Robert Brown and Brownian Movement: Radicalism, Spontaneous Generation and Microscopy in Nineteenth Century England

by James Strick, Program in History of Science Princeton University

John Turberville Needham, in a 1748 paper which later became well known for suggesting the possibility of spontaneous generation, described seeing the production of small particles released and moving actively around in water in which seeds or other vegetable matter had been infused. These

Bodies were seen to move in a manner very different from Atoms in a fermenting liquid, and yet not so seemingly spontaneous as microscopical Animalcules,...(so that) it must be so; that these were detached organical Parts, and that the Seeds, and particularly the Germs of Seeds in Plants, must necessarily abound with them more than any other substances. (1748, p.634)

Needham worked on these experiments partly with Georges-Louis de Buffon, since Buffon was interested in the details of reproduction of plants for the text he was writing on natural history, while Needham desired "to discover which among these moving Bodies were strictly to be looked upon as Animals, and which to be accounted mere Machines." (ibid, p.634).

In the end, Buffon and Needham explained the phenomena by slightly different theories. Buffon (1797, pp.313-19) invoked a dynamic view of nature and organic materials, continually breaking down upon death into 'organic molecules' common to both plants and animals. These were then able to be taken up by other organisms and incorporated or, alternatively, to group together to form sperm and eggs (hence the reference above to why they would be found more concentrated in seeds or organs of generation); or even to form entire organisms by spontaneous generation, in the case of microscopic organisms and intestinal worms. Buffon thus argued strongly against 'univocal' generation, in which an organism must always be produced directly by a parent of the same species, suggesting that 'equivocal' generation occurs just as often. Needham invoked a 'vegetative force' instead of organic molecules. He also argued for univocal generation in higher organisms, but both theories agreed that equivocal generation could occur in microscopical animals which were, he claimed "a class apart; and their greatest characteristic is, that they neither are generated...or generate in the ordinary way." (1748, p.657; see Farley 1977, pp.23-25 for discussion of Needham and Buffon's ideas). Shirley Roe, in her analysis of Needham's position, points out that the term"equivocal implied chance origins, (and) was used to designate theories permitting the accidental emergence of life out of materials such as decomposing flesh or heated mud...In Needham's day, most people were opposed to the idea of

equivocal generation." Roe highlights the nature of this opposition: "That chance could play a role in the production of living organisms, at any level, provided clear grounds, opponents argued, for materialism and atheism." (1983, p.171-172).

Needham was aware that this charge might result; particularly in response to his portrayal of matter as self-active. Thus, even as early as in his 1748 paper, he addressed a lengthy conclusion to the president of the Royal Society, defending himself against such a charge and explaining why he saw the need to offer such a defense:

Not that I imagined that either you, or any of the Gentlemen of the learned *Society* in which you preside, would think my Principles in any way tending to Materialism, from which no one can be more distant or averse than myself...But I was willing to guard against the Misapprehension of others less acquainted with Matters of this sort, and into whose hands this paper might come, and have therefore taken these Precautions. (1748, p.665)

Needham denied that his was a theory of equivocal generation in later years and tried to explain away the appearance that he had argued in his earlier publications that this could be true of even microscopic organisms; for, if he was astute enough to realize the potential danger-by-association of material-ism with arguments about spontaneous generation, the developments of the remainder of the eighteenth century fully vindicated his caution. Needham's work, along with the generative powers discovered in the polyp *Hydra* by Abraham Trembley, was used by the philosophes Diderot and d'Holbach as evidence for a new, extreme form of materialism, in which

spontaneous generation seemed to validate the broadest claims of the philosophes' thoughtthat the causes of all development must be sought for inside, not outside of nature...The impact of such...views...was indirect, resulting in the association of the doctrine of spontaneous generation with materialism, atheism, and political radicalism to a much greater extent than before. *As a result...attacks upon spontaneous generation became a central tenet of the Christian faith...*the doctrine also became associated with...the horrors of the French Revolution. (Farley 1977, pp.28-29, italics in original).

In England, where the political reaction against the Revolution was very pronounced, this association had a powerful effect on British reception of any work which included sympathy for spontaneous generation. J.B. Lamarck's transmutationist theory (1984), in which spontaneous generation played a significant role, was a good example of this: it was severely attacked in scientific circles, with so few willing to defend it in public that they were exceptions which proved the rule (Secord 1991).

This paper will show that the discovery of 'active molecules' by the Scottish botanist Robert Brown in 1827 had a reception very much influenced by this context, and will argue that Brown faced choices in some ways similar to those of Needham with, however, even more at stake both personally and politically.

I. Brown's Education and Background to Early Career

Brown was born in 1773, the son of Bishop James Brown of the Episcopal Church of Scotland. His father was one of a tiny minority of those bishops so strongly opposed to the English King that they refused to pledge allegiance to him, even when the heir to the Stuarts (the historically favored choice) in 1788 was Henry, a cardinal of the Roman church. Living in the household of a man so politically aware and strongly committed to principles must have produced a greater than average awareness in Robert of the political consequences which could follow from unorthodox ideas. Throughout his childhood, for instance, his father was forbidden under penal laws from ever addressing more than four people at once in any one room! Though in later years Robert was thought to be a nonbeliever, the political power of the Anglican Church orthodoxy must have made an early and indelible impression. (Mabberley 1985, pp.15-18). As Ramsbottom expressed it: "The stern uprightness *and diplomacy* of the father were inherited to the full by the son." (1932, p.18; my italics); or "Robert retained his father's intellectual honesty, but lost his uncompromising religious faith." (Stearn 1970, p.517).

