences and observations rather than a speculative supposition.

The autochthony view, however seemingly fanciful, was the product of strict scientific reasoning – its advocates emphasized. The main links in the chain of reasoning were the following (disregarding several subsidiary "buts" and "ifs"). (1) Species are not eternal, as shown by historical geology, but originate, disappear and are replaced by new ones. (2) Species are fixed, as shown by universal experience. (3) Creation of species by divine interference is not a scientifically meaningful interpretation. (4) Evolution in the sense of transformation of one species into another goes against observation. (5) Ergo, species have originated in a natural way and abiogenetically, from lifeless matter. This was the logical "tertium datur:" if no creation and if no transmutation of species, then there must have been in operation a non-miraculous process of origin *de novo* of full-blown species. Thus the very first appearance on earth of a species was envisaged as an instantaneous development, possibly from vital germs that under special circumstances had coalesced from inanimate matter. To some authors this process of abiogenesis represented the incarnation of ideal types by means of a separate life force, to others no more than the workings of properties of matter that did not fundamentally differ from those forming crystals.

By the middle of the nineteenth century, spontaneous generation had become controversial, and the advocates of autochthonous generation with regret admitted that today the process appears not to be taking place, not of higher organisms and most likely not even of single-celled organisms or of small, primitive entozoa – the common "suspects" of spontaneous generation at the time. If small, primitive organisms were not generated spontaneously, how could large ones, let alone entire communities of these? Yet there seemed to be no imaginable way around abiogenesis; after all, even if today all species are perpetuated by means of parental procreation, the origin of the first parents must still be explained; moreover, at some moment in the past, life as such must have originated from lifeless matter and, this admitted, the process could conceivably have repeated itself at later stages and for higher forms of life. All that had been needed were special circumstances. Whereas "transmutation" resorted to the imperfection of the geological record and the length of geological time, "autochthonous generation" had recourse to unusual conditions, such as those occurring in the wake of geological catastrophes. The process might have temporarily operated vigorously following mass extinctions. This one-and-only admissible option of autochthony, however far-fetched to later generations of biologists, was nevertheless the one to which during the middle of the nineteenth century a number of botanists, zoologists and palaeontologists was drawn – if not driven.

Autochthonous generation had a bearing not just on the issue of origins but also on problems of phyto- and zoogeography, which at the time, in the wake of the Humboldtian turn towards geographic distribution (Rupke 2001), had gained widespread interest. Botanists linked their discovery of well-defined realms of plant distribution with the notion of multiple local origins that were "centres of creation" ("creation" here, for most authors, did not mean "special creation" but "origin") or, less ambiguously, "centres of appearance." Also, autochthony had fewer problems than Mosaic creationism explaining the occurrence in widely separated regions of representatives of one and the same species. For example, freshwater fish belonging to a single species but living in wholly unconnected bodies of water presented a migrational problem to the notion of a single centre of creation/dispersal but could be readily attributed to multiple spontaneous generations triggered by identical conditions. Human racial variety across the globe, too, was explained in terms of autochthonous generation. Such simultaneous multiple origins had anti-creationist implications as they went against the stories of Paradise and Flood, adding to the non-theological, scientific appearance of the theory.

In the course of the first half of the nineteenth century autochthonous generation became the leading paradigm of organic origins in the German-speaking world. Following the appearance of the *Origin of Species*, the theory of autochthony rapidly disappeared and soon appeared forgotten. It is not explicitly identified in the secondary literature, not even in the monographs on the history of the spontaneous generation debate by Farley (1974), Fry (2000) and Strick (2000). Bowler, in his comprehensive history of the idea of evolution, gives the theory short shrift, devoting no more than a single sentence to it. Speaking about the period 1800-1859, he writes: "The idea of a series of spontaneous generations was retained by a few German naturalists but now was abandoned generally" (Bowler 1984, 111). It would appear that this particular theory of the origin of species has been erased from the map of nineteenth-century scientific thought. It has become so unfamiliar that scholars reading the original literature fail to recognize the theory when they encounter it and, in a number of instances, have misidentified it as a form of either creationism or evolutionism (see below).

A contributory cause of misidentification may have been a misunderstanding of the word "creation." At the time, it became invested with new meanings. It could still refer to a miraculous act of God – a *creatio ex nihilo*, but increasingly scientists used the word simply to mean "origin." To the advocates of autochthonous generation it was a synonym for "spontaneous generation." Richard Owen (in)famously stated that to a mid-nineteenth century zoologist "creation" meant nothing more than "a process he knows not what" (Rupke 1994, 236), the vagueness of which definition was subsequently ridiculed by Charles Darwin (1988, xvii-xviii). In the case of Owen and other Christian biologists, use of the word "creation" helped soften the impact of the fact that they no longer meant divine intervention but a natural process of evolution, however much they interpreted that process as divinely ordained. Later process theologians, too, by introducing the term "continuous creation," employed the word as a verbal fig leaf to cover the embarrassment of their acceptance of an evolutionary world view. By contrast, to materialists such as Carl Vogt and later Ernst Haeckel, using the word "creation" ("Schöpfung") to denote a natural origin, represented a conscious act of appropriation of religious vocabulary – a laying claim to this word in a secular scientific context and a disinvesting it of any Christian meaning.

The main purpose of this paper is to establish that there indeed existed a third way in mid-nineteenth century thinking about the origin of species, for which purpose I document the views of some of its protagonists. The sources I use are all familiar and my argument is based on a re-reading of these, not on new material. My approach is that of a conventional history of ideas; yet the implications and results are not quite so conventional, as indicated at the end, where I briefly reflect upon the historiographical significance of the theory and on its post-*Origin* disappearance from the record.

The Advocates and their Explications

The post-Romantic years in the history of science (c. 1840-1859; some place the *terminus a quo* five years earlier) saw a steep decline both of creationism with its connection to church and clergy, and of *Naturphilosophie* with its predilection for mystical analogies and correspondences. During these years, too, Darwin returned from his *Beagle* journey around the world (1831-1836), developed his theory of natural selection with a "Notebook on the Transmutation of Species" begun in 1837, an 1844 essay on natural selection and in 1859 his *magnum opus*. Simultaneously, a number of mainly German naturalists went public with their anti-creationist, anti-evolutionist and pro-autochtonist views. From among them, I select the following three biologists/paleontologists for concise close-ups of their stance on the question of the origin of species: the Halle and Buenos

Aires zoologist Hermann Burmeister, the Heidelberg paleontologist Heinrich Georg Bronn, and the Giessen and Geneva zoologist Carl Vogt.

Figure 1. Karl Hermann Burmeister (Burmeister 1872, frontispiece).

Karl Hermann Konrad Burmeister (1807-1892) (Figure 1)

Burmeister's career took place in two major stages. In Germany he acquired a reputation as an entomologist, in 1842 becoming professor of zoology at Halle University. An admirer and protégé of Alexander von Humboldt, he undertook major journeys of exploration to South America. In 1861 he emigrated to Argentina, becoming the following year director of the Museo Público in Buenos Aires (Argentina's National Museum of Natural History) (Birabén 1968; Ulrich 1972; Streicher 1993). In 1843 he published the popular *Geschichte der Schöpfung*, which between its year of first publication and 1872 enjoyed eight editions, as well as a Dutch, French, and Spanish translation (Salgado and Floria 2001). Burmeister interpreted the history of life on earth as a teleological process towards *Homo sapiens*, of which the successive steps had been progressive and had shown adaptation to changing environmental conditions – an at the time widely accepted interpretation of the paleontological record. The consecutive stages of organic development had not evolved one from the other but originated independently in a process of ideal types becoming incarnated through secondary laws. Species were fixed and had originated from "elemental raw materials" (Burmeister 1848, 311), i.e., in a process of abiogenesis. Burmeister admitted that scientific evidence for the occurrence today of spontaneous generation is weak but assumed that the process was real nevertheless, because no incontrovertible proof to the contrary existed and the only alternative to this natural process of origins would have been divine, miraculous intervention, which however ran counter to the results of science.[2] The possibility of an abiogenesis of all species was indicated by the fact that at present infusoria and parasitic entozoa still seemed to originate in a process of "Urbildung." In the past all

[2] "Letztere [the advocates of spontaneous generation] lehren nämlich, von jenem Entstehen fremder Organismen in anderen ausgehend, die Möglichkeit des Entstehens aller Organismen auf dieselbe Weise in frühester Zeit, und nehmen für jetzt nur die Bildungsfähigkeit niedriger, unvollkommen entwickelter organischer Körper aus elementaren Stoffen an. Ob diese Annahme einen positiven Grund habe, steht gegenwärtig noch dahin, wenn sich gleich die meisten Stimmen der Zeitgenossen dawider erklären; wir wollen sie indeß einstweilen beibehalten, weil in der That kein einziger streng wissenschaftlicher Gegenbeweis vorliegt, und ohne dieselbe das Entstehen der Organismen auf der Erdoberfläche nur durch unmittelbares Eingreifen einer höheren Macht denkbar ist, dafür aber aus dem ganzen übrigen Entwickelungsgange des Erdkörpers kein hinreichendes Motiv nachgewiesen werden kann, vielmehr ein solches unmittelbares Eingreifen der Gottheit allen anderen wissenschaftlichen Resultaten widerspricht" (Burmeister 1848, 312).

organic forms had been brought forth that way, not only lower but also higher organisms; Burmeister specifically included humans.

If primitive organisms still come into being through a process of spontaneous generation, why do higher organisms at present no longer do so – why do advanced species derive exclusively from reproductive parents, Burmeister asked? The reason for this was that in nature's economy only the necessary, never the superfluous, is allowed: when once self-reproducing organisms have formed that are capable of perpetuating the species to which they belong, matter loses its independent procreative potency ("Zeugungsmacht").[3] A further reason was that the organic matter from which living organisms first originated is no longer available in sufficient quantity. The organic matrix ("organische Grundmaterie") – Burmeister believed – had been produced in copious quantities during the early phase of the history of the earth, when the conditions were ideal, with a plentiful supply of nitrogen- and calcium-compounds, and with high temperatures and high humidity; but this matter had been used up in the production of organic life. Therefore today only tiny infusoria could originate spontaneously or, if larger ones such as parasitic entozoa, this was because they develop inside host organisms that provide them with the necessary organic matter.[4]

Thus the origin of the first representatives of species had been the result of the free procreative capacity of matter itself ("die freie Zeugungskraft der Materie selbst") and the discontinuation of this capacity for higher organisms the result of general laws of nature's parsimoniousness.[5] This parsimony did not mean, however, that species were produced in the form of a single individual or a single pair. On the contrary – Burmeister argued – species had originally come into being at various different locations across the globe. This had been particularly true of humans, the secure survival of which had been assured by nature when it produced more than a single pair and at different locations on earth.[6] By analogy to present-day processes the "Urbildung" had not produced adult individuals but more likely juvenile ones.[7]

[3] "Durch diese Betrachtung scheint sich auch die Frage zu erledigen, warum in gegenwärtiger Zeit keine höheren Organismen mehr durch Urbildung neu entstehen, da sie doch nach der Meinung der Naturforscher, welche die Generatio *aequivoca* annehmen, vormals auf solche Weise entstanden waren. Denn da alle diese höheren Organismen mit eigenthümlichen Fortpflanzungsorganen versehen sind, so besitzen sie in ihnen die Mittel zum selbstthätigen Erzeugen ihres Gleichen in hinreichendem Maaße, um für die gleichmäßige Fortdauer der Art, deren Glieder sie sind, sorgen zu können. Sie brauchen daher nirgends neu zu entstehen" (Burmeister 1848, 313-314).

[4] "Auch fehlt es vielleicht an der materiellen Grundlage, woraus sich neue Geschöpfe bilden könnten, da bei weitem die meiste organische Substanz der Gegenwart sich bereits in lebendigen Organismen befindet, und kein Vorrath zur Entstehung neuer Individuen in anderer Weise als durch Zeugung vorhanden zu sein scheint" (Burmeister 1848, 314).

[5] "Wollen wir also nicht zu Wundern und Unbegreiflichkeiten unsere Zuflucht nehmen, so müssen wir die Entstehung der ersten organischen Geschöpfe auf der Erde durch die freie Zeugungskraft der Materie selbst einräumen und die Gründe, warum diese Zeugungskraft jetzt nicht mehr für höhere Organismen fortdauert, aus allgemeinen Naturgesetzen, denen zufolge nur das Nothwendige, nicht das Ueberflüssige statuirt worden ist, deduciren" (Burmeister 1848, 314).

[6] "Konnte die Natur zu einer gewissen Zeit ein Menschenpaar schaffen, so konnte sie auch mehrere erzeugen, ja sie mußte das, wenn sie die Existenz ihres Geschaffenen für immer gesichert wissen wollte" (Bur-

Burmeister was infamous for a certain arrogant stubbornness that made him stick to opinions, once he had formed these (Pooth 1966, 370). His view on autochthonous generation formed no exception to this rule, and he did not change his position in the later editions of his *Geschichte der Schöpfung*, not even when Darwinism had arrived in Argentina (Montserrat 2001; Salgado and Floria 2001) , relegating a single expression of disagreement to a brief footnote. In the last edition of his *Geschichte der Schöpfung* (1872), Burmeister merely corrected himself with respect to the issue of the spontaneous generation of entozoa, which he admitted does in fact not happen, insisting at the same time that the different conditions that had reigned during primordial times justified the assumption of the abiogenetic origin of species (Burmeister 1872, 352).

Figure 2. Heinrich Georg Bronn (Junker 1991, p. 207).

Heinrich Georg Bronn (1800-1862) (Figure 2)
Equally explicit with respect to autochthonous mega-generation as Burmeister was none other than one of the greatest palaeontologist of the period, Georg Heinrich Bronn, who from 1833 was professor of natural science at Heidelberg. He did fundamental work in the systematics of fossils, his *Lethaea geognostica* (1835-38; later edns) as well as the *Index palaeontologicus* (third part of the *Handbuch einer Geschichte der Natur*, 1841-43) being for many years essential reference works in the field. Bronn's *Untersuchungen über die Entwickelungs-Gesetze der organischen Welt während der Bildungs-Zeit unserer Erd-Oberfläche* (1857) won the prize of the Academy of Sciences of Paris in answer to a prize question, set in 1850 and repeated in 1854, about the nature of the fossil record (Bronn 1858, 1859). Bronn formulated a number of general laws of paleontological history, stressing progressive development towards increased complexity that followed a definite plan. Like Cuvier, he focused on adaptation to environment but diverged from the Cuvierian tradition by emphasizing that changes had been gradual. The course of life had been determined by a "terripetal" law, organic adaptations having kept pace with the evolution of the earth's crust, from pelagic to littoral, coastal and finally continental environments.

meister 1848, 314). "Nimmt man dagegen mehrere Autochthonen an verschiedenen Stellen der Erde an, denen alle eine gleiche typische Idee zum Grunde lag, was der spezifischen Uebereinstimmung wegen gewiß der Fall war, so stoßen wir durchaus nicht auf irgend eine Schwierigkeit bei der Erklärung der wahrnehmbaren Unterschiede" (Burmeister 1848, 552).

[7] "Wenn wir demgemäß annehmen, daß die ersten Geschöpfe nicht unmittelbar in vollendeter Gestalt entstanden, sondern vielmehr in normaler Weise als jugendliche, unvollkommene Individuen unter Processen, die dem heutigen Entwickelungsgange ähneln, sich bildeten; so haben wir damit sogleich Alles gesagt, was über ihren Ursprung füglich sich sagen läßt, und können demnach in die Einzelheiten ihres Bildungsganges nicht weiter eingehen" (Burmeister 1848, 316).

In other words, Bronn postulated a spontaneous generation force that repeatedly throughout geological history had produced the myriad of plant and animal species, in the appropriate numbers for original survival, in their different locations, and in integral connection with the changing environmental conditions of an evolving earth.

Already in his *Handbuch einer Geschichte der Natur* Bronn extensively addressed the issue of the "Schöpfung der organischen Welt" and in particular the phenomenon of "Urzeugung." If spontaneous generation of simple organisms took place at present, one could reasonably infer that under special external conditions during earlier stages of earth history also all other, higher species had spontaneously come into being. Bronn went into considerable detail discussing pros and cons of the belief in spontaneous generation today and admitted that such a belief did not appear supported by facts. Even so, in order to explain the origin of new species one had to postulate the former existence of a procreative capacity ("Zeugungs-Kraft") of abiogenesis that in more recent times had expired.[8] In other words, he saw no alternative to autochthonous mega-generation.

