



Darwinism and the Origin of Life: The Role of H.C. Bastian in the British Spontaneous Generation Debates, 1868–1873

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Abstract. Henry Charlton Bastian's support for spontaneous generation is shown to have developed from his commitment to the new evolutionary science of Darwin, Spencer, Huxley and Tyndall. Tracing Bastian's early career development shows that he was one of the most talented rising young stars among the Darwinians in the 1860s. His argument for a logically necessary link between evolution and spontaneous generation was widely believed among those sympathetic to Darwin's ideas. Spontaneous generation implied materialism to many, however, and it had associations in Britain with radical politics and amateur science. Huxley and the X Club were trying to create a public posture of Darwinism that kept it at arm's length from those negative associations. Thus, the conflict that developed when Huxley and the X Club opposed Bastian was at least as much about factional in-fighting among the Darwinians as it was about the experiments under dispute. Huxley's strategy to defeat Bastian and define his position as "non-Darwinian" contributed significantly to the shaping of Huxley's famous address "Biogenesis and Abiogenesis." Rhetorically separating Darwinism from Bastian was thus responsible for Huxley's first clear public statement that a naturalistic origin of life was compatible with Darwin's ideas, but only in the earth's distant past. The final separation of the discourse on the meaning of Brownian movement and "active molecules" from any possible link with spontaneous generation also grew out of Huxley's strategy to defeat Bastian. Clashes between Bastian and the X Club are described at the BAAS, the Royal Society, and in the pages of *Nature* and other journals.

Keywords: H.C. Bastian, T.H. Huxley, John Tyndall, spontaneous generation, X Club, Brownian movement, active molecules, Royal Society, materialism, abiogenesis

Henry Charlton Bastian has a place in the history of British biology and medicine, but unless one does some digging that place is almost exclusively grounded in his reputation as the "loser" in the last public spontaneous generation controversy of the nineteenth century. In that debate his chief opponents were T.H. Huxley and John Tyndall, X Club captains and defenders of the party line in Darwinism. Because of this it has been widely supposed for more than a century now that no respectable supporter of Darwin seriously advocated spontaneous generation in mid-Victorian Britain. Bastian is portrayed as something of an oddity on the British scene, more like some displaced

1850s German materialist, or like Ernst Haeckel perhaps, than a British scientific naturalist.¹ This paper seeks to overturn such historiography and to show that many evolutionists in Britain, even if not the X Club, agreed with Bastian that spontaneous generation and evolution were linked theories. An examination of Bastian's early training and the beginnings of his scientific career in the 1860s will show that from very early on he believed himself, and was also perceived by others, to be one of the most promising young talents among the Darwinian stable.

The X Club had begun in 1864 as an informal group of friends who met monthly to discuss the agenda of professionalizing science and advancing Darwin's cause, among other things. The nine members, including Huxley, Tyndall, J.D. Hooker, Herbert Spencer, George Busk, and chemist Edward Frankland, among others, met regularly for over 25 years and exercised enormous influence on the development of British science and science education during this period. In general they supported each other's individual initiatives, and they held many important scientific offices, most notably in the Royal Society of London.² The reputation of Darwinism, and of their project of professionalizing British science, stood to be judged by Bastian's claims precisely because he and his experiments were so widely seen, by both friends and enemies of evolution, as part and parcel of evolutionary science. Indeed, near the pinnacle of his fame in this role, Bastian summed up the debate on the subject to date in a book titled *Evolution and the Origin of Life*.³ Both Huxley and Tyndall could recall the debacle for evolution among scientists in the wake of *Vestiges of the Natural History of Creation*, a book that linked evolution with spontaneous generation but was judged to be amateur science.⁴ Even worse, as Adrian Desmond has made clear, spontaneous generation had unsavory associations with medical reformers and their radical politics.⁵ Thus, Huxley and Tyndall were at some pains at first to steer Bastian's publishing on spontaneous generation in a way they thought most beneficial for the public profile of the Darwinian camp. By the summer of 1870, however, they concluded that Bastian was too strong-willed to be controlled by their fatherly advice as mentors. First Tyndall, then a few months later Huxley came out publicly attacking Bastian's work. Yet even after this, the X Club Darwinians were still faced, through most of the 1870s, with a significant minority faction among evolutionists who found Bastian's synthesis of evolution and present day spontaneous generation persuasive, or at least plausible.

John Farley's masterful analysis of the experimental dispute between Bastian and his opponents shows clearly the extent to which the outcome was underdetermined by experimental evidence alone. Farley shows, for example, that John Burdon Sanderson's role as an "expert witness," testifying to

Bastian's experimental skill, contributed significantly to supporting Bastian during Huxley and Tyndall's attacks on his technical competence.⁶ I discuss elsewhere some further experimental issues, including what Harry Collins has called "the experimenter's regress."⁷ This paper is intended to complement Farley's account by showing that a number of other forces in the Darwinian community, more or less independent of the experiments themselves, also played crucial roles in the outcome of the debate. Only in the context of Bastian's role as a leader of a rebellious evolutionary faction can a truly symmetrical story of these debates finally begin to take shape around a fully detailed, fully historical figure.⁸ Finally, I will show that the conflict with Bastian significantly shaped the outlines of the Darwinian position on the origin of life question, in ways that persist to the present day.

Bastian's Background

Henry Charlton Bastian was born in Truro, Cornwall on 26 April 1837. As a young man his interest in natural history led to Bastian publishing a complete "Flora of Cornwall" at the age of nineteen, and a collection of all the ferns of Great Britain a year later, which won a prize from the Royal Cornwall Polytechnic Society.⁹ A few years later, after obtaining his B.A. (1859), M.A. (1861) and M.B. (1863) degrees at University College London, and while working toward his M.D., Bastian carried out an extensive study in his spare time of the Guineaworm ("taken from the extremities of a well-known surgeon from Bombay")¹⁰ and of the entire group of Nematoid worms, parasitic and free. The latter project resulted in a monograph in which 100 new species were described. Bastian became an early and enthusiastic convert to evolutionary thinking by reading both Charles Darwin and Herbert Spencer during his university education.

At University College London Bastian studied anatomy under G. Viner Ellis, zoology under Robert Grant, and physiology under William Sharpey. He won gold medals in botany, comparative anatomy, anatomy and physiology, pathological anatomy, and medical jurisprudence. By 1860 he had been appointed Assistant Conservator of the UCL Museum of Morbid Anatomy under Sharpey's direction.¹¹ He held this post for three years, until his departure to take up a residency in neurology at the State Asylum for Criminal Lunatics at Broadmoor from October 1863 until the end of 1865.¹² By early 1866, Bastian had returned to London, qualified for his M.D. at the University of London, become engaged, and become Assistant Physician and Lecturer in Pathology at St. Mary's Hospital Medical School. By 1866, his position also included Curatorship of the St. Mary's Museum of Pathology.¹³ During his eighteen months at St. Mary's, Bastian came to know Ernest Hart, the

dean, and Henry Lawson, the teacher of histology and later Lecturer on Physiology, both of whom later offered support for his publications in journals they edited.¹⁴ Francis Sibson, F.R.S., of the St. Mary's staff, wrote enthusiastically of Bastian's credentials, saying "I count his accession to St. Mary's as the most important addition that our staff has yet acquired, and I feel that it will add to the growing prosperity of the school . . ." ¹⁵

Bastian's reputation spread to scientific circles beyond the medical community. On 15 June 1865 his memoir on the Nematoids was read to the Royal Society. It was proposed for inclusion in the *Philosophical Transactions*, and was thus assigned to two reviewers, T.H. Huxley and George Busk. Both Huxley and Busk were enthusiastic about the scientific talent of the young Bastian. Both cited the evident commitment of time and care to original investigations, Bastian's disciplined systematic approach, and his commitment to evolutionary thinking. Huxley was firmly against Bastian's speculation that "the Nematoids are aberrant Echinoderms," but otherwise praised the paper highly.¹⁶ Busk agreed about the paper's strong points and, while disagreeing with Bastian's speculation, concluded "I see no reason in this why he should not be allowed to express his own views in the way he has done."¹⁷ With two enthusiastic recommendations, the paper, including the speculations on affinity to the echinoderms, was voted for acceptance into the *Philosophical Transactions* on 26 April 1866.¹⁸ On the strength of his research publications, Bastian was proposed for Royal Society membership by Busk on 7 February 1867. The petition for membership was signed by Busk, Darwin, Lubbock, William Carpenter, Huxley protégé William Flower, Lionel Beale, and President of the Royal Microscopical Society James Glaisher,¹⁹ among others, as well as several medical men. After the collection of sufficient signatures, Bastian was elected F.R.S. on 4 June 1868.²⁰ With such widespread support among the Darwinians, whom Roy MacLeod has dubbed "the young guard," his memoir was also extracted in the journal that they currently looked to as their organ of expression, *The Reader*.²¹

In late 1867 Bastian won appointment as Professor of Pathological Anatomy in the medical faculty back at his alma mater, University College, as well as becoming assistant physician at University College Hospital.²² Within a few months he had caught the eye of University College Professor of Medicine John Russell Reynolds and become his protégé, both at UCL and as Assistant Physician at the National Hospital at Queen Square.²³ Reynolds commissioned Bastian to write a number of sections on pathology and morbid anatomy of various brain and spinal cord disorders for his new text *A System of Medicine*.²⁴ Another young physician who joined the staff at Queen Square shortly before, Thomas Buzzard, later reminisced with William Gowers about their colleague Bastian's rapidly rising stature at this time, saying that Gowers

and Bastian brought “into the hospital an atmosphere of scientific precision and method” that contrasted sharply with the “decidedly commercial tone” with which the hospital had been run up to that point.²⁵ Bastian’s research continued in the area of pathological neurology, including aphasia (speech loss due to neurological damage).²⁶ He publicly debated with Alexander Bain over the “muscular sense”²⁷ and was a leading advocate of the doctrine of psychophysical parallelism in England.²⁸ Bastian’s interest now also moved into the realm of more basic physiology, alongside Michael Foster under the tutelage of William Sharpey in the lab at University College. He took a great interest in the phenomenon of the movement of white blood corpuscles out of capillaries into the surrounding body fluid, later called diapedesis.²⁹ Bastian had also published a technical article on the details of staining procedures he had developed.³⁰ By July of 1868, Arthur Durham was lauding Bastian’s skill as a microscopist, in his Presidential Address to the newly formed Quekett Microscopical Club, while in the same breath touting microscopy as guaranteed to develop “the Moral Qualities,” most of all patience, saying “everyone who works well with his microscope during such opportunities as he may have, cannot fail to become in more senses than one ‘a wiser and a better man.’”³¹ Thus, as both distinguished physician and Victorian man of science, Bastian’s reputation was widely established by the relatively young age of thirty-one.

Evolution and Spontaneous Generation as Bedfellows

How living things originate, including the earliest ones, was a question of great interest to Charles Darwin. Especially after the publication of *On the Origin of Species* in 1859, many in Darwin’s audience, enthusiastic about his totally naturalistic method of explaining life, wondered whether this also implied a non-miraculous process for the origin of life. Historically, reasoning and experiment on this question had been highly contentious and usually fell under the rubric of “spontaneous generation.” Advocates of spontaneous generation claimed that organisms could sometimes arise by the right combination of nonliving materials under appropriate conditions. Opponents claimed this was never possible; that living things must always come from parents like themselves. The term “spontaneous generation” was considered antiquated and simplistic by most of the principals in the debate by the late 1860s. As Farley has pointed out, the term also obscures a crucial distinction drawn by most of the participants: that between what was commonly called “heterogenesis” or “heterogeny” and what was called “abiogenesis” or “archebiosis.” Heterogenesis is the process of living things allegedly appearing from degenerating material, which itself was derived from previously

living things (e.g. meat or vegetable infusions). Archebiosis is the process of living things allegedly appearing from inorganic starting materials. While participants argued for one or the other position, many journals, especially those aimed at a non-scientific public, continued to describe any supporter of either doctrine as an advocate of “spontaneous generation,” thus often lumping together individuals with significant disagreements.

Spontaneous generation (especially archebiosis) was opposed strongly by many scientists in the Christian cultures of Western Europe because it implied the possibility of a universe without any necessity for a Creator God. Materialism, the belief that matter contained within it all the necessary properties to form life, was also implied. Darwin’s theory of transmutation by natural selection encountered a very similar reaction for the same reasons. There was an implicit logical connection between the two theories: if all living things came from fewer and fewer common ancestors the further back in time one went, as Darwin’s theory proposed, then a few or perhaps even one original organism would eventually be reached whose origin still required a naturalistic explanation. If the forces and atoms of the physical world were the same as those in the living world, then to oppose spontaneous generation, to posit an unbridgeable gap between complex organic molecules and extremely simple living things, suggested a break in the continuity of nature and of evolution that underlay Darwin’s theory. In addition, Lamarck in his *Philosophie Zoologique* and Robert Chambers in *Vestiges* had popularized earlier theories of transmutation in which spontaneous generation continuing to present times played an integral part, strengthening the linkage between the two theories.

