Experimental Method and Spontaneous Generation: The Controversy between Pasteur and Pouchet, 1859–64

NILS ROLL-HANSEN

HE introduction of the experimental method in biology has traditionally been seen as the main ingredient of the nineteenth century revolution in that science. Louis Pasteur was perhaps the greatest prophet of the new experimental biology, and his contribution to settling the question of spontaneous generation has been taken as a model of proper experimental method. He combined theoretical boldness and imagination with a critical scientific attitude and thorough methods of testing disputed hypotheses. In contrast to his opponents, Pasteur was not led astray by his prejudices, but built methodically on experimental work done largely by himself. The facts behind this interpretation deserve some reexamination. What influence, for instance, did religious or political views have on the conclusions that were drawn? John Farley and Gerald Geison have tried to show 'the very real significance of the extra-scientific, political aspects of the debate.'1 They claim to have revealed a 'direct influence of extrinsic factors on the conceptual content of serious science,'2 and they accuse Pasteur of violating the rules of the experimental method.³ Farley and Geison are 'persuaded that external factors influenced Pasteur's research and scientific judgement more powerfully than they did the defeated Pouchet.'4 This reinterpretation of the controversy is repeated by

^{1.} John Farley and Gerald L. Geison, 'Science, politics and spontaneous generation in nineteenthcentury France: the Pasteur-Pouchet debate,' Bull. Hist. Med., 1974, 48, 161-198, p. 162.

^{2.} Ibid.

^{3.} Ibid., pp. 190-193.

^{4.} Ibid., p. 197.

The author is grateful to Mirko D. Grmek for a thought-provoking conversation on the methodologies of Pasteur and Bernard, to David Hull for comments on the draft, and to the referees and editors of this journal for very helpful criticisms, corrections, and editing of the paper.

John Farley in his book titled The Spontaneous Generation Controversy.⁵

No doubt political conservatives and Roman Catholics in France were in the 1860s hostile to the doctrine of spontaneous generation. Spontaneous generation was generally associated with Darwinism and other radical views. Pasteur was conservative rather than radical, and he used his refutation of Pouchet's claims about spontaneous generation as an argument against 'materialism' and atheism, and in support of 'spiritualism' and traditional values. Pasteur's development of an organismic, antireductionist research program and his tenacity in defending antireductionist ideas in organic chemistry and fermentation studies, may also owe something to his social outlook. But it is a far cry from such a correspondence between Pasteur's political and religious views and his scientific ideas to the specific claims of external influence made by Farley and Geison. For instance, Farley maintains that Pasteur's victory in the debate with Pouchet 'was achieved because Frenchmen were already convinced of the impossibility of spontaneous generation.'6 The implication must be that if Frenchmen had not already been convinced that spontaneous generation was impossible. Felix Archimède Pouchet would have won, or that an unprejudiced Academy of Sciences would have decided in favour of Pouchet rather than Pasteur.

The experiment that Pouchet carried out together with Nicolas Joly and Charles Musset in the Pyrenees in the summer of 1863 formed the starting point of the second round of the controversy, which attracted most public attention. Even classical and friendly accounts of Pasteur's work, like those by Emile Duclaux,⁷ René Dubos,⁸ and François Dagognet⁹ have found Pasteur's success in this phase of the controversy to rest on a doubtful scientific basis. They suggest that Pasteur won a too easy victory because Pouchet did not have the nerve to pursue his case before the Academy of Sciences. According to Farley and Geison it was Pouchet rather than Pasteur who was the sober scientist who based his conclusions on sound logic and thoroughly tested facts.

By contrast I find that it was Pasteur who best understood and applied the experimental method and was most careful to avoid appeals to religious

^{5.} John Farley, The spontaneous generation controversy from Descartes to Oparin (Baltimore and London, 1977). In 'The social, political, and religious background to the work of Louis Pasteur,' Ann. Rev. Microbiol., 1978, 32, 143-154, Farley has repeated the same claims once more, but with no additional evidence.

^{6.} Ibid., p. 114.

^{7.} Emile Duclaux, Pasteur histoire d'un esprit (Sceaux, 1896), pp. 140-142.

^{8.} René Dubos, Louis Pasteur, free lance of science (1950; reprint ed., New York, 1976), p. 175.