Bronn returned to the question of this creative force ("Schöpfungs-Kraft") in his *Untersuchungen über die Entwickelungs-Gesetze der organischen Welt*, reiterating that today no species, not even primitive and simple ones, appear to originate spontaneously, but all organic forms are produced by pre-existent living beings. The evidence against *generatio spontanea* put forward by the microbiologist Christian Gottfried Ehrenberg and the physiologist Theodor [Ambrose Hubert] Schwann appeared irrefutable. Yet although there existed a certain organic variability that can lead to the origin of races, none of the higher taxonomic entities – species, genus, let alone order or class – could have come about by a process of transmutation from one into another, as assumed by Jean Baptiste [Pierre Antoine de Monet] de Lamarck, Étienne Geoffroy St. Hilaire and Lorenz Oken.[9] Species were fixed. Historical geology showed no gradual transformation of old species into new ones, "but the new ones have everywhere come into being without help of the previous ones" ("sondern die neuen sind überall neu entstanden ohne Zuthun der vorigen" (Bronn 1858, 80)).

Neither was divine creation an option. Science explained the physical world in terms of natural laws, and it would be inconsistent not to do this in the case of the origin of species and instead have recourse to supernatural intervention. Moreover, it would be unbefitting the Deity to reduce Him to a common gardener by making Him carry out innumerable little acts of planting life on earth. Thus there had been in operation in the geological past an as yet unknown force of nature that by operating according to its own laws had spontaneously brought forth from raw matter all plant and animal species,

[8] "Läßt sich jene [Urerzeugung or *Generatio spontanea*] aber, wie es scheint, nicht erweisen, so müssen wir gleichwohl die Urerzeugung, wenn auch als eine jetzt völlig erloschene Zeugungs-Kraft der Erde zu Hülfe rufen, um die erste Entstehung der Arten zu erklären" (Bronn 1858, 30).

[9] "Was die Umbildung der Thier- und Pflanzen-Formen in andere neue und vollkommenere betrifft, so finden wir zwar, dass Varietäten einer Art eine gewisse Beständigkeit erlangen und zur 'Rasse' werden können, welche aber auch wieder in die Urform zurückzukehren im Stande ist. Aber keine Erfahrung spricht dafür, dass wirklich eine Art, eine Sippe oder sogar eine Ordnung und Klasse in eine andere übergehen könne" (Bronn 1858, 79).

down to the details of their natural existence, and always in accordance with the gradually changing environmental conditions.[10]

Unlike Burmeister, Bronn reacted with sympathetic engagement to Darwin and the *Origin of Species*, and even translated the *Origin* into German – on Darwin's repeated urging, he let it be known – thus significantly contributing to the spread of Darwinism in the German-speaking world (Junker 1991). Yet Bronn, too, did not change his mind, or at least not quite. In a critical epilogue to "this wonderful book" (Darwin 1863, 525) he took issue with a number of Darwin's stances and in particular the one on the primordial origin of life. Darwin did not postulate spontaneous generation of life from lifeless matter, not even in the case of the most simple living speck, but rather inconsistently – and disingenuously – invoked old-fashioned creationism. Towards the end of the *Origin*, Darwin wrote: "I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed" (Darwin 1859, 484), and he referred at the very end to life "having been originally breathed into a few forms or into one" (Darwin 1859, 490). If we resort to a divine act once, why should we not be allowed to do this more than once, Bronn asked?

Carl Vogt (1817-1895) (Figure 3)

The most vociferously anti-creationist and anti-evolutionist among the autochthonists was Vogt. A student of Justus von Liebig at Giessen, he himself in 1846 was appointed professor of zoology at that university. Vogt's political radicalism during and in the wake of the 1848 Revolution, which took the form of outright anarchism, forced him to flee to Switzerland where in 1852 he became professor of geology at the Geneva Academy, significantly contributing to this institution's transformation in 1873 into the city's university (Best 1998). Not just politically, also philosophically he was a radical, known together with [Friedrich Karl Christian] Ludwig Büchner and Jakob Moleschott for his uncompromising materialism (Gregory 1977; Pont 1998). He took a leading role in the so-called "Materialismusstreit," and in a famous exchange of treatises with the Göttingen

[10] "Entweder ist dieser successive Entwickelungs-Gang während Millionen Jahren eine jederzeitig unmittelbare Folge der Plan-mässigen Thätigkeit eines selbstbewussten Schöpfers gewesen, welcher dabei jedesmal nicht allein die Ordnung des Auftretens und die Bildung, Organisation und irdische Bestimmung jeder der Millionen Pflanzen- und Thier-Arten, sondern auch die Zahl der ersten Individuen, den Ort ihrer Ansiedelung, Alles im Einzelnsten erwogen, beschlossen und ausgeführt hat, obwohl es in seiner Macht gelegen hätte, Alles auf einmal zu schaffen; - oder es bestund irgend eine uns bis heute durchaus unbekannt gebliebene Natur-Kraft, die vermöge ihrer eigene Gesetze Pflanzen- und Thier-Arten bildete und alle jene zahllosen Einzelverhältnisse ordnete und schlichtete, welche Kraft aber in diesem Falle in unmittelbarstem Zusammenhange mit und in vollkommener Abhängigkeit von denjenigen Kräften stehen musste, welche die allmählich fortschreitende Ausbildung der Erdrinde und die allmähliche Entwickelung der äusseren Lebensbedingungen für immer zahlreichere und immer höhere Organismen in Folge dieser Ausbildung bewirkt habe" (Bronn 1858, 81).

"Wir glauben daher, dass alle Pflanzen- und Thier-Arten durch eine uns unbekannte Natur-Kraft ursprünglich geschaffen, nicht aber durch Umbildung aus einigen wenigen Urformen entstanden sind, und dass jene Kraft mit den die Oberfläche ausbildenden Kräften und Ereignissen im innigsten und nothwendigsten Zusammenhange stund" (Bronn 1858, 82).

Figure 3. Carl Vogt (Vogt 1859, frontispiece).

professor of anatomy Rudolph Wagner ridiculed the belief in the existence of an immaterial human soul (Rupke 1994, 307-308). Vogt's contributions to the earth and life sciences ranged widely, and included influential popular books, textbooks, as well as specialised studies in marine biology.

During his younger years Vogt regarded "revolutions" as the decisive moments in the development of the history of life and of society. Repeatedly in the course of geological time all life had been exterminated by catastrophes, but out of the ruins new "creations" had emerged, constituting a sequence of increasing perfection. This sequence could be compared to that through which an embryo goes in its development towards birth. Yet Vogt's comparison of the succession of life on earth with ontogeny did not imply that he accepted an evolutionary phylogeny. On the contrary; he aggressively rejected the notion of species transmutation, just as he strongly denounced the belief in miraculous creation. The revolution-restitution pattern of geological history, and thus also the repeated re-emergence of life, were an expression of material laws and of innate properties of matter (Bröker 1973, 51-55). Thus to Vogt abiogenesis was not heterogenesis, in the sense that life and its manifestations were nothing more than configurations of matter and its natural laws.

Ironically, Vogt's early anti-evolutionary utterances took the form of footnotes added to his translation into German of the anonymous *Vestiges of the Natural History of Creation* (1844), the pro-evolutionary treatise by the Edinburgh publisher Robert Chambers. Vogt rejected what to many people was the essence of the book, namely the notion of the spontaneous origin of life from lifeless matter and the evolutionary origin of species by natural laws that had been divinely enacted. It is not clear why Vogt should have wished to spend his time during the time-consuming turmoil of the revolutionary years translating a book the central thesis of which he disagreed with, but he simply may have approved of the anti-establishmentarian furore it had created (Gregory 1970, 70-71; Rupke 2000, 220). In any case, Vogt's translation, which came out under the title *Natürliche Geschichte der Schöpfung des Weltalls, der Erde und der auf ihr befindlichen Organismen, begründet auf die durch die Wissenschaft errungenen Thatsachen* (1851; 2nd edn 1858), shows him to have been a decided opponent of spontaneous generation and of species transformation.

Vogt denounced evolution as a "view that had come from nature philosophy" ("aus der Naturphilosophie hervorgegangene Ansicht" (Vogt 1851a, 98)). This theory of the gradual transmutation of successive creations contrasted with his own "theory of revolutions that let continually new faunas appear on earth" ("Revolutionstheorie, die stets neue Faunen auf der Erde auftreten läßt" (Vogt 1851a, 124)). Geological revolutions had repeatedly destroyed all life, and new organic worlds had followed in the wake of the destructions, not as a result of old forms being transformed into new, nor because of a transcendental Creator, who would be a ridiculous figure to try twenty-five times or more to get things right or just as ridiculous to enact laws to do the work for Him while

He Himself had gone into early retirement. Matter itself and the laws of nature could explain all.[11] Vogt's pronouncements on the geological record were somewhat contradictory. In his *Lehrbuch der Geologie und Petrefactenkunde* (1854) he reiterated that the development of life on earth took place by fits and starts, in the sense that from time to time a revolution called into life new faunas and floras on the surface of the earth; yet revolutions had not always led to the extinction of all species, and the formational and paleontological boundaries were not absolute and impermeable (Vogt 1854, 338-389).

Vogt's hero of factual, observational science, the man who formed a polar opposite to the deductive and dogmatic nature philosophers, was Georges Cuvier, and closely bracketed with him were Johann Friedrich Meckel, Karl Asmund Rudolphi and Friedrich Tiedemann. Against the nonsense of the nature philosophers who believed that the animal kingdom formed a single chain of being based on one and the same structural blueprint, they had demonstrated on the basis of a study of comparative anatomy of living animals, of their embryonal development, and of their occurrence throughout the geological record, that there existed several irreducible fundamental types (Vogt 1851b, 14-19). Vogt concurred with Cuvier and Karl Ernst von Baer that the animal kingdom is not characterized by a unity of type, which Geoffroy St. Hilaire had argued for, but is divided into several different, irreducible types or "embranchements" (Vogt 1851a, 148). Moreover, Lamarck's theory of species transformation through an inner urge was nonsense, because no animal had cravings that go beyond its structure in the first place (Vogt 1851a, 170). This was just as much nonsense as the creationist view (Vogt 1851a, 229).[12]

Part of the theory of transmutation as envisaged by Lamarck and by the anonymous author of *Vestiges* was the spontaneous generation of primitive life from lifeless matter, and Vogt strongly objected to it: experience taught us that infusoria came from germs ("Keime") carried by the air (Vogt 1851a, 135); with respect to entozoa, the evidence pointed to propagation by means of their own eggs (Vogt 1851, 136-137). Already in his *Physiologische Briefe* (Vogt 1847, 300-318) and also in his *Zoologische Briefe* (Vogt 1851b, 52-54) Vogt emphatically denied the reality of spontaneous generation. Neither infusoria nor parasitic entozoa originated that way (Vogt 1847, 313).[13]

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[11] "Wir glauben auch, daß keine Species aus einer Formation in die andere übergegangen sei, sondern daß mit jeder geologischen Revolution auch eine völlige Vernichtung der Organismen und eine Erneuerung derselben verbunden gewesen sei; aber deshalb nehmen wir noch gar nicht einen Schöpfer an, weder im Anfange, noch im Verlaufe der Erdgeschichte, und finden, daß ein selbstbewußtes, außer der Welt stehendes Wesen, welches dieselbe erschafft, ebenso lächerlich erscheint, wenn es fünf und zwanzig Mal oder noch öfter die Erde mit ihren Organismen ändert, bis es endlich das Rechte trifft, als wenn es, nach Erschaffung der Welt und nach der Gebung der Naturgesetze, sich pensioniert und in Ruhe setzt, wie unser Verfasser es will. Die Materie (die Welt) ist für uns so wenig erschaffen, als die Naturgesetze gegeben – beide sind nothwendige, gegenseitig bedingte Dinge, die keinen Dritten zum Urheber haben" (Vogt 1851a, 124).

[12] "Die eine Theorie, wonach der Schöpfer eine Menge von Thieren nur zu dem Zwecke schafft, um andere Geschöpfe nutzloser Weise zu quälen, ist wahrlich ebenso abstoßend, wie die des Verf., wonach die Thiere sich freiwillg, absonderlichen Gelüsten folgend, ihre Lebensform wählen und dann in Folge der getroffenen Wahl ihre Organisation modificiren sollen" (Vogt 1851a, 185).

[13] "Wir verwerfen also gänzlich und unbedingt die sogenannte Urzeugung als ein Hirngespinnst, oder vielmehr als einen theoretischen Deckmantel für unsere factische Unwissenheit" (Vogt 1851b, 54).

Yet in the past, abiogenesis must have been active, although not in the way that the nature philosophers had imagined. The organic "Urschleim" of the *Naturphilosophen* was a chemical impossibility; an organic substratum of global extent and of long duration from which life had emerged made no sense, because organic matter decomposes when left to itself. Rather, in the wake of geological revolutions, life had re-emerged rapidly during a short period of time and in restricted areas, and new organic forms had originated from the remnants of the extinguished life.[14] Given this autochthonous origin of species, it seemed likely that also today some form of spontaneous generation continued to take place. Changing his mind on the issue, Vogt in an 1852 essay on "Die Erzeugung der Jungen" (Vogt 1852, 91-312) not only reaffirmed his view that species had originated by a coming together of elemental matter[15] but took issue with Ehrenberg and Schwann in arguing for the spontaneous generation of infusoria and parasitic entozoa. He even gave credence to stories about "electrical mites" – mites that had been generated by electrical discharges. Life could conceivably be generated in the laboratory, chemistry being able to produce organic substances such as urea. The organic form and, with that, organic life itself, was still outside the reach of organic chemistry, but in nature, under certain favourable circumstances, simple organisms could well be originating from dead matter, especially because, in the case of infusoria, this matter existed in the form of organic fibers, proteins and chlorophyll. In a twin essay on "Untergegangene Schöpfungen" (Vogt 1852, 313-418; republished in Vogt 1859) Vogt once more attacked the transformism of the nature philosophers and the creationism of Agassiz. The origin of species – he reiterated – was a function of the properties of matter and had taken place in the wake of

[14] "Perioden dieser Art [when life is exterminated by a geological revolution] können im Gegentheile niemals in allgemeiner Ausdehnung, sondern nur in beschränkter räumlicher Verbreitung existirt und müssen auch dann eine ausnehmend kurze Dauer gehabt haben, welche gestattete, die aus der vorigen Schöpfung herstammende Materie zu neuen Lebensformen umzugestalten" (Vogt 1851a, 339).

[15] Von philosophischem Standpunkte aus kann es nicht geläugnet werden, daß die Möglichkeit einer solchen Urzeugung allerdings nicht nur gegeben ist, sondern daß auch, *mit denselben Elementen*, wie diejenigen, aus denen jetzt unsere Erde mit ihren Bewohners besteht, diese Urzeugung schon öfters, zu wiederholten Malen stattgefunden haben muß; die Frage ist freilich müßig, was zuerst bestanden habe, die Eichel oder der Eichbaum – aber daß es eine lange Epoche unserer Erde gab, wo weder Eicheln noch Eichbäume existirten, das können wir mit eben so viel Sicherheit nachweisen, als man überhaupt eine wissenschaftliche Wahrheit feststellen kann. Es muß also eine Epoche gegeben haben, wo die Eiche entstand, um mich eines gewöhnlichen, wenn auch falschen Ausdruckes zu bedienen, wo sie geschaffen wurde, das heißt, wo diejenigen chemischen Elemente, welche das Eichenholz, seine Rinde, Blätter und Wurzeln bilden, in derjenigen organischen Form zusammentraten, in welcher wir sie als Eiche erkennen. Die gleiche Schlußfolgerung gilt für die Thiere. Die ganze Schöpfung, welche uns jetzt umgibt, hat in einer früheren Epoche der Erdgeschichte nicht existirt – sie muß einmal ins Leben getreten sein – es muß ein Zeitpunkt vorhanden gewesen sein, wo die Elemente, welche die Thierkörper bilden, in dieser Form zusammentraten.