When Louis Pasteur’s experiments of 1860–1862 were reported, Darwin, Huxley and Spencer took considerable interest. The standard account goes as follows: Felix Pouchet had shown that bacteria could appear in previously boiled and sealed infusions made from, e.g., hay. Pasteur had shown with other infusions (he did not use hay) that various ingenious precautions that prevented dust from entering his flasks and tubes could reliably prevent any growth in them. Pasteur’s experiments persuaded many that present-day spontaneous generation was impossible, he routed his opponent Pouchet and received a prize from the French Academy of Sciences for having, in its judgment, finally laid the question to rest.³² Darwin, Huxley, and Spencer were impressed by Pasteur’s technique and found his work persuasive.³³ This is no small irony, considering that a large part of the reason the French Academy declared against Pouchet may have been because of the unpalatability of Darwin’s theory in the conservative Second Empire of Catholic France and the fact that spontaneous generation was clearly seen there as linked with evolution.³⁴

In the powerful natural theology tradition of British science up to this time, and in the context of Pasteur's widely accepted success at disproving spontaneous generation, Darwin was unusual in that he was even willing to consider the possibility of spontaneous generation. Robert E. Grant, one of the few British Lamarckians who did support spontaneous generation, proves the point.³⁵ His was a non-mainstream voice whose science was clearly merged with his radical political interests, such that Huxley said "within the ranks of the biologists at that time [1850s], I met with nobody, except Dr. Grant of University College, who had a word to say for evolution – and his advocacy was not calculated to advance the cause."³⁶ Despite finding Pasteur's work highly suggestive, Darwin did still keep a somewhat open mind on the possibility of spontaneous generation all through the 1860s and early 1870s.

At the 1868 meeting of the British Association for the Advancement of Science, Huxley announced the discovery of a gelatinous substance that he took to be protoplasm, from ten-year-old samples of deep ocean sediment. As this appeared to be a primordial, undifferentiated creature that fit well into Haeckel's recently coined category of "Monera," Huxley named it *Bathybius haeckelii*. Haeckel was honored and quickly went much further than Huxley, claiming this was precisely the kind of relatively undifferentiated protoplasm one would expect for organisms formed by spontaneous generation.³⁷ Haeckel announced triumphantly that *Bathybius* was the validation of Oken's concept of 1805, that all life originally emerged via spontaneous generation by way of "Urschleim" from the sea.³⁸

In his famous lecture on "The Physical Basis of Life" in the *Fortnightly Review* in Feb. 1869, Huxley described protoplasm as a purely chemical substance, common to all living things. Particularly in the wake of *Bathybius*, it appeared to many that Huxley was supporting the idea of spontaneous generation and materialism, despite his typically cagey denial and his claim to be disproving materialism. He was attacked by anti-evolutionists and vitalists like Lionel Beale and lauded by evolutionists, both assuming that his "physicalist" agenda was headed toward support for spontaneous generation.³⁹

A number of influential articles appeared during 1869, suggesting that evolution, and the correlation of forces in the living and nonliving world, made it unlikely that any uncrossable boundary existed between life and nonlife. A cogent but anonymous nine-part series in the *British Medical Journal* ran through the entire year and was perhaps most influential of all. Its author cited Huxley's "Physical Basis of Life" as well as John Tyndall's recent lectures on conservation of energy and correlation of forces. Huxley later admitted to doing experiments during this period, to find out whether spontaneous generation of microbes indeed ever seemed possible. However,

in his public statements during this time, he remained noncommittal and insisted that he did *not* believe in materialism. Thus during 1868 and 1869 there was a strong association in the public mind in Britain between evolution and spontaneous generation. There was as well a perception that Huxley was trying to avoid publicly admitting this connection and broaching the materialist implications, yet kept hinting around the edges, flying tentative trial balloons.

The *British Medical Journal* series won over steadily more people of scientific importance. For some months it was not widely known that Henry Charlton Bastian was the author of these articles, but even anonymously his case was sufficiently strong to attract the attention of Huxley, Busk, Sharpey and Frankland, and to provoke a countercampaign by Tyndall. Once he went public in April 1870, and long after Grant or other early supporters of spontaneous generation had faded from the scientific limelight or died, Bastian was seen by all of the English-speaking scientific world to be the most talented, eloquent and vociferous evolutionist ever to make the case for spontaneous generation.

Bastian's established place among the evolutionary young guard can be gauged, among other things, by the fact that Alexander Macmillan in Britain and Edward L. Youmans in America both eagerly sought to publish his works. These men were noted from the 1860s onward as seeking to advance the cause of evolutionary science, as well as of public education in science; they were in full agreement with the "young guard" and saw Tyndall and Huxley as ideal models of how to put this agenda into practice.⁴⁰ Youmans had cordial relations with Bastian, as well as with the older Darwinians, and was always scouting for young talented authors whose works he could publish to further the cause of scientific naturalism. He edited the *North American Review* and later began *Popular Science Monthly*, using both as venues for this cause.

It was Youmans, for instance, who spotted young Spencer enthusiast John Fiske at Harvard and recruited him in November 1863,⁴¹ and who also published a critique of Spencer by Frances E. Abbot in October 1868.⁴² The latter specifically challenged Spencer to be consistent with the "first principles" of evolutionary naturalism, and to admit that spontaneous generation was necessarily implicit, just as much as evolution was. Spencer forcefully denied the possibility of rapid spontaneous generation, and insisted that "the facts and arguments [of his denial] had the unqualified endorsement of Huxley, Tyndall and Frankland."⁴³ Youmans was interested in highlighting the potential inconsistency to which Abbot pointed, and he seemed to feel that the question was open still for experimental resolution. Thus, when Spencer wrote him in response to Abbot's essay asking for publication of a

rebuttal, Youmans replied “Is it, in fact, needful for you to commit yourself to either side of the question at present contested?”⁴⁴ Nor was Youmans alone in drawing this conclusion. As I have shown elsewhere, Richard Owen, Gilbert Child, Jeffries Wyman, and Henry Lawson were making similar arguments.⁴⁵ Spencer admitted that this “seems to be a necessary inference. Nevertheless, the inference [of the necessity of spontaneous generation] is one which I did not intend to be drawn.”⁴⁶ Spencer’s claim remained unpublished until 1870.

Bastian Enters the Spontaneous Generation Debate

To recap briefly: after his discovery of *Bathybius haeckelii* in August 1868 and his “Physical Basis of Life” paper in February 1869, the public perception was that Huxley was supportive of the idea of spontaneous generation. James Moore has summed up Huxley’s difficult public relations dance very well. Ernst Haeckel, the most vociferous advocate of Darwin’s ideas in Germany, immediately began to declare that *Bathybius* proved that spontaneous generation was a necessary correlate of evolution.⁴⁷ Back in England, through 1868, Huxley tried to distance himself from Haeckel, but George Lewes wrote in the radical *Fortnightly Review* defending Haeckel’s spontaneous generation theory and claiming that “Mr. Darwin has reason to be proud of his disciple.”⁴⁸

Bastian’s entry into the debate was the anonymous series in the *British Medical Journal*, which explicitly pressed the connection between Huxley’s ideas and spontaneous generation. The path of Bastian’s experimental and clinical work that led him toward spontaneous generation has been discussed by Alison Adam.⁴⁹ Here I wish to emphasize the broader theoretical concerns of evolutionary naturalism, plainly displayed in the *Bathybius* / “Physical Basis” debate and in Bastian’s *BMJ* articles, which seem at least as important as his microscopic observations, in steering him toward spontaneous generation. The *BMJ* articles argued for applying the physicalist doctrines of Huxley and Tyndall to the origin of life question, along with the principle of continuity in nature. Bastian suggested that the boundary between living and nonliving was just as likely to be crossed by natural processes as was the boundary between electricity and magnetism or heat.⁵⁰ The widely respected scientific instrument maker John Browning agreed with the basic position of Bastian’s *BMJ* series, saying

There is no boundary line between organic and inorganic substances. . . . Reasoning by analogy I believe we shall before long find it an equally difficult task to draw a distinction between the lowest forms of living matter and dead matter.⁵¹

As described by a status report on the debate of 23 July 1869 in the *Journal of the Quekett Microscopical Club*, Bastian's first few installments in the *BMJ* were found persuasive and widely accessible.⁵² Club president Arthur Durham seemed fully willing to grant serious consideration to Bastian's arguments that the possibility of spontaneous generation was fully in line with tenets such as evolution and the "correlation of forces" (related to the conservation of energy). Thomas Clifford Allbutt, a colleague who was also rising rapidly in medical circles, wrote Bastian⁵³ to say that he was of a similar opinion especially over the principle of continuity implying spontaneous generation as likely.⁵⁴ A similar assessment was given in an editorial by Darwinian sympathizer Henry Lawson in the 28 April 1869 issue of the weekly *Scientific Opinion*:

It seems to us a little strange that many among the fiercest opponents of spontaneous generation are yet most implicit believers in the law of natural selection, and, indeed, in the general principles of evolution. . . . On merely *à priori* grounds, we cannot see how the Darwinian disciples can reject heterogeny . . .⁵⁵

By this time, microscopist Lionel Beale and Scottish philosopher James H. Stirling had launched aggressive counterattacks against Huxley's physicalism, and against the implied doctrine of spontaneous generation.⁵⁶ Bastian, hoping to mediate, attempted to get a less dogmatic response from Stirling by writing to him privately. Although Stirling was effusive in his reply regarding the accolades he'd heard about Bastian's talents, the letter indicated that the Scot was more rigid than ever in not giving Huxley credit for anything but confusion in his logic.⁵⁷ Bastian despaired of any compromise with Stirling, and, in a scathing review of Stirling's pamphlet early in 1870, he defended Huxley, the "molecularists" (spontaneous generation supporters Richard Owen and John Hughes Bennett), and "physicalists" in a manner which showed his belief that he, Owen, and Huxley ought all to be in agreement over spontaneous generation. Stirling, in his attack, had specifically said Huxley seemed to be implying spontaneous generation, and on this issue Bastian's defense was ardent. He hinted that scientific naturalism might well be about to prove the existence of spontaneous generation, and that would be just the victory Huxley had been anticipating.⁵⁸

Huxley witnessed several of Bastian's experiments in the spring of 1870 and was advising the younger man about how to proceed. It was surprising then for Bastian, when Huxley, at first helpful and sympathetic, soon began to distance himself more and more, as the young pathologist made his claims about spontaneous generation more public and more forcefully. Matters had become more complicated when Huxley's friend and X Club compatriot

John Tyndall, in late 1869, became a convert to Lister's and Pasteur's germ theory of disease. In a widely publicized Friday evening public lecture at the Royal Institution, titled "Dust and Disease," Tyndall in January 1870 very clearly committed himself to the germ theory and criticized the majority of doctors who remained skeptical of it. In line with Pasteur's arguments about the origin of the germs, Tyndall insisted in a letter to the London *Times* in April, that microbes and their germs could never be spontaneously generated. The sooner doctors could see things from the more scientific viewpoint of Pasteur's chemistry or his own field of physics, Tyndall argued, the sooner they would find the solutions that they had been muddling around for, e.g. the cause of infectious disease. British doctors were not the first group Tyndall had managed to offend, but in this case many who kept up with the latest experimental evidence and were proponents of evolution and the new scientific naturalism had come to the opposite conclusion from Tyndall. Bastian led the doctors' response to Tyndall. Although he had great regard for Tyndall's work in physics, he thought it pointed toward support for a mechanistic origin of life. He criticized the physicist in a reply to the *Times*, saying that Tyndall was trying to avoid the logical implications of scientific naturalism – that continuity in nature required the ability of life to form from nonliving matter. Bastian further chided Tyndall for so heavy-handedly declaring medical knowledge on infectious disease less scientific than the physical sciences.⁵⁹ More than that, he pointed out that Tyndall was totally ignorant of some of the main evidence doctors considered persuasive against a simple germ infection model, namely the variable constitution of people within the same town, or even household, some of whom got sick during epidemics and some of whom did not.⁶⁰ The two men exchanged several more public letters throughout April 1870, steadily increasing in rancor. Because Tyndall had chosen as public a forum as the *Times* for his initial letter, and because that letter was so insulting to many medical men who up to that point thought him an ally in the cause of evolutionary science, by the end of the exchange both Tyndall and Bastian had become publicly committed to highly polarized positions on the issue in full public view – a scenario that rarely lends itself to further calm and easy compromise.

Bastian had told the noted publisher of the scientific young guard, Alexander Macmillan, that presenting a paper at the Royal Society would be his first major public move in publicizing his views on spontaneous generation.⁶¹ In the end, the paper was not actually presented when Bastian realized that, "owing to the accumulation of many papers and other causes, no evening could be allotted on which it might be read and discussed."⁶² However, feeling that there was need to get an in-depth statement of his position and experiments before a scientific audience without undue delay,

especially after his public confrontation in the *Times* in April 1870, Bastian opted to submit the paper to *Nature*. Norman Lockyer, the editor, was on friendly terms with Bastian, and Macmillan, the journal's publisher, had been planning to publish Bastian's book-length treatment of the subject for more than six months already, advertising it as forthcoming in the pages of *Nature* since January.⁶³ Thus it was agreed that Bastian's long paper would appear *in extenso* in three installments in the 30 June and 7 and 14 July issues.

Huxley observed all this with some dismay. After working for years with the X Club for order in the ranks of the "young guard" of Darwinians, the camp was fragmenting in full public view into two factions sharply divided over spontaneous generation. Clearly this was a millstone that scientific naturalism, embattled on many fronts as it was, did not need around its neck right now. He counselled both Tyndall and Bastian to cool off.