^{9.} François Dagognet, Méthodes et doctrine dans l'oeuvre de Pasteur (Paris, 1976) pp. 158-161.

or political authority in his scientific argument. In one way this is a disappointing result. Pasteur was one of the most productive and influential scientists of the nineteenth century. His work was inspired by controversial antireductionist principles which had clear ideological affinities at the time. His research, therefore, appears as one of the more promising places to look for external influence on important developments in science. However, Pasteur had adversaries with a better grasp of the experimental method than Pouchet, for instance Marcellin Berthelot, Justus von Liebig, Claude Bernard, and Robert Koch.

Explicit philosophical and methodological statements are sparse in Pasteur's writings. Pasteur differed from his great contemporary in French experimental biology, Claude Bernard, in spending little effort on epistemological justification of his methods. On the other hand, Pasteur's numerous controversies provide unique material for reconstruction of his methodology. Through the views that he opposed, one can delineate Pasteur's own position.

Pasteur's concept of scientific method was part of a general view of science and its social role. One basic tenet was that a properly conducted scientific inquiry led to absolute truth; another, that there is complete harmony between scientific and social progress. Today both tenets are difficult to accept. From our doubts about the happy harmony between scientific and social progress arises the urgency of the problem of external factors.

Pasteur's view on the practical application of science conformed to this picture of science as the motor of social progress. In a public appeal for more support for science after the disastrous war with Prussia in 1870, Pasteur attacked the view that the character of scientific research was changing. He denied that the theoretical sciences were gradually giving way to the applied sciences. Such views were wrong and dangerous and could only diminish the practical benefits of science. According to Pasteur there was no special category of science which one might call applied, only 'science and the applications of science.'¹⁰

Similarly, Pasteur's conception of the relation between science and religion was part of the same general view. Science, in its own field, produced absolute truth which religion had to respect. If science supported our religious views that was good, but religion had nothing to do with establishing scientific truths. The autonomy of scientific method produced an asymmetric relationship: scientific truth might be used to support religion, but

^{10.} Louis Pasteur, Oeuvres de Pasteur, 7 vols. (Paris, 1922-39), VII, 215.

religion could not be used to support scientific claims. Religious authority was irrelevant in scientific disputes.

This separation of science from religion permitted Pasteur to use the results of his experiments on spontaneous generation as arguments against materialism and atheism at the same time as he claimed that his scientific conclusions were not prejudiced by philosophical or religious doctrines. In the famous popular lecture on spontaneous generation which Pasteur delivered in April 1864, he reminded the audience what a victory it would be for materialism if it could support itself on 'the verified fact of matter organizing itself, making life on its own.'11 Pasteur asserted that philosophical and religious doctrines had no importance for him as a scientist and that spontaneous generation was only a question of fact. 'I set out with no preconceived idea, equally prepared to declare, if experience had forced me to that confession, that there is spontaneous generation, as I am now convinced that those who believe in it are blindfolded,'12 he declared. Pasteur may have exaggerated his own openness of mind, but he made as objective and open-minded a test of Pouchet's claims as one could humanly expect and as proper scientific procedure demanded. While Pasteur did not mention religious doctrines in his scientific arguments, and never claimed that consistency with religious doctrines provided any support for his scientific conclusions, Pouchet was eager to establish that heterogenesis was not only consistent with orthodox religious doctrines but actually fitted them better than did Pasteur's theory.13

In 1882 Pasteur discussed the relation between science and religion in his speech on his election to the French academy. He criticised French positivists, in particular Emile Littré and Auguste Comte, for misunderstanding and misusing the scientific method by trying to apply it to political and religious problems. He said that it was an illusion to believe that the scientific method can solve all problems. The humanist Littré confused the method of observation, which usually cannot give rigorous demonstrations, with the experimental method of science which properly applied leads to an unambiguous conclusion, Pasteur declared.¹⁴

Pasteur described with enthusiasm the special virtues of the experimental method. He stressed that experimental science is not limited to the facts available. Its task is to investigate unknown possibilities: 'The unknown in

^{11.} Ibid., II, 332.

^{12.} Ibid., p. 334.

^{13.} See, for instance, the account in Farley and Geison (n. 1), pp. 169-172.

^{14.} Pasteur, Oeuvres (n. 10), VII, 334.

the possible, and not in that which has been, that is its domain. . . .¹⁵ Pasteur's scientific method conformed well to the hypothetico-deductive ideal. He stressed on the one hand that scientific conclusions must always build on empirical facts and on the other that hypotheses are needed to guide scientific inquiry.