From age thirteen to sixteen, Robert attended Marischal College in Aberdeen, where a number of the faculty had formed strongly negative opinions of Buffon's and Needham's work. particularly their ideas on equivocal generation (Wood 1987, pp.177-181). Curriculum reform there and also at King's College in Aberdeen in the 1750s specifically increased the amount of instruction given in mathematics and the natural sciences, and faculty from the two met with interested men in the community in a number of clubs to discuss moral and natural philosophy, among other things. A prominent figure in these circles until he moved to Glasgow in 1764 was Thomas Reid. In his lectures Reid criticized Buffon's ideas as materialistic on numerous grounds, having a great interest in systematics and other areas of natural history himself. He and his contemporary John Stewart, Edinburgh Professor of Natural Philosophy felt that Buffon and Needham had "carried the activity of matter to the highest pitch." (quoted in ibid, p.177). Though these public statements began in the 1750s, Reid was still making them at least into the late 80s, when Robert Brown was imbibing the academic atmosphere of Scotland. Marischal College had a tradition in botany (Mabberley 1985, p.18), and Brown had an interest in botany early on, and would likely have attended closely to these debates, e.g. Reid attempting to substitute a nonmaterialist theory of generation, which he first outlined in the mid-1770s; it was important in an acceptable alternative that the power of organization not be intrinsic to matter itself:

Reid postulated the existence of various species of 'organized atoms', which contained in embryonic form the rudiments of the plants and animals into which they would mature. Once they found their proper matrix, these organized atoms were, he claimed, united to a 'vital principle' which initiated the process of growth...Reid thus allowed that an immaterial principle superadded to matter was involved in generation, but he was emphatic that the initial organization of the atoms was the work of God alone. (Wood

1987, p.177).

By the time of Brown's university studies at Marischal, 1787-90, with the departure of Reid, the death of Stewart and others "the Aberdeen Enlightenment was transformed" with at least one natural history professor beginning to cite Buffon as an authority and teach his works in class, though "the question of the Frenchman's materialism clearly continued to exercise his Aberdonian readers." (ibid, pp.184-85).

When James Brown moved to Edinburgh late in 1790, Robert moved with the family and began medical studies at the University there, continuing through 1793, though his father died in 1791. Robert continued living in the city with his mother until 1795. Edinburgh during this period was a noted center of activity among parliamentary reformers, whose ranks were swelling with "new working-class members of the metropolitan and provincial radical societies." (Goodwin 1979, p.22). The French Revolution had focused the atten-tion of such groups "on the surviving relics of feudalism in Britain--the game laws, the tithe system, the Scottish feudal land laws...In doing so it pointed the way to a radical reform of Parliament..." (ibid, p.23). Although in their demands, these groups still did not suggest infringing on royal powers or on the constitutional position of the House of Lords, the fact that the reform movement had shifted more into the hands of urban and working-class radicals was perceived as threatening and subversive enough by the conservative reaction; e.g. followers of Edmund Burke. One such occasion was when

In December 1792 representatives from eighty Scottish reform societies assembled in the city of Edinburgh at the Convention and demanded universal suffrage and annual parliaments. When a young advocate, Thomas Muir, who was prominent in the Friends of the People in the Glasgow area, suggested that the Convention should accept an address from the Society of United Irishmen in Dublin and make its stance republican and nationalist, the Convention refused his lead and stressed that its aims were a peaceful reform of Westminster. (Thomis and Holt 1977, pp.9-10).

Agitation by these groups for a national convention of elected representatives, to adopt constitutional reforms, was seen as threatening in light of the aftermath of the 1789 assembly in France. Despite such claims of moderation from the majority in the reform movement, it was seen as a seedbed for the spawning of more radical demands. "These enemies of the reformers now questioned their motives and methods and depicted them as anarchists, Levellers, atheists, and in general as the ignorant dupes of French republican propaganda...criticisms of the established church by Priestley...were easily misrepresented...as 'subversive' of the constitution." (Goodwin 1979, p.25)

Thus, the previous associations of natural philosophic discourse with political ideas became still further strengthened during this period. Other prominent English natural philosophers who fell into greater suspicion and disrepute with conservatives included Erasmus Darwin and chemist cum medical reformer Thomas Beddoes. Golinski describes the manner in which to some extent an Enlightenment model of public science was able to hold its own in such a political climate, sheltering in the circles of enlightened provincial intellectuals such as the remaining Lunar Society of Birmingham members (1992, p. 158). The London scientific establishment, especially the Royal Society under the presidency of Joseph Banks, remained firmly allied with the conservative political and orthodox Anglican establishment, which it perceived as its main 'audience', throughout the 1790s, shunning and isolating the likes of Priestley¹ and Beddoes from 1791 on (ibid, p.69). Banks's project to consolidate and centralize all scientific communication, patronage and authority under the Royal Society and thus his own personal control, had begun immediately in 1778 when he assumed the presidency (ibid, p.124). His power and influence continued to grow and solidify in an unbroken manner until shortly before his death in 1820, as he retained the presidency of the Royal Society over that entire span of time. Banks's power and patronage assumed an even larger role in Brown's career than for most British scientists of the time, as will be described below.

The political climate of Edinburgh grew steadily more polarized during the entire time Brown continued to live there. The British Convention held there in November 1793

although short-lived was, in reality, a landmark in the history of British radicalism. It was organized by the Scottish societies...who now dissociated themselves from their parent society in London and committed themselves to the full program of radical reform...The attempted summons of an English Convention in the spring of 1794 followed directly from the dissolution of the Edinburgh convention and the consequent radical confrontation with the government occurred, at least partly on the recommendation of the English delegates imprisoned at Edinburgh. The challenge was readily accepted by the government as a means of stopping the radical movement in its tracks and demonstrating to the country, now in danger of invasion,...the reality and extent of the radical threat to established institutions and social order...the real motivation of the state trials of 1794 was political. (Goodwin 1979, pp.26-27).

The Irish radical nationalist risings were met with force. Newly raised regiments of Fencible infantry were trained and sent to Ireland to restore and maintain order and to resist any possible French landing there. Brown joined the Fifeshire regiment in late 1794 and was sent to Ireland from 1795-1800. He saw little action however, and spent much time botanically collecting local flora.

¹With regard to the subject of spontaneous generation, Priestley's position was highly ironic. Though in agreement with so many of the politically suspect philosophical positions of the other men mentioned, he actually quickly reversed his initial belief in spontaneous generation in 1779 and afterwards opposed those such as G.R. Treviranus, Jan Ingenhousz, and Erasmus Darwin, who attempted to use his experimental findings to continue justifying such claims (see Farley 1977, p.43-4 and Abrahams 1964). In 1803, in response to Darwin's *Temple of Nature*, Priestly wrote that such a belief in equivocal generation "is unquestionably atheism. For if one part of the system of nature does not require an intelligent cause, neither does any other part, or the whole." (1809, p.120-1).