In private, Huxley began to have doubts around the beginning of May 1870 about Bastian's experimental technique for sterilization. He urged Bastian to postpone publication until he was absolutely certain of all the details. Bastian, however, had already satisfied himself about the difficulties with many of the experiments and wanted to get his results published soon,⁶⁴ since his debate with Tyndall had caused him to show his hand. Though he withheld from his *Nature* paper the experiments about which Huxley had the most doubt, going ahead with publication against the advice of the elder statesman of scientific naturalism made things worse in Huxley's view – now the public splitting into camps was occurring in the very journal he had hoped would finally act as the unified voice for scientific naturalism. Further, citing Huxley and Tyndall specifically, Bastian pointedly stated that their work tended to show why heterogenesis was implicit in the other tenets of scientific naturalism. Only a vitalist, he insisted, would argue for an arbitrary divide between the chemistry of solutions crystallizing and the simplest living matter forming by some analogous process.⁶⁵

Clearly among the British scientific naturalists there was an extraordinary amount of historical contingency in the forming of their final public positions on spontaneous generation. This must be taken into account above and beyond the experimental evidence that was under debate. John Tyndall, for instance, had expressed deep admiration for Bastian's experimental skill *prior* to their run-in in the columns of the *Times*.⁶⁶ Bastian's strong-headed insistence on going public despite the clear advice of the "Lord Mayor" or "General" of the evolutionist army also greatly conditioned Huxley's regard for the younger man's experimental talent. While prior to Bastian's publication Huxley was encouraging and cautiously positive about Bastian's work, after Bastian published Huxley immediately began pronouncing him an incompetent experimenter, at first in letters to other Darwinian intimates,⁶⁷

but by September 1870 in the glare of publicity of the BAAS meeting in Liverpool, where Huxley had the public relations advantage of serving as president of the Association and delivering the opening President's address.

Huxley's Attitude Toward Young Men of Talent

To fully understand Huxley's reaction, we need to look briefly at his manner of guiding the careers and *Bildung* of rising young evolutionary scientists. Huxley viewed himself as mentor and guide to a number of young scientists interested in evolution. These included Ernst Haeckel, Anton Dohrn, William Flower, Michael Foster, Alexander Kowalevsky, St. George Mivart, E. Ray Lankester, and the philosopher John Fiske, as well as Bastian. Huxley was attentive to the development of their character and to acquainting them with his idea of the proper code of behavior for a professional evolutionary scientist, at least as much as to the details of their experimental and theoretical development. An early exchange with Anton Dohrn illustrates this quite well. Dohrn, at 28, had been corresponding with Huxley for some time, and had developed a theory on arthropod evolution that he was so excited about that he sent an article off to be published even though Huxley had cautioned him that the theory seemed shaky and advised waiting for further evidence and reflection before hasty publication. (Recall Huxley's very similar warning to Bastian in his 1865 reviewer's report, as well as in April–May 1870.) By the time the article appeared in 1868, Dohrn had reconsidered and felt very sheepish at not having followed his mentor's guidance. Huxley responded

As you know, I did not think you were on the right track with the arthropods, and I am not going to profess to be sorry that you have finally worked yourself to that conclusion. As to the unlucky publication . . . you have read your Shakespeare and know what is meant by "eating a leek." Well, every fine man has to do that now and then, and I assure you that if eaten fairly and without grimaces, the devouring of that herb has a very wholesome cooling effect on the blood – particularly in teaching sanguine temperament. Seriously, you must not mind a check of this kind.⁶⁸

That this was a general principle for Huxley, not just an ingenious one-time literary trope for this occasion only, is clear from Huxley's repeated use of the "leek eating" expression to encapsulate his notion of that particular rule of scientific decorum when illustrating the point for his young protégés. For instance, in 1875 when *Bathybius haeckelii* was found to be an artifactual chemical precipitate from seawater by the scientists aboard the Challenger expedition, and not the primitive protoplasmic form Huxley had declared it to be in 1868, Huxley wrote punningly to Michael Foster

The “Challenger” inclines to think that *Bathybius* is a mineral precipitate! in which case some enemy will probably say that it is a product of my precipitation. . . . Old Ehrenberg suggested something of the kind to me, but I have not his letter here. I shall eat my leek handsomely, if any eating has to be done.⁶⁹

And at the 1879 BAAS meeting at Sheffield, when the issue of Huxley’s mistake was brought up again, he again gracefully ate the leek, according to his *Life and Letters*.⁷⁰ Foster showed that the lesson had been internalized when he wrote to “my Lord Mayor . . . you did that *Bathybius* business with the most beautiful grace – I wish you would sell me a little morsel of that trick.”⁷¹

In another letter to Dohrn, Huxley maintained the same humorous and fatherly tone, but described a bit more explicitly his self-appointed role as critic of the younger Darwinians:

What between Kowalevsky and his ascidians, Mikluko-Maclay and his fish brains, and you and your arthropods – I . . . spend my time mainly in that pious ejaculation “Donner und Blitz,” in which you know I seek relief. Then there is our Bastian. . . . Now I think that the best service I can render to all you enterprising young men is to turn devil’s advocate, and do my best to pick holes in your work . . . I have been [Herbert Spencer’s] devil’s advocate for a number of years, and there is no telling how many brilliant ejaculations I have been the means of choking in an embryonic state.⁷²

A number of events occurred in 1869 that led to a crucial shift in Huxley’s attitude. One major event was the increasing furor over spontaneous generation with frequent attribution of his “Physical Basis of Life.” Another was the defection, on 15 June 1869, of St. George Mivart, one of the brightest young Darwinians Huxley was grooming. Mivart during the next two years published some of the most technically well-informed, and therefore damaging, critiques of Darwinism of any yet produced. And it was no surprise, since he had for years been studying at Huxley’s elbow, acquiring the technical expertise that made his treason so publicly damaging to the cause of Darwinism. This made Huxley exquisitely sensitive during these months to any behavioral impropriety among the younger ranks, which might hint of another damaging defection to come. It is in the context of Huxley’s larger role as self-appointed policeman of the younger ranks, and of his heightened sense of having completely overlooked a defection-in-the-making during the last months of 1869 and the opening months of 1870, that his encounters with Bastian at that time must be viewed. Let us look again, in this light, at how their relations developed.

Huxley and Bastian: Initial Interest Turns Sour

Bastian had corresponded with Tyndall about his experiments as early as January 1870, and advertisements for his forthcoming book *The Beginnings of Life* were placed by Macmillan on the front page of *Nature* for many weeks, beginning with the first issue of that year.⁷³ The subject of the origins of microorganisms was debated at an X Club meeting on 3 February, given Tyndall's recent "Dust and Disease" lecture and an editorial in *Nature* that same day questioning the validity of the germ theory and suggesting that Pasteur's claims about air-germs were just begging the question.⁷⁴ Huxley's interest in Bastian's work began or intensified at this point. He had been consulted by Bastian and was present, along with Busk, Frankland and Sharpey, to witness numerous experiments in March and April 1870, both at the sealing and the opening of tubes when any possible contamination due to experimental error might occur.⁷⁵ Furthermore, the support of the publisher Alexander Macmillan for Bastian's work was an important factor in its serious reception at this time. Dr. Gilbert Child of Oxford wrote to *Nature* to express support for Bastian on 21 April, and Macmillan responded to a complaint about publishing this, saying "I think you may rely that Lockyer will allow no partisanship in such questions to influence the fair discussion of them."⁷⁶

Nonetheless, after Bastian's recent clash with Tyndall in the columns of the *Times* (a forum inherently distasteful to the X for such a disagreement),⁷⁷ Huxley began to have a more guarded and skeptical view of Bastian's experiments. In particular, he was disturbed when some spiral fibers and a structure resembling a leaf of the moss *Sphagnum* showed up in some of Bastian's sterile tubes; because the tubes had been hermetically sealed, Bastian was willing to believe that those must have originated from nonliving matter in the solutions. On 1 May 1870, Huxley met with Bastian and cautioned the younger man that he felt such objects must be contaminants. Bastian responded in a letter the next day, that he had looked at some *Sphagnum* leaves for comparison and agreed that the one in the solution must indeed be "an accidental fragment of a *Sphagnum* leaf." However, he maintained that the spiral fibers seemed to be genuinely spontaneously generated. Bastian went on to say he hoped to get help from chemist Edward Frankland in trying to understand the apparent result that organisms had appeared in some solutions supposedly containing no carbon, but he agreed that he would certainly not say anything for the present about such a surprising finding.⁷⁸

Huxley replied, endorsing Bastian's course and, hoping Bastian would not be angry at advice, urging him not to publish until "all results tested."⁷⁹ Bastian wrote back on 12 May after having done more experimental work on the fibers, and seemed to have taken the advice in a collegial spirit:

So far from being at all angry, I am very much obliged to you for the advice contained in your letter, which I know was dictated by the best of motives . . . I can understand that there is reason for the most extreme caution in bringing before the world supposed new organisms – which may be not organisms at all and not living.⁸⁰

Bastian stated that some of his experimental results were much more certain and directly contradicted crucial claims of Pasteur, however, and he felt that those results should be published as soon as possible.

In the context sketched above, including the recent treason of Mivart, we can easily understand why this behavior on Bastian's part would strike Huxley as intolerably cocky. He was refusing advice from his elder and showing signs that he might eventually resist "eating his leek," and this over spontaneous generation, an issue particularly explosive for the status of evolution as respectable science.

Bastian's eagerness to believe in spontaneous generation, and his need to be corrected by Huxley on something as complex and familiar as a *Sphagnum* leaf, had greatly shaken Huxley's confidence in Bastian's abilities in the laboratory. Thus, even though his tone to Bastian was such that the younger man remained convinced of his support, in a letter to Anton Dohrn at this same time Huxley's tone suggested that he was now much more skeptical of Bastian's claims:

. . . Bastian . . . is making living things by the following combination:

R_x Ammoniae Carbonatis, Sodae Phosphatis, Aquae destillatae quantum sufficit, Caloris 150 Centigrade, Vacui perfectissimi, Patientiae.

Transsubstantiation will be nothing to this if it turns out to be true . . .⁸¹

Huxley's attitude toward Bastian may still have been ambivalent in early May of 1870 when he looked in on Bastian's experiments for the last time. However, as we have seen, Bastian went ahead against Huxley's advice and published a large part of his experimental case for spontaneous generation in *Nature* in three installments beginning 30 June 1870. Thus, by midsummer, as Huxley carried out more of his own experiments on the origins of bacteria, molds and yeasts, he had made up his mind that Bastian's scientific demeanor was improper and unmanageable. A series of letters exchanged among Hooker, Darwin and himself at this time leaves no more room for any benefit of the doubt toward Bastian.

Hooker opened the exchange by commenting to Darwin that "Bastian's paper in *Nature* is full of curious matter, but eminently unsatisfactory in treatment. I think it poorly written."⁸² Darwin agreed that the paper was not convincing, but continued "[a]gainst all evidence, I cannot avoid suspecting

that organic particles (my gemmules from the separate cells of the lower creatures!) will keep alive [during boiling] and afterwards multiply under proper conditions. What an interesting problem it is.”⁸³

Huxley, however, replied to Hooker describing experiments of his own and saying of Bastian’s that

The wonderful and significant fact about Bastian’s *Sphagnum* leaves is not that they were in his tubes, but that he had not sufficient histological knowledge to be led to suspect their real nature. He brought a specimen, shut up, to me in order to put an end to my doubts about the generation of living things in his tubes – and I had much ado to convince him of the real nature of the specimens . . . I put not the slightest faith in Bastian’s work. He is a clumsy experimenter and an uncritical reasoner.⁸⁴

Over the summer Huxley decided to try to cut Bastian and the controversy associated with him off from the camp of true scientific naturalists, and to use his upcoming Presidential Address to the British Association for the Advancement of Science (BAAS) meeting in September as a powerful weapon in that service. In that speech Huxley defined the new party line position on the origin of life question – spontaneous generation in the present day had been conclusively disproved by Pasteur, he said, but in the earth’s distant past scientific naturalism surely called for it to have happened at least one original time.⁸⁵ Lest it be mistaken that this position was arrived at or announced independent of Bastian, it should be noted that Huxley’s clever rhetorical strategy rules this out. I discuss this further below. Huxley barely mentioned Bastian in his address, but in a debate in the Biological Section six days later, he went beyond his earlier caution about Bastian’s speculation being not sufficiently grounded in the Nematoid paper, and directly attacked Bastian’s experimental competence.⁸⁶

Brownian Movement

I have discussed elsewhere how the phenomenon of Brownian movement, from its initial discovery in 1827, was bound up with discussions about spontaneous generation, a linkage that especially hinged upon the appearance that Brown’s microscopically visible “active molecules” were self-animated. The writings of John Hughes Bennett and Richard Owen, in 1868–1869, revived this linkage by explicitly referring to the “molecules” that led to heterogenesis.⁸⁷ Soon after Bennett’s “molecular theory” became popular, the well-known Manchester instrument maker and microscopist John Benjamin Dancer, though attempting to dismiss any claims that the particles are self-animated, had stated that Brownian movement’s cause was still not accounted

for. His own theory was that the movement was in some way connected with the absorption and radiation of heat.⁸⁸ A few years earlier, Christian Wiener, a professor at Karlsruhe, claimed to have experimentally investigated and explained the phenomenon. Wiener, like Dancer, stated that Brown originally believed that the active molecules might bridge the gap between the living and the nonliving, but claimed himself to have proved that their motion was of a purely physical character.⁸⁹ Dancer's theory was not identical to Wiener's, but he likewise made light of any attempts, including those still current, to link the phenomenon with spontaneous generation:

Many instances have come under the author's notice, in which these objects have been regarded by microscopists as animalculae. They have given rise to many very ingenious speculations, some of which are connected with spontaneous generation. . . . Some writers who commented on [Brown's] experiments, but who had not carefully followed his communications, asserted that Dr. Brown imagined these particles to be animated, – and this statement was generally believed.⁹⁰

A similar caution had been stated by physician (and friend of Tyndall) Henry Bence Jones in his *Life and Letters of Faraday*, which appeared at the end of 1869. Dancer's colleague, the economist and mathematician William Stanley Jevons, had also advanced a theory of his own to explain the movement as purely physical in nature.⁹¹ All this discussion from so many quarters suggests a case of "the lady doth protest too much": why would multiple authors bother to restate, at just this time, than an issue was disproved long ago and no longer to be taken seriously, except because the revival of the spontaneous generation controversy to such a heated level in 1869 had also revived such speculations?