The successful prediction of novel facts was to Pasteur a crucial test of a true theory. For instance, he proposed to Justus von Liebig, during their controversy over acetic acid fermentation, a kind of public scientific duel under the auspices of the Paris Academy of Sciences. They should perform a new experiment, one which nobody had carried out before, and for which their respective theories predicted contradictory results. It is 'a property of true theories that they give rise to logical deductions whose truth can be affirmed *a priori*,' Pasteur claimed ominously.¹⁶ Liebig wisely did not respond to this challenge.

Such confidence in clear-cut results from so-called crucial experiments appears naive to us today. It nevertheless demonstrated Pasteur's clear understanding that the force of the experimental method lies not only in technical manipulation but also in a method of reasoning. This method works analytically by isolating one causal factor at a time and testing its effects. A main tool in this connection is the control experiment. This is, ideally at least, an experiment identical in every respect except the causal factor that is to be tested. Such a procedure may not be foolproof. Hidden factors may invalidate the experimental results. But the procedure is essential to practical research and was a central feature of the experimental method that penetrated so many fields of scientific research in the nineteenth century. The systematic use of control experiments, and the meticulous care about details which the experimental method demanded, was characteristic of Pasteur's work. In Pouchet's work, by contrast, control experiments were absent or insufficiently developed.

Pasteur's belief that science could produce absolute truths resulted in a certain dogmatism. He held that scientific hypotheses could in many cases be definitively verified or falsified. In a dispute with Berthelot he insisted that his hypothesis that fermentation is life without oxygen was no longer open to doubt. Berthelot, on his side, had great respect for Pasteur's factual discoveries, but found this claim, among others, to be a quite speculative hypothesis.¹⁷

15. Ibid., p. 335. 16. Pasteur, Oeuvres (п. 10), п, 365. 17. Ibid., pp. 586-615.

Claude Bernard also criticised Pasteur's dogmatism. In February 1877 Bernard wrote some notes for the preface of his long planned, but never finished, treatise *Principes de médecine expérimentale* which contained remarks critical of Pasteur's scientific method. Bernard opposed what he called the a priori method. From observations the a priori method induced theories which were not tested further by experience. 'One must not let oneself be dominated by one's induced idea which is at bottom nothing but a hypothesis,' Bernard warned. He expressed his own ideal in almost Popperian terms: '... I try to destroy my hypothesis rather than to verify it.' These warnings against a priori thinking were clearly addressed to Pasteur. Bernard did not directly call him an a priorist, but he was put in the same class. Bernard contrasted Pasteur's method with his own 'experimental a posteriori' method. Pasteur let his ideas dominate the facts, Bernard claimed, and wanted to dictate the experimental results to nature according to his own ideas.¹⁸

There is one somewhat pathetic episode in Pasteur's life which both reveals his lack of finesse in political matters and illuminates his prophetic vision of science as a driving force for social progress. In 1876 Pasteur attempted to be elected for the senate. His election manifestos reflected his contempt for ordinary politicians. Pasteur emphasised that he was not linked to any political party and only wanted to serve his country by serving science. As proof of his qualifications he put on his scientific decorations.¹⁹ 'It is thus science in its purity, its dignity and its independence that I will represent in the Senate, if you will honour me with your votes,' were the words which concluded Pasteur's first appeal to the electorate.²⁰ Few people voted for him.

Pasteur's attitude was characteristic of the class of professional scientists that emerged in the nineteenth century. They worshipped the experimental method, and the Paris Academy of Sciences was one of their bastions. Pasteur was a purebred exemplar of this race, a superb scientist, in some respects so extreme that he became a caricature.

In the controversies over spontaneous generation, Felix Archimède Pouchet, director of the Museum of Natural History in Rouen and corresponding member of the Paris Academy of Sciences, appealed to immediately observable phenomena and disdained theoretical arguments. To him spon-

20. Louis Pasteur, Correspondance de Pasteur 1840-1895, ed. Pasteur Vallery-Radot, 4 vols. (Paris, 1940-1951), II, 612.

^{18.} Claude Bernard, Principes de médecine expérimentale (Paris, 1947), pp. xxv-xxvi.