As Brown's interest in botany deepened, his thoughts on making a career of it would invariably have led him to realize the importance of Joseph Banks. In addition to his powerful position as president of the Royal Society, Banks had made his reputation as the botanist on one of Captain Cook's expedition and had, at Soho Square, perhaps the most extensive personal botanical library and herbarium in Britain by the 1790s. Though Brown's diary was extremely detailed, he began keeping it only after he had already managed to make Banks's acquaintance, so that exactly how he managed this is unclear. It is clear however, that beginning immediately from the time he was first able to visit Banks's library while on leave in London in 1798, he actively lobbied to meet Banks personally and to be appointed by him as naturalist on an expedition being prepared for New Holland. He succeeded in getting offered the appointment on 17 December 1800 and promptly packed, sold all unnecessary belongings, and made a bee-line for Soho Square to call on Banks (Ramsbottom 1932, pp.19-20) and to put himself at the service of "the most powerful patron of science in Britain" (Golinski 1992, p.124). Brown's service on this voyage was the making of his scientific career, much as Darwin's Beagle was for him. For the next thirty years, Brown was to manage his connection with Banks (and with Banks's estate after the older man's death) with an extremely shrewd approach: "his admitted business ability...He was very careful about his own position," gradually securing for himself access to the nation's largest herbarium collections, to a secure legacy of an annuity income and the lease of Banks's house in Soho Square, and eventually his project to "make a stronghold for Botany in the British Museum." (Ramsbottom 1932, p.32).

Brown also was very circumspect in matters scientific. His published works were models of painstaking attention to detail in plant anatomy, and his knowledge of the plant kingdom was encyclopedic. He argued persuasively for a 'natural' system of plant taxonomy such as that of de Jussieu, to replace the Linnean system, but avoided any conclusion of transmutationism which Lamarck was just at this time drawing from zoological taxonomy. According to Desmond and Moore, the political danger of these ideas was as great in the late 1820s as it had been in the 1790s: such "godless evolutionary talk was anathema in conservative Britain...In the long Tory-dominated years after the Napoleonic Wars, pedantic specialization and description were safer occupations for a savant." (1991, p.35). Materialist ideas generally were still extremely charged, even in Edinburgh, where the young Charles Darwin attended a meeting of the Plinian Society in 1827 at which materialism was discussed, only to provoke such outrage that the entire record of it was stricken from the minutes of the meeting. Such typical conservative reactions came from all quarters of the land:

The staunch *Quarterly Review* wanted it legally suppressed...In an undemocratic age, with Church and state striving to keep order, the reaction was shrill. Nor was it surprising: the Cato Street conspiracy, a plan to assassinate the Cabinet

in 1820 and proclaim a democracy, was still fresh in the authorities' minds. (ibid, pp.38-39).

The shrillness was bound to increase to a fever pitch by 1828-9, when the pressure on the Anglican orthodoxy finally built up to a level sufficient to snap its monopolistic hold: parliament "passed bills allowing Dissenters and Catholics to hold public office for the first time in centuries." (ibid, p.74). By 1832 this was followed politically by the First Reform Act in parliament. Hole stresses the unity of the edifice whose bastions which were crumbling:

Much of the (historical) work...on the reform of the constitution has over-emphasised its secular nature; 1831-2 has been divorced from 1828 and 1829, and Repeal and Emancipation, when they were noticed much at all, were seen as part of that Liberal Toryism which preceded the Whig reforms. An over-concentration on working-class agitators...has led to the 1832 Reform Act being explained almost exclusively in terms of industrialization and class-consciousness which, while meaningful and relevant to the secular values of late-twentieth century society, distorts its full contemporary context. (1989, p.238).

It is likely that Brown, seeing what he had, and now having reached a quite comfortable and promising place in the patronage system, could plainly enough read this handwriting on the wall. In 1819, he was offered the Chair of Botany at Edinburgh, and tempted with the additional provision of directorship of the new Botanic Garden there which would make the post extremely attractive. He objected that he was not a medical man, and the University promptly offered him a medical degree to remove that obstacle. Then, perhaps more importantly, he cited "the extreme delicacy of my situation here...I think you will agree that I ought not to propose entirely and suddenly to leave Sir Joseph Banks." (quoted in Ramsbottom 1932, p.33). Banks's failing health was a factor, but so too, surely, was Brown's estimate of what he stood to lose in terms of inheritance from his patron. Indeed, when Banks died in 1820, Brown was legally declared Keeper of his collections and library until his own decease (at which time they would go to the British Museum) as well as provided with an annuity of L 200 for life, as mentioned above. I do not wish here to insist on a major role for conscious strategies of concealment in Brown's career, but rather to clearly establish the intellectual context in which he worked and that he was keenly aware of the political connotations of scientific ideas; indeed, given his upbringing alone, it may

7

not make sense to view such caution as a conscious strategy isolated in any sense from his entire

personality, which Asa Gray described as follows:

He is very fond of gossip at his own fireside, and amused us extremely with his dry wit, but in company he is silent and reserved. I have found also that it does not do to ask him any question directly about plants. He is...the driest pump imaginable. (quoted in ibid, p.35).

In this light, it is interesting to consider Charles Darwin's comment that Brown was

"remarkable for the minuteness of his observations and their perfect accuracy," but that he kept

most of his vast knowlege to himself, as Darwin speculated, "owing to his excessive fear of ever

making a mistake." Darwin goes on to relate an episode, probably just prior to publication of

Brown's (1831) discovery of the cell nucleus and of cytoplasmic streaming:

He poured out his knowledge to me in the most unreserved manner, yet was strangely jealous on some points...on one occasion he asked me to look through a microscope and describe what I saw. This I did, and believe now that it was the marvellous currents of protoplasm in some vegetable cell. I then asked him what I had seen; but he answered me, who was then hardly more than a boy and on the point of leaving England for five years, "That is my little secret." I suppose that he was afraid that I might steal his discovery. (Darwin 1958a, pp.103-4).

Even accounting for the hazards of accepting such a retrospective account, recalled almost 50 years after the fact and possibly embellished in light of later events, such remarks might still be taken as suggestive of a personality concerned with a calculated approach to publicly releasing speculative ideas of a new or possibly controversial nature.