By 1870, Huxley had adopted the belief that the movements were purely physical in character, and this in fact was immediately brought to bear upon his campaign to distance himself from Bastian. Huxley opposed spontaneous generation in his Presidential Address, opening the BAAS meeting in Liverpool in September 1870. His frontal attack on Bastian began a few days later with the delivery of a paper about Huxley's experiments on "Penicillium, Torula, and Bacteria" at the Liverpool meeting⁹² that was published soon afterwards. In that paper, Huxley stated:

When you examine . . . Bacteria with the very highest powers . . . they have *two distinct kinds* of movements. . . . These two kinds of movement are not to be confounded. They must be explained as due to very different causes; and it seems to me that it is a confusion of these two which is at the bottom of the mistakes made in the assertions as to the survival of Bacteria, &c., after the application of very high temperatures. . . . Dr.

Bastian was good enough to unseal a flask in my presence, which had been closed at a temperature of 150 °Centigrade; and I saw there and then Bacteria, exhibiting these active, trembling movements, which, had they come from any other solution, I should have *then* considered as a proof of their being alive. . . . The first kind of movement (the trembling) is no doubt the Brownian movement, first shown by Robert Brown to be exhibited by minute particles of a variety of substances, when placed in liquid. . . . This discrimination is of the utmost importance. I cannot be certain about other persons, but I am of opinion that observers who have supposed that they have found Bacteria surviving after boiling have made the mistake which I should have done at one time, and, in fact, have confused the Brownian movements with *true living* movements.⁹³

Bastian responded to this charge on the spot and repeatedly afterwards, yet found that it continued to be used against him as if he had no good defense.⁹⁴ His argument was that the organisms in his tubes were alive because they multiplied, not because they showed movement. Ironically enough, it was Bastian himself who had first *made* that distinction in conjunction with spontaneous generation experiments, at the very beginning of his paper in *Nature* not three months before this.⁹⁵ He pointed this out, implying both plagiarism and treachery on Huxley's part:

This statement concerning the two kinds of movements of Bacteria and the power of boiling water to arrest only one of them, is almost word for word what appeared in *Nature* for June 1870. I thought at the time that the statement was new in certain respects – at least I cannot refer to any similar statement in the writings of others previous to that time. I was somewhat surprised, therefore, on reading the quotation, . . . to find that Prof. Huxley, on Sept. 13, 1870, mentioned such distinctions as if they were quite novel, and with the tacit suggestion that I was unaware of them.⁹⁶

Bastian quoted from Huxley's article and concluded that "what follows is certainly a suggestion that I had been misled by these phenomena, apparently because I was unaware of the distinction then pointed out by Prof. Huxley,"⁹⁷ The fact that Huxley was able to get away with this, despite the truth of Bastian's claim about having priority in publishing exactly the distinction at issue, is testimony probably not only to Huxley's oratorical skills, but also to his wide reputation in the scientific community, bolstered by much X Club maneuvering. But this was not the only, or even the most important, of Huxley's successes over his opponent at the 1870 meeting in Liverpool.

Controlling the Terms of the Debate

Huxley's Presidential Address was an outstanding rhetorical success. He used the terms "biogenesis" and "abiogenesis," which caught on quickly, to define the poles of the debate (biogenesis meaning life only from other life, and abiogenesis meaning life from nonliving matter). It is more than a little ironic that Huxley hijacked the term "biogenesis" from Bastian, who was using it up until that time to mean exactly the opposite, i.e. spontaneous generation!⁹⁸ This ranks perhaps highest among the rhetorical coups pulled off by Huxley, in which defining the terms of the debate and making his terms stick was the single most effective stroke that guaranteed him victory. Indeed, we must credit him in this case with even more than his usual degree of skill: the fact that this term (biogenesis) is often listed as among those *coined* by Huxley himself reveals how much more completely than usual he managed to deny his opponent the rhetorical advantage, taking the opponent's own terms away and giving them opposite meaning, which eventually stuck in the history books, and more importantly, in the biology textbooks, when those began to appear.

Bastian attempted to play catch-up by devising his own new terms. He coined the word "archebiosis" to refer to the origin of living matter from inorganic starting materials. In the scientific naturalist view that Bastian shared with, e.g. Huxley and physiologist Michael Foster, arguing for "spontaneous" generation at any point in the earth's history had become undesirable, as the term "spontaneous" was seen by many to "carry with it the idea of irregularity."⁹⁹ Bastian was attempting, as was Huxley with his new coinage, to cut himself off from the older term and its connotations. "Archebiosis" implied *lawful* processes of development, as lawful as, and analogous to, those by which inorganic crystals formed from a saturated solution. This distinction was recognized by some, and seen to be to Bastian's credit.¹⁰⁰ Bastian's term, however, was not perceived to have the same elegance as Huxley's "abiogenesis." One reviewer somewhat sympathetic to Bastian's evidence and arguments nonetheless said "[t]he nomenclature adopted by Dr. Bastian is very peculiar. The hideously ugly word 'archebiosis' is coined to express an idea, which, when it is examined, is closely allied to that of heterogenesis."¹⁰¹

Huxley and Tyndall in their arguments against Bastian continued to describe his views as favoring "spontaneous generation." In the long run, Bastian's failure to separate himself clearly enough from the associated and scientifically unpopular implication of randomness, was a serious rhetorical weakness. The success of Huxley's "abiogenesis," on the other hand, because it carried the desirable notion of lawfulness, helped produce, over time, the public perception that Huxley's views were more up to date than Bastian's.

Huxley also set a strong precedent in his talk for shifting the discussion of spontaneous generation to the distant past, as required by Darwinian theory. He gained accolades from some for his open-mindedness in declaring that, though he saw “no reason for believing that the feat [of spontaneous generation] has been performed yet,” nonetheless “I must carefully guard myself against the supposition that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past. . . . That is the expectation to which analogical reasoning leads me; but I beg you . . . to recollect that I have no right to call my opinion anything but an act of philosophical faith.”¹⁰² It is increasingly clear from this time forward that almost all scientists sympathetic to evolution began more and more to acknowledge an abiogenetic origin of life, but to allow of this possibility only in the distant past. In the months immediately following Huxley’s BAAS address that Darwin’s famous remark finally gave a more explicit and concrete basis for this argument:

It is often said that all the conditions for the first production of living organisms are now present, which could ever have been present. But if (and oh what a big if) one could conceive in some warm little pond with the right amounts of ammonia and phosphoric salts, – light, heat, electricity, etc. present, thus a protein compound was chemically formed, ready to undergo itself such complex changes, at the present day such matter would be instantly devoured or absorbed, which would not have been the case before living creatures had formed.¹⁰³

Bastian’s relatively young career handicapped him in such a dispute among scientific titans, But his strategic naivete was just as important a source of weakness. He had cut himself off from a significant traditional support base in the spontaneous generation community, the “molecularists” as Stirling called them, by accepting the physicists’ claim to Brownian “molecules” and their characterization as “fantastic” of any views that the movement was vital. By simultaneously trying to be a good Darwinian evolutionist and Huxleyan physicalist *and* to vocally support the possibility of present day spontaneous generation and butt heads with Tyndall, Bastian was also alienating the X Club, perhaps not sufficiently grasping its intense desire to be “respectable.” Thus, despite his own widely acknowledged oratorical skills, he was soon cut off from those under the influence of the X, and had also burned the Brownian movement bridge behind him. Bastian did attempt to respond to Huxley’s presidential pronouncements in letters to *Nature* written over a period of a month after the 1870 Liverpool BAAS meeting. He raised many of the issues that had previously been raised in private with Huxley, and accused Huxley of pretending to have always had clear views that “abiogenesis” was not possible, when not six month previously Huxley had observed Bastian’s work with

the belief that it might well be proving the opposite.¹⁰⁴ His tone was sharp in response to Huxley's public accusations that his technique was sloppy (a much more high-powered attack than Huxley ever adopted in private when attempting to correct young scientists). Huxley replied with an equally sharp tone, now saying sweepingly that "what Bastian got out of his tubes was exactly what he put into them," i.e. contaminants. And privately Huxley wrote to Norman Lockyer, editor of *Nature*, to inquire:

I have been obliged much against my will to take notice of Bastian's "Reply" – What was his reason for going out of his way to be so offensive? He knew exactly what I thought about his work and therefore must have known that in my judgement the kindest thing I could do was to be silent about him.¹⁰⁵

Bastian's Defenders

Bastian clearly did not think that Huxley's public attack at the BAAS constituted a strategy of "being silent about him." And his supporters in the medical community were not daunted by Huxley's attempts to declare him scientific *persona non grata*. In addition, a significant minority faction of Darwinian scientific naturalists continued to support Bastian (or at least to consider the issue still open) for many years after this. No matter how seriously he might attack or undermine the man, Huxley could not get rid of Bastian's case for spontaneous generation being integral to evolution. Many reviews of the controversy sympathetic to scientific naturalism continued to accuse Huxley and Tyndall of trying to avoid the uncomfortable materialist consequences of their own work. A reviewer in the medical journal *Practitioner*, for instance, wrote:

It need hardly be said to readers of the *Practitioner*, that in rejecting all theories of life that imply the existence of any special vital force essentially different from the physical forces of the universe, Dr. Bastian has our entire sympathies. . . . We believe him to be right in saying that the present anomalous position of a few prominent physiologists . . . is due to an unwarrantable shrinking from what they suppose is a career which must land them in the regions of atheism and materialism. . . . [Thus,] the brunt of conflict which Dr. Bastian will have to sustain . . . is not with the dwindling sect of vitalistic biologists, but with the far larger and more influential section of scientific men [i.e. Tyndall and Huxley] who candidly acknowledge the substantial identity of the forces that originate life with those of the physical world, and yet cannot bring themselves to think it even possible that living things should originate from not living

matter. . . . Once the first shock of the idea is got over, however, . . . the reader will find far less to astonish him in the . . . evidence produced by Bastian . . . to an extent that no previous Darwinian philosopher has attempted to prove.¹⁰⁶

The *British and Foreign Medical-Chirurgical Review* added:

We regard the alleged refutation of the probability of heterogenesis, made by Professor Huxley at the Liverpool meeting, as entirely unsatisfactory, based, as it was, upon not a single cited original experiment. The advocates of spontaneous generation have a right to demand a demonstration of the impossibility of their statements, instead of a mere allegation of their improbability. Dr. Bastian has certainly carried out a long series of experiments . . . and the character both of his experiments and of his views is such as to call for a serious re-examination and discussion.¹⁰⁷

Not everyone at the time was swayed by Huxley's dazzling rhetorical skills.