Wir wissen, so weit wir mit unbewaffnetem Auge blicken können, daß diese Eigenschaft der Erde, neue Organismen entstehen zu lassen, für jetzt schlummert, daß die höheren Thiere nur durch Aelternzeugung sich fortpflanzen. Aber ob dies Gesetz gleichmäßig für alle Organismen ohne Ausnahme gilt, das ist eine andere Frage, ob es namentlich für diese niedersten Wesen, deren Form eine sehr einfache ist, ebenso entschieden und kategorisch gilt, das ist eine Frage, über welche *a priori* durchaus nicht abgesprochen werden kann. Die einzige Schwierigkeit liegt in der Hervorbringung der organischen Form, nicht in der Erzeugung der zusammensetzenden Elemente, die alle in denjenigen Flüssigkeiten, in welchen diese Organismen sich finden, in zureichender Menge und Mischung vorhanden sind" (Vogt 1852, 104-105).

geological revolutions. Species had originated autochthonously and their patterns of distribution across the globe reflected favourable conditions for their spontaneous origin and were not the result of migrations.[16] In the case of today's fauna and flora as well as in that of all the previous extinct ones, species must have originated *in situ*, in the regions where they now live, in approximately the same numbers of individuals as they at present have, and all at the same time, because the economy of their existence depended on the totality of their interactions.[17] "Those species which consist of many individuals, also originated in large numbers" ("Diejenigen Arten, welche zahlreich an Individuen sind, entstanden auch in zahlreicher Menge" (Vogt 1854, 387)). The belief that species came from a single pair in a single location was a theological notion and derived from the story of Noah's Ark. This made no scientific sense, because survival of species was only possible if they existed as communities of, in many instances, large numbers of individuals.

Like Bronn, Vogt took an active part in the Darwinian debates, and not long after the appearance of the *Origin of Species* he became a fervent supporter of Darwin. At the same time that Thomas Henry Huxley published his *Evidence as to Man's Place in Nature* (1863), Vogt came out with *Vorlesungen über den Menschen, seine Stellung in der Schöpfung und in der Geschichte der Erde* (1863), which was translated into English and French. In the last of these lectures, Vogt admitted that in the wake of Darwin's book he had changed his views, dropping his opposition to the theory of evolution. His opposition to the creationist view, which postulated a God who from time to time refurbished the earth with new furniture after having destroyed the old, had remained. Before the *Origin of Species* – he rather disingenuously asserted – he had not known what to put in the place of creationism (Vogt 1863, vol. 2, 259). With Darwin's theory, this now had changed.

Early Autochthonists

Vogt's style, I repeat, was aggressively adversarial and his utterances about the origin of species for the most part negative, attacking at length the transformism of Lamarck and the creationism of Agassiz. When it came to describing his own views about the origin of species, his writings were, excepting the 1852 essay "Die Erzeugung der Jungen," notably

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16 "So stellt es sich denn klar heraus, daß für unsere jetzige Schöpfung mehrere wichtige Gesetze existiren, welche schon beim Beginn derselben obgewaltet haben. *Die Arten sind Autochthonen* – d.h. mit geringen Ausnahmen, welche sich meist historisch nachweisen lassen und nur einzelne wenige Species betreffen, sind alle Arten an denjenigen Orten entstanden, welche ihnen noch jetzt als Wohnsitze angewiesen sind. *Die Verbreitungsbezirke sind nicht Resultate von Wanderungen*, sondern von Entstehungen zur Stelle und zwar ist es oft geschehen, dass dieselbe Art an verschiedenen Orten zugleich erschien, wo eben die Verhältnisse ihrer Existenz günstig waren" (Vogt 1859, 370).

17 "*Die Arten*, welche unsere Schöpfung zusammensetzen, *müssen endlich in ähnlichen Verhältnissen der Zahl*, in welchen sie sich noch jetzt vorfinden, *und zwar zu gleichen Zeiten entstanden sein*, da die ganze organische Oekonomie der Erdoberfläche auf dieser gleichzeitigen Existenz beruht und diese Verhältnisse nur innerhalb sehr geringer Gränzen, nicht aber in ihrer Gesammtheit, geändert werden können, indem solche Aenderungen den Untergang der ganzen Schöpfung herbeiziehen würden.

Die gleichen Gesetze werden wohl für die vorhergehenden Schöpfungen gelten müssen" (Vogt 1859, 371).

more parsimonious. And he had good reason to be, because his theory of autochthonous generation itself had theological as well as nature philosophical antecedents.

Among the earliest writers to advance the theory of autochthonous generation were two relatively minor figures, namely August Michael Tauscher, an independent scholar ("Privatgelehrter") whose claim to legitimacy as a naturalist was his membership of the Imperial Society of Sciences in Moscow, and Johann Georg Justus Ballenstedt, a scientifically engaged Lutheran pastor. In 1818, unbeknown to one another, they each published a treatise promoting the notion that species were not created once by divine interference but continuously by natural processes that, however, operated according to divinely enacted laws (Ballenstedt 1818; Tauscher 1818). Ballenstedt discussed the two books in his short-lived periodical *Archiv für die neuesten Entdeckungen aus der Urwelt*, expressing the joint view that the origin of new species, also of higher animals, was a process that had continued up to the present day (Ballenstedt 1819). The process of nature's creation was continuous, not restricted to the six days of Genesis 1.[18] *Ornithorhynchus*, the duck-billed platypus, for example, was a form of life that most likely had come recently into being rather than that it had recently been discovered. Continuous generation took several forms, namely bastardisation, domestication, spontaneous generation of infusoria and parasitic entozoa, and the periodic origin *de novo* of new species in the wake of revolutionary changes on the surface of the earth. Humans as well as the higher animals had originated as products of a major "Erdrevolution." [19] Today's species lived on top of the remnants of older, extinct but similar forms, and this continuity of form could be due to the existence of a fixed number of lasting models ("Muster") or organic archetypes ("organischer Urformen"), according to which actual organisms are structured.

On one point the two authors diverged. Whereas Tauscher saw a close similarity between his view and the Mosaic creation story, Ballenstedt did less so and in particular took issue with Tauscher's belief in the origin of humans from a single pair. More likely – Ballenstedt argued – humans had originated in various independent forms across the world, which explained the existence of different races. In a later work *Die Vorwelt und die Mitwelt* (1824), he reiterated that there were no grounds for assuming the unity of the human race, whether in the sense of Adam and Eve in Paradise or of Noah and his family after the Deluge. Humans may well form a single species, but they were of multiple, autochthonous origins – a polygenetic species.[20] Moreover, these origins had not been as single pairs, because carnivores would have readily exterminated them. Nor had they involved thousands of individuals, because there would not have been sufficient food for so many. Both authors were concerned, however, to make sense of the biblical creation

[18] Thus "continuous creation," as used by Ballenstedt, meant that the origin of species had not been a one-off event but was a continuing sequence of spontaneous generations. As such, the expression had a different meaning from the one given to it by later process theologians (see above).

[19] "Die Erzeugung von Menschen und Thieren größerer Art ist nur das Werk einer allgemeinen Erdrevolution, dergleichen bei dem Untergange der Urwelt statt fand" (Ballenstedt 1819, 267).

[20] "Denn wenn auch alle Menschen nur zu Einer Species oder Art gehören, wie man behauptet, so folgt daraus doch nicht, daß sie alle von einem und demselben Paare und aus einem Lande herstammen. Konnte denn nicht die Natur auf mehrern Punkten Menschen von einerley Art entstehen lassen? Mußte es also nicht gleich anfangs mehrere Stämme und Raçen von Menschen geben?" (Ballenstedt 1824, xi).

story in the context of the new Cuvierian history of the earth, and Tauscher's booklet in particular showed that autochthony was in some ways merely the Genesis account of creation minus the miracle of supernatural intervention.

The first origin of species, i.e., without "parents," had involved the mixing of materials from all elements ("durch eine Mischung von Stoffen aus allen Elementen" (Ballenstedt 1818, 86)). The reason that continuous generation of successively higher floras and faunas had taken place was that the survival of the higher communities required the pre-existence of the lower. Trees, for example, need soil, but this had to accumulate over a period of time through the action of less developed plants, insects etc.. The process of continuous creation had levelled off and today few if any major new species originated.[21] The procreative force set free by the last geological catastrophe had lost its vigour and become largely dormant, yet some of it remained. By continuing to produce certain organisms *de novo* up to the present day, nature was giving us an indication, as it were, of how originally the majority of today's species had come into being.

Ballenstedt became carried away with the idea of continuous generation, maintaining in a later publication that organisms were spontaneously generated not just on land and in the sea, but also in the air, like hailstones and – he believed – like meteorites. Organic bodies grow from, and later decompose into, elementary substances. Why should nature, through a fresh mixture of these substances, not be capable of continuously producing new forms? Given the fact that the "ocean of air" contains all the elements that make up organic bodies, should the "Bildungstrieb," "die bildende Kraft der Natur," not be capable of producing new germs, new seeds, new plants and new animals when the circumstances happened to be right? Our planet itself was a living organism, not a clump of dead matter, and the atmosphere in particular contained mechanical, organic as well as immaterial, electrical forces, capable of producing specific life forms. Products of the air that were known to have come down in showers included inorganic objects such as meteorites but also organic products such as caterpillars, butterflies, locusts, and even fish, frogs, snakes, and tortoises (Ballenstedt 1824, 102).

This verged on nature philosophical fantasy, although Ballenstedt has been counted among the representatives of the German Enlightenment rather than of *Naturphilosophie*. All the same, autochthonous generation had advocates among the latter, too, instances of which were Friedrich Siegmund Voigt, Ferdinand August Maria Franz von Ritgen, and Lorenz Oken himself. Voigt was professor of medicine at Jena and director of the

[21] "Freylich ist jetzt die bildende Kraft der Natur nicht mehr so thätig und wirksam, als anfangs. Wir sehen jetzt keine beträchtliche neue Arten von Pflanzen und Thieren mehr in die Reihe der Wesen eintreten und sie vermehren. Aber man bedenke, wie sehr sich die Umstände geändert haben! Unmögliche Dinge muß man von der Natur nicht verlangen. Als die große Catastrophe eintrat, welche der jetzigen Schöpfung ihr Daseyn und Entstehen gab, waren die Umstände dazu viel vortheilhafter. Es ging ein neuer Zeitraum an; es galt einer neuen Umwandlung und Verneuung unsers Erdballs. Es entstand ein neuer Boden und eine neue Oberfläche der Erde. Neue, oder jetzt ruhende Kräfte der Natur wurden damals in Thätigkeit und Bewegung gesetzt. Jetzt nun ist aber alles in Ordnung, hat sich alles gleichsam gesetzt und entwickelt, und geht seinen ordentlichen Gang fort. Es darf aber nur einmal eine neue allgemeine Revolution und Umkehr auf der Erde entstehen und eine neue große Catastrophe anfangen; so wird man eine neue Schöpfung anheben sehen, deren Produkte uns ganz fremd und ein Räthsel sein werden" (Ballenstedt 1818, 90).

botanical garden, who acquired a reputation for his work in the field of plant systematics and for his *Lehrbuch der Botanik* (1827). A contributor with Bronn and other leading scientists to various encyclopedic manuals of natural history, Voigt produced among his first publications the *Grundzüge einer Naturgeschichte* (1817) that marked him as a representative of the nature philosophical movement in botany and zoology. He nevertheless set himself apart from the Lamarckians, arguing that variability in plants and animals went no further than the diversification of genera into species, and that the generic forms had originated by *generatio originaria*, like today many lower forms of life still did (Voigt 1817, 418-433, 493-494; see also 1823, 812-817). Applying the nature philosophical reasoning-by-analogy to the geographical distribution of plants, he addressed the question of the "inner causes" of this distribution. The various vegetations across the globe could not be explained by a process of dispersion of seeds. Nor should we assume that there had been a divine gardener who had sown seeds from above. The many tens of thousands of known plant species had originated autochthonously, but not from the few and simple soils that cover the surface of the earth. There was a deeper cause, namely the life force of the earth itself, which, striving outwards in manifold radial directions, had produced the vegetation and its patterns. The earth should be thought of as the body of an animal, as it had a soul, a living unifying principle that kept its mass together. Every animal produces across the surface of its body a pattern of feathers (in birds) or hair (in mammals). This pattern was produced from within and the cause of distribution of plants across the globe should be thought of in the same way.[22]

Speculations of a similar kind were put forward about the origin of humans, among others by Ritgen, who was professor of surgery and obstetrics at Giessen and later professor of psychiatry there. He adhered to a world view of a nature philosophical holism in the tradition of Carl Gustav Carus, Lorenz Oken and Johann Bernhard Wilbrand (Bühne 1992, 183-186). Ritgen postulated that the totality of all individualities had simultaneously been created as potentialities, and that their appearance as realities in the course of time had happened at moments when this corresponded to their own purpose and the purposes of all selves. The human form and all human individuals, past, present and future, existed from the beginning of the world as incorporeal germs. The first entrance of human individuals into the corporeal world, "without a mother," had probably taken place in the form of eggs formed in mud and along the shore of a large body of water. These eggs had combined animal and plant elements, the plant-like features having consisted of the eggs' leathery covering opening out like the outer layer of a *Rafflesia*. This plant connection helped us visualize the growth of primordial human eggs as a process of fungiform gemmation.[23] The raw materials would have come from the air,

[22] "Wenn wir nun durchaus keinen Beweis auffinden können, dass die Vegetation, die unsern Erdball bedeckt, durch irgend einen Gärtner erst in den Erdboden hineingepflanzt und gesäet worden sey, was bleibt dann der Vernunft anders übrig, als den ersten Grund ihrer Entstehung von innen heraus anzunehmen?" (Voigt 1838, 623).

[23] "Richtiger dürfte es daher sein, ein im Uferschlamm sich entwickelndes Menschenei anzunehmen und so die ersten Menschen aus Eiern entstehen zu lassen. Denkt man um ein solches Menschenei nur einige dicke lederartige Hüllen gelegt, welche wie die Aussendecken der *Rafflesia* sich entfalten: so schmilzt das Pflanzliche und Thierische ziemlich gut zusammen. Man wird auf diese Weise eine Pilzknospe und ein Menschenei

from the earth and also from remnants of dead animals and plants. The occurrence of different human races made it likely that *Homo sapiens*, like animal species, had originated in different parts of the world. Ritgen cited many examples of organisms that, he believed, today still originate without "procreating parents," e.g., various fungi, infusoria, entozoa, mites, lice, molluscs, worms, insects, fish, various plants – lower and higher – among the latter, given certain~~s~~ conditions, poplars (Ritgen 1832, 47). No new human races were forming *de novo* anymore, however, because with humanity nature had reached its highest form and ever since the arrival of humans the process of abiogenesis had been retrogressive.[24]

Ritgen's "Anthropogenie" was reminiscent of what Oken had written on the subject. Because of his *Lehrbuch der Naturphilosophie* (1809-1811; 2nd edn 1831) Oken has often been counted among the early advocates of evolutionary transformism and was bracketed by Bronn and Vogt with Lamarck and Geoffroy St. Hilaire. Yet in a paper on the origin of the first humans, he rejected the notion of a gradual transformation of animals into humans, denigrating it as "childish and without thought" ("kindisch und gedankenlos") and pitiable (Oken 1819, 1122). The first humans had originated in a uterus that was the ocean, and at a time when the water temperature had been as high as that of a mother's womb. From the kind of slime from which still today infusoria and jellyfish originated, membrane-encapsulated embryos of humans had formed that extracted nourishment and oxygen from the water. Children may be able to survive on their own, without maternal support, from the age of two, when they have grown teeth and are capable of eating worms, shellfish, snails, or also fruit etc.. The floating embryos had therefore been "ripe" when the enclosed child was like a two-year-old, and Oken published a sketch of what such an embryo might have looked like (Figure 4). These had originated by the thousands, some thrown ashore prematurely and left to die, others crushed against rocks, yet others

Figure 4. Spontaneously generated human foetus, at the developmental stage of a two-year old child (Oken 1819, plate 13, fig. 5).

für weniger fremdartig halten und das Hervorwachsen des letztern wie der erstern aus der Erde nicht als ganz ungereimt abweisen" (Ritgen 1832, 47).

24 "In Ansehung der Thiere zweifelt Niemand, daß verschiedene Welttheile häufig dieselbe Thierart unter einiger vom Welttheil abhängiger Nüanzirung in ihrem Schooße unmittelbar haben entstehen lassen, wie z. der Tapir, das Nashorn, der Bär, Tieger u.s.w. Nach dieser Analogie müßte daher der Neger in Afrika, die Rothhaut in Amerika u.s.w. entstanden sein. Es läßt sich übrigens behaupten, mit der Erscheinung der Menschenform sei das Höchste erreicht worden, was auf unserm Planeten zu erreichen war und daher seie von da an ein Rückschreiten im ursprünglichen Schaffen erfolgt, weshalb wohl noch Thiere und Pflanzen, aber keine Menschen mehr neu aufgetreten seien" (Ritgen 1832, 50).

eaten by predatory fish. Enough would have been beached on soft sand at the right age, however, and the children broken out of their membranous enclosure to survive by starting to eat simple foods (Oken 1819, 1122).