^{19.} Maurice Perrod, 'Pasteur candidat au Senat,' Mem. Soc. Emul. Jura, 1923.

taneous generation was an obviously observable fact, and Pasteur's objection, that what Pouchet and others had observed was not really spontaneous generation, appeared as scholastic sophistry. Insistence on the verdict of observed 'facts' was also characteristic of Henry Charlton Bastian, the English physician who challenged Pasteur's view on spontaneous generation in the 1870s. Bastian would not accept the experimental strictures that Pasteur derived from his germ theory, but nevertheless Bastian claimed to have refuted the germ theory.²¹ Both Pouchet and Bastian accused Pasteur of disregarding the empirical method of science.

In Pouchet's first major scientific work in 1842, a treatise on the fertilisation of mammals, the emphasis on positive empirical facts is prominent.²² Basing his work on observation, experiment (*expérience*), and logic, Pouchet wanted to trace rigorously the laws that governed fertilisation, the most fundamental phase in the generation of animals. In his own view Pouchet postulated no theory of generation like Aristotle, Hippocrates, and Buffon, but limited his account to verified facts.²³

In 1859 in his first book on spontaneous generation, Pouchet stated explicitly his preference for observable facts. In the preface he explained that the work consisted of two parts: an experimental part, which was 'the only fundamental one,' and a theoretical part, which formed only 'an accessory fragment.' Pouchet's aim was 'to demonstrate a fact, not to discuss essences or nebulous theories.'24 When in 1864 Pouchet published a new book on spontaneous generation, his exasperation with Pasteur's obstinacy in the face of the 'facts' was evident. This single scientist, complained Pouchet, continues to deny generally accepted facts and continually changes his theories in an arbitrary way.²⁵ Since the Academy of Sciences was in general very sceptical of Pouchet's 'facts,' Pouchet's picture of Pasteur's lone opposition to reason was somewhat exaggerated. But Pouchet apparently had the majority of interested laymen and the nonscientific press behind him. He did not lack public support. Pouchet also criticised the biological ambitions of the chemists. Referring to the prominent French naturalist Isidore Geoffroy Saint-Hilaire, Pouchet claimed that in the physiological domain chemistry could act only as the vassal of bi-

^{21. &#}x27;The Commission of the French Academy and the Pasteur-Bastian experiments,' Nature, 1877, 16, 277-279.

^{22.} F. A. Pouchet, Théorie positive de la fecondation des mammisfères, basée sur l'observation de toute la serie animale (Paris, 1842), p. 1.

^{23.} Ibid., p. 5.

^{24.} F. A. Pouchet, Héterogénie ou traité de la génération spontanée (Paris, 1859), p. viii.

^{25.} F. A. Pouchet, Nouvelles expériences sur la génération spontanée et la resistence vitale (Paris, 1864), p. xii.

ology. Such appeals to authority were typical of Pouchet's way of supporting his scientific claims. Pouchet used contemporary rhetoric according to which 'facts' were considered to be the basis of science and 'theories' to be speculative and suspect, but his grasp of experimental reasoning was feeble.

Pasteur and Pouchet based their investigations on two rival theories. Pouchet's theory was heterogenesis which held that under appropriate conditions new living organisms could arise from matter that was or had been living. The new organisms did not have parents of the same kind as themselves, hence the word *heterogenesis*. According to Pouchet's theory, in the present world heterogenesis gave rise only to microorganisms. Pasteur upheld the germ theory according to which microorganisms always arose from parents of the same species, either directly or through germs produced by the parent. Such germs were present in great numbers almost everywhere—in dust, in air, in water, etc.—and germs could therefore explain the 'spontaneous' growth of microorganisms on various kinds of nutrient media.

Methodologically the two rival theories had a symmetrical relationship to each other. They both needed to build their own factual basis and weaken that of their rival, that is, they both needed to produce novel facts to support their own theory as well as to undermine the 'facts' which appeared to support the rival theory. The problem was not whether the doctrine of spontaneous generation could be disproved or not.

The opponents of spontaneous generation should not be placed in the logical dilemma described by John Farley. He claims that their thesis, 'all organisms arise from parents,' cannot be proved but only falsified, while the thesis of the heterogeneticists, 'some organisms can indeed arise directly from matter,' can be proved but not falsified. 'Logically speaking, therefore, opponents of spontaneous generation could do no more than invalidate particular experiments said to illustrate its occurrence,' says Farley.²⁶ However, one should not be misled by the occurrence of 'all' and 'some' in these statements. The heterogeneticists were also subject to the logic of experimental science and had to make general claims which were accessible to testing, for example, that under certain conditions heterogenesis regularly occurs. To the extent that a logical dilemma really existed, it pertained equally to both theories.