In the mid-1820s, possibly wishing to improve his financial position (Mabberley 1985,

p.261), Brown entered into negotiations with the British Museum to turn over the collections of

Banks, with himself as Keeper to be paid a salary. During the period of negotiating

Whigs and radicals, including many of Brown's acquaintance, notably James Mill, proposed founding (London University)...In March 1827, an offer was made to Brown...of the Chair of Botany...Brown went...to explain the difficulties of Banks's codicil, by which, if he took the chair, his annuity would cease...Paradoxically, in offering Brown the Chair, the radical new university was playing into the hands of the establishment, for the Museum would have got Banks's library free. (ibid, pp.262-4)

Brown kept the Museum officials in the bargaining, holding out for more money and an assistant, to top the salary offered by the University and hoping perhaps to work in the less radical surroundings; however, he kept the University offer on the line in the meantime. After driving the bargain he wanted with the Museum in September 1827, he accepted that position and, now

secure for life, allowed the University Chair to go up for grabs. (It was soon filled by John Lindley, then still in his twenties, after W.J. Hooker also passed it up.) (ibid, pp.264-5.)

II. The Discovery of Active Molecules

Brown had carried out microscopic studies at about this time on the unfertilized ovule in flowers. Meanwhile in France in December 1826, Adolphe Brongniart reported on extremely detailed and careful microscopic studies he had carried out on pollen grains, using a new Amici compound microscope capable of magnifying up to 630x. Brongniart took a particular interest in tiny microscopic granules within the grains, which he thought might be the 'organic molecules' discussed by Buffon in the 1740s. Two observations seemed to him to discount this possibility however: the granules varied in size from one species to another, and such motion as they exhibited seemed to Brongniart not to be spontaneous, but to be due to temperature (1827). Brown, having seen only an abstract of Brongniart's results, in June 1827 began his own microscopic work on pollen grains, using a simple microscope capable of magnifying about 320x. Brown (1828) found that while observing pollen grains suspended in water, even under the best observing conditions these grains were never still, but vibrated slightly and irregularly. Brown first convinced himself that the motion was not caused by the vibration of his equipment nor by convection currents nor by evaporation of the liquid. He observed the motion in drops of water suspended in oil, completely preventing evaporation, and saw that the motion continued for many days, whereas differences in temperature would have equalized themselves in a fairly short time. Among the pollen grains Brown observed smaller particles, roughly spherical, which showed a more active vibration than the larger grains. These he called 'active molecules'. He also saw them in water suspensions of other parts of many other plants, including dried specimens preserved for over 100 years in herbaria. He says:

Reflecting on all the facts with which I had now become acquainted, I was disposed to believe that the minute spherical particles or molecules of apparently uniform size...were in reality the supposed constituent or elementary molecules of organic bodies, first so considered by Buffon and Needham, then by Wrisberg with greater precision, soon after and still more particularly by Muller...I now therefore expected to find these molecules in all organic bodies: and accordingly on examining the various animal and vegetable tissues...they were always found to exist; and merely by bruising these substances in water, I never failed to disengage the molecules in sufficient numbers to ascertain their apparent identity in size, form, and motion, with the smaller particles of the grains of pollen. (1828, pp. 165-6)

Thus, in his claim that the motion did inhere within the molecules themselves, and that their size was uniform from all organic sources, Brown directly contradicted Brongniart's findings. Sloan (1986) outlines in detail the exchanges between the two researchers over the technical details of plant pollen. Brown, however, went on to test gum resins and other organic substances, even

coal, and found the same result. Upon seeing molecules of identical appearance form from *petrified* wood in even greater quantity than from non-mineralized organic matter, startled, Brown

proceeded to examine, and with similar results, such minerals as I either had at hand or could readily obtain, including several of the simple earths and metals with many of their combinations. Rocks of all ages, including those in which organic remains have never been found, yielded the molecules in abundance. Their existence was ascertained in each of the constituent minerals of granite, a fragment of the Sphinx being one of the specimens examined. To mention all the mineral substances in which I have found these molecules would be tedious... (ibid, pp. 166-7)

Brown himself seemed to suggest that the molecules were the universal fundamental constituents of matter, in observing that they were all of the same shape (spherical), but tried to remain non-commital in his wording:

From the number and degree of accordance of my observations, however, I am upon the whole disposed to believe the simple molecule to be of uniform size...I shall not at present enter into additional details, nor shall I hazard any conjectures whatever respecting these molecules, which appear to be of such general existence in inorganic as well as in organic bodies... (1828, p. 169)

However, the very choice of the term 'molecule' by Brown was a tell-tale sign of allegiance to Buffon's concept. As Kim has pointed out, the obvious term in use in Britain at the time for such concepts was 'atoms'. Though this term was often mistakenly translated by the French for their term 'molecule', the two were definitely not conceptually equivalent. 'Atom' had an empirically determinable connotation, while "in the French literature, 'molecule' tended to carry...speculative dimensions" (1992, p.3); everything from Buffon's theory of generation to Hauy's theory of crystal formation (Stevens 1984).

III. Points of Contention

That Brown, who at 54 had been for 25 years and more a bastion of conservative scientific respectability, would suddenly dare to publish on such topics is striking indeed. Surely it cannot be mere coincidence that, although he discovered these phenomena in mid-1827, during his sensitive negotiations over a position at the British Museum and with the University of London, he waited a full year before publishing the discovery on 30 July 1828; i.e. until the position for life at the British Museum was secure and he had settled into it for nine months or so. Perhaps, feeling as though he had stumbled on to something very important, he felt once his position was thoroughly secure, that he could publish, and that if he kept his wording sufficiently philosophically neutral, the brunt of any controversy would not fall on him personally.