Opposition

The autochthonists developed their views in the context of a double-pronged attack on creationists and evolutionary transmutationists. Conversely, representatives of the two oppugned groups criticised the autochthonous theory. Let us briefly look at the critiques by one representative of each group, the creationist Johann Andreas Wagner, professor of zoology at Munich, and the evolutionist Franz Unger, professor of plant anatomy and physiology at the University of Vienna. In his *Geschichte der Urwelt* (1845), Wagner extensively argued against the "Lehre von den Autochthonen" (Wagner 1845, 408-420). The theory required spontaneous generation, which is a chimera, because nothing organic comes from something inorganic and, moreover, spontaneous generation was an hypothesis that increasingly was losing ground. Also, nothing was to be gained by investing raw nature with creative power.[25]

Wagner then discussed in detail the views of the autochthonous origin of humans as put forward by various authors, including Oken and Ritgen, and in particular focused on the thesis by Burmeister that the geographically dispersed nature of human races supported an autochthonous origin. Multiple autochthonous origins had of course anti-creationist implications, as Vogt was to make clear. To Wagner, like any orthodox Christian scientist, humans came from a single pair, and his *Geschichte der Urwelt* closely followed the Mosaic account of creation, quoting Genesis 1 as a source of scientific information (Wagner 1845, 503).

Unger, too, objected to the theory of the autochthonous generation of species but from an evolutionist's point of view. In his *Versuch einer Geschichte der Pflanzenwelt* (1852b) he took it for granted that plants and animals had at some stage in the past resulted from "mutterlose Zeugung," but the new forms had been primitive ones, and the many later, higher organisms had come from already existing species. The autochthonous view was problematic, because in the course of geological history the number of plant species had increased, the current flora being the richest. Thus the number of generative events would have had to increase, too, and the spontaneous generation of new plant species should be occurring in front of our very eyes. Yet this is not happening – even spontaneous generation of the primitive, simple forms of life did not occur today. Unger therefore believed that the origin of species was not attributable to some external, general force of nature but to the world of plants itself. Autochthonous generation would have produced an irregular aggregate of species throughout earth history; but we encounter a

[25] "Das Räthsel von der Entstehung der organischen Welt wird daher ebenfalls nicht gelöst, wenn den Naturgewalten schöpferische Thätigkeit beigelegt wird; ja es wird noch weniger begreiflich, wenn sie statt auf einen allweisen göttlichen Willen auf eine blinde Naturnothwendigkeit zurückgeführt wird" (Wagner 1845, 409).

regular pattern, which can only have resulted from later species deriving from earlier ones (Unger 1852b, 340-345; see also Unger 1852a).[26]

Autochthony Sympathisers

There were of course creationists other than Wagner, such as Agassiz, and evolutionists other than Unger, for example Hermann von Schaaffhausen; yet the critics of autochthony were in the minority, and the three autochthonists and their predecessors, discussed above, had many sympathisers, not only in the German-speaking world but also elsewhere. In substantiation of this, let us have a few names pass the revue. During the heyday of *Naturphilosophie* Karl von Raumer, professor of natural history and mineralogy at Erlangen, interpreted plant fossils as embryos that had not fully developed and been stillborn, or rather unborn, inside the womb of mother earth (Raumer 1819, 166).

In moving away from nature philosophical obfuscations, Johannes Peter Müller, professor of anatomy and physiology at the University of Berlin and a founder of modern biomedical science, came close to explicit autochthony. In his classic *Handbuch der Physiologie des Menschen für Vorlesungen* (1st edn 1834-40) Müller denied present-day abiogenesis, yet at the same time defended a strict constancy of species, which entities – he asserted – had come into being as the result of a natural procreative force ("Schöpfungskraft") (Müller 1844, vol. 1, 24-25),[27] not by way of transmutation of one into another (Müller 1844, vol. 2, 769).[28] From the moment of their origin, species had been independent and unchangeable. Müller was an influential teacher, counting among his pupils Rudolf Carl Virchow, whose views by the middle 1850s bore the distinctive marks of the autochthonous generation theory (Virchow 1856, 24).

One of the greatest scientific trend setters of the period, Alexander von Humboldt, in the first volume of *Kosmos* (1845) stuck out his neck from a customary carapace of

[26] "Was bei der Ansicht, die die Pflanzenarten unabhängig von einander entstehen lässt, nothwendig vorausgesetzt werden muss, ist ihre primitive Erzeugung, welche also nicht sowohl der Urpflanze an und für sich, sondern, – da jene Art eine ursprüngliche ist, – daher auch jeder Urart zukommen müsste. Es folgt daraus, dass jener Schöpfungsact der Pflanzenwelt sich so oft wiederholte, als es Pflanzenarten gibt, und da dieselben nicht auf einmal, sondern nach und nach und zwar in immer wachsender Anzahl erschienen, dieser Schöpfungsakt statt seltener zu werden, sich mit jeder folgenden Weltperiode im umgekehrten Verhältnisse vermehrte. Nach dieser Ansicht würde demnach unsere gegenwärtige Periode am reichsten in den Pflanzenschöpfungen sein, es wäre auch nicht abzusehen, warum sie nicht fort und fort vor unseren Augen vor sich gingen und wir daher nicht auf direkte Weise von diesem Produciren neuer Pflanzenarten uns überzeugen sollten" (Unger 1852b, 342).

[27] "… jede Art ist an gewisse physische Bedingungen ihrer Existenz auf der Erde, an eine gewisse Temperatur und bestimmte physisch-geographische Verhältnisse gebunden, für welche sie gleichsam erschaffen. In dieser unendlichen Mannichfaltigkeit der Geschöpfe, in dieser Gesetzmässigkeit der natürlichen Klassen, Familien, Gattungen und Arten, äussert sich eine das Leben auf der ganzen Erde bedingende gemeinsame Schöpfungskraft. Aber alle diese Arten des Organismus, alle diese Thiere, die gleichsam eben so viele Arten, die umgebende Welt mit Empfindung und Reaction zu geniessen, sind, sind von dem Zeitpunkt ihrer Schöpfung selbstständig; …" (Müller 1844, vol. 1, 23-24).

[28] "Die Arten der Thiere bieten keine entfernte Möglichkeit einer Erzeugung der einen aus der andern dar. Diese müssen vielmehr nach Allem, was jetzt in der Geschichte der thierischen Welt vor sich geht, einzeln und unabhängig von einander geschaffen sein" (Müller 1840, vol. 2, 769).

cautious agnosticism – just barely – by sympathetically quoting St. Augustine who had raised the possibility of an autochthonous origin of plants and animals following the flood, to explain the occurrence of life on remote islands (Humboldt 1845, 345-346). Humboldt's first German biographer, Hermann Klencke, also toyed with the "third way" in thinking about the origin of species. He interpreted the creation story of Genesis 1 as an allegory and derided the notion of evolutionary transmutation as laughable nonsense. Every species, every type represented an independent and separate idea in nature, and Klencke conceived of the origin of species and of humans in particular as a process of germination of "Keime" under particular conditions of humidity and temperature (Klencke 1850, 38-39). Humans had not originated as a single pair in one location but autochthonously on the tablelands of Asia, Africa, America and Europe (Klencke 1854, 192-194).

Further afield, Alphonse de Candolle, too, in his *Géographie botanique raisonnée* (1855), while prevaricating, toyed with the notion of autochthony. How were the many plant forms that have succeeded each other in the course of earth history connected – by means of a material bond ("liaison matérielle") or did they constitute new creations that were independent of preceding ones ("des créations de formes nouvelles indépendentes des précédents") (Candolle 1855, xiii))? New creations had been produced either by an unknown physical law working on inorganic matter, or by an external power creating something out of nothing or possibly out of inorganic matter. In either case, the moment of creation had features we can not see, touch or even comprehend, and lay outside our range of observation. The origin of species was therefore "extra-natural." Also the notion that species evolved, one from another, still had to resolve the problem of the origin of the earliest form of life from lifeless matter. Moreover, it multiplied the number of extra-natural moments innumerable times by supposing species transmutations, which, however, we never saw (Candolle 1855, 1107).

To plant geographers, the attractiveness of autochthonous generation existed in the fact that it helped explain the existence of "floral provinces." Distribution areas of plants could be thought of as produced by "Schöpfungscentren" or "Schöpfungsherde," locations where particular plant species had come into being. This line of thought was followed by the Humboldtian botanist and Göttingen University professor August [Heinrich Rudolf] Grisebach. While expressing partial agreement with Darwin, Grisebach continued to argue in 1865 and 1866 essays, i.e., after the appearance of the *Origin of Species*, that plant species had come into existence autochthonously, at various locations across the globe, and that this mode of origin was of crucial significance in explaining geobotanical patterns (Grisebach 1872, vol. 1, 1-9; 1880, 274; 1880, 307-334).

A systematic re-examination of the primary literature is likely to add more names to this list, also for the French-speaking world. In Britain, among the leading scientists who appeared inclined towards the theory of autochthonous generation was Charles Lyell. In fact, Lyell was cited in support of autochthony by both Bronn (1858, 78) and Vogt (1859, 366-368). In the second volume of his *Principles of Geology* (1832) Lyell had famously criticised Lamarck's theory of transmutation, upheld the constancy of species, and speculated that, on the basis of a gradual, uniformitarian rate of extinction and origin of species, in a region the size of Europe, only once every 8000 years or more a mammal would disappear and emerge, making it a difficult process to authenticate (Lyell 1830-33, vol. 2, 182-183). Lyell did not specifically state that he believed in a natural origin of species but later admitted that he left this to be inferred (Lyell 1881, vol. 1, 467). Bronn, for one, did infer just that and attributed to Lyell the view that species past, present and future had been/were being/would be originated by primordial generation ("Urerzeugung") (Bronn 1858, 78), in a slow and imperceptible process of change of the world's fauna (Bronn 1843, 40-41).

Both Joseph Dalton Hooker and Thomas Henry Huxley, before they joined the Darwinian cause in the wake of the appearance of the *Origin of Species*, expressed doubts about species variability. Huxley, in his notoriously scathing review of *Vestiges* strenuously opposed "transmutation" (Huxley 1854) as well as divine guidance and intervention. In a review of Candolle's *Géographie botanique raisonnée* Hooker expressed agreement with Huxley (Hooker 1856, 252) but at the same time prevaricated on whether multiple creations or creation by transmutation had taken place (Hooker 1856, 151-157, 248-256). It is likely that in the cases of Hooker and Huxley, as in the German instances of pre-*Origin* scientists, the combination of anti-miraculous creation and anti-transmutation went hand in hand with an open-mindedness *re* the possibility of autochthonous generation.

To be sure, in the English-speaking world there were far fewer adherents to the theory of autochthonous generation than in Germany. Secularisation of science was less advanced, and creationism continued to be a firmly held belief, not least at Oxbridge. In such a context, the tenets of *Naturphilosophie*, which by this time had become outdated in Germany, represented in London a cutting-edge force of modernization, promoted by Richard Owen and various other scientists of repute. One indication of this was the appearance of an English translation of Oken's *Lehrbuch der Naturphilosophie* under the title *Elements of Physiophilosophy* (1847) (Rupke 1994, 151, 187, 230). The accompanying notion of species transmutation was not considered old hack but revolutionary, a "Victorian sensation" (Secord 2000).

Thus the "politics of evolution" in the English-speaking world differed from what it was in Germany. In London, forces of radical socio-political reform latched on to Lamarckism (Desmond 1989). In Berlin and other German university cities, by contrast, the revolutionaries were more likely to be autochthonists who ridiculed Lamarck. Burmeister and Vogt belonged to the liberal-to-radical left in politics. They were "Forty-eighters," who sympathised with or also took part in the Revolution of 1848 with its programme of German national unification coupled with demands for democratic reform (after 1859, many of the "Forty-eighters" went over to Darwinism (Junker 1995)). Both men were members of the "Deutsche Nationalversammlung," Vogt prominently so. In 1849, Burmeister entered the Prussian Parliament where he represented the extreme left. His later emigration to Argentina has been attributed ultimately to Burmeister's disillusionment with the reactionary political developments at home that followed (Pooth 1966, 364-366). Vogt was more radical yet and, having been accused of high treason and dismissed from his chair, was – as mentioned above – forced into exile (Best 1998). To these men, science was an essential part of the political reform programme and had to be spread among the German *Volk* as part of the people's education to political maturity. Writing popular books about natural history and propagating in these a secular theory of origins represented a form of socio-political renewal. "Miraculous creation," by contrast, stood for the old order of absolutism, feudalism and monarchy, and the creationist Wagner indeed defended these, abhorring the Revolution of 1848, which he interpreted as divine punishment for the secularisation of society (Soulimani 1999, 102-113).

The doctrine of autochthonous generation formed a common ground where the heterogeneous constituency of the liberal-to-radical left in science could meet – a "common context" for a latitudinarian variety of non-Mosaic opinions and values, ranging from idealist to mechanistic-materialist ones.[29] The issue that in the post-*Origin* years proved the most contentious, namely the descent of humans from apes, was kept off the scientific agenda, because *Homo sapiens* was said to have originated, like all other species, directly from "the earth." Autochthonous generation had well-defined socio-political coordinates. Vogt in particular was always as much a political agitator as a pioneering scientist, and his attack on creationism doubled as an attack on the Prussian monarchy. In his *Zoologische Briefe* (1851b, 13-16) he attributed the emergence of new, fruitful directions of scientific research to political revolutions. The French Revolution had led to the fact-based theories of Cuvier. The Revolution of 1848 was leading again to further fact-based scientific advances. Linneus had founded his classification on external morphological features; Cuvier had improved upon him by using internal, anatomical structures of adult individuals; the latest advances added the embryonal development: "[A]t present a new direction is similarly blazing a trail, on the development of which the Revolution of 1848 is probably destined to exert an equally fertile influence as the one of 1789 on the Cuvierian. After all, everywhere one has become aware of the fact that political storms engender the most powerful intellectual stimulus, switching to other areas – of art and science, of trade and industry – as soon as the area of political action becomes blocked off [by reactionary political developments]."[30]

Historiographical Significance

To sum up: the theory of autochthonous generation was present at a range of leading institutions of higher education across the German-speaking world, at Berlin, Erlangen, Giessen, Göttingen, Jena, Heidelberg, Zürich and, yet further afield, in Geneva. Especially Giessen appears to have been a hotbed of autochthony, and the theory's connection with organic chemistry and the latter's applications to agriculture and physiology deserve further study. By the middle of the nineteenth century, the flourishing earth- and

[29] The notion of a common context was put forward by Robert Young in the late 1960s and applied to British scientists and their shared adherence to natural theology (Young 1985, 126-163).

[30] "… so bricht sich jetzt eine neue Richtung Bahn, auf deren Entwickelung die Revolution von 1848 vielleicht bestimmt ist einen ähnlichen befruchtenden Einfluß zu üben, wie diejenige von 1798 [sic] auf die Cuvier'sche. Hat man doch überall bemerkt, daß durch politische Stürme die mächtigste geistige Anregung erzielt wird, die sich auf andere Gebiete, der Kunst und der Wissenschaft, des Handels und der Industrie wirft, sobald ihr dasjenige des politischen Handelns verschlossen wird" (Vogt 1851b, 16).

life sciences in Germany were part of a paradigm of origins that centred on the notion of autochthonous abiogenesis. This was the leading theory of the origin of species at the time that Darwin wrote his *Origin of Species*, adhered to in Germany by several of Darwin's friends-to-be and in England sympathised with by some of his closest colleagues. Yet, as pointed out in the introduction, it never entered the history books about theories of the origin of species. The question should be asked how such "forgetting" could have happened?