Pasteur avoided a general discussion of the two theories and concen-

^{26.} Farley, Spontaneous generation (n. 5), p. 4.

trated on their factual bases. He showed that when proper care was taken to exclude the germs, there was no growth of microorganisms. From the germ theory he derived predictions of novel facts which he then verified experimentally. Pasteur thought the latter to be the most distinguished kind of proof that a theory could get.

From the beginning Pasteur raised doubts about the validity of Pouchet's experimental evidence. In a letter to Pouchet written in February 1859, Pasteur was clearly in favour of the germ theory. He said that he respected Pouchet's belief in spontaneous generation because it is hard in such questions not to have a preconceived notion, but that he rejected the experimental evidence. In the letter Pasteur sketched an experiment which he thought ought to convince Pouchet that his apparent success with spontaneous generation might well be due to contamination with germs.²⁷

Pasteur did not participate in the first confrontation of Pouchet with the Academy of Sciences. In December 1858 Pouchet presented to the academy two experiments in support of spontaneous generation.²⁸ He claimed to have reversed the outcome of a classical experiment by Theodor Schwann against spontaneous generation. Oxygen was often thought to be a crucial factor in spontaneous generation. It was known that one could prevent the appearance of microorganisms in organic matter indefinitely by violent heating in a closed vessel, but when fresh air or oxygen was introduced growth would occur. In 1836 Schwann had shown that by heating the oxygen before it was brought into contact with the nutrient medium, he could prevent the appearance of microorganisms.²⁹ Schwann's experiment indicated that the air or oxygen contained something, which was destroyed by heating, namely, germs. Pouchet claimed that he consistently got the opposite result. And he concluded, therefore, that germs in the air could not be the source of the microorganisms.

Pouchet's ideas and experiments were received very unfavourably by the academy. The most extensive comment was given by Henri Milne-Edwards who found the theory of heterogenesis so improbable in relation to general biological knowledge, and Pouchet's experiments so deficient, that he asked the audience to excuse him for taking so much of its time.

^{27.} Pasteur, Oeuvres (n. 10), II, 627-630.

^{28.} F. A. Pouchet, 'Note sur des proto-organismes végétaux et animaux, nés spontanément dans l'air artificiel et dans le gaz oxygène,' C.r. Séanc. Acad. Sci., Paris, 1858, 47, 979–983. F. A. Pouchet and Auguste Houzeau, 'Expériences sur les générations spontanées. Deuxiéme partie: Développement de certains proto-organismes dans l'air artificiel,' C.r. Séanc. Acad. Sci., Paris, 1858, 47, 982–984.

^{29.} Theodor Schwann, Vorläufige Mittheilung betreffend Versuche über die Weingährung und Fäulniss,' Annln Phys. Chem., 1837, 41, 184-193.

Milne-Edwards found that Pouchet had not taken sufficient precautions to prevent the intrusion of viable germs. For example, Pouchet introduced hay which had been heated in a flask in a steambath for thirty minutes. All germs in the hay would be killed by this treatment, Pouchet argued, because when he boiled spores of Penicillium in water for fifteen minutes they were seriously deformed. Milne-Edwards remarked tersely: 'cette raison ne me satisfait pas.'³⁰

Milne-Edwards's opinion was supported by other speakers.³¹ Jean-Louis-Armand de Quatrefages presented some observations on the distribution of germs in the air. Jean-Baptiste-André Dumas and Claude Bernard discussed the difficulties of destroying all viable germs and preventing the intrusion of new ones. Not a single voice was reported in favour of Pouchet.

In his reply to this unanimous rejection, Pouchet tried with some apparent success, to meet the technical points of the criticisms. He announced, for example, that he would soon publish the results of an experiment in which hay had been heated to 200°C., or even carbonised, and yet gave as much generation of microorganisms as before.³² Only later did Pasteur point out the crucial source of germs in Pouchet's experiment. The manipulation of the flasks where the generation took place was conducted under the surface of a vessel containing mercury to prevent the access of air. Pasteur showed that the mercury carried dust particles from the air. Pouchet's precautions to exclude germs from the air were therefore insufficient.³³ As a new argument for spontaneous generation Pouchet added that the species generated in the flasks were different from the ones growing outside. In his experiments, he claimed, the species of animalcules in the flasks had *never* been the same as the ones teeming outside.³⁴ Pouchet did not say on what kind of control experiments he based this claim.