For instance, even though invoking Buffon's and Needham's concepts, Brown does not clearly claim that his observations support processes of spontaneous generation; indeed, he is very cautious in his wording. The tone of his paper was sufficiently suggestive that many other investigators responded, both positively and negatively, to what they read as the implication that these molecules were some kind of transisitional stage between living and non-living, akin to Buffon's 'organic molecules' only even more universal in being produced from both organic and inorganic sources. To believers in spontaneous generation, this would be evidence for the more radically materialist idea of abiogenesis (formation of life from basic inorganic substances), not just of the heterogenesis (formation of life from degenerated matter of living or once living tissues) described by Buffon. Brown's insistence on the 'self-active' nature of the molecules, in reply to Brongniart, was not a minor technical point between two botanists. Whether matter could be self-active or required a transcendental superadded principle in order to become lifelike was one of the most central points at issue in debates over atheist, materialist philosophy; and the point had been thoroughly delineated as such in the exchanges between William Lawrence and John Abernethy and others, between 1815 and the early 1820s (Jacyna 1983).

In addition, the active molecule seemed to be the material fulfillment of the metaphysical principal of plenitude and the principal of continuity implicit therein, important to both Buffon and Leibniz's theories. Leibniz's monad theory was still influential among many biologists at this time.² Carl A.S. Schultze in Freiburg was one contemporary who immediately suggested that Brown's concept could be traced back to the Leibnizian monadology. Assessing the connection between the ideas of Leibniz, Buffon and Brown some 40 years later, DuBois-Reymond concluded that the kinship was too close to be coincidental. For him, Buffon and Needham must have accepted the metaphysical 'monad' concept, and:

what was intended for the mind's eye, the physical eye wanted to see; and if one did not literally attempt to discover monads with the microscope, one did, however, believe to have discovered them or something similar when the microscope actually showed every drop of an infusion teeming with small, seemingly simple creatures... Buffon believed that he had recognized, in infusoria and spermatozoa, living organic molecules which were incessantly active and which were indestructible by fire or by decay...Buffon did not call these so-called organic molecules monads, and he did not take this opportunity to remind us of Leibniz. However the, so to speak, materialized Leibnizian Thought can not be denied in his concept, and perhaps Buffon avoided disclosing the source of his theory because it would not have been accepted at that time, in France where the reputation of Leibnizian philosophy had been undermined by Voltaire...70 years later, when Robert Brown discovered the motion of small particles suspended in liquids which is named after him, Buffon's theory emerged again...Brown believed that he had discovered living elementary particles of all organic and inorganic bodies, indestructible even in fire, exactly as Buffon...had conceived of them. (1870, pp.843-5)

Lest it be thought that the intellectual climate of Britain was more receptive to the Leibnizian concept than Voltaire's France had been, Adrian Desmond reminds us that

²So much so "that even under Schwann's cell-theory, Johannes Muller and Henle spoke of the cells as 'organische Monaden'." (Thompson 1942, p.73)

Oxbridge dons belabored...complimentary aspects (of Lamarckism). Sedgwick dismissed 'the doctrines of spontaneous generation and transmutation of species, with all their train of monstrous consequences,' and Whewell listed four auxilliary hypotheses necessitated by the theory of transmutation: (1) the existence of monads, (2) a tendency to progressive development, (3) the force of external circumstances, and (4) spontaneous generation. (1984, p.189)

The first, third, and fourth of these all seemed rolled into Brown's discovery, by his association of it with Buffon and Needham. And the scientific establishment was declaring in no uncertain terms that this entire complex of ideas was still as seditious as ever. Nor did Brown's seeming philosophical neutrality in any way deter other scientists from speculating privately, or political radicals from picking up on his discovery and making use of it, confirming the conservatives in their views:

An acceptance of 'living atoms' was almost universal among the flaming democrats. It gave a scientific basis to their belief in free men controlling their own destinies, so important in an age of democratic demands. It provided the perfect political analogy--power from below, 'mandating' upwards, rising from the 'social atoms'--the people--rather than reigning down from a godhead or monarch. The notion of self-organizing atoms was spreading like wildfire through the democratic press. Looking at Brown's swarming atoms Darwin too became convinced... accepting that the atoms themselves were alive. He was switching a Cambridge tradition of inert matter powered by God for a more secular one. (Desmond 1991, p.223).

In this intellectual climate, it is not surprising, despite his attempts to be very careful about language, that "some writers who had not carefully followed his communications, asserted that Dr. Brown imagined these particles to be animated,--and this statement was generally believed." (Dancer 1868, p.163). Schultze, in his 1828 response to Brown, argued that the motion of the 'active molecules' was not a lifelike motility but, rather, was probably caused by quivering motion of the fluid. Thus, he did not believe the molecular movement to be a manifestation of life; however, interestingly, Schultze did believe that he had observed the production of monads from particles of dust from books! (1828, p. 31). Others, particularly Continental scientists, also read Brown's report as a claim that the molecules were lifelike, and also objected to this claim. A number of accounts of the objections have been written (e.g. Brush 1968, Goodman 1972, Mabberley 1985); however, the debate is uniformly treated as about technical issues such as the ability of temperature, light, or other factors to cause the movement, without considering the larger social context which supplies a major driving force to the need to find such an external, inanimate cause for the movements.

IV. Brown's Defenses

Several questions present themselves: First, if, as Brown claimed, the movements had been seen, but not understood, by Needham, Gleichen, Muller, Ingenhousz (1971, van der Pas 1971), Drummond (1815), and numerous others³ and had the same year been misinterpreted by Brongniart, why was Brown's view given such weight even though Brongniart had arguably the better microscope (Sloan 1986), so much so that today one may hear of 'Brownian movement' as a phenomenon⁴ which was simply 'discovered by Brown'? This is even more puzzling in light of the fact that most current references of that sort also describe his initial view as *wrong* from the point of view of latter-day understanding of the phenomenon, e.g. "he discovered the...Brownian movement in 1827. This was before the recognition of the existence of protoplasm. He...at first associated the movement with vitality, though he soon gave up this idea." (Ramsbottom 1932, p.33).⁵ Secondly, if Brown was so plainly breaching such taboo subjects, how did his reputation in Britain and Europe as a whole continue to prosper, while that of his contemporary Robert Grant dwindled so that Grant "by the 1840s...had virtually ceased publishing and was reduced to living in a 'slum'''? (Desmond 1984, p.189). I will turn to these questions roughly in the order in which I have raised them.