Of course, our picture of the past, both individual and collective, involves forgetting as well as remembering, and omissions of one kind or another are an inevitable and integral part of historiography (e.g., Schinkel 2004, 45-48). Yet the phenomenon of forgetting may prove significant when one considers the political purposes that can lie hidden behind historians' amnesia, and in this case, I believe, there existed such a purpose. Several of the major as well as the minor figures who objected to creation/transmutation have entered the history books, yet not as the outspoken autochthonists they were but as forerunners of Darwin. For example, Ballenstedt, Tauscher and Voigt have been described as a "Triumvirat" of early-nineteenth century advocates of the theory of evolution and as predecessors of Darwin (Schindewolf 1941; 1948, 91). A worse misidentification has taken place in the case of Ritgen, who has been described as a creationist who nevertheless also helped pave the way for Darwin's theory of evolution (Bühne 1992, 176-183). Burmeister, too, has been inadequately characterized as an anti-Darwinist rather than properly as an autochthonist (Montserrat 2001, 4-5). Because Bronn helped translate Darwin's *Origin of Species* into German, he has acquired a reputation as a Darwinist, and Vogt more emphatically yet has been put forward as a follower/forerunner of Darwin, these reputations having been retroactively extended to cover the entire career of the two men (Baron 1961; Vogt 1896, 129-137). Even Humboldt, who died shortly before the *Origin of Species* saw the light of day, was turned by Emil Heinrich Dubois-Reymond into a "pre-Darwinian Darwinist" (see Rupke 2005, 63).

The historiography of theories about the origin of species was to a large extent inspired by the success of Darwin's *magnum opus*, and Darwin himself set the trend by adding to the fourth edition of the *Origin of Species* (1864) a "Historical sketch on the progress of opinion on the origin of species" in which he provided a template for the forerunner historiography of his theory, constructing a "creation-*vs*-evolution" model: "Until recently the great majority of naturalists believed that species were immutable productions, and had been separately created. This view has been ably maintained by many authors. Some few naturalists, on the other hand, have believed that species undergo modification, and that the existing forms of life are the descendants by true generation of pre-existing forms" (Darwin 1988, xiii). In the introductory sketch Darwin then identified and discussed the people who had preceded him in formulating species variability and natural selection. Ever since, the production of "forerunners of Darwin" literature has not ceased, and the preoccupation with "predecessors" has never quite ended. The craze reached a climax during the 1959 centenary of the *Origin of Species*, when even Owsei Temkin, who more than any author of secondary literature on Darwinism came close to identifying the theory of autochthonous mega-generation, turned his discussion of pre-Darwinian spontaneous generation into a "forerunners of Darwin" story (Temkin 1959).

Of course, Darwin scholarship has greatly diversified since, yet the mid-nineteenth century autochthonists, whether Christian or materialist, have escaped the attention of recent as well as early Darwin scholars.

What was the purpose of Darwin and his followers to compose an ever lengthening list of predecessors? One could argue that it was intended by Darwin himself to give fair credit to those who had preceded him. To the later historians of particular forerunners, the purpose may have been to demonstrate the significance of their heroes and of the city, the region or the country to which they belonged. More in general, historians have treated forerunners as part of the context of discovery of Darwin's theory. Yet there existed a further aspect to the forerunner frenzy. The oversimplification of "creation-*vs*-evolution," the misidentification of autochthonists as "creationists" or "evolutionists," and the making up of a long list of "forerunners," served the politics of institutionalisation. First, it strengthened Darwin's position by contrasting his theory with the straw man doctrine of creationism, which among Europe's leading scientists was by then a long-slain dragon. Second, it kept the many supporters of autochthonous generation, who for the most part joined the Darwinian cause, out of the firing line. Darwin's ridicule of the belief in an instantaneous origin of species was directed against the creationists, not the autochthonists, even though his words could, *mutatis mutandis*, just as well have applied to them: "These authors seem no more startled at a miraculous act of creation than at an ordinary birth. But do they really believe that at innumerable periods in the earth's history certain elemental atoms have been commanded suddenly to flash into living tissues? Do they believe that at each supposed act of creation one individual or many were produced? Were all the infinitely numerous kinds of animals and plants created as eggs or seed, or as full grown? And in the case of mammals, were they created bearing the false marks of nourishment from their mother's womb?" (Darwin 1959, 483).

A discussion of the theory of autochthonous generation was censored from the start of the Darwinian historiography of evolutionary theory. Whereas "evolution-*vs*-creation" was a useful construct, "evolution-*vs*-spontaneous mega-generation" was not, because – I repeat – the autochthonists by and large joined the Darwinian camp, and it served no purpose to attack or ridicule the past views of comrades-in-arms by exposing their in retrospect bizarre and fanciful doctrines. Nearly to a man, the autochthonists were appropriated on behalf of the Darwinian cause, together with their scientific possessions and valuables. Thus an army of Darwinian foot soldiers was recruited from the historical past to advance Darwinism. The scientific accomplishments that were theirs now became the legitimate inheritance of the evolutionary *Weltanschauung*. Darwin undercut the belief in a teleological development of life, yet no such anti-teleology was put in practice when it came to the historical evolution of the evolution theory, Darwinism retrospectively being made to look like the logical-rational outcome of the mainstream of nineteenth-century science. The historiography of Darwinism was an integral part of the process of its institutionalisation, and excising from the historical record the pages on autochthonous generation served the "politics of Darwinian evolution theory."

Such partisan appropriation of the past by Darwin scholarship is not unique and similar to what has gone on in, for example, Humboldt scholarship (Rupke 2005). It calls for a metahistorical examination of the purposes – political as well as scientific – of the massive body of literature on Darwinism, starting with Darwin's own contributions to the history of his life and work (for such an approach to social Darwinism see Moore 1986). This is not the only historiographical desideratum that emerges from the identification of the "third way" in mid-nineteenth thinking about the origin of species. Revisionist work that needs to be done should take account of the "geography of knowledge" approach (Livingstone 2003) and includes the further identification across the Western world of the scientific constituency that favoured autochthonous generation, a full description of what the theory entailed, a comparative study of the institutional and socio-political location of the theory in the various countries and locations where it had adherents, a study, too, of its situatedness within pre-*Origin* paleontology and physiology, an exploration of the reasons why some of the leading autochthonists so readily switched to Darwinism, and of the extent to which in the popular literature autochthonous mega-generation persisted.

Finally, we may want to examine whether developments in evolutionary biochemistry of recent decades have their intellectual antecedents in mid-nineteenth century autochthony – more so at least than in Darwinism. Helpful in this connection might prove a "What if …?" scenario: what would evolutionary biology be like today if the theory of autochthonous generation and, with that, abiogenesis as the central moment in the study of organic origins, had not been terminated by Darwinism? Would we by now be closer than we are to solving the problem of the origin of life and to producing life in the laboratory? Would the concept of self-organization have gathered strength well before its 1970s and its current "emergence"? If possibly so, the history of the "third way" in mid-nineteenth century thinking about the origin of species could and should be brought to bear on these scientific trends.

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Was there a Darwinian Revolution?

Michael Ruse

Abstract
Some twenty five years ago I published a book called The Darwinian Revolution: Science Red in Tooth and Claw. No one then doubted that I was talking about a real subject, namely the Darwinian Revolution? Since then, that happy assumption has come under attack and in this paper I examine the question about the Darwinian Revolution. First, is it proper to talk at all about scientific revolutions? I argue that it is, so long as one does not assume that all such revolutions are the same. I also argue that the Darwinian Revolution is a paradigm of such revolutions. Second, even if there is a revolution, is it properly Darwin's? I agree that if you restrict the question narrowly and refuse to look at later developments, then Darwin becomes somewhat less important. But I argue also that such restrictions are unfair and if we look at the full scope of the issue, Charles Darwin is very significant and deserves the name of the revolution. Finally, what kind of revolution was the Darwinian Revolution? I argue that it is only in a limited sense like that discussed by Thomas Kuhn, but that the form-function dichotomy that runs through the Darwinian Revolution does have non-rational elements as well as empirical backing.

Twenty five years ago I published an account of the Darwinian Revolution: *The Darwinian Revolution: Nature Red in Tooth and Claw*. The first reader of any book is the author, and this was certainly the case for me and this book. I had been a scholar (meaning that the bulk of my education was now over) for about ten years and my field was the philosophy of science, with a particular interest in evolutionary theory. Although the predominant philosophy of science (in the Anglophone world) of the late 1960s was so-called "logical empiricism" – chief gurus: Carl Hempel, Ernest Nagel, R B Braithwaite, and, somewhat off to the side, Karl Popper – the really exciting book of the day was Thomas Kuhn's *The Structure of Scientific Revolutions*. Above all else, Kuhn argued that philosophers of science needed to take seriously the history of science. For me and for others – notably my long-time friend and mentor David Hull – this was a call we were happy to hear and obey.

As it happened, this was a wonderful moment to get involved in the history of evolutionary theory. History of science as a profession was now developing fast. There were terrific conceptual issues (internalism versus externalism, for instance) and there was good advice about the way to move forward (go to the archives, for instance). More than this, the history of evolutionary theory – the history of Darwinism particularly – was attracting attention, and there were wonderful archival resources at Darwin's own university, in Cambridge, England.

Although I say it myself, I worked hard at my tasks, and actually spent a year in Cambridge, working in the archives and trying to acquire the skills of a professional historian of science. I learnt much from Robert M. Young, Martin Rudwick, and the late Roy Porter, then still a graduate student but with more energy than the rest of us put together. But the one thing I did feel the need of was a good overview of the Darwinian Revolution. Something that someone like me could read and, as it were, get up to speed on the topics. So, at the end of the 1970s, I wrote just such a book! And, without being unduly modest, I think I did a pretty good job. It has been a good standard account from that day to this, and still sells as many copies each year as it did almost from the beginning.

Actually, let me be a little more modest, because I think my book succeeded for one very good reason. People started to take serious, professional interest in the Darwinian Revolution around the time of the centenary of the *Origin*, in 1959. By the end of the 1970s, we had therefore had twenty years of hard work by scholars (most of them young and enthusiastic), and much of the basic groundwork had been done. We had a pretty good idea of what was going on – a pretty good idea based on published material and the very large archives. My book was unashamedly a synthesis of the work of us all, and so this is why it has stood the test of time. I do not mean that nothing new has been done – one thinks for instance of Adrian Desmond's (1989) terrific work ferreting out the working-class evolutionists in Britain in the 1830s, or of Robert J. Richards's (2002) very important work on German biology – but basically we were on top of things, and my book reflects that.[1]

But things do not stand still and let us be thankful for that. In this new century, it is clear that there remains massive interest in the Darwinian Revolution – both internally by scholars and externally (in the USA particularly) because of current controversies over evolution – and things are only going to get more intense as the decade proceeds. 2009 is the two hundredth anniversary of the birth of Charles Darwin and the 150th anniversary of the publication of the *Origin of Species*. What I think is fair to say is that emphasis has now moved from finding out the facts and the immediate issues to broader questions of interpretation. How do we make sense of what happened in the nineteenth century in biology, with the coming of evolution?

There are some very different interpretations and understandings being put forward. *The Journal of the History of Biology* (Spring 2005) has just published a symposium on the Darwinian Revolution, and it would be hard to imagine more different visions of the

[1] What were the important books and articles of the day? Most important of all was the transcription and publication of Darwin's notebooks by Sir Gavin de Beer and co-workers. A new improved transcription is Barrett et al (1987). For me, Robert M. Young's articles were simply mind-blowing. These have been collected as Young (1985). Camille Limoges's (1970) doctoral thesis was very insightful. We all owed a debt to David Hull (1973) for collecting together the responses to Darwin. Michael Ghiselin's (1969) overview of Darwin was quirky, irritating, and stimulating. Martin Rudwick (1972) gave great background. Absolutely crucial for understanding the reception of Darwinism was Ellegård 1958. This is just a sample and for more information go to the bibliography of *The Darwinian Revolution*.

event. Hence – and once again I am writing this piece with myself as first reader, trying to understand the issues – what I intend to do here is to ask some basic questions about the Darwinian Revolution. Three in all. Was there a Darwinian *Revolution*? Was there a *Darwinian* Revolution? Was there a *Darwinian Revolution*?

Was there a Darwinian *Revolution*?

Let us start right at the beginning. A number of scholars today are saying that the whole talk of "revolutions" is mistaken and misleading. One is applying categories of today to the past, and forcing the past into constraints and structures which are not really accurate or informative. In the particular case of science, it is a mistake to talk of scientific revolutions – an unfortunate influence of Thomas Kuhn's book – and we should drop all such talk. There was really no Scientific Revolution in the sixteenth and seventeenth centuries and there was really no Darwinian Revolution in the nineteenth century. Of course things happened, but not revolutionary. Even if one keeps the term for political events – the American Revolution, the French Revolution (even here a bit misleading because they were two very different events) – it is not helpful to transfer the term to science.

The leading debunker of the Darwinian *Revolution* interpretation – what he refers to amusingly as the "evo-revo" school – is the eminent historian Jonathan Hodge (2005), who states flatly that "historians of science should abandon any notion of a Darwinian revolution." He thinks it misleading, because in some sense it focuses us on the Darwinian period, when we should be looking at the whole history of evolutionary theory. It forces us to think that this was when the really significant action occurred, when we should be realizing that it was one episode among many. Hodge believes that the very notion of a Darwinian *Revolution* was an invention of Darwin's supporters after the *Origin* was published, and hence was a function of propaganda needs rather than serious conceptual analysis. Drop it!

Let us sort out the gold from the dross here. If the complaint is that by focusing on the Darwinian Revolution we ignore or trivialize the rest of the history of biology, of evolutionary biology in particular, then (supposing it is true) Hodge has a legitimate complaint. We should certainly not force everything into one short time period, or at least we should not do so simply because of our metaphor without having thought the matter through. And certainly, one has to agree that there is some truth in what Hodge says. I myself went straight to Darwin because of the "obviousness" of the Darwinian Revolution. I was (and am) primarily a philosopher. Why did I not go first to Aristotle, say, or Descartes or Leibniz? It was not just nationalistic chauvinism (I am English-born) but because that was where I "knew" the action lay.

Having said that, however, it is simply not true that now we ignore the rest of the history of evolutionary biology. There is now much discussion of Aristotle, even by me, but much more so by people like Hodge himself, James Lennox (2001), and Marjorie Grene and David Depew (2004). And the post-Darwinian era gets massive attention, from me (Ruse 1996), from Peter Bowler (1988, 1996), from William Provine (1971), from Jean

Gayon (1992), and from a host of younger scholars like John Beatty (1987), Joe Cain (1993), and Betty Smokovitis (1996). So let us not be overly worried on this score.

If the complaint is that one should not use a term like "revolution" because it means so many different things – or applies to so many different events – that it becomes trivial and misleading, I agree fully that one should not assume that one revolution is going to be like another. Especially in science, one should not assume that all revolutions are alike. (Especially, one should not assume that they are all Kuhnian. I will speak to this particular matter later.) But the word revolution does have a standard meaning, and it is useful. It means a dramatic change from one state to another. The American Revolution was certainly revolutionary in this sense. Before, the country was ruled by the British; after, it was not. This made all of the difference.

Similarly in other cases. The Information Revolution makes a lot of sense. I remember when one had to book ahead to make a telephone call to England from Canada, on Christmas day. Yesterday, sitting in a hotel room in Bogotá, Colombia, I did a radio phone-in show in Boston, along with Richard Dawkins in his college rooms in Oxford, and the head of the Discovery Institute out in Seattle. Anybody over forty sitting at their laptop today, grabbing information from the internet, needs no more proof that something pretty revolutionary has occurred.

Of course, this in itself does not imply that the Darwinian Revolution was revolutionary, but by any measure it surely was. At the beginning of the nineteenth century, by and large people did not believe in evolution. At the end of the nineteenth century, by and large people did believe in evolution. More than this, they accepted that it applies to our own species, *Homo sapiens*. This was a terrific move. I will allow completely it was not necessarily purely a scientific revolution. Perhaps it was not even primarily a scientific revolution, being more one to do with religion – Does God still exist and what does He care about us? – or culture or whatever. But it was a revolution and moreover, whether the most important overall factor or not, science was a very important factor. I would go further and say that it was the prime causal factor, for without the scientists I do not see how you could have had a shift to what is (after all) a scientific claim: organisms, including humans, evolved.

So Hodge is going too far here. But I suspect that underlying his complaints is a third, deeper objection. Hodge is a professional historian. The one thing that is drummed into professional historians today is that you must not judge the past by the present, above all you must not assume that the present is the best and the past is the worst. This is the dreadful sin of "Whiggishness." By highlighting the Darwinian Revolution, you are putting today's categories on the nineteenth century and moreover you are portraying it all as a move from dark to light. Anathema!

Fortunately, I was first a philosopher before I became a historian, so I can see the strengths and weaknesses of this objection. It is indeed true that one should not simply mine the past for support for the present – the kind of thing that one often sees at the beginning of science textbooks – Darwin, Mendel, double-helix. But the very fact of interpretation is not bad history. Indeed, it is essential for history. Without interpretation one just has one fact after another – chronology. Martin Rudwick (1986), probably the best living historian of geology, once wrote a book that offered no interpretation or reference to the present. Interesting but a failure. Rather boring really. So interpretation is essential.