Pouchet also argued that to explain the generation of such a variety of microorganisms the air would have to contain such an immense number of germs as to be visibly and palpably filled with them, which was obviously not the case.³⁵ In this argument he did not consider the organisms'

^{30.} Henri Milne-Edwards, 'Remarques sur la valeur des faits qui sont considérés par quelques naturalistes comme étant propres à prouver l'existance de la génération spontanée des animaux,' C.r. Séanc. Acad. Sci., Paris, 1859, 48, 23-36, p. 27.

^{31.} Comments by Payen, Quatrefages, Bernard, and Dumas, ibid., pp. 29-36.

^{32.} F. A. Pouchet, 'Remarques sur les objections relatives aux protoorganismes rencontrés dans l'oxygène et l'air artificiel,' C.r. Séanc. Acad. Sci., Paris, 1859, 48, 148-158, p. 149.

^{33.} L. Pasteur, 'Nouvelles expériences relatives aux générations dites spontanées,' C.r. Séanc. Acad. Sci., Paris, 1860, 51, 348-352, p. 352.

^{34.} Pouchet (n. 32), p. 150.

^{35.} Ibid., pp. 153-154.

power to multiply rapidly in a suitable medium. Pouchet seems to have assumed that there has to be one germ for each microorganism that appears in the medium. This assumption was quite unacceptable to Pasteur because of his experience with the growth of yeast. But as late as 1864 Pouchet repeated the same view.³⁶

In October 1859 Pouchet published his *Héterogénie ou traité de la génération spontanée*, and on 30 January 1860 the Academy of Sciences announced a prize for experiments which could 'throw new light on the question of spontaneous generation.' The commission that was appointed by the academy demanded precise and rigorous experiments taking all relevant circumstances into consideration.³⁷ Their stress on exact experimentation suited Pasteur very well and at the following meeting he presented his first results to the academy. Clearly he had been working on the problem for some time³⁸ and was now well prepared to take up the challenge of Pouchet.

Between February 1860 and January 1861 Pasteur presented five short papers to the academy containing his main results on germs and spontaneous generation. He systematically described and discussed these results in 1861 in his large memoir on spontaneous generation.³⁹

In his first paper Pasteur described how he collected dust by drawing air through a cotton plug, and by microscopical examination satisfied himself that the dust of the air contained a considerable amount of particles with a form and structure which indicated that they might be spores or germs of living organisms.⁴⁰ Pasteur also described his elegant experiments with flasks that have open but curved necks, his so-called swan-neck flasks. The curvature and small diametre of the neck caused all dust particles to settle before they reached the interior of the flask though the air could pass freely. In the flasks there was no growth of microorganisms. This experiment suggested strongly that the microorganisms arose from particles carried by the air and not from the air itself.

In his third paper Pasteur made a more direct attack on the doctrine of

36. Georges Pennetier, 'Cours de M. Pouchet. Les générations spontanées (1),' Rev. Cours Scient., 1864, 1, 265-270, p. 266.

37. C.r. Séanc. Acad. Sci., Paris, 1860, 50, 248.

38. In August 1857 Pasteur was carrying out a series of experiments on spontaneous generation, testing Schwann's results among other things. See 'Mémoire sur les corpuscules organisés qui existent dans l'atmosphere. Examen de la doctrine des générations spontanées,' in Pasteur, Oeuvres (n. 10), II, 210-294, pp. 235-236.

39. Ibid.

40. L. Pasteur, 'Expériences relatives aux générations dites spontanées,' C.r. Séanc. Acad. Sci., Paris, 1860, 50, 303-307.

heterogenesis by designing a crucial experiment. If it were the air itself which caused generation in previously sterile flasks, one should expect any volume of air to be equally effective. On the germ theory, however, one should expect the generative power to be distributed unevenly because germs are particles which will be distributed discontinuously through the air. The discontinuous distribution of germs implied that there would always be a certain chance of drawing a sample of air which contained no germs and which, therefore, would not cause the generation of microorganisms. One should also expect that the chances of obtaining such a germfree sample of air would vary from place to place according to the frequency of germs in the air. Pasteur succeeded in verifying both expectations.⁴¹

The experimental technique used by Pasteur may be sketched briefly as follows: round flasks of 250 ml. were filled with about 100 ml. of growth medium which was boiled. While the medium was still boiling, the neck of the flask was elongated and closed in a flame. On cooling the flasks contained a partial vacuum in the space above the nutrient medium. Thus, when the tip of the closed neck was broken off, air entered the flask. Pasteur could then reseal the neck of the flask and observe the effect of the sample of air on the nutrient medium.