³Lindley points out that others had clearly seen the movement, who even Brown seemed unaware of: Amici "sometime before 1824" and Guillemin in 1825. He reports that "In June 1827 I was shown the motion by Dr. Brown, who subsequently published some valuable observations...without however noticing those of either Amici or Guillemin. The most important addition that was made by Brown to the knowledge that previously existed, consisted in the discovery of...two kinds of active particles in pollen..." (1835, p.158-9). Note the significant difference between this and more current accounts of Brown as discoverer. ⁴Mabberley (1985, p.273) incorrectly attributes this term to H.C. Bastian(1871). He seems unaware that by that time Bastian was replying to Huxley (1870) who had used it already. It seems to have caught on in Britain in the late 1860s as a rendering of the expression 'Brunonian movement' which was already in common use on the Continent by the time of Brown's death (anon., 1858).

⁵In many cases the story is truncated (and distorted) still further, e.g. "Brown's discovery...was not his observation of the motion of microscopic particles in fluids; that observation had been made many times before; instead it was his emancipation from the previously current notion that such movements had a specifically organic character. What Brown showed was that almost any kind of matter, organic or inorganic, can be broken into fine particles that exhibit the same kind of dancing motion; thus he removed the subject from the realm of biology into the realm of physics." (Brush 1968, pp.2-3). This kind of condensing of the story obscures just that process of negotiating and quickly fumbling for new positions during a controversy which I hope to show for Brown. It also obscures the period of limbo in which the

Undoubtedly, Brown's previously established reputation as a scientist of first rank both in England and on the Continent (Mabberley 1985, p.253) was one of the most significant factors in the acceptance of his views, or at least in the willingness of many to give him the benefit of the doubt while a controversial matter was still unresolved. One whom von Humboldt had dubbed 'facile botanicorum princeps' and whose reputation had been built on the most careful and painstaking detail work, especially skill in microscopy and sharpvision, for over 20 years, was not to be lightly challenged or treated as some young firebrand whose ideas were running out ahead of his laboratory skill. When Brown claimed that his simple microscope was capable of delineating the phenomena in question perfectly clearly at only about 320x, such established men as J.S. Henslow were quite willing to accept his advice on this point (despite agreeing with Brongniart against Brown on the question of the self-activity of the molecules), and it was to Brown that Henslow sent the young Charles Darwin before the *Beagle* voyage for advice on what kind of microscope would be useful for the marine invertebrate studies Darwin wished to pursue while away (Sloan 1986, p.424). Darwin obtained the same type of microscope suggested by Brown, since studying the 'atoms of life' (in an attempt to develop some of Grant's hypotheses about zoophytes) seems to have been specifically what he had in mind. Particularly in what he saw as the connection to transmutationism. Darwin certainly realized the explosive potential of Brown's 'molecules' and thus kept all his notes and ideas on this, and the early 'monad theory of evolution' which it led him to, completely private for almost another 40 years (Sloan 1985, 1986, Gruber 1985).6

As far as developing and maintaining his reputation on the Continent, Brown surely stands out in this period in Britain, not only for cultivating early on the ability to read and translate German (Stearn 1970) and thus to be at the cutting edge of a whole new influential wave of biological thought in the 1830s (Sloan 1986, pp.396-8). He also received many foreign visitors and "strengthened his connexions with the Continental schools by travelling there frequently" (Mabberley 1985, p.253). Thus, although misinterpretation of his intent in the 1828 paper did occur, he was challenged much more gently than some might be, with challengers less likely to

phenomenon existed from 1829 until 1863 when it was in fact first claimed by physicists (Wiener 1863, Exner 1867, Mabbereley 1985, p.273).

⁶In many cases the story is truncated (and distorted) still further, e.g. "Brown's discovery...was not his observation of the motion of microscopic particles in fluids; that observation had been made many times before; instead it was his emancipation from the previously current notion that such movements had a specifically organic character. What Brown showed was that almost any kind of matter, organic or inorganic, can be broken into fine particles that exhibit the same kind of dancing motion; thus he removed the subject from the realm of biology into the realm of physics." (Brush 1968, pp.2-3). This kind of condensing of the story obscures just that process of negotiating and quickly fumbling for new positions during a controversy which I hope to show for Brown. It also obscures the period of limbo in which the phenomenon existed from 1829 until 1863 when it was in fact first claimed by physicists (Wiener 1863, Exner 1867, Mabbereley 1985, p.273).

suggest carelessness or to bluntly impute philosophical bias, which has no doubt helped to conceal from historians the highly charged nature of the issues at stake. No less a figure than Michael Faraday spoke up to gently remind people of Brown's trusworthiness and laboratory skill, in a Friday evening lecture at the Royal Institution on 13 February 1829:

Mr. Brown, supposed to be careless and bold, is used to microscopical observations--has not yet been corrected--assisted by Dr. Wollaston--so that carelessness can hardly be charged. Then, what does Mr. Brown say? Simply that he cannot account for the motions...motion cannot be considered as distinctive of vitality--connection with atomic or molecular philosophy. (quoted in ibid, p.272).

In attempting to explain the misunderstandings (he was sure they were no more than that) which had occurred, Faraday did point out that, as discussed above, by using the term 'molecule', which Faraday distinguished from 'ultimate atoms', Brown had invited difficulties "because the subject connects itself so readily with general molecular philosophy that all *think* he must have meant this or that." (Jones 1870, p.403). Brown did not rely only on his individual reputation; in addition, he "demonstrated the movements to Franz Bauer and, among others, Bicheno, Fitton, Forster, Lindley, Maton and Wollaston, a fact he was careful to record. Alphonse de Candolle was visiting Britain in the following spring (1828) and he wrote to his father of Brown's (also showing him)." (Mabberley 1985, p.270). Brown was aware, it seems, of the power of obtaining testimony by 'gentlemen witnesses' in British science and of including them in the published report, a strategy dating back to Boyle and Newton (Shapin and Schaffer 1985, Schaffer 1989).

Mabberley and other authors point out that Brown's forceful literary style and other features of the way in which he reported the discovery also played a role in the association of his name with the phenomenon, rather than one of the earlier 'discoverers', e.g. Ingenhousz:

Ingenhousz's remark is buried in a paper on another topic: indeed it was overlooked until 1971, a technical 'first' with little importance in the history of science... Brown's paper is a *tour de force*, a beautifully constructed crescendo on the widening implications of his findings...however...it is true, that with mention of the Sphinx, it verges on the theatrical. (1985, pp.272-3).