What about the use of revolutions for interpretation? I do not see that necessarily one implies that things are getting better because of revolutions. The Russian Revolution of 1917 seems to me to be a clear counter example to this. Many (I am not one, except when a student emails me when I am on vacation in Paris) think that the Information Revolution has been altogether too much of a good thing.

Having said this, I do not at all see why we today should not look back with interest on the things that we see as affecting us today. And evolution certainly does. Nor do I see why we should not look back on things that we think true today. And I believe evolution certainly is. Admittedly, we should look back also on the things we think wrong, if only to see why we think them wrong and compare them to the things we think right. But one can do this in the case of the Darwinian Revolution. Indeed, many of us do precisely this. I have just published a book on the history of American Creationism (Ruse 2005).

So, all in all, I think Hodge's worries are not well taken, and I am happy to remain a prominent member of the evo-revo school. I invite young scholars to join us!

Was there a *Darwinian* Revolution?

How much credit does Charles Robert Darwin merit for the revolution that carries his name? In one sense, no one can deny that he deserves some, a lot in fact. Before the *Origin of Species* appeared in 1859, the idea of evolution was a minority position and in many respects not very respectable. After the *Origin*, it became in many circles – middle class and working class, religious and not -- the accepted position on origins. More than this – with a defiant nod at Hodge – Darwin put forward the mechanism of natural selection, and today this is generally accepted as the right mechanism. Darwin got it right about causes.

But there is more to the question than this. Start with the period before Darwin. We now know, thanks to the massive research of scholars, that there was a lot more acceptance of evolutionary ideas than we realized. In Germany, as the above-mentioned Robert J. Richards has shown, the *Naturphilosophen* were a lot more inclined to evolution than we once thought. Even Goethe, towards the end of his long life, embraced the idea. In France, the opinion used to be that Lamarck was something of an oddity, and that Cuvier's anti-evolutionism was the universal norm. Thanks to the Italian scholar Pietro Corsi (1988, 2005), we now know that there was a whole group of evolutionists around Lamarck. And this continued through the century. We know of Etienne Geoffroy Saint-Hilaire who upset Cuvier around 1830 with his evolutionism, but he was not alone. And there were others in other countries.

Britain too yields many evolutionists, starting with Charles Darwin's own grandfather, Erasmus. There was a tendency rather to dismiss Erasmus as a fat fool, who was a bad poet and too much given to sexual pursuits. But now we realize that he had more influence than we knew. For instance, his major work *Zoonomia* was translated into German and read (and commented on) by the aged Immanuel Kant (Ruse 2006). Charles Darwin himself had also read *Zoonomia.* Then, as I also have mentioned above, Adrian Desmond (1989) has shown the large number of radical evolutionists in London in the 1830s. Particularly important was the professor of anatomy at UCL, Robert Grant, with whom (as an undergraduate in Edinburgh) Darwin had on many occasions discussed matters biological.

There was also the anonymous evolutionist, the author of *The Vestiges of the Natural History of Creation.* We have long known of the influence of this author, later in the 19th century revealed as the Scottish publisher Robert Chambers. For instance, he was clearly a major inspiration for the poet Alfred Tennyson as he struggled to finish (what rapidly became) his much-loved and read poem *In Memoriam.* Recently, James Secord (2000) has shown just how widespread was the influence of Chambers. Finally, we might mention the general man of letters and science, Herbert Spencer, in the 1850s just beginning his dizzying rise upwards as the people's philosopher in Britain and the rest of the world. In the decade before Darwin, he was publishing evolutionary ideas including a clear statement of natural selection (Ruse 1996; Richards 1987).

So there can be no claim that Darwin was the first evolutionist or even the first with natural selection. (There were others who also had glimpses of selection, as well of course as Alfred Russel Wallace, whose sending to Darwin in 1858 an essay with a clear expression of natural selection was the immediate spur to Darwin's writing the *Origin*). Moreover, having said this, there is also the fact that Darwin rarely if ever had an original idea in his life. He was a great packrat, forever gathering together the ideas of others. For a start, to get to natural selection, he had to learn all about artificial selection from the breeders – apart from some desultory experiments with pigeons, he was not into the practical aspects of any of this. He was not really into the theoretical aspects either, getting his information from others. Then there was the influence of Archdeacon William Paley (1802) who sold Darwin on the idea that the world is design-like and that any natural mechanism for the creation of organisms had better take this fact into account. And let us not forget the crucial importance of Thomas Robert Malthus (1826), who argued that food supplies are outstripped by population pressures and that there will be ongoing struggles for existence. This goes straight into the *Origin of Species.*

> A struggle for existence inevitably follows from the high rate at which all organic beings tend to increase. Every being, which during its natural lifetime produces several eggs or seeds, must suffer destruction during some period of its life, and during some season or occasional year, otherwise, on the principle of geometrical increase, its numbers would quickly become so inordinately great that no country could support the product. Hence, as more individuals are produced than can possibly survive, there must in every case be a struggle for existence, either one individual with another of the same species, or with the individuals of distinct species, or with the physical conditions of life. It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms; for in this case there can be no artificial increase of food, and no prudential restraint from marriage. (Darwin 1859, 63)

More broadly, there was the overall influence of Charles Lyell, whose *Principles of Geology* (1830-33) not only inspired Darwin in his early days as a geologist but whose general philosophy, of explaining the past by reference to causes now in operation, was the ruling method that Darwin used in his evolutionary theorizing. And complementing this was the influence first of the empiricist philosopher John F W Herschel (1830, 1841), whose writings probably drove Darwin to make so much in the *Origin* of the artificial-natural selection analogy. Plus the rationalist philosopher William Whewell (1837, 1840), whose argument that the best science shows a "consilience of inductions" – different areas of science brought together under one causal hypothesis – is precisely the argument of the second half of the *Origin*. Darwin surveys instinct, paleontology, biogeography, morphology, embryology, systematics, arguing that all of these are explained by evolution through selection and in turn all of them make plausible evolution through selection. A classic consilience and Darwin was proud of the fact.

One could keep going. Recently Robert J Richards (2004) and I (Ruse 2004) have been arguing about the influence on Darwin of German thought, he claiming that it was the major influence on Darwin and I denying this fact and going for British influences. This is an argument about degree. Neither of us denies that Darwin was influenced by German thinking, either directly or through others, notably Richard Owen's theory of archetypes.

So, having poured so much water on the altar, what can one say in response. Simply that Darwin's genius was to take so many different ideas and to make something of them. Others set the problem. Evolution: right or wrong? The "mystery of mysteries," as Herschel called it. There may have been many evolutionists. No one had come up with a way to make the idea plausible. It was very much in the realm of what I have called "pseudo science," like phrenology and mesmerism. Darwin made the idea of evolution not just plausible but, for most folks, absolutely compelling. The way he tied everything together in a consilience was definitive – back then and now. Darwin did this in the *Origin*.

The same is true of natural selection. Darwin may have borrowed from others but it was he who made something of it all. Take Malthus. He was using the struggle to argue that there can be no overall, lasting change! He argued that any attempts at state help, trying to improve the general status of humankind, were simply bound to make things worse. You feed the poor in this generation and you have more of them in the next. Darwin turned this on its head, showing how the struggle can make for ongoing change. Similarly with artificial selection. It was the standard argument against the possibility of ongoing change. You cannot turn horses into cows. Indeed, in his essay, Wallace (1858) devoted much time to arguing that we should not take the analogy seriously and thus it is no bar to evolution! Again, Darwin saw the potential and used it – used it moreover to make an experienced case of change to argue for unexperienced change, just the kind of argument that the empiricist Herschel was demanding.

Any fool can take pigments and paint a picture of flowers. It took Van Gogh to paint the sunflowers. It took Darwin to write the *Origin*. This now takes us to the time after the *Origin*. Agreed that Darwin put together the idea of evolution and made it compelling, and that it was because of him (together with his various supporters) that people were converted to the idea of evolution. However, after the *Origin*, it is well known that natural selection was a flop. No one took it up, and it languished until the 1930s, when the population geneticists like Ronald A. Fisher (1930) in Britain and Sewall Wright (1931, 1932) in America melded Darwinian selection with Mendelian genetics to make the new theory, so called neo-Darwinism or the synthetic theory of evolution. Hence in major respects even if you grant that there was a revolution it was not very Darwinian. Indeed the historian Peter Bowler has gone so far as to write a book with the title, *The non-Darwinian Revolution*! (Adding to the negative case, let us not forget that the way to any favourable reception of the *Origin* needed to be prepared by others, for instance by the enthusiasts for *Higher Criticism* who were blasting the stuffing out of conventional theology, especially literal readings of Genesis.)

As it happens, Bowler (1996) and I (Ruse 1996) have a major difference in interpretation about the history of evolutionary theory after the *Origin*. He thinks that good, professional-quality work was done – in Britain, Germany, France, and increasingly in America – and that this led into the synthetic theory. It was just that the work was not very Darwinian, in the sense of using natural selection. I think that the years after the *Origin*, measured by the standards of mature professional science, were generally speaking an absolute disaster. I do not deny that there was some professional work, but I think it tended to the decidedly second-rate – phylogeny tracing -- and it was increasingly out of touch with reality. I think that the main use that was made of evolution – by Darwin's supporters like Thomas Henry Huxley – was as a kind of Christianity substitute, a sort of secular religion, to promote the kind of society that they wanted to create. There was no wonder that Herbert Spencer, who was right into this sort of thing, was more influential than Darwin.

I would also say that I am not convinced that natural selection fell absolutely flat. After Darwin, Wallace (1870) and above all H. W. Bates (1862) did wonderful work on Lepidoptera and their markings, using selection as a tool of research. Then later in the century we find others also using selection – E. B. Poulton (1890) at Oxford and Raphael Weldon (1898) then in London, to name but two. Also I am inclined to think that, even if people did not accept selection – and everyone thought it had some little role – the very fact of having a mechanism, even if not accepted, helped. It showed how things could be done, if not done properly.

Having said this, one must agree that natural selection was not a great success. Scientists generally did not pick it up and use it. There was not a new field of selection studies. Partly, this non-development was scientific. There were perceived problems with selection. Without a good theory of heredity, no one could see how the effects of selection could be long lasting (Vorzimmer 1970). Also the age of the earth was a problem. Not knowing about radio-active decay and its warming effects, physicists thought that the earth is much younger than it really is, and it seemed that a leisurely process like selection would never get the job done in time (Burchfield 1975). Partly, there were other factors for the non-development, some in science and some outside. Someone like Huxley was never really that interested in adaptation and design, so for him selection was not really needed anyway (Desmond 1994, 1997). Evolution was what he needed. Outside science, the religious were happy to accept evolution, but they still wanted a bit of guidance to get organisms, especially humans. So they were into directed mutations and so forth, and eschewed the full implications of the blind, cruel process of selection.

So, no Darwinian Revolution in this sense. But I still go back to the point I made earlier. I am just not prepared to take the present out of the picture. Darwin did get it right about selection! It took seventy-five years for this to become apparent, but then it did and it has stayed that way. So, if you are prepared to use the present as a guide to the past – and I have defended this practice – so long as you do not gloss over how history took time to develop, I see no reason to deny Darwin's role in the Darwinian Revolution. And much good reason to think that the revolution is appropriately named.

Was there a *Darwinian Revolution*?

Here my question is about the nature of the revolution, meaning more about what kind of revolution it really was. In other words, my question is more of a philosopher's question, trying to understand the nature of science and the way in which it changes. The name and work of Thomas Kuhn lurks large. So let us phrase the question in Kuhnian terms: Was there a change of paradigms in the sense described by Thomas Kuhn in his *The Structure of Scientific Revolutions*? Was there a switch of world views – perhaps even a switch of worlds – that required more of a leap of faith than an appeal to reason? Or was the change smoother, as more conventional philosophies of science might lead one to expect. Was the change more (say) in a Popperian vein, where basically the facts told against the older position and people shifted because this was the reasonable thing to do?

After nearly forty years of looking at the revolution, my answer is an unequivocal – yes and no (and maybe)! At a broad level, there are certainly Kuhnian aspects to the revolution. Most strikingly, there were people who simply could not see the other side's point of view – clever people, that is, who knew the ins and outs of the issues. Most prominent of these was the Swiss-American ichthyologist Louis Agassiz (E. C. Agassiz 1889). He had staked out an idealistic position before the *Origin* (Agassiz 1859) – one that came directly from his *Naturphilosophen* teachers (Friedrich Schelling and Lorenz Oken) when he was a student in Munich – and try as he might, he could never accept evolution, even a Germanized form that was being pushed by people like Ernst Haeckel (1866). To Agassiz's credit, he really did try – his students around him, including his own son, were becoming evolutionists in the 1860s -- but it was not for him.

Something like this makes perfect sense on the Kuhnian scenario and, in my opinion, fits uncomfortably into the Popperian (1959) scenario – although in fairness to the memory of Popper, I should add that he himself thought that the Darwinian Revolution was more metaphysical than purely science and so probably would not think Louis Agassiz a refutation of his position (1974). So let me simply say, without trying to attribute positions to people, that Agassiz does not fit comfortably into a philosophy of science that makes rational choice the sole criterion of theory change.

However, in other respects the Darwinian Revolution seems clearly very non-Kuhnian. There have been major arguments about what precisely Kuhn meant by "different worlds" – my own inclination is to say, on rereading the first edition of *The Structure of Scientific Revolutions*, that Kuhn meant a real ontological change, although tempered by the fact that ontology (in a kind of Kantian sense) has to be mediated through the observer. But however you read Kuhn, he does argue that people see the facts differently – it is not just a question of interpretation, but of the facts themselves. This is simply not true of the Darwinian Revolution. Everything I have said above about Darwin himself denies this claim.

Darwin was not the Christian God, making things from nothing. He was much more like Plato's Demiurge, shaping what he already had. This applied to ideas as well as facts. Everyone knew about Malthus, for instance, but it was Darwin's genius to put the ideas into a theory of change rather than a theory that argued that change is impossible. Likewise, the facts of the successes of animal and plant breeders were well known. It was again Darwin's genius to make something of these facts. The same is true of so much else – the vaguely progressive fossil record, for instance, and the peculiarities of biogeography. Particularly important for Darwin were Ernst von Baer's discoveries in embryology. Darwin seized on the similarities of embryos and made this a key support for the arguments of the *Origin*.

Let me underline this point by drawing attention to the time after Darwin and returning briefly to the students of Agassiz. It was notorious then – and it is a burden of historians of science now – that it is often absolutely impossible from reading their papers to tell if they have crossed the evolutionary divide or not. Alpheus Hyatt (1889) was a first-class invertebrate paleontologist, yet a foggy writer by anyone's standards – his papers drove Darwin to despair – and part of that fogginess is that one simply does not know where he stands on the issue of evolution. I do not know how else you can describe this phenomenon except by saying that the facts remained the same and the interpretation mattered.

Although having said this, I think in respects we should switch back to something a bit Kuhnian. If you take natural selection out of the picture – and most people at that time did – then it seems to me that the switch to evolution was in a way more of a metaphysical switch, a switch to a natural world, than simply one of science. Imagine if you read a molecular biology paper of the late 1950s, and you could not tell if the author accepted the double helix! It would be impossible.

So I do not want to say that the facts in the Darwinian Revolution were unimportant. People were very impressed by the information in the *Origin*. But I do want to say that there was more than just a rational-choice-powered switch. I would also be inclined to say something similar in the case of natural selection. There is no doubt that Ronald Fisher was a fanatically committed Englishman, and the heritage and glory of Charles Darwin was part and parcel of this. (Fisher was a friend of and fellow eugenicist with Darwin's youngest surviving son, Major Leonard Darwin. Indeed, the rich, childless Darwin helped out the always-underfunded, large Fisher family.) I would entertain sympathetically an argument that said that, for Fisher, natural selection was more than just something justified by the facts and also something with deep cultural significance and hence to be cherished.

But let us not get too carried away by this line of argument. Take Theodosius Dobzhansky. Although initially, in the first edition of his *Genetics and the Origin of Species* (1937), he does not give an overwhelming role to natural selection, by 1941 when he published the second edition he was moving to selection. This was certainly not fueled by a love of the English and their traditions – he could not stand Julian Huxley, the major public spokesman for English evolutionism. Dobzhansky's switch came simply from the facts of variation that he was finding in his fruitflies, especially those in the wild. They showed cyclic, seasonal changes that he simply could not explain by other processes – genetic drift for instance – and so he moved to a more selectionist stance (Lewontin 1981). A rational decision if ever there was one.