Publishers Youmans and Macmillan also strongly supported Bastian's right to continue the debate with Huxley, and they published Bastian's first book as well as his 1872 two-volume magnum opus *The Beginnings of Life*. Huxley and the X Club began to feel concern that *Nature* was slipping away from them. Now less than a year after the journal's origin, Lockyer and Macmillan's desire to keep it impartial, and thus open to dissenting arguments like Bastian's, threatened to make it a center of divisiveness among the evolutionists rather than the voice Huxley wanted under orthodox Darwinian editorial control.¹⁰⁸

Despite Huxley's position, the highly respected evolutionist Prof. Jeffries Wyman of Harvard, agreed with the criticisms of Spencer's inconsistency over spontaneous generation.¹⁰⁹ Wyman had himself done widely known experiments on the subject in the 1860s, and when he learned of Bastian's experiments on a visit to London in August 1870,¹¹⁰ he read Bastian's papers on the subject. He then wrote to encourage the younger man to pick up where he (Wyman) had left off in attempts to solve the spontaneous generation problem experimentally:

I am fully aware of the incompleteness of my experiments and how I lived in the hope of making new ones. But since reading your own results I have thought it far wiser to leave to others the battle. . . . My primary standpoint is this: if there ever was a time when organic life did *not* exist on the surface of the earth, the transition to the period when it *did* exist was through spontaneous generation. If the question is approached from a scientific point of view I see no other alternative. The experimental proof

may be slowly completed, but I believe the cumulative evidence in favor of it is becoming day by day stronger.¹¹¹

Other Darwinians also remained convinced that spontaneous generation was necessary along with evolution, for any naturalistic science to be consistent and for continuity in nature not to be violated. Rev. Thomas Stebbing, for instance, published just such an argument early in 1871 in his *Essays on Darwinism*.¹¹²

Even more importantly, Alfred Russel Wallace was so persuaded by Bastian's evidence and powerfully argued case, that he glowingly reviewed Bastian's best known book, *The Beginnings of Life*, when it appeared in 1872, pointing out especially Bastian's argument that continuous creation of new microbial life and rapid transformations by heterogenesis could greatly speed up the rate at which evolutionary change occurs. This, he suggested, could provide an answer to the stultifying challenge of William Thomson's claim that earth had not existed in a cooled state long enough to allow the amount of time required by Darwin in *The Origin of Species*, for evolution to have occurred.¹¹³ Wallace's review appeared in *Nature* and went on to say that one of Bastian's most persuasive points was that a single origin of life in the distant past violated the uniformitarian continuity in nature so fundamental to the theory of evolution.¹¹⁴ Charles Darwin himself carefully read *The Beginnings of Life* at the suggestion of Wallace. His comments on the book reflect a deep ambivalence over the issue of spontaneous generation and thus bear quoting at some length. In 1872, Darwin concluded that Bastian

seems to me an extremely able man, as indeed, I thought when I read his first essay. His general argument in favour of Archebiosis is wonderfully strong, though I cannot think much of some few of his arguments. The result is that I am bewildered and astonished by his statements, but am not convinced, though on the whole, it seems to me probable that Archebiosis is true. I am not convinced partly I think owing to the deductive cast of much of his reasoning; and I know not why, but I never feel convinced by deduction, even in the case of H. Spencer's writings. If Dr. B's book had been turned upside down, and he had begun with the various cases of heterogenesis, and then gone on to organic and afterwards to saline solutions, and had then given his general arguments, I should have been, I believe, much more influenced. I suspect however that my chief difficulty is the effect of old convictions being stereotyped on my brain. . . . Perhaps the mere reiteration of the statements given by Dr. B. by other men whose judgment I respect and who have worked long on the lower organisms would suffice to convince me. Here is a fine confession of intellectual weakness; but what an inexplicable frame of mind is that of

belief! . . . I should like to live to see archebiosis proved true, for it would be a discovery of transcendent importance. . . . If ever proved, Dr. B. will have taken a prominent part in the work. How grand is the onward rush of science; it is enough to console us for the many errors we have committed and for our efforts being overlaid and forgotten in the mass of new facts and new views which are daily turning up.¹¹⁵

Wallace replied

I quite understand your frame of mind and I think it a natural and proper one. You had hard work to hammer your views into people's heads at first, and if Bastian's theory is true he will have still harder work, because the facts he appeals to are themselves so difficult to establish.¹¹⁶

Edward Youmans was reinforced in his high opinion of Bastian by Wallace's strong support. He sought from Macmillan the rights for an American edition of Bastian's forthcoming book *The Beginnings of Life*.¹¹⁷ He later ran a review of *The Beginnings of Life* in the November 1872 *Popular Science Monthly* that echoed Wallace's enthusiasm and cited Wallace's opinion as proof that spontaneous generation was fully compatible with evolution.¹¹⁸ Youmans also continued to cordially encourage Bastian in his efforts in the spring of 1874, printing Bastian's argument in installments in the *Popular Science Monthly*.¹¹⁹ He wrote Bastian, encouraging him to keep up a critique that the publisher saw as healthy for intellectual debate within the Darwinian community.¹²⁰ As late as 1875, Youmans still felt strongly enough about Bastian's scientific credentials to run a feature on him as "Scientist of the Month" in the *Popular Science Monthly*,¹²¹ to sign him on, along with Tyndall and Huxley, to write a book for the new International Scientific Series, and to pay him an advance of £100.¹²² And when Tyndall stepped up his attack on Bastian's scientific reputation in public, Youmans seems to have urged him to behave in a more respectful fashion toward Bastian, regardless of their differences.¹²³

A useful indicator of the prevalence of positive assessments of Bastian is the comment in the *Annual Register* at year's end in 1872:

The subject which stands out pre-eminently this year as riveting the attention of men of science, and producing wonder in the minds of those who have but to take the results of investigation and analysis as they are propounded by the skilled experimentalist, is the spontaneous generation doctrine advocated, and it is said all but established, by Dr. Bastian. . . . Whether or not Dr. Bastian's statements of fact are all capable of verification, it seems to be generally admitted that a great stride had been made in biological science by his investigations, and that a further

elucidation has been attained of that unity and continuity of Nature's laws which is so marked a result of modern scientific research.¹²⁴

Thus it is clear that many evolutionists and supporters of that cause saw Bastian's version of scientific naturalism *with* spontaneous generation as an equally valid competitor to the X Club's version without it – perhaps as having an even better claim to be the version most compatible with the doctrine of continuity. Further, Bastian's scientific credentials were widely viewed as more than sufficiently strong to entitle him to challenge Huxley, Tyndall and Spencer as peers.

Similarly, in medical circles, Bastian's reputation continued to grow based on his clinical work as well as his scientific work. In 1870 he had been elected to fellowship in the Royal College of Physicians.¹²⁵ William Sharpey continued to speak highly of Bastian's work, especially among those sanitarian reformers skeptical of a too-simplistic germ theory.¹²⁶ Bastian was courted to write numerous articles on neurological and pathological subjects, including bacteria and the germ theory of disease, for Richard Quain's new *Dictionary of Medicine*.¹²⁷

Thus, within a year of publicly entering the British debate, Bastian had become the leading figure supporting spontaneous generation, with previous heterogenists Owen, Child, Wyman and Bennett having withdrawn to much more peripheral roles by the end of 1870. Bastian's stature in the medical and laboratory science communities, and in the evolutionary "young guard" gave the doctrine a level of publicity previously unheard of in Britain. The same year saw the consolidation of a new vocal opposition as well. Because of his high profile, Bastian was the lightning rod upon which most of this reaction was discharged.

The Younger Generation of Scientific Naturalists

Once a considerable level of animosity developed between Bastian and the orthodox X Club Darwinians, it is not surprising to see that some of the younger evolutionary scientists such as Anton Dohrn and William Thistleton-Dyer soon got the message that Bastian-style evolutionism was incompatible with orthodox Darwinism. In an article in the *Quarterly Journal of Microscopic Science* on 1 October 1870, hot on the heels of the Liverpool BAAS meeting, Dyer wrote

The interval which the evolutionist is modestly content to conceive deductively bridged, is an nothing to the leaping powers of the so-called heterogenist who boldly widens the gap and passes easily from ammonium tartrate to a Penicillium. . . . A believer in spontaneous generation is

not really an evolutionist, but is only a vitalist minus the supernatural; the special creation which the one assumes is replaced by the fortuitous concurrence of atoms of the other.¹²⁸

The sharply negative rhetoric spreading against Bastian among the younger evolutionists is also clear in an 1872 letter from Dohrn to Darwin. Dohrn had already written Darwin about Wallace's "sad falling away," over several issues, including human evolution.¹²⁹ Now a new letter was prompted by Wallace's favorable review of Bastian in *Nature*:

Poor Wallace completely drifts away, and now most unfortunately associates himself with such a man as Bastian! His two articles in *Nature* are the worst thing he ever did in his life, – and it becomes really difficult for his friends to speak with respect of him.¹³⁰

Some younger evolutionists agreed with Bastian, provoking attempts to convert them from that view. In New York, Columbia College president Frederick Barnard wrote in 1873 that, to him, spontaneous generation and the correlation of the physical and vital forces were two doctrines relatively inseparable from evolution.¹³¹ He noted, however, that Bastian's success in convincing him of this connection was a source of real intellectual distress to him, just the kind of distress that Huxley was trying to help supporters of evolution avoid, by such rhetorical moves as creating the term "agnostic" to describe a neutral position on religion. Barnard was rather more explicit about his distress than was consistent with the larger project of Huxley and the X Club; his exceptional frankness bears quoting at some length:

We are told, indeed, that the acceptance of these views need not shake our faith in the existence of an Almighty Creator. It is beautifully explained to us how they ought to give us more elevated and more worthy conceptions of the modes by which He works His will in the visible creation. We learn that our complex organisms are none the less the work of His hands because they have been evolved by an infinite series of changes from microscopic gemmules, and that these gemmules themselves have taken on their forms under the influence of the physical forces of light and heat and attraction acting on brute mineral matter. Rather it should seem we are a good deal more so. This kind of teaching is heard in our day even from the theologians [Stebbing, Kingsley]. . . . It is indeed a grand conception which regards the Deity as conducting the work of His creation by means of those all-pervading influences which we call the forces of nature; but it leaves us profoundly at a loss to explain the wisdom or the benevolence which brings every day into life such myriads of sentient and intelligent beings, only that they may perish on the morrow of their

birth. But this in not all. If these doctrines are true, all talk of creation or methods of creation becomes absurdity; for just as certainly as they are true, God himself is impossible. . . . But we are told it is unphilosophical, in the pursuit of truth, to concern ourselves about consequences. . . . To this canon I am willing to subscribe up to a certain point. But if, in my study of nature, I find the belief forced upon me that my own conscious spirit . . . is but a mere vapor, which appeareth for a little time and then vanisheth away forever, that is a truth . . . for which I shall never thank the science which has taught it me. Much as I love truth in the abstract, I love my hope of immortality still more; and if the final outcome of all the boasted discoveries of modern science is to disclose to men that they are more evanescent than the shadow of the swallow's wing upon the lake, . . . give me then, I pray, no more science. Let me live on, in my simple ignorance, as my fathers lived before me, and when I shall at length be summoned to my final repose, let me still be able to fold the drapery of my couch about me, and lie down to pleasant, even though they be deceitful dreams.¹³²

Spontaneous generation seemed to bring home to many evolutionists the stark materialist implications of what Huxley had called "the physical basis of life."

Tyndall, after reading Barnard's article, wrote to urge him that no such drastic intellectual suicide was necessary. He pursued the strategy developed by Huxley: sever the linkage by attacking Bastian's credibility as an experimenter:

I am not surprised to find you attaching so much importance to Bastian's work. Still there is not a man of my acquaintance of any scientific weight, and I number among my acquaintances many who know Bastian's calibre and method of work, who attach any importance to his results. All his more startling ones are to be ascribed to the fact that a man undisciplined in experiment has taken up a subject which requires for its treatment the most consummate experimental tact.¹³³

It is interesting to contrast this with Tyndall's positive assessment of Bastian *before* the younger man had publicly confronted him and before Huxley had changed his mind about Bastian's technical skill.¹³⁴ In Tyndall's statements, as with other foes with whom he clashed publicly, his opponent became increasingly demonized as time passed.

Methodist minister and biologist William Dallinger was another young supporter of Darwin, who, like Barnard, was trying to prevent conflict between evolutionary doctrine and moderate religious views. Dallinger belonged to the Christian Evidence Society, a group of "moderate and evangelical churchmen . . . who were not identified with either the ritualist or the

rationalist extremes of the Church of England,” and who sought defense of the faith against doubt and atheism by seeking evidence in support of Christianity. The Presence of such men as Dallinger among its ranks “helped to ensure that the Society could not be fairly charged with being hostile to new discoveries or to speculations concerning the origins of the universe.”¹³⁵ Dallinger took an active role in looking for evidence of “life cycles” among the protozoa or “monads,” since this could powerfully undermine spontaneous generation claims for microbes just as it had for parasitic worms shown to develop through many stages dissimilar in appearance to one another. At the height of the 1870s spontaneous generation controversy in Britain, he and a friend, John Drysdale of Liverpool, published a series of articles in the *Monthly Microscopical Journal* that influentially made just that argument.¹³⁶ These articles were widely cited by Huxley and Tyndall as evidence against Bastian.

Harvard philosophy professor John Fiske, whose admiration for Tyndall, Huxley, and especially Spencer verged on worship,¹³⁷ at first believed that spontaneous generation was still experimentally an open question. In late 1873, while visiting the scientific lights of London, he wrote of Bastian and Huxley in a tone that lends Bastian as much authority as Huxley:

It is perfectly in keeping, for example, for two upholders of the Doctrine of Evolution, as well as for two scientific specialists committed to no general doctrine, to hold opposite views concerning the hypothesis of spontaneous generation. Since this is a strictly scientific hypothesis, . . . and since there is no reason . . . why it should not sooner or later be established or overthrown by some crucial experiment; there is nothing anomalous in the fact of two such thoroughly scientific evolutionists as Prof. Huxley and Dr. Bastian holding opposite opinions as to its merits.¹³⁸

By late 1875 at the latest, however, Fiske, too, had gotten the message that evolution and spontaneous generation were not to be presented as compatible. His new book completed in February 1876 had a noticeably cooler treatment of Bastian.¹³⁹

Henry Lawson of St. Mary’s Hospital Medical School was described above as an early and vocal supporter of heterogenesis in general and of Bastian in particular. As editor of the weekly *Scientific Opinion* from its beginning in November 1868 until June 1870, of the new *Monthly Microscopical Journal* from January 1869 until his death in 1877, and of *Popular Science Review*, which he took over in January 1869,¹⁴⁰ Lawson wrote frequent editorials in support of Darwinism, pangenesis, and heterogenesis, which he argued was a necessary corollary to evolution. Lawson’s jabs at the

orthodox Darwinians, for their resistance to making the logical jump from evolution to spontaneous generation, have been cited above. Although these journals targeted a popular, mostly middle class audience, it was precisely the audience that was more and more merging with the group that actually practiced science professionally as a result of the long efforts of Huxley and others.¹⁴¹ Lawson and his journals, then, had been a big thorn in Huxley's side.