Schaffer (1989) has also discussed the strategy of stabilizing a new discovery by multiplying its sites of replication; this was next on Brown's agenda as well. Within three weeks of publishing the discovery, by 18 August 1828, Brown was demonstrating the molecules at a scientific institute in Paris. As the controversy heated up the following year, Brown again took the show on the road, this time demonstrating the molecular movements for Martius, Oken and others in Munich, for Sommering in Frankfurt, and for an entire convention of German naturalists assembled at Heidelberg from 18-24 September (at which five others from Britain, including William Whewell, were attending). "Brown demonstrated 'movement' to the physicists who, as Oken wrote... considered themselves honored... They were all convinced, and Brown later

demonstrated his method to the botanical and zoological sections of the meeting." (Mabberley 1985, pp.282-5).

Nevertheless, as mentioned in the previous section, there were numerous objections from both British and Continental scientists to some of Brown's conclusions, especially the claim that the particles were self-active and of the same size, implying Buffon's organic molecules and equivocal generation. Many possible physical explanations of the motion were advanced (Raspail 1828, Schultze 1828, Bakewell 1829, Brewster 1829, Muncke 1829, Dancer 1868), despite the clear controls by which Brown claimed to have ruled this out. David Brewster, a physicist and editor of the *Edinburgh Journal of Science*, was not oblivious to Brown's arguments, but felt that even if a complete explanation by physical causes was not yet possible, such causes would eventually be found, and that in the meantime it was improper to attribute the motion to animal life. Thus, if my reading of Brown is correct, his one cautious venture with such ideas was enough of a bad experience with the volume of criticism provoked to cause an immediate withdrawal to a safer position.

By the time a year had passed since his original report, Brown had either realized he could not avoid criticism since his discovery might support claims for materialism or spontaneous generation, or decided that he had at least been misinterpreted as implying such ideas. Although the suggestive passages in his 1828 paper imply the former, Brown claimed the latter. In 1829 he wrote:

In the present supplement to that account, my objects are, to explain and modify a few of its statements...In the first place, I have to notice an erroneous assertion of more than one writer, namely, that I have stated the active molecules to be animated. This mistake has probably arisen from my having communicated the facts in the same order in which they occurred, accompanied by the views which presented themselves in the different stages of the investigation; and in one case, from my having adopted the language, in referring to the opinion, of another inquirer. (1829, p.161)

One seemingly implicit claim Brown particularly wished to distance himself from was that the molecules were the same, whatever source they came from. He refers to one line of reasoning he had pursued in 1828 as:

a supposition which, though professedly conjectural, I regret having so much insisted on, especially as it may seem connected with the opinion of the absolute identity of the molecules, from whatever source derived. On this latter subject, the only two points that I endeavored to ascertain, were their size and figure: and although I was, upon the whole, inclined to think that in these respects the molecules were similar from whatever substances obtained, yet the evidence then adduced in support of the supposition was far from satisfactory; and I may add, that I am still less satisfied now that such is the fact. But even had the uniformity of the molecules in those two points been absolutely established, it did not necessarily follow, nor have I anywhere stated, as has been imputed to me, that they also agreed in all their other properties and functions. (ibid, p. 162) Checking his observations, originally made with high-powered single lens microscopes, using "the best achromatic compound microscopes," he confirmed them, summing up his position thus:

That extremely minute particles of solid matter, whether obtained from organic or inorganic substances, when suspended in pure water...exhibit motions for which I am unable to account, and which from their irregularity and seeming independence resemble in a remarkable degree the less rapid motions of some of the simplest animalcules of infusions...I have formerly stated my belief that these motions of the particles neither arose from currents in the fluid containing them, nor dependence ded on that intestine motion which may be supposed to accompany its evaporation. (ibid, p.162-163)

Thus Brown now tried to claim that the movements were not vital, even if he could not account for them physically.

V. Microscopes and Cell Theories

Many, in England perhaps the majority, accepted this new 'non-aligned' stance, so that by 1858 the motion was described in Brown's obituary notice as "these movements, the full import of which is at present not understood" (anon. 1858, p.786). However, on the Continent the debate about original *perceptions* of Brownian movement as allegedly biological evidence continued to be linked to the spontaneous generation controversy. Ehrenberg, for instance, opposed the possibility of spontaneous generation, and in 1832 cited Brown's discovery as merely a careful measurement "of inorganic solid bodies, and also of organic ones," fixing "the size of the smallest particles which could be observed, and which he (Brown) himself saw in spontaneous motion...at 1/20,000 to 1/30,000 of an inch...in diameter." (1832a, p.25). Indeed Ehrenberg discussed Brown, along with work by Koelle and numerous other advocates of spontaneous generation (via elementary particles, 'organic atoms', 'zymom', etc.), all of whom would no doubt advocate reading Brown's results as confirming such a process. Ehrenberg however, wrote in 1832:

It has not here been my intention to give a collection of the opinions of natural philosophers and chemists respecting atoms, but to call to memory only a few of those statements...of the magnitudes of the smallest particles of bodies which have been observed and calculated, in order to add to them the results of more recent observations...and to lay down a scale for them. The most recent theoretical statements do not give any very great degree of minuteness to the ultimate particles of bodies; the observations of Mr. R. Brown very nearly approximate to those statements. The common opinion that infusoria or mould could be made by pouring water on dead organic matter I must pronounce to be completely contradicted by the whole series of my observations. (ibid, pp.28-9)