There is one more point I want to make before I close this section. Like many historians of biology, I have long been fascinated by the distinction between form and function. (See Russell 1916 for a still major discussion.) Is the right way to understand organisms as having basic forms – archetypes or *Baupläne* – with adaptations added on top? Or is the right way to understand organisms as being adaptive wonders (or machines) that show shared form because of shared ways in which adaptation works? No one denies either form or function, but which is prior? Is it final cause first and pattern second, or pattern first and final cause second?

I would say that form and function perspectives have features of Kuhnian paradigms. Not entirely obviously, because I argue that all biologists recognize both simultaneously. But some biologists see form as prior, that this is a basic commitment, and that they simply cannot see why others do not share this vision. And some biologists see function as prior, and cannot see why others do not share this vision. In the *Origin*, Charles Darwin recognized both form and function – Conditions of Existence and Unity of Type – and clearly came down in favor of function over form. He thought that shared patterns were the consequence of evolution, and that the real driving force and issue was adaptation. It was to this that natural selection spoke.

However, whatever Darwin himself thought, the Darwinian Revolution was a failure if you think that historically it represents the victory of form over function. On this issue, there was no revolution. Before Darwin there were formalists. Goethe was one, the English biologist Richard Owen with his theory of archetypes was another. Before Darwin there were functionalists. Archdeacon Paley was one, the great French comparative anatomist Georges Cuvier was another. Most of these people – Paley and Cuvier for sure, and Goethe and Owen for much of their careers – were not evolutionists. If you need a formalist who was not an evolutionist, then add Louis Agassiz.

What I find fascinating is that, after Darwin, there were (evolutionary) functionalists. Bates was one. Later in time one can add Weldon and Fisher. Coming to the present, one has Richard Dawkins (1986) – who describes himself as being somewhat to the right of Archdeacon Paley on the issue of adaptation – and many other evolutionists, including this author. But there were also (evolutionary) formalists! Darwin's bulldog, Thomas Henry Huxley, was one, and in Germany the promoter of *Darwinismus*, Ernst Haeckel was another. After that we have, at the beginning of the twentieth century, people like the Scottish morphologist D'Arcy Wentworth Thompson (1917). And coming down to today, Dawkins's great rival in the popular field, the late Stephen Jay Gould (2002). All of his arguments about spandrels were designed to promote form over function. Others of Gould's ilk include those that think that the laws of physics create form – the "order for free" school – including the American Stuart Kauffman (1993) and the Canadian-Englishman Brian Goodwin (2001).

My gut instinct as a partisan is to say that one side is right and the other wrong, but that is what partisans say. My more reflective opinion as a historian is that we have rival and ongoing world pictures, very much akin to Kuhnian paradigms. Not strictly paradigms, for apart from the fact that both sides see at least something in the opinions of the rivals, there is not a sequential process, with one paradigm beating out the other. Rather there are ongoing world pictures, and in this sense the Darwinian Revolution was no revolution.

Epilogue

When I set out to write the *Darwinian Revolution: Science Red in Tooth and Claw*, I had no worries about my topic. There was a Darwinian Revolution and I was going to write a book about it. I did just this, and neither I nor my early readers – most friendly, some critical, a few vitriolic – doubted that there was a phenomenon there about which I had written. Now, nearly thirty years later, as you can see, I think matters are rather more complex. But let me say that I do not see this as a matter of regret, and certainly not a cause to feel sad that I have spent so much of my life as a scholar talking and writing about a phenomenon that may or may not exist. If scholars felt that way, then most theologians would be out of business by lunchtime. Rather, I now see how much more complex – and interesting – things are than what I and others once thought. And that is an excellent thing.

Having said this, if you want a good entry into the debate, might I recommend *The Darwinian Revolution: Science Red in Tooth and Claw*. It is a great place to start!

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Holism, Coherence and the Dispositional Concept of Functions

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Abstract
I argue that the originally interest-relative dispositional concept of biological functions can be narrowed in a way that makes functions natural but holistic properties of self-reproducing systems. The additional constraint needed is a coherence relation that obtains exactly between those capacities of an organism's parts that, together, best explain how the organism can self-reproduce. The basic relation that gives rise to this kind of coherence is the contribution that a certain capacity makes to another capacity of the containing system. After developing this account, I show that a system of functions so construed shows the characteristics of a holistic system, strongly resembling a system of beliefs as conceived by semantic holists. The implications for a general conception of holism such as Michael Esfeld's are discussed.

1. Introduction: Holism in the Biological Sciences

Holism is a recurring theme in the history of biology (Weber and Esfeld 2003). General metaphysical ideas in biology that are committed to a form of holism include vitalism (see Weber 1999) and emergentism (Kim 1999, Stephan 2005). In addition, there are specific substantive biological theories and concepts that contain an element of holism, for example, some theories of group selection (Sober 1980), the conception of species as individuals (Hull 1976), F.E. Clements's theory of plant succession (Clements 1936), R. Goldschmidt's theory of the gene (Goldschmidt 1946), and many more. It is probably fair to say that all of these ideas had a difficult time to be accepted by the scientific and philosophical communities, and these difficulties may be partly due to their inherent holism. By contrast, in physics there exists a very important and uncontroversial theory that instantiates a form of holism: Quantum systems are thought to be holistic because they show the phenomenon of non-separability (entangled states). In philosophy, precise holistic claims have been defended with respect to meaning and confirmation (semantic holism) and with respect to intentionality and rule-following (social holism). These cases show that holism is not a fundamentally confused or obscure idea (Esfeld 1998, 2001). However, a convincing case for holism in biology has yet to be made.

In this paper, I try to provide a rationale for the widely shared intuition that living organisms are holistic systems in some sense. This will involve an attempt to show that there exists a form of biological explanation that is essentially holistic, namely a particular species of *functional* explanation. This type of explanation is characterizable by the so-called dispositional account of functions originally due to Robert Cummins (1975). I have previously shown that this account can be supplemented with a coherence condition in order to avoid a certain kind of relativity to the investigator's interest (Weber 2005, p. 35-39). I will present a modified version of this account in Sections 2 and 3. In Section 4, I will briefly discuss the general conception of holism due to Michael Esfeld (1998, 2001). Finally, in Section 5 I try to defend the claim that dispositional functions under the coherence constraint are holistic properties in Esfeld's sense, strongly analogous to a system of beliefs as construed by semantic holists. A comparison to an etiological account of functions that is also claimed to be holistic (McLaughlin 2001) and a brief examination of the implications for a general conception of holism such as Esfeld's conclude this essay.

2. The Dispositional Account of Functions

After Cummins (1975), functions may be defined in the following manner:

X's function in system S is ϕ exactly if X's capacity to ϕ is part of an adequate analytic account of S's capacity to ψ

To illustrate this account, we may use a classical example of a biological function:

The heart's function in the circulatory system is to pump blood exactly if the heart's capacity to pump blood is part of an adequate analytic account of the circulatory system's capacity to deliver nutrients and oxygen to the body's cells.

What is crucial with this account is that function ascriptions according to this definition do not explain the *presence* of the function bearer in the system. In other words, the identification of something as a function entails nothing about why this thing is part of the system. In contrast, the etiological account of functions (Wright 1973) holds that this is precisely what a functional ascription explains. I shall come back to a variant of the etiological account in the final section. Here, my main concern is the dispositional account.

According to the dispositional account, a function ascription explains how the function bearer's activities contribute to some systems capacity or disposition (hence the name). It is in relation to such a systems capacity that some components of a system acquire their status as functions:

When a capacity of a containing system is appropriately explained by analyzing it into a number of other capacities whose programmed exercise yields a manifestation of the analyzed capacity, the analyzing capacities emerge as functions (Robert Cummins 1975, p. 765).

Thus, according to Cummins, functions are relational with respect to some capacity (ψ in the definition given above) of the containing system. This raises the obvious question how this capacity of the containing system is identified. Why see the heart's function in contributing to blood circulation and not to the body's carbon dioxide production or glucose consumption? Can't we just choose as the overall systems capacity whatever we find interesting, thus making functions interest-relative? Can functions be seen as natural properties on such an account?

For Cummins himself, these are simply not desiderata of functional analysis. He fully accepts the consequence that, on his account, the overall systems capacity is ours to choose, and it does not appear to be among his goals to naturalize functions (see McLaughlin 2001, pp. 119-124). However, it seems to be a goal of biological science to identify the *natural* functions of some organ and structure. A biologist who says "I happen to be interested in blood circulation, therefore I see the heart's function in pumping blood" would appear rather unusual. Biologists want to discover what the function of some biological structure *is*, and they want their functional explanations to be made true by natural facts. Thus, Cummins' desiderata for functional analysis and those of a modern biologist appear to be different.[1]

This raises the question of whether there is an analysis that renders functions natural properties of some part of a system. One possibility, of course, is to endorse an *etiological* account of functions (also known as "proper" functions; see Millikan 1984). The usual versions of this account tie functions to natural selection. The function of a thing or structure is the activity for which it was selected in the organism's evolutionary history. However, there are some well-known difficulties with this account. First, it will not admit anything as a function that has just arisen anew (for example, by spontaneous mutation) without having experienced the influence of natural selection yet. Second, biologists sometimes attribute functions without knowing the evolutionary past of some part or structure. Of course, one could argue that this is so much the worse for the standard usage of the term in biology (if there is a single standard usage, which is questionable). But to make sense of scientific practice it is necessary to give an account (or several accounts, should there be different concepts of functions used in biology) that picks out those things as functions that biologists ascribe functions to. There is an alternative version of the etiological account that has recently been developed by Peter McLaughlin (2001) that might be adequate to this task. I shall briefly discuss this account in Section 5. Now, I will discuss an additional constraint to the dispositional account that could also make functions natural properties.

3. Introducing a Coherence Constraint

As we have seen, it is an intended consequence of Cummins's account that the overall systems capacity in relation to which some capacities emerge as functions can be chosen freely by the investigator. Let us consider the example of the heart again in order to examine the options. Cummins requires that, in order to ascribe a function to the heart, we need to pick a systems capacity and show how some capacity of a part contributes to the exercise of this capacity of the whole. For all we know, the heart has the capacity to pump blood, which contributes to the circulatory system's capacity to deliver oxygen and

[1] Cummins' main interest is not in biology, but in psychology and the philosophy of mind (see Cummins 1983). It is beyond the scope of this paper to assess the adequacy of his account in these areas.

nutrients to all body cells. But the circulatory system does many other things: For example, it delivers signaling molecules such as hormones and removes metabolic waste from the cells for chemical decomposition in the liver or dialytic removal in the kidneys. It also carries platelets (for repair), antibodies and immune cells such as B- and T-lymphocytes through the body. For simplicity, let us treat these various activities of the circulatory system as one capacity, the *transport capacity* of the circulatory system. The question now is whether biologists have chosen this capacity just so, because they happen to be interested in transport. This seems not right. Intuition prompts us to say that the transport capacity is the *salient* capacity of the circulatory system. The circulatory system also generates heat and carbon dioxide, uses up energy-rich compounds, makes noises, forms blood clots and hence causes disease and death, but these capacities are not salient.

But why is the transport capacity salient? An obvious answer is that the transport capacity is the circulatory system's *function,* while generating heat and carbon dioxide, using up energy-rich compounds, making noises and forming blood clots are not. But now note what we have done: We have picked the transport capacity as an overall systems capacity in order to ascribe a function to the heart on the basis of it being a function *itself.* This raises the obvious question of what underwrites the functional status of the circulatory system's transport capacity. Perhaps it is the fact that the transport capacity contributes to a variety of other capacities that are *also* functions: cell respiration, immune defense, catabolic waste removal, metabolic coordination, sexual differentiation, and so on.

At this point, it is obvious that this procedure for ascribing functional status generates a regress. Remarkably, it also leads to circularities: For example, cell respiration, which we have marked as one of the systems capacities to ascribe a function to the circulatory system, is a capacity that contributes to the blood-pumping capacity of the heart. We have come full circle.

I suggest that the picture we have here is reminiscent of what certain epistemologists say about a person's beliefs: A belief may be justified by virtue of being entailed or probabilified by a set of other beliefs. So what justifies these other beliefs? The fact that they, too, are entailed or probabilified by other beliefs. Foundationalists about knowledge believe that there is a set of privileged beliefs that can ground all the other ones (for example, beliefs that are directly justified by sense experience). These beliefs, according to foundationalists, are necessary in order to break the regress and to avoid circularity in a system of beliefs. By contrast, coherentists maintain that such grounding beliefs are not necessary. A belief can be justified by cohering with a system of other beliefs without this system needing any kind of grounding. Neither the regress nor the possibility of circularity prevent such a system of beliefs from engendering justification and, hence, knowledge (Lehrer 1990, p. 87-111).

This analogy suggests that we can analyze functions by means of coherence:

X's function in system S is ϕ exactly if X's capacity to ϕ coheres with other capacities belonging to (parts of) S

The concept of coherence as understood here designates a complex relation between a large number of capacities. The basic relation on which this coherence relation is based consists in a capacity's contribution to another capacity. The exemplary case is the heart's contribution to the circulatory system's capacity to transport solutes and cells through the body. It is obvious that this basic relation differs from the basic relation in coherence theories of knowledge. There, the basic relation is usually thought to be an *inferential* relation, including deductive and inductive inferences (such as inference to the best explanation). But I see no reason why the contributory relation between capacities that is under discussion here should not be able to generate a coherent system as well. For example, it is no obstacle that the basic contributory relation is asymmetrical. Most inferential relations are also asymmetrical, and in particular those that are thought to be involved in generating knowledge.

Having specified the relevant basic relation for coherence, we can spell out whence coherence consists in. Let us say that a system of capacities is coherent if it contains a sufficiently complex net of such contributory relations between the various capacities, such that many capacities contribute to other capacities that contribute themselves to other capacities and so forth. By contrast, an *in*coherent system would be one where most of its capacities do not stand in such a network.[2] As an example, we may consider a heap of sand. Its parts – the sand grains – have various chemical and physical capacities, but these do not contribute to other capacities that are instantiated within the sand heap, which themselves contribute to other capacities, and so on. All they do is to exert some repulsive and frictional forces that keep the sand heap stable. This is not a sufficiently complex web of capacities; hence, a sand heap is not a functionally organized system.[3]

By contrast, biological organisms contain an elaborate network of capacities that contribute to other capacities. Here is just a small section through such a network: The function of certain ion channels in nervous membranes is to regulate ion permeability because this capacity is part of an account of the nervous membrane's capacity to fire action potentials. But the nervous membrane's capacity to fire action potentials is part of an account of the nervous system's capacity to process information. Therefore, it is a function of nervous membranes to fire action potentials. Furthermore, the nervous system's capacity to process information is part of an analytic account of the organism's

[2] In epistemology, a coherent system also needs to be *self-consistent* in addition to containing a sufficient number of inferential relations. There is no equivalent for this condition in the present use of the concept of coherence (capacities are not the sorts of things that can contradict each other), but none is required. Coherence in general is about how things hang together or dovetail with each other. Note also that consistency is a much weaker relation than coherence; a system of beliefs can be fully consistent yet lack coherence because there are no or not enough inferential relations.

[3] This account raises the question of where the border is between functionally organized systems and systems that are not so organized. However, there doesn't have to be a sharp border. I do not shy from the possibility of "functionally organized" being a vague predicate. After all, being alive could be a vague predicate just like being bald. As for artificial systems with a functional organization such as a car or a TV set, its functions are parasitic on the engineer's intentions. Therefore, human artifacts are an entirely different issue. For something to have *biological* functions it must be a self-reproducing system, that is, it must be able to continuously replace its parts and maintain a stable state under a variety of external conditions. I take it that this condition means that, in order to have biological functions, a system needs a certain complexity. Of course, nothing in principle prevents human artifacts from becoming self-reproducing some day, in which case they would become candidates for function ascriptions in the same sense as in biology.

capacity to locate food and sexual partners. Therefore, it is a function of the nervous system to process information. The organism's capacity to locate food is part of an analytic account of its capacity to ingest energy-rich compounds and nutrients, which are part of analytic accounts of the liver's capacity to synthesize purines and pyriminides and of the muscles' capacity to transform chemical energy into motion. By the way, this capacity of the muscles is involved in the organism's capacity to ingest energy-rich compounds; here is the first circle.