However, Lawson, like the other young Darwinians, also reversed himself and fell into line within a year or two. In an October 1871 editorial he was already beginning to question Bastian's claims, with no mention at all of his previous enthusiastic support.¹⁴² And over the next few years Lawson's tone became more and more directly opposed to Bastian in all the journals he edited. Indeed, until his untimely death in October 1877 at age 37, his journals became an important venue for the publication of new evidence from Bastian's opponents and steady editorial criticism of Bastian. By April of 1875, Lawson was reviewing Bastian's latest book, *Evolution and the Origin of Life*, and he now voiced many of the same criticisms as Huxley, William Dallinger and others, especially that bacteria might produce "germs" that could survive temperatures much greater than the adult bacteria ever could. "Dr. Bastian has received many hard blows in this controversy," Lawson noted.¹⁴³

Bastian, Huxley and the Royal Society

Despite his initial difficulty getting a presentation of his views on the calendar, Bastian had not given up on the Royal Society, and as the spontaneous generation controversy became public and heated, he applied for a research grant in the fall of 1870 that Sharpey as Secretary passed on to the Society Council for him.¹⁴⁴ Bastian was eventually able to schedule the reading of several of his experimental papers at the Thursday evening meetings of the Royal Society¹⁴⁵ and all of these papers were quickly accepted, by vote of the Society, for publication in the *Proceedings*.¹⁴⁶

In 1872 when Huxley replaced Sharpey as the Secretary of the Royal Society responsible for biological sciences, Bastian's reception became more problematic.¹⁴⁷ After Bastian's paper of 30 January 1873, Bastian wrote to Huxley in that capacity to ask for the insertion of a footnote to his paper, prior to its printing in the *Proceedings of the Royal Society*. Huxley cordially agreed.¹⁴⁸ However, by the time of Bastian's paper of 1 May, Huxley had begun to exercise the Secretary's prerogative to edit papers for the *Proceedings* in a way that Bastian found less to his taste.¹⁴⁹ At the reading of the paper to the Society, Huxley as Secretary requested the removal of a note

from Bastian's MS prior to its publication. The following morning Bastian replied that he had no objection, but went on to request

At the same time, for the mere sake of the principle involved I should be very glad if you will kindly inform me to what extent it is permissible for one of the officers of the Society to alter any communication after it has been read. . . . I must say it seems to me rather important that there should be a clear understanding on this subject . . .¹⁵⁰

This suggests that Bastian already considered the possibility that Huxley would use his position unfairly and was trying diplomatically to secure against that possibility. In a series of letters exchanged over the next two months, Bastian continued to press his point that as he understood scientific decorum, especially in such an official forum as the Royal Society of London, what had actually been read aloud to the public meeting of the Society must remain inviolate in print unless the author consented to changes.

Huxley did not see this principle as sacred, and asserted greater latitude upon the part of Society officers (such as himself) as a matter that was of greater importance to the Society.¹⁵¹ This of course confirmed Bastian's suspicions, since according to his view of fair procedure this must appear to violate a cardinal principle of respect for another's work, implying possible intent to alter his published words by one who had, since late 1870, publicly declared himself Bastian's bitter opponent on the subject of the article. Not surprisingly, Bastian became still more guarded and formalistic in his tone so that relations were likely to deteriorate even if Huxley's intent was not at all malicious. For Bastian, that was beside the point: no one but the author should have the right to judge whether a given editorial change did not "affect any statement or fact, nor weaken any argument" he had used. In his next letter he said he had received the revise of his article, but not the proof from which it was made up (Bastian's own original MS, presumably), which he wished to compare with the revise to see exactly how great the changes had been. When he inquired of the sender, Bastian was told that the proof had been sent to be preserved in the Society Archives. Again stressing his point of principle, he asked Huxley, "since without it I cannot readily ascertain what omissions you wish or what modifications you think desirable. As the paper has already been read at the Society, I suppose I am to have some voice as to any proposed alterations."¹⁵² A note in Huxley's hand on the margin of the letter indicates that Huxley directed the proof to be sent to Bastian and had written to tell him so. Both men, while sticking to their different points of principle, seemed to want to avoid having them clash with one another.

Once having seen the extent of Huxley's revisions, Bastian offered to assent to some that, he agreed, might be seen as language not sufficiently

genteel. He felt, however, that some of the alterations were for more heavy-handed reasons. As he put it: "I am anxious to meet the wishes of the Council – but whilst quite willing to make modifications in the direction indicated, I can scarcely suppose that the Council of the Royal Society would wish to suppress any show of independent opinion." He marked on the revise some changes and re-insertions with which he would be satisfied.¹⁵³ Bastian urged that these were really all the alterations he felt he could fairly be called upon to make, and hoped Huxley would agree that he was showing every sincere desire to find a mutually acceptable solution. Huxley's reply has not been preserved, but he seems to have balked, for Bastian's next letter is still trying to persuade him that the fact that passages were actually aloud to the public meeting of the Society ought to guarantee those passages' safety from editorial tampering without the author's permission. He retained his confidence that the Council of the Royal Society would definitely agree with him, wishing to avoid any appearance that they might be suppressing dissenting opinion within their ranks. He urged more bluntly than ever that if "whole paragraphs or qualifying phrases may be blotted out by the editor . . . even in the absence of any intrinsic unsuitability in the passages themselves. . . . This, . . . I venture to think, would involve a great departure from existing rules." But Bastian did not feel that all hope of respectful interchange was gone. He closed by repeating that, "I hope when you find that the passages marked . . . were actually read to the Society, you will not still think it necessary to cancel them, in opposition to my own wishes."¹⁵⁴

Huxley continued to wrangle over the question of whether the passages actually had been read, the two men comparing differing recollections on this subject until it became almost comic. Finally Bastian gave up hope of avoiding a confrontation and begged Huxley to lay the matter before the Council of the Society directly:

I can scarcely think that the Council will insist upon the removal of the words and passages in question. . . . Should the Council desire to impose restrictions against expressions of opinion of this kind, I, of course, must bow to their decision and consent to the publication of my paper in the curtailed form. . . . I am therefore quite content that its publication should stand over until the Council shall have come to their decision upon this subject . . .¹⁵⁵

Bastian seems to have had faith in his interpretation of the rules of fair play, and in the Council's desire to avoid negative publicity. But he was also hedging by trying to guarantee that if he was to be censored, at least it would have to be done by an official act, so that other members of the Society would know that suppression against his wishes had occurred and could happen to

any of them as well. Huxley, on the other hand, seems to have had no doubt that the Council would fully support his position in the matter. The conflict must be viewed in the context of the X Club's consolidation of power in the Royal Society during these years, so thoroughly explicated by Ruth Barton. Since the X Club had at least three members on the Council at all times during the 1870s (five during 1873)¹⁵⁶ and already exerted wide influence in Society matters by the time of the dispute, Huxley could rely upon a friendly jury for the dispute and knew this from the beginning, as Bastian may not have. Perhaps more importantly, Secretaries of the Royal Society during the nineteenth century were in general granted enormous latitude by the Council and thus had considerable personal power. Huxley was no exception.¹⁵⁷ Thus Huxley graciously agreed to Bastian's suggestion. Perhaps he had been trying to handle things without such an embarrassing defeat for Bastian.

In the event, the Council fully supported Huxley. Further, they did not even admit to seeing Bastian's concerns as reasonable ones. They advised Huxley to communicate to Bastian "That the Council see no reason to interfere with the action which has been taken by the officer in the matter of Dr. Bastian's paper." Huxley offered an olive branch to the loser:

I hope that you will believe that nothing but a sense of my duty to the Royal Society has led me to exercise my editorial functions in a way which, I fear, had been disagreeable to you and that I have done my best to avoid the suggestion of any omission which would really tend to weaken the strength of your arguments . . .¹⁵⁸

Nonetheless, it was very clear to Bastian that the Royal Society was no longer a fully supportive – nor in his view fully objective – forum for his scientific papers to be presented. He began to explore other possible venues for his views, for instance the Pathological Society of London.

Conclusion

The X Club Darwinians had attained positions of power and become the London scientific establishment since 1870. This gave them an important advantage over Bastian in controlling his access to at least some important platforms for presentation of his views. And this strategy was to prove more and more effective through the mid-1870s, until by 1878, well before the acceptance of the germ theory of disease among a majority in science or medicine, Bastian's reputation was so damaged that he was essentially forced to withdraw from public debate on spontaneous generation.¹⁵⁹ He remained an important figure on the London Medical scene, being promoted to full Professor at University College Medical School in 1878, and continuing to

publish respected work in neurology for two full decades more. But the linkage between spontaneous generation and evolution was effectively destroyed by Huxley and Tyndall. When Huxley and his students wrote the next generation of biology textbooks, they wrote as if Bastian and that linkage had never existed. The success of their version of the story can be gauged by the amount of historical work required here, to reconstruct the fact that a camp of Darwinian spontaneous generation advocates not only *existed* but was a serious intellectual force in the 1870s, as different “Darwinisms” struggled among themselves for the survival of the fittest.

The in-fighting among Darwinian factions over spontaneous generation significantly shaped the discourse on this subject in biology. Most importantly, once and for all the debate forced acceptance of a linkage between evolution and some kind of naturalistic explanation of the origin of life. However, Huxley’s new term “abiogenesis” encapsulated a new assumption that, along with the term, came to dominate until the present day: that such an event could only have happened in the earth’s distant past. By differentiating “abiogenesis,” which included this qualifying proviso, from “spontaneous generation,” Huxley finally succeeded in getting all the amateur science and radical political implications of that earlier doctrine, from around the neck of “Darwinism” as we have since come to know it. Furthermore, the importance of differentiating Brownian movement from true living movement became, in Huxley’s hands, a weapon for the defeat of Bastian and “archebiosis,” despite the fact that the very distinction at issue was first used by Bastian himself in support of archebiosis. Huxley’s success cut off from the history of spontaneous generation disputes a previously significant related discourse: that of Brown’s “active molecules,” later called “histological molecules,” observations of which were seen to support the possibility of heterogenesis.

As noted at the start, I do not intend to suggest that the experiments themselves were unimportant in the debate. The “duelling experiments” narrative of spontaneous generation debates has been well told before, best of all by John Farley. Those experiments alone, however, were not sufficiently persuasive to determine the final marginalization of Bastian. What I have shown here is that the social context of the Darwinian scientific naturalists is also a crucial part of the story and that the spontaneous generation debates of the 1870s in Britain are misunderstood if they are seen only as a debate over experiments, or as a struggle between Darwinian science and outsiders.

Acknowledgments

I am grateful for permission to quote from the following archival collections: the Huxley papers at Imperial College London; Tyndall papers at the Royal Institution; J.D. Hooker papers at Royal Botanical Gardens, Kew; Wyman

papers at Countway Library of Medicine; Wilder papers at Cornell University; the Wellcome Institute London, University College Archives, London; the Royal Society; the Royal College of Physicians; Columbia University, New York; the American Philosophical Society; Macmillan papers at the British Library; and the Owen papers at the British Museum of Natural History. Much of the research was done under a National Science Foundation dissertation improvement grant, and much of the writing under a Dibner Institute postdoctoral fellowship, for both of which I am deeply indebted. I would like to thank Gerry Geison, Lynn Nyhart, William Bynum, Adrian Desmond, James Moore, Ruth Barton, Bernard Lightman, Susan Goldsmith, and two anonymous reviewers for reading and commenting on earlier drafts of this paper.

Notes

1. See for example the description of Bastian in Adrian Desmond and James Moore, *Darwin* (New York: Warner, 1992), pp. 594–595. Desmond has since moved considerably in the direction of the interpretation I present here. See, e.g. his biography *Huxley: From Devil's Disciple to Evolution's High Priest* (Reading, Mass.: Addison-Wesley, 1997), pp. 392–393.
2. Roy MacLeod, "The X Club," *Notes and Records of the Royal Society* **24** (1970): 305–322; and Ruth Barton, "'An Influential Set of Chaps': the X-Club and Royal Society Politics 1864–1885," *Brit. J. Hist. Sci.* **23** (1990): 53–81.
3. Bastian, *Evolution and the Origin of Life* (London: Macmillan, 1874).
4. See also the controversy over Andrew Crosse's spontaneous generation experiments, in James Secord, "Extraordinary Experiment: Electricity and the Creation of Life in Victorian England," in D. Gooding, T. Pinch, and S. Schaffer, eds., *The Uses of Experiment* (Cambridge Univ. Press, 1989).
5. Desmond, *The Politics of Evolution* (Chicago: Univ. of Chicago Press, 1989); also Desmond and Moore, *Darwin, op cit.*, pp. 40–41, 223–224.
6. John Farley, *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore: Johns Hopkins Univ. Press, 1977), esp. ch. 7.
7. Strick, "Purity and Contamination: John Tyndall, H. Charlton Bastian, Louis Pasteur and Spontaneous Generation, 1870–1878," in Bernardo Fantini and Jed Buchwald, eds., *Pasteur, Germs and the Bacteriological Laboratory* (Cambridge, Mass.: MIT Press, forthcoming).
8. The attempt is for as fully detailed a portrait of Bastian as possible, given the lack of preservation of his personal papers. After two years of searching finally tracked down surviving heirs, I ascertained that those papers were lost and/or sold to collectors over the years. The number of Bastian letters I have located in various extant collections (Huxley, Lockyer, Macmillan, Owen, Tyndall, Wyman, Wellcome, Univ. College, Royal Society, Linnean Society, Amer. Phil. Society, etc.) probably represents the most complete sample ever assembled to date.
9. Edward Youmans, "Sketch of Dr. H.C. Bastian," *Pop. Sci. Monthly* **8** (1875–1876): 108–10, p. 109.