In November 1860 Pasteur reported to the academy the following results: of twenty flasks opened in the countryside at the foot of the Jura Mountains, eight produced microorganisms. Of twenty opened at the top of the Jura Mountains, about 850 metres above sea level, five produced microorganisms. And of twenty opened on a glacier at an altitude of 2,000 metres, only one produced organisms.⁴²

On the basis of such experiments Pasteur challenged the supporters of spontaneous generation with the claim that it is always possible in any location to obtain a sample of air which is incapable of causing the generation of microorganisms.

It is always possible to extract at any given time and place a sizable volume of ordinary air, which has not been subjected to any physical or chemical alteration, and is nevertheless completely unfit to give birth to infusoria or moulds, in a liquid that is always very quickly altered in free contact with the atmosphere.⁴³

^{41.} Pasteur (n. 33).

^{42.} L. Pasteur, 'Suite a une précédente communication relative aux générations dites spontanées,' C.r. Séanc. Acad. Sci., Paris, 1860, 51, 675-678.

^{43. • . . .} il est toujours possible de prélever dans un lieu et a un instant donné un volume considérable d'air ordinaire, n'ayant subi aucune espèce d'altération physique ou chimique, et néanmois tout à fait impropre a donner naissance à des infusoires ou à des mucédinées, dans une liqueur qui s'altère très vite et constamment au libre contact de l'air.' Pasteur (n. 33), p. 350.

In the autumn of 1863 the explicit rejection of Pasteur's experiment by Pouchet, Joly, and Musset became the starting point of a second round of controversy.

During 1860 and 1861 Pouchet also communicated the results of a series of investigations to the Academy of Sciences. His main answer to Pasteur was that the atmosphere contains too few germs to provide an explanation for the generation observed. Most of Pouchet's observations were vague and the arguments he drew from them were weak by the standard of experimental method used by Pasteur. In various ways Pouchet sought to analyse the atmospheric air for germs. He constructed for this purpose an apparatus that drew air through an aperture of less than 0.5 mm. against a glass plate.⁴⁴ Here, according to Pouchet, the dust particles collected within an area of a few square millimetres. To ensure that no particles escaped, the glass could be covered with a sticky substance. With the help of this instrument Pouchet confirmed his earlier contention that 'spores of plants and eggs of infusoria' are 'infinitely rare' in the atmosphere. For instance, in one cubic metre of air from his own laboratory, Pouchet did not find a single germ.

Pouchet challenged the germ theorists to demonstrate the presence of the right kind of germs in the air and not just assume that they were there.⁴⁵ But Pasteur evaded this challenge. The collection and identification of the germs floating in the air was an extremely difficult task with the methods then available.

From the scarcity of germs in the air, Pouchet argued that the microorganisms generated in his vessels could not possibly have come from the air but had to be generated spontaneously. He experimented with the addition of atmospheric dust to his vessels and found no increase in the number of microorganisms with the addition of atmospheric dust, as the germ theory would imply.⁴⁶ Pouchet still disregarded the rapid multiplication of microorganisms in suitable media. In a communication of October 1860, he claimed explicitly that multiplication of organisms by division played no role in his experiments and that there should be as many germs in the air employed as there were animalcules produced in the medium.

^{44.} F. A. Pouchet, 'Moyén de rassembler dans un espace infiniment petit tous les corpuscles normalement invisibles contenus dans un volume d'air déterminé.' C.r. Séanc. Acad. Sci., Paris, 1860, 50, 748-750.

^{45.} Ibid., p. 750.

^{46.} Ibid., p. 749.

In these experiments I have made sure, as in all former ones, that propagation by division has not played any role and that it is the same way in normal reproduction. It should follow that, to explain the phenomena I have observed, one must find as many eggs in the air as there are animalcules produced.⁴⁷

In June 1860 Pouchet reasserted his claim of a year and a half before: 'proto-organisms' are generated 'in a hermetically closed apparatus, heated to 100 degrees and receiving only air washed in sulphuric acid or raised to a temperature of glowing red.' According to Pouchet this fact had not been seriously contested. Furthermore, he had now improved the procedure so that the experiment was constantly successful. In Pouchet's view 'clearly the organisms could not have been introduced from outside.'⁴⁸ The presence of organisms in the vessel at the close of the experiment was an undisputed fact, but this was hardly the kind of fact that could clinch Pouchet's argument for heterogenesis. And three months later Pasteur could point to the dust in the mercury bath as a crucial source of germs. Pouchet had failed to establish that germs could not have been introduced from the outside.

In this same paper Pouchet repeated his claim that the organisms generated in the vessel usually belong to species different from those found outside, therefore, both could not have arisen from the same source, namely, the germs in the air.⁴⁹ Pouchet also asserted that 'all physiologists unanimously agree that no egg, no animal, no plant can resist a humid temperature of 100 degrees.'⁵⁰ Pasteur had shown in May 1860 just a month before that in milk a temperature of 100°C. was not enough to kill all germs.⁵¹

Most revealing of Pouchet's naive belief in the unequivocal nature of observed facts, and his uncritical use of them, was a paper he communicated to the Academy of Sciences in 1861. In it he claimed that the biology of the yeast of alcoholic fermentation was quite different from that described by Charles Cagniard de Latour, Eilhard Mitscherlich, Theodor

48. F. A. Pouchet, 'Genèse des proto-organismes dans l'air calciné et à l'aide de corps putrescibles portés à la temperature de 150 degrés,' C.r. Séanc. Acad. Sci., Paris, 1860, 50, 1014-1018.

49. Ibid., p. 1016.

50. Ibid., p. 1017.

51. L. Pasteur, 'De l'origine des ferments. Nouvelles expériences relatives aux générations spontanées,' C.r. Séanc. Acad. Sci., Paris, 1860, 50, 849-854.

^{47. &#}x27;Dans ces expériences, je me suis assuré, comme dans toutes celles que j'ai déjà faites, que la scissiparité n'a joué aucun rôle et qu'il en a été de même de la réproduction normale. Il eût donc fallu, pour expliquer les phénomènes que j'ai observés, rencontrer dans l'air autant d'œufs qu'il s'est produit d'animalcules...,' F. A. Pouchet, 'Analyse mécanique de l'air atmosphérique en différents lieux, pour servir à l'histoire des générations spontanées; extrait d'une Note adressée, de Messine,' C.r. Séanc. Acad. Sci., Paris, 1860, 51, 524.

Schwann, and Louis Pasteur, namely, that the yeast globules are simple plants which can propagate by budding. According to Pouchet the yeast globules were generated spontaneously in the fermentation medium and were in fact the germs of a mould, an *Aspergillus*. Pouchet had observed how the yeast globules gave rise to webs of mould mycelium, and he claimed confidently that the yeast globules were never propagated by budding.⁵² Although the science of microbiology in 1860 was still in its infancy and the techniques for cultivating and studying microorganisms were poorly developed, Pouchet stated his mistaken conclusions with a certainty that is hard to justify.

In November 1862 Pouchet withdrew from the competition⁵³ and on 29 December Pasteur was awarded the prize of the academy for experimental contributions to the problem of spontaneous generations.⁵⁴

Pasteur had repeatedly challenged the heterogeneticists with his claim that in any location one might obtain samples of air that did not provoke generation of microorganisms. Pouchet had made contrary statements, for example, that in a series of experiments on the Mediterranean coast a cubic decimetre of air had always generated an abundance of ciliated infusoria,⁵⁵ without directly challenging the correctness of Pasteur's experimental claims. But in September 1863 Pouchet, Joly, and Musset presented to the Academy of Sciences experimental evidence in explicit defiance of Pasteur's challenge.⁵⁶ They began by quoting Pasteur's claim which they stated had already been disproved by numerous experiments. Pouchet and his collaborators now wished to remove all remaining doubts by using air from high mountains. Pasteur had asserted that such air contained particularly few germs and that one could, therefore, obtain samples of sterile air more easily in high mountains than in other locations.

Pouchet and his collaborators carried out their experiment in the