Thus while he used the term 'monads' to describe many infusoria, Ehrenberg intended it to have almost *none* of the connotations which it previously had, but to serve only as a quaint historical

reminder of notions he considered to be outdated.⁷ He challenged Brown's interpretation of the active molecules as the smallest constituents of matter by citing examples from his own work, of vesicles within infusorians which were many times smaller. Inherent in his argument against spontaneous generation we see the use of more powerful microscopes, especially those of higher magnification, touted as crucial to the ability to confirm the findings, a theme which would continue in spontaneous generation debates (see, e.g. Vandervliet 1971, p.38). With his best microscope, magnifying up to 800x, Ehrenberg claimed that he could in fact make out clearly "the smallest animal form, to which I have given the name *Monas termo*," which by the dimensions he gives (only 1-1.5um long) seems more likely a bacterium (1832a, p.30). With a "solar microscope" he reports seeing "wandering shadows of smaller monads, which could not by a vast deal reach to" the dimensions of *M. termo*,

and which I could not at all discern in the same water with the most powerful magnifier of Chevallier's microscope: perhaps their transparency might be one reason... it follows from the observation that 1/2000 of a line (c. 1 um) is not at all the limit of organized beings for observation...from this point a new system of organized beings may easily be opened by means of increased power of vision...These calculations...plainly demonstrate an unfathomableness of organic life in the direction of the smallest conceivable space; and if the word infinity be too much for what we know at present, let the word unfathomableness, which I have purposefully employed, avert from me the reproach of exaggeration, and establish the point of view which the physical, chemical and physiological inquiries of our days, should they be rendered fruitful by new powers, have to take... (ibid, p.34)

It would be misleading to see the technical improvement in microscopes as entirely and immediately helpful in clarifying these debates, or as lending support exclusively to the kind of arguments Ehrenberg puts forward. In 1829, for instance, the new compound microscopes began also to be improved with achromatic lenses, first pioneered by J.J. Lister. Recall that, when checking his results with new achromatic compound microscopes, Brown confirmed his original observations. As late as 1834, Lister published some observations, using his new invention, describing the 'dynamic granules' in the ova of the same polyps which Darwin was away studying. His description could well have been used to reinforce the central claim of 'self-activity':

the...particles rushed out with the vivacity and rapid motions of bees swarming. Their action could not at all be referred to currents in the water, and was very different from the dancing of inorganic molecules; such, indeed, that it was difficult not to believe them possessed of vitality. (1834, p.376)

⁷DuBois-Reymond warns against such attempts at clever naming: That Otto Friedrich Muller, one of the most important of Mr. Ehrenberg's predecessors, introduced into the zoological nomenclature the name *Monas* for such (infusorian) forms, was only one of those terminological jests which also, along with Linnaeus, gracefully enliven the dryness of the system. This allusion indicates a manner of thinking then current, which in imaginitive personalities led to severe errors. (1870, p.843)

Lister himself, though he felt the movements important and impressive, could not quite bring himself to make that logical step. His explanation reveals again that the particles at this time still carried their charged implications:

...it would be highly interesting to ascertain distinctly how they are produced, and what is the office they perform, as well as the true character of their remarkable activity and seemingly spontaneous motions; *for the hypothesis of their individual vitality is too startling to be adopted without good evidence.* (ibid, p.377, italics mine).

Secord has shown that as late as 1837, equivocal generation claims were certainly as unpalatable as ever (1989).

Brown's microscopic observations from 1828 to 1831 were especially influential in dramatic transformations in the development of the cell theory at this time (Stephenson 1932). In addition to such contributions as naming and emphasizing the importance of the nucleus, negotiations over the significance of Brown's 'molecules' also were seen within the theoretical context of cell theory at the time. So-called 'globulist cell theories' of this period were challenged by Schwann's new doctrine, based on observations which "relied upon extensive use by Schwann and others of the achromatic microscope...It is sometimes assumed that this technical advance itself made possible the development of cell theory," says Jacyna; however, the amount of detail now reported, in fact produced tremendous confusion. "There was a continuing need to impose some 'intellectual order' upon the mass of information accumulated by observers"; an example, Jacyna argues, of Fleck's claim that meaning can only be found amid empirical data by fitting it into some pre-established thought structures (1984, pp.26-7). The globulist theories of the early nineteenth century saw all tissues to be composed of microscopic 'globules' in different arrangements and some, e.g. Unitarian minister and sanitary reformer Thomas Southwood Smith in 1827, connected this tradition with the belief in infusorians as 'monads'--the infusorians being supposedly independent, but otherwise identical to the basic globules of tissues (ibid, p.21). Sloan shows how Ehrenberg had success in undermining theories of the infusorial monad as spontaneously generated and as the "elementary biological atom, as had been commonly claimed by William Sharp Macleay, Robert Edmond Grant,...Smith, and others" by painstakingly revealing the detail of the inner structures in those infusorians. This, however, only "seemed to demonstrate unequivocally...that the real ultimates were the granular particles, as Brown, Lister, Brongniart, Henslow and Darwin...had already claimed in various ways." (Sloan 1986, pp.435-6, italics in original). The end result has been described thus "the doctrine of organic molecules was to be swallowed up by the cell theory" (Brush 1968, p.2), though Jacyna shows how the common elements between the earlier ideas of Smith and the theory of Schwann helps explain why cell theory was accepted easily and quickly in Britain (1984, p.22). We see the beginnings of this

united ingestion of 'globules' and 'organic molecules' already in the new 1833 physiology text of Johannes Muller:

In fine, this theory of the composition of tissues by the aggregation of globules, which are supposed to be more than 1/2000 of a line in diameter, is rendered exceedingly improbable by the discovery of Ehrenberg...On account of the difficulty of distinguishing by the microscope between inequalities and globules, this theory still remains a mere hypothesis. At any rate, the organic molecules are merely the most minute forms in which the compound organic matter appears; they are not the atoms of the organic combination. The hypothesis that all the tissues of the animal body are, in their perfect state, composed of globules aggregated together in different forms, is now known to be wholly incorrect. The nervous fibers, for instance, are delicate tubes...enclosing a fine granular substance... (1843, p.21)

Throughout the 1830s and, especially with the wide acceptance of Schwann's doctrine in the 1840s, the notions of 'cell' and 'infusorial monad' as much more complex than a 'globule' probably afforded a more appealing focus of attention, emphasizing structural detail less "startling" and philosophically sensitive than the swarming and rushing around of the 'active molecules.

Thus, the discovery and reception of Brown's active molecules has been shown to be embedded in the context of political and religious turmoil of Britain in the early nineteenth century and of Brown's career in particular. The later definition of what came to be called 'Brownian movement' was very much a product of this earlier history, since even those who accepted that its cause was still unknown were strongly predisposed by the cultural context to believe that the cause would eventually be shown to be physical, nonvital forces.

Many thanks to James Secord for suggesting several of these references which proved extremely helpful, as well as for valuable suggestions about the period in general.

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