10. Mercer Rang, "Henry Charlton Bastian, 1837–1915," *Univ. Coll. Hospital Mag.* **39** (1954): 68–73, p. 68.
11. *Idem*, *The Life and Work of Henry Charlton Bastian*, unpub. ms., 1954, in University College Medical Library, p. 4; Bastian to (Sharpey?), 8/6/1863, UC Library, College Correspondence file, unsorted; Youmans, "Sketch" *op cit.*, p. 109.
12. Sir John Meyer to Bastian, 1/5/66, Wellcome Institute London, autograph letters collection.
13. Zachary Cope, *The History of St. Mary's Hospital Medical School* (London: Wm. Heinemann, 1954), pp. 85, 124, 139.
14. *Ibid.*, pp. 168–169, See also W.H. Brock, "Patronage and Publishing: Journals of Microscopy, 1839–1989," *J. Microscopy* **155** (1989): 249–266, p. 253 and R. Barton, "Just before *Nature*: The Purposes of Science and the Purposes of Popularization in Some English Popular Science Journals in the 1860s," *Ann. Sci.* **55** (1998): 1–33.
15. F. Sibson to Grayson Orme, 4/28/66, Wellcome Institute London, autograph letters collection.
16. Huxley Reviewers Report, RR.6.18, Royal Society Archives.
17. Busk Rev. Report, RR.6.37, Royal Society Archives.
18. Bastian, "On the Anatomy and Physiology of the Nematoids, Parasitic and Free," *Phil. Trans. Roy. Soc.* **156** (1866): 545–624.
19. Glaishier was President of the RMS from 1866–1870. See G. Turner, "The Origins of the Royal Microscopical Society," *J. Microscopy* **155** (1989): 235–248, pp. 246–247.
20. Certificate of a Candidate for Election for H.C. Bastian, Royal Society Archives.
21. Indeed, Roy Macleod lists Bastian as a solid member of that young guard in "Seeds of Competition," *Nature* **224** (1969): 431–434, p. 431; see also Gordon Holmes, *The National Hospital Queen Square, 1860–1948* (London: E. & S. Livingstone, 1954), p. 39.
22. Bastian to UCL, 11/15/67, UCL Archives, College Corresp. Coll., unsorted.
23. Holmes, *op cit.*, pp. 38–39; on Russell Reynolds, see p. 36.
24. J. Russell Reynolds, ed., *A System of Medicine* (London: Macmillan, 1868). Bastian contributed in vol. II the descriptions of pathology and morbid anatomy for pp. 413–429 "Congestion of the Brain," pp. 430–433 "Cerebritis," pp. 434–477 "Softening of the Brain," and pp. 478–503 "Adventitious Products in the Brain."
25. T. Buzzard to W. Gowers, 12/6/1905, Royal College of Physicians, autograph letters, Buzzard collection #23. On Buzzard, see Holmes, *op cit.*, p. 37.
26. On Bastian's ongoing neurological research, see "On the Various Forms of Loss of Speech in Cerebral Disease," *Brit. For. Med.-Chir. Rev.* **43** (1869): 209–236, 470–492; "Sensation and Perception," *Nature* **1** (1869–1870): 213–214, 309–311; and "Consciousness," *J. Mental Sci.* **15** (1869–1870): 501–523.
27. Bastian, *On the "Muscular Sense" and on the Physiology of Thinking* (London: H.K. Lewis, 1869).
28. See *Beginnings of Life* (London: Macmillan, 1872), I: 35–47. On psychophysical parallelism as a constitutive element in scientific naturalism see Frank Turner, *Between Science and Religion* (New Haven: Yale Univ. Press, 1974), p. 15.
29. W. Sharpey to Bastian, 4/24/68, Wellcome Institute London, autograph letters collection. This research resulted in a note on "Passage of Red Blood Corpuscles Through the Walls of the Capillaries in Mechanical Congestion" in the *British Medical Journal* **1** (1868): 425–426.
30. Bastian, "Mounting and Tinting of Sections of Animal Tissues," read to Royal Microscopical Society 1/13/69, published in *Monthly Micr. J.* **1** (Feb. 1869), and *Sci. Opin.* **1** (1869): 330–331.

31. A.E. Durham, "President's Address, Delivered at the Annual Meeting, July 24th, 1868," *J. Quekett Micr. Club* **1** (1868–1869): 95–110, quote on p. 110.
32. Technical note: Pouchet was a believer in heterogenesis, but not in the more radical doctrine of abiogenesis or archebiosis. Henry Charlton Bastian believed in both; Huxley and John Tyndall in neither, at least not in either occurring during present times.
33. For Huxley, see his *Lectures to Workingmen* (London, 1863); for Darwin, see Darwin to Henry Holland 1/15/62 and Holland's reply, 4/62 (Darwin Papers, American Philosophical Society Library, Philadelphia, hereafter APS).
34. John Farley and Gerald Geison, "Science, Politics and Spontaneous Generation in Nineteenth Century France: the Pasteur-Pouchet Debate," *Bull. Hist. Med.* **48** (1974): 161–198.
35. See, e.g., Robert Grant, *Recent Zoology* (London: Walton and Maberly, 1861), pp. 5–6, 9.
36. Quoted in *Life and Letters of Charles Darwin* (London: Macmillan, 1887), II: 188. For Grant's radical political interests, see Desmond, *Politics, op cit.*
37. Huxley, "On Some Organisms Living at Great Depths in the North Atlantic Ocean," *Q. J. Micr. Sci.* **8** (1868): 203–212. For Haeckel's reaction going much further than Huxley, see P. Rehbock, "Huxley, Haeckel and the Oceanographers: the Case of *Bathypus haeckelii*," *Isis* **66** (1975): 504–533.
38. Friedrich Lange reported this in his very popular *Geschichte des Materialismus*, as did other authors. See E. Haeckel, "Beiträge zur Plastidientheorie," *Jenaische Zeitschr.* **5** (1870): 499–519, p. 500, quoted in F. Lange, *History of Materialism* (New York: Harcourt and Brace, 1877), II: 20n.
39. See, e.g. L.S. Beale, "Protoplasm and Living Matter," *Monthly Micr. J.* **1** (1869): 277–288.
40. On Macmillan's crucial role, see R. Macleod, "Macmillan and the Young Guard," *Nature* **224** (1969): 435–461. On Youmans, see a collection of essays edited by him arguing the importance of science education, *Modern Culture: Its True Aims and Requirements* (London: Macmillan, 1867).
41. See E. Fiske, *The Letters of John Fiske* (New York: Macmillan, 1940), pp. 109–111.
42. F.E. Abbot, "Review of *The Principles of Biology* by Herbert Spencer," *North Amer. Rev.* **107** (1868): 377–422.
43. Spencer to Youmans, 3/69, *Life and Letters of Herbert Spencer* (New York: Appleton, 1908), I: 190.
44. Youmans to Spencer, 3/69, *Ibid.* I: 190–191. Spencer's rebuttal, "On Alleged 'Spontaneous Generation' and on the Hypothesis of Physiological Units," written 12/5/68, was eventually published as an Appendix to the next (1870) edition of *Principles of Biology*, I: 479–492.
45. J.E. Strick, *The British Spontaneous Generation Debates of 1860–1880: Medicine, Evolution and Laboratory Science in the Victorian Context*, Ph.D. dissertation, Princeton University, January 1997. Forthcoming as a book from Harvard University Press.
46. Spencer, "On Alleged," *op cit.*, p. 479.
47. James Moore, "Deconstructing Darwinism: the Politics of Evolution in the 1860s," *J. Hist. Biol.* **24** (1991): 353–408, pp. 386–387.
48. George H. Lewes, "Mr. Darwin's Hypothesis," *Fort. Rev. n.s.* **3** (1868): 353–373, quote on p. 357n.
49. Alison Adam, *Spontaneous Generation in the 1870s: Victorian Scientific Naturalism and its Relationship to Medicine*, Ph.D. dissertation, Sheffield-Hallam University, Jan. 1988, pp. 200–202.
50. [Bastian], "The Doctrine of the Correlation of the Vital and Physical Forces," *BMJ* **1** (1869): 50–51; "Vital Functions and Vital Structures," *Ibid.* pp. 288–289; and the seven

- part “The Origin of Life,” *Ibid.*, pp. 312–313, 569–570 and *BMJ* 2 (1869): 157–158, 214–215, 271–272, 473–474, 665. These all appeared in a revised form in 1872 as part 1 of volume 1 of Bastian’s best known book, *The Beginnings of Life*.
51. John Browning, “On the Correlation Between Microscopic Physiology and Microscopic Physics,” printed by Lawson in both *Month. Micr. J.* 2 (1869): 15–21 and *Sci. Opinion* 2 (1869): 347–348.
 52. A.E. Durham, “Presidential Address of July 23rd, 1869,” *J. Quekett Micr. Club* 1 (1868–1869): 240–248, esp. pp. 244–247.
 53. The two men had just served together as joint Secretaries to the Medical Section at the annual meeting of the British Medical Association in Leeds in August 1869. See H.D. Rolleston, *The Right Honourable Sir Thomas Clifford Allbutt: A Memoir* (London: Macmillan, 1929), p. 43.
 54. Allbutt to Bastian, 12/10/69, Wellcome Institute London, autograph letters collection.
 55. Lawson, *Scientific Opinion* 1 (1869): 483.
 56. See Gerald Geison, “The Protoplasmic Theory of Life and the Vitalist–Mechanist Debate,” *Isis* 60 (1969): 273–292. J.H. Stirling gave a lecture “As Regards Protoplasm,” severely criticizing Huxley’s “Physical Basis” on 4/30/69. It was published as a separate monograph in 10/69 and reprinted repeatedly.
 57. Stirling to Bastian, 11/6/69, Wellcome Institute London, autograph letters collection.
 58. H.C. Bastian, “Protoplasm,” *Nature* 1 (24 Feb. 1870): 424–426, esp. p. 426.
 59. See e.g. *Brit. Med. J.* 2 (10 Dec. 1870): 632, for a sample of the conflict seen by the actors themselves as a professional turf battle.
 60. I discuss in considerably greater detail elsewhere the relationship between Bastian’s and Tyndall’s ideas on the cause of disease and their views on spontaneous generation. See J.E. Strick, “Purity,” *op cit.* See also the papers in the same volume by William Bynum and Mick Worboys, and a paper by Terrie Romano “The Cattle Plague of 1865 and the Reception of ‘The Germ Theory’ in Mid-Victorian Britain,” *J. Hist. Med. Allied Sci.* 52 (1997): 51–80.
 61. Macmillan to Bastian, 1/7/70, Macmillan letterbook, Ms 55390, British Library.
 62. Bastian, “Facts and Reasonings Concerning the Heterogeneous Evolution of Living Things,” *Nature* 2 (1870): 170n.
 63. Macmillan to Bastian, 1/3/70, Macmillan letterbook, Ms 55390; *Nature* 1: 253 (title page of 6 Jan. issue), (20 Jan., p. 301), (17 Feb., p. 398), (3 March, p. 450), (10 March, p. 474), (28 Apr., p. 668).
 64. Bastian to Huxley, 5/2 and 5/12/70, Huxley papers, Imperial College Archives, 10: 238–239.
 65. Bastian, *Facts and Reasonings, op cit.*, p. 175.
 66. Tyndall to Bastian, 1/25/70, Tyndall papers, Royal Institution Archives, T6/C10, tss. p. 149.
 67. See, e.g. Huxley to Hooker, 8/10/70, Huxley papers, 2: 163.
 68. Huxley to Dohrn, 7/4/68, Huxley papers 13: 166–167. The expression comes from the play *Henry V*, V-I: 10, “. . . look you now, *of no merits*, he is come to me, and prings me pread and salt yesterday, look you, and bid me *eat my leek!*” My thanks to John Dettloff for help in locating this passage.
 69. Huxley to Foster, 8/11/75, Royal Coll. Phys., Autograph letters collection.
 70. *Life and letters of Thomas Henry Huxley* (London: Macmillan, 1900), hereafter *LLTHH*, II: 5.
 71. Foster to Huxley, 9/14/79, Huxley papers 4: 214–215.
 72. Huxley to Dohrn, 4/30/70, Huxley papers 13: 174.

73. Tyndall to Bastian, 1/25/70; Letters between Macmillan and Bastian, 1/3/70, 1/5, 1/7, 2/10, Macmillan letterbook, Ms. 55390, British Library.
74. X Club notebook, Tyndall papers 4/B8, on meeting of 2/3/70. Anon., "The Atmospheric Germ Theory," *Nature* **1** (1870): 351.
75. *LLTHH*, I: 355.
76. Macmillan to Lionel Beale, 4/21/70, Macmillan letterbook, Ms. 55390 (2): 891, British Library.
77. Joseph Hooker in particular thought this such a bad strategy that he had advised even Darwin to avoid carrying out such disputes in public. Hooker to Darwin, 5/63, *Life and Letters of Sir Joseph Dalton Hooker* (London: Macmillan, 1918), II: 51.
78. Bastian to Huxley, 5/2/70, Huxley papers 10: 238. On Frankland, see Colin Russell, *Edward Frankland: Chemistry, Controversy and Conspiracy in Victorian England* (Cambridge: Cambridge U. Press, 1996), esp. chapters 11 and 13.
79. *Ibid.*, Huxley's note on the margin of Bastian's letter, indicating the content of his reply.
80. Bastian to Huxley, 5/12/70, Huxley papers 10: 239.
81. Huxley to Dohrn, 4/30/70, Huxley papers 13: 174 (*LLTHH*, I: 357–358).
82. Hooker to Darwin, 7/7/70, Darwin papers, APS.
83. Darwin to Hooker, 7/12/70, *More Letters of Charles Darwin* (London: Macmillan, 1903), I: 321–322.
84. Huxley to Hooker, 8/10/70, Huxley papers 2: 163.
85. T.H. Huxley, "Biogenesis and Abiogenesis," pp. 232–274 in *Discourses Biological and Geological* (London: Appleton & Co., 1925).
86. Huxley, "On the Relations of Penicillium, Torula and Bacteria," delivered 9/20/70 to BAAS mtg. in Liverpool, pub. six weeks later in *Q. J. Micr. Sci.* **10** (1870): 355–362.
87. See my Ph.D. dissertation, cited above, esp. Chapters 2 and 5; also Philip Sloan, "Darwin, Vital Matter, and the Transformism of Species," *J. Hist. Biol.* **19** (1986): 369–445.
88. John B. Dancer, "Remarks on Molecular Activity as Shown Under the Microscope," *Proc. Lit. Phil. Soc. Manchester* **7** (1868): 162–164, p. 164.
89. Christian Wiener, "Erklärung des Atomistischen Wesens des Tropfbar-flüssigen Körperzustandes, und Bestätigung Desselben durch die Sogenannten Molekularbewegung," *Ann. d. Physik* **118** (1863): 79–94.
90. Dancer, *op cit.*, pp. 162–163.
91. *Life and Letters of Michael Faraday* (London: Longmans, Green, 1870), I: 403–404; William S. Jevons, "On the So-Called Molecular Movements of Microscopic Particles," *Sci. Opin.* **2** (1869): 155; full text in *Proc. Manchester Lit. Phil. Soc.* **9** (ser. 3, 1870): 78–84.
92. Anon., "The British Association," *Nature* **2** (22 Sept. 1870): 408–410, and *Lancet* **2** (8 Oct. 1870): 514.
93. Huxley, "Penicillium," *op cit.*, pp. 358–359.
94. Bastian's argument was that the organisms in his tubes were alive because they multiplied, not because they showed movement.
95. The passage that gave Bastian priority for this distinction was in his article "Facts and Reasonings . . .," *op cit.*, p. 171. See Stephen Brush, "Brownian Movement from Brown to Perrin," *Arch. Hist. Exact Sci.* **5** (1968): 1–36, esp. p. 6.
96. Bastian, *The Beginnings of Life*, *op cit.*, I: 319. Bastian seems to have gotten the date wrong on which Huxley's paper was given, first publicly raising this issue.
97. *Ibid.*
98. See H.C. Bastian, *The Modes of Origin of Lowest Organisms* (London: Macmillan, 1871), pp. xi–xii.

99. Michael Foster, *A Text Book of Physiology* (London: Macmillan & Co., 1877), p. 2n.
100. See, e.g. anon. review of *Beginnings of Life* in the medical journal *The Practitioner* **9** (1872): 291–294.
101. Anon., “Bastian on Lowest Organisms,” *Brit. For. Med.-Chi. Rev.* **49** (1872): 450.
102. Huxley, “Biogenesis and Abiogenesis,” *op cit.*, pp. 258–260.
103. Darwin to Hooker, 2/1/71, Darwin papers, APS Library, Philadelphia.
104. Bastian, *Nature* **2** (1870): 411–413, 431–434, 473, 492.
105. Huxley to Lockyer, 10/8/70, Huxley papers 21: 252–253.
106. Anon., “Review of *The Beginnings of Life*,” *Practitioner* **9** (1872): 291–294.
107. Anon., “Bastian on Lowest Organisms,” *Brit. For. Med.-Chir. Rev.* **49** (1872): 448–451, p. 451.
108. Peter Bowler, “Darwinism in Britain and America,” in D. Kohn, ed. *The Darwinian Heritage* (Princeton: Princeton U. Press, 1985), p. 668.
109. Wyman to Burt Wilder, 11/25/69, Wilder papers, Box 1, Cornell University Archives.
110. Wyman to Wilder, 10/16/70, Wilder papers, Box 1.
111. Wyman to Bastian, 10/21/70, APS Library, 509 L56.42, misfiled under “Bartram.” I am indebted to Toby Appel for this reference and the preceding two. For more on Wyman’s overall support of evolution and his stance on spontaneous generation, see Toby Appel, “Jeffries Wyman, Philosophical Anatomy, and the Scientific Reception of Darwin in America,” *J. Hist. Biol.* **21** (1988): 69–94.
112. Thomas R.R. Stebbing, “Note on the Hypothesis of Spontaneous Generation,” pp. 126–146 in his *Essays on Darwinism* (London: Longmans, Green, 1871), pp. 127–128.
113. Wallace, “Review of *The Beginnings of Life*,” *Nature* **6** (1872): 284–287, 299–303; Wallace to Darwin, 8/4/72, in Marchant, ed., *Alfred Russel Wallace: Life and Reminiscences*, I: 224–225. On Thomson’s argument about the cooling of the earth, see Joe Burchfield, *Lord Kelvin and the Age of the Earth* (Chicago: Univ. of Chicago Press, 1990).
114. Wallace, “Review,” *op cit.*, p. 303.
115. Darwin to Wallace, 8/28/72, in Marchant, ed., *op cit.*, I: 225–226; an expurgated version is in *Life and Letters of Charles Darwin*, III: 168–169. Darwin expressed himself similarly to Haeckel on 9/2/72 (Darwin papers, APS Library). See also Darwin’s copious notes in his copy of the book, in M.A. DiGregorio, ed., *Charles Darwin’s Marginalia* (New York: Garland, 1990), pp. 34–35.
116. Wallace to Darwin, 8/31/72, Marchant, ed., *op cit.*, I: 226.
117. See Macmillan to Youmans, 2/1/72, Macmillan papers, Ms. 55392(1), p. 164, British Library.
118. *Popular Science Monthly* **2** (1872): 83–93, esp. pp. 91–93.
119. Bastian, “Evolution and the Origin of Life,” *Pop. Sci. Month.* **4** (1874): 713–728.
120. Youmans to Bastian, 4/16/74, Wellcome Institute London, autograph letters collection.
121. Youmans, “Sketch of Dr. H.C. Bastian,” *Pop. Sci. Month.* **8** (Nov. 1875): 108–110.
122. Personal communication 10/15/1994 & 2/5/96 from Michael Collie. See also Bastian to Richard Owen, 10/28/76, Owen papers, BMNH 2: 324–326. The book *The Brain as an Organ of Mind* was published in 1880.
123. J. Fiske, *Edward Livingston Youmans: Interpreter of Science for the People* (New York: Appleton, 1885), p. 320.
124. *Annual Register 1872*, p. 368, cited in J. Friday, “A Microscopic Incident in a Monumental Struggle: Huxley and Antibiosis in 1875,” *Brit. J. Hist. Sci.* **7** (1974): 61–71.
125. *Munk’s Roll*, v. 1, pp. 174–175.
126. Sharpey to Bastian, 11/16/71, Wellcome Institute London, autograph letters collection.

127. Bastian, "Bacteria," "Germs of Disease," "Aphasia" in *Quain's Dictionary*. Bastian's articles on these topics continued to be included even up to the 1890 edition of the *Dictionary*.
128. William Thistleton-Dyer, "On Spontaneous Generation and Evolution," *Quart. J. Micr. Sci.* **10** (1870): 333–354, quote on p. 335.
129. Darwin to Dohrn, 2/3/72, *The Naples Zoological Station at the Time of Anton Dohrn*, Christiane Groeben, ed. (Woods Hole, Mass., 1980), pp. 84–85.
130. Dohrn to Darwin, 8/21/72, Christiane Groeben, ed., *Charles Darwin – Anton Dohrn Correspondence* (Naples: Macchiaroli, 1982), pp. 40–41.
131. Frederick A.P. Barnard, "The Germ Theory of Disease and Its Relations to Hygiene," *Public Health Reports and Papers* **1** (1873): 70–87; p. 79.
132. *Ibid.*, pp. 79–80. It is interesting that this passage was one of very few excerpted from a bowdlerized version of Barnard's essay published in Gert Brieger, ed., *Medical America in the Nineteenth Century* (Baltimore: Johns Hopkins, 1972), pp. 278–292; passage expurgated on p. 286.
133. Tyndall to Barnard, 12/10/73, X mss. collection, X973/c72/f, Columbia University Library. I am indebted to Michael Collie for this reference.
134. For Tyndall's initial respect for Bastian's technical talent, see Tyndall to Bastian, 1/25/70, Tyndall papers, T6/C10, tss. p. 149.
135. Dale A. Johnson, "Popular Apologetics in Late Victorian England: the Work of the Christian Evidence Society," *J. Religious Hist.* **11** (1981): 558–577, pp. 558, 559. See also J.W. Haas, "Late Victorian Gentlemen of Science: William Dallinger and the Wesley Scientific Society," paper presented 9/13/94 at Royal Institution, London.
136. See obituary on Dallinger in *Proc. Roy. Soc. London*, **B 82** (1910): iv–vi. The influential articles appeared as "Investigation into the Life History of the Monads," *Month. Micr. J.* **10** (1873): 53–58, 245–249; **11**(1874): 7–10; **13**(1875): 190–198.
137. See, e.g., Fiske's letters to his wife of Dec. 1873 and Jan. 1874 in E. Fiske, ed., *The Letters of John Fiske* (New York: Macmillan, 1940), pp. 270–301.
138. J. Fiske, *Outlines of Cosmic Philosophy, Based on the Doctrine of Evolution* (London: Macmillan, 1875), I: 129.
139. John Fiske, *The Unseen World* (Boston: Houghton Mifflin, 1876), p. 49.
140. William H. Brock, "Patronage and Publishing," *op cit.*, p. 253.
141. Ruth Barton, "Just before *Nature*," *op cit.*
142. Lawson, "Spontaneous Generation," *Pop. Sci. Rev.* **10** (1871): 416–417.
143. [Lawson] "On Spontaneous Generation," *Pop. Sci. Rev.* **14** (1875): 184–185.
144. 10/17 and 11/2/70 Bastian to Sharpey, RS, MC.9.129 and 9.133; Bastian to Wyman, 11/11/70, Wyman papers, Countway Library of Medicine, Boston.
145. These included, "On Some Heterogenetic Modes of Origin of Flagellated Monads, Fungus Germs, and Ciliated Infusoria," *Proceedings of the Royal Society* **20** (1872): 239–264; also "Note on the Origin of Bacteria, and on their Relation to the Process of Putrefaction," *Proceedings of the Royal Society* **21**: 129–131.
146. Bastian's next papers were "On the Temperature at Which *Bacteria, Vibriones*, and their Supposed Germs are Killed when Exposed to Heat in a Moist State, and on the Causes of Putrefaction and Fermentation," *Proc. Roy. Soc.* **21**(1873): 224–232; and "Further Observations on the Temperature at Which . . .," *Ibid.*, 325–338. It should be noted, however, that publication in the *Proceedings* was not nearly as restricted and therefore prestigious, as publication in the *Philosophical Transactions*, which required favorable reports by two FRS referees chosen confidentially by the Council.

147. Marie Boas Hall, *All Scientists Now: the Royal Society in the Nineteenth Century* (Cambridge: Cambridge U. Press, 1984), p. 136.
148. Bastian to Huxley, 1/31/73, MC.9.484, Royal Society (hereafter RS) Archives.
149. Secretaries of the Royal Society exercised enormous powers over many such matters, including the awarding of research grants. See Boas Hall, *op cit.*, pp. 135–136, and Roy M. MacLeod, “The Royal Society and the Government Grant: Notes on the Administration of Scientific Research, 1849–1914,” *Historical J.* **14** (1971): 323–358, esp. pp. 339–340, 343, 348–350.
150. Bastian to Huxley, 5/2/73, MC.9.543, RS Archives.
151. Huxley to Bastian, 5/5/73, MC.9.545, RS Archives.
152. Bastian to Huxley, 5/11/73, MC.9.551, RS Archives.
153. Bastian to Huxley, 5/14/73, MC.9.553, RS Archives.
154. Bastian to Huxley, 5/18/73, MC.9.555, RS Archives.
155. Bastian to Huxley, 5/19/73, MC.9.557, RS Archives.
156. Ruth Barton, “Influential Set,” *op cit.*
157. MacLeod, “Royal Society,” *op cit.*, p. 343.
158. Huxley to Bastian, 6/19/73, MC.9.587, RS Archives.
159. I explore in another paper the further clashes of Bastian at the Society with Huxley and Tyndall, during 1876 and 1877. See “Purity and Contamination,” *op cit.*