It is obvious that biologists could tell many endless stories like this one. Any organism of some complexity will reveal zillions of such explanatory relations; this is what it means to possess a functional organization (and perhaps, to *be* an organism). What I am suggesting here is that, if there is a unique way of laying such a coherent functional organization over an organism it is the place of a given capacity in such a coherent system that underwrites this capacity's status as a function, and not its selection history nor the investigator's interests.

The crucial question is obviously whether there is a *unique* coherent system of capacities. Doubts are in order; it is quite conceivable that there are many ways of knitting various causal dispositions of the parts of an organism into a coherent system in the manner just outlined. However, what seems less likely is that there are several systems that are *explanatorily equivalent*. It is possible that, for any type of organism, there exists exactly one coherent system of capacities that best explains how the organism can self-reproduce. By "self-reproduction" I mean not procreation, but the organism's capacity to maintain its form or identity for a certain appropriate duration (see McLaughlin 2001). This appears to be the most universal property in biology (note that not all organisms procreate!), and it is certainly the property that biologists ultimately want to understand. For these reasons, it is appropriate to take self-reproduction as the capacity that a system of functions must explain.[4]

I will now investigate whether the coherence account of functions instantiates a substantial form of holism.

4. Esfeld's General Conception of Holism

Having been introduced by J. Smuts (Smuts 1926), the term "holism" has traditionally suffered from a certain conceptual obscurity. Formulations such as "the whole is more than the sum of its parts" are either trivial or false, or it is not clear what they mean.

[4] In an earlier work, I have argued that a biological function is a capacity that either contributes to a capacity of a containing system that is itself a function or that contributes directly to self-reproduction (Weber 2005, p. 39). The latter clause was intended to break the regress. The present coherence account is both an elaboration and a modification of this earlier account. First, I now think that functional relations do not necessarily have to stand in a *vertical* hierarchy (thanks to Michael Herzog for pointing this out to me). Second, I realize now that the distinction between capacities that contribute to other functions and capacities that contribute to self-reproduction *directly* makes no sense. Only the whole system of functions explains self-reproduction, and there are no more or less direct contributions to self-reproduction. Once these two points are understood, the coherence account presented here follows naturally.

However, Michael Esfeld has given an account of holism that is both precise and substantial. In this section, I shall briefly present this account.

Esfeld's invites us to "regard a system as holistic if and only if the things which are its parts have some of the properties that are characteristic of them solely within the whole" (Esfeld 1998, p. 367). Thus, the basic content of the concept is a certain kind of *ontological dependence* of the parts of a system from its whole. This dependence is such that the parts would not have some of their characteristic properties, would they not be part of the system. Hence, a sand heap is not a holistic system because the sand particles can have their characteristic properties (shape, size, chemical composition) in isolation. Similarly, an ordinary electronic circuit is not a holistic system, because its parts (transistors, capacitors, resistors, etc.) can have their characteristic properties even when they are not assembled to a circuit. Thus, holism on Esfeld's account is different from the trivial claim that some systems have properties that the parts of the system lack, or that a part of the system may be causally influenced by being a part of the system. These claims are trivial because anything that deserves to be called a system will have properties that the parts lack, namely properties that arise from the interactions between the parts. Furthermore, in any system of interacting parts the properties of the parts may be causally affected by the overall state of the system. By contrast, genuinely holistic systems show a very specific kind of dependence of the parts on the whole.

It is crucial for Esfeld's conception that the dependence of the properties of the parts of the system on the properties of the whole is not *causal* dependence. Holism in this sense is not committed to controversial ideas involving macro-determination or top-down causation. In holistic systems *sensu* Esfeld, something else is going on: Any individual part of the system that has property F is, with respect to its being F, ontologically dependent on some other individual that is G where this dependence is not causal (F and G can be identical). Properties that show this characteristic may be termed holistic properties.

For example, it has been suggested that systems of *beliefs* are holistic systems, and that beliefs have holistic properties. This is so because – so it is argued – beliefs can only have conceptual content or be confirmed if they are suitably connected to other beliefs that have content or are confirmed. This dependence is not *causal*; that is, the claim is not that beliefs can only arise as a causal consequence of there being other beliefs (in fact, some beliefs may be directly caused by sensory input). The claim is rather that something can only have some of its characteristic properties if it is part of a system with other things that that have some of these properties. (I am not defending this claim; this is just to illustrate holism). Esfeld analyses this dependence as *generic ontological dependence*.

Generic ontological dependence is intended as a broad category that applies not just to genuinely holistic properties, but also to straightforward relational properties. My being a sibling is ontologically dependent on someone else also being a sibling, namely my sister. Without her, I couldn't exist *as* a sibling. Thus, there is nothing mysterious about generic ontological dependence; it is a straightforward consequence of there being relational properties.

But relational properties do not necessarily lead to holism. In order for some system to be holistic, according to Esfeld, some additional requirements must be met: There must be, for every constituent of a system *S*, a "family of qualitative properties which make something a constituent of an *S* in case there is a suitable arrangement" (Esfeld 1998, p. 375). For holistic systems, the following conditions must be satisfied for all its constituents:

> with respect to the instantiation of some of the properties that belong to such a family of properties, a thing is ontologically dependent in a generic way on there actually being other things together with which it is arranged in such a way that there is an *S* (*ibid.*).

As a corollary, the following definition of a *holistic property* can be given (*ibid.*): A relational property is holistic exactly if

(1) It belongs to a family of properties which make something a constituent of an *S* in case there is a suitable arrangement.
(2) Nothing can instantiate this property unless there actually are other things together with which this thing is arranged in such a way that there is an *S*.

Esfeld's major candidates for holistic systems and holistic properties are systems of beliefs, social systems (with respect to individuals having intentional states) and quantum systems. In the context of beliefs, there exists a family of qualitative properties (conceptual content, confirmation) that can be arranged in such a way that makes them constitutive parts of a system. Presumably, the arrangement consists in standing inferential relations in this case. With respect to any belief instantiating any of the qualitative properties, it is ontologically dependent on there being other things that instantiate some of these properties and on there being a suitable arrangement – at least according to semantic holists such as W.V.O. Quine (Quine 1953).

What interests us here is whether biological functions are holistic properties and, *mutatis mutandis*, biological organisms holistic systems. Esfeld himself is skeptical concerning this possibility:

> A thing which is a piece of flesh with respect to certain non-relational properties can merely not exercise the function of a blood pump if there is no blood. But, independently of its being a constituent of an organism, such a thing has a number of properties which make it function as a heart in case it is arranged with other things in a suitable way. […] Therefore, a functional definition of organs is not sufficient to make a case for holism. I do not intend to rule out that organisms are holistic systems. But the dependence on other things in order that the properties which a thing already has are exercised in such a way that this thing fulfills a certain function is not sufficient for a substantial case of holism (Esfeld 1998, p. 376).

As is evident in this passage, Esfeld takes functional properties to be simple relational properties that supervene on a thing's causal dispositions and its immediate arrangement with other things. These properties are not holistic in Esfeld's sense because the causal dispositions themselves show no ontological dependence on the properties of other constituents in the system. However, as we have seen in Section 2, not any of the relational properties of the parts of an organism should count as biological functions, and the immediate causal relations of something are not sufficient to give them functional status. I have presented an account of functions according to which it is the place of certain capacities in a coherent system of capacities that underwrites their status as functions. On this view, nothing counts as a function unless there are lots of other things that are also functions and this system of functions provides the best explanation for the organism's capacity to self-reproduce. In the following section, I shall examine whether this overturns Esfeld's own diagnosis with respect to holism and biological functions.

5. Functions as Holistic Properties of Complex Systems

A part of an organism may have any number of causal capacities. The heart, for example, has a capacity to pump blood, to produce carbon dioxide, to make noises, to shake the surrounding tissues, to respond to nervous and to hormonal signals, to stimulate immune cells and develop chronic inflammation, to cause blood clots, and so on. Capacities are traditionally not viewed as relational properties. Even though the heart's capacity to pump blood is only *realized* in the presence of blood (or, perhaps, some surrogate fluid), the disposition *itself* is intrinsic. Thus, the causal dispositions themselves are not even candidates for holistic properties, as holistic properties must be relational. So far, Esfeld's diagnosis is correct. However, his claim that a "functional definition of organs is not sufficient to make a case for holism" seems to be based on a very wide sense of "function", namely function as immediate causal role. This non-teleological sense of "function" and "functional" is customary in the philosophy of mind. In the philosophy of biology, by contrast, the term "function" is used in a more narrow sense, namely in a teleological or quasi-teleological sense (depending on the exact sense of "teleological"). It is this sense of function that we are trying to explicate here.

I have proposed an analysis of biological functions that individuates functions by their place in a coherent system of capacities that together provide the best explanation for the organism's capacity to self-reproduce. The heart's capacity to pump blood is a biological function by virtue of its being part of an explanation of the circulatory system's capacity to carry various solutes and cells around the organism, which is in turn a part of an explanation of various other capacities of other parts of the organism, and so on. Thus, having a certain biological function is clearly a relational property.

But are biological functions in my sense also *holistic* properties? Let us check if Esfeld's two conditions are satisfied:

(1) Biological functions belong to a family of properties that make something a constituent of an *S* in case there is a suitable arrangement.

(2) Nothing can instantiate a biological function unless there actually are other things together with which this thing is arranged in such a way that there is an *S*.

It will be readily appreciated that condition (2) is satisfied for biological functions if they are construed in accordance with the coherence account. Here, "S" should be read as a coherent system of capacities that stand in suitable contributory relations; its extension will be a proper subset of all the capacities of an organism's parts. Any of these capacities can only have a biological function by virtue of being part of such a coherent system. Therefore, any biological function is generically ontologically dependent on other functions. It is important to note that any part of an organism has its *causal dispositions* independently of the other constituents. The thesis under consideration here is not that these dispositions or capacities are holistic properties, but that whether or not some capacity is a biological function depends on there being a suitable arrangement of capacities such that many other capacities are also functions. On the present account, a function is not merely an immediate causal role but a certain relation of many causal roles.

Condition (1) seems more problematic at first. First, is there really a *family* of properties in the case of biological functions? It seems that we have only one property here: the property of having a biological function. However, Esfeld does not require that all the members of the family of properties are holistic. For example, it is consistent with his account to say that a system of beliefs is holistic only with respect to confirmation, but not with respect to meaning.[5] By analogy, we could say that a system of capacities is holistic only with respect to their functional status. Note also that to have a biological function is a qualitative generic, determinable property like mass or meaning – nothing can have a biological function *simpliciter*; if something has a biological function it has a specific biological function (the heart does not have a biological function *simpliciter*; it has the specific function of pumping blood). Thus, biological functions are equivalent in all respects with other relational properties that are thought to be holistic by some philosophers, for example, meanings.[6] The meanings of beliefs are determined by the beliefs' place in a web of inferential relations, while the biological functions of the parts of an organism are determined by the parts' place in a system of capacities that together best explain the organism's capacity to self-reproduce.

Note also that, in semantic holism, the relevant holistic system is a system *of* beliefs, not the person who has these beliefs. It would be odd to say that someone's beliefs are constituents of this person. At best, they are constituents of someone's *mind*, which is not a thing but a complex set of properties. Analogously, we don't say that an organism's functions are *constituents* of the organism. Its guts, mesoderm and epidermis and their various differentiations are the organism's constituents. The biological functions are parts of a coherent system of capacities, which are intrinsic and specific (determinate) properties and which are functions themselves. In other words, individual functions are constituents of a system *of* functions. Hence, the relevant holistic system *S* here is not the organism, but it's system of biological functions. In this manner, biological functions as construed under the coherence account satisfy Esfeld's condition for holistic properties, and an organism's system of functions those for a holistic system.

The second problem with condition (1) is the issue of whether it is really the possession of a biological function that makes something a constituent of a system *S*. However, I suggest that this is also a matter of interpreting the system *S* correctly. Intuitively, one might feel that the relevant system ought to be the organism. But in this case, condition (1) is hardly applicable, because it is not the having of a particular function that makes

[5] For Quine, holism with respect to confirmation implies holism with respect to meaning because he holds a verificationist account of meaning (Quine 1953). But without further assumptions this is not implied by saying that confirmation is a holistic property.
[6] Note that some philosophers have tried to reduce meanings to biological functions (see Millikan 1984); this is not my goal here.

something like the heart a constituent of an organism. A heart is a constituent of an organism simply because it is mereologically contained in it. But the heart is a constituent of a system of capacities that have biological *functions* solely by virtue of there being a suitable arrangement of capacities such that many other things emerge as functions, too. The heart can only have a function (in the sense explicated here) if it has a capacity that contributes to the circulatory system's capacity to transport solutes and cells, which in turn contribute to a host of other capacities, and so on. This is what I mean by a coherent system of capacities. As there might be more than one such coherent system, I have required that only the system that best explains the organism's capacity to self-reproduce constitutes its functional organization. The relevant kind of system *S*, then, is a system of select capacities, not the organism itself.[7]

Thus, I conclude that, on the coherence account, biological functions are holistic properties in a similar way in which beliefs are (provided that semantic holism is correct). I would now like to draw a comparison to a form of holism that is instantiated by a version of the etiological theory of functions.

Peter McLaughlin (2001) has proposed an alternative to the classical etiological account according to which functional status is attributed on the basis of the contribution that a function bearer makes to an organism's self-reproduction. By this, McLaughlin means the maintenance of an organism's identity through time by the continuous replacement of its parts (I also use the term in this sense here). According to McLaughlin, something that contributes to self-reproduction in a sense explains its own presence in the system because it is reproduced along with the whole system to the self-reproduction of which it contributes. This explaining of a function bearer's presence in the system is what renders this a species of etiological function.

As McLaughlin notes (2001, p. 210-212), his account leads to a certain kind of holism. Self-reproduction is an activity that is exerted by the whole organism through continuously replacing its own parts. This activity explains why a specific function bearer such as the heart is present in a token organism, and its function is determined by the contribution it makes to the process of self-reproduction. Now, it seems to me that this whole notion requires *downward causation*, a notion that has been shown to be problematic.[8] Thus, the holism that is inherent in McLaughlin's account is a causal holism. By contrast, the holism of the coherence account of functions that I have outlined here is not committed to holistic causality; it is a form of metaphysical holism that arises because of the special relational nature of biological functions. Thus, the coherence account of functions comes metaphysically less expensive than McLaughlin's version of the etiological account.

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[7] One could ask here if it is really desirable to detach an organism's system of functions from the organism itself? Are these two systems not numerically identical? I think they are not, for the following reason: An organism's parts contain many capacities that are not functions (e.g., the heart's capacity to make thumping noises). An organism's system of biological functions supervenes on the causal dispositions of its mereological parts, but it is not identical with it.

[8] See, e.g., Kim (1992), Hoyningen-Huene (1994). McLaughlin does not seem to be sure whether his account really requires downward causation; sometimes he also speaks of "apparent holism".

It is an interesting question whether the successful application of Esfeld's conception of holism to an organism's functional organization – should it actually turn out to be successful – shows that this holism is trivial after all. As we have seen, Esfeld himself uses the example of functional properties in order to show that his conception is non-trivial, because it *excludes* cases like functional properties. However, as already indicated, Esfeld uses the terms "function" and "functional" in a much broader sense than I do in this work, namely as immediate causal role. Such causal roles are not holistic in any interesting sense; they are merely relational. But on the present account, we have whole *systems* of causal roles each of which become constituents of a functional organization only by contributing in a suitable manner to the self-reproduction of the whole system.

Finally, it is appropriate to ask if this somewhat formal result has any kind of *prima facie* plausibility. Why should organisms have holistic properties? I think there is a robust intuition that supports the view developed in this paper: While the parts of an organism have all of their *causal* dispositions independently of the other parts, it is necessary to determine how the parts play together to ensure the self-reproduction of the system in order to single out some of these causal dispositions as biological functions. So far, this is an epistemic claim, but it has ontological reasons, namely that not every system is capable of self-reproduction (while any system can be analyzed into its capacities). Only living things are, and many great thinkers have shared the intuition that, in some sense, living things are holistic system.

In this paper, I have tried to provide reasons for this intuition. I have suggested that the holism of living systems can be construed as a form of holism that resembles the holism claimed for systems of beliefs and some other systems and that satisfies the conditions of Esfeld's general conception. We are dealing with a non-trivial holistic claim because the claim is not merely that functions are relational or that the whole system has properties that its parts in isolation lack. What we have is rather a situation where the parts of a system have some of their characteristic properties – namely, their biological functions – only because they form a coherent system with other components that have biological functions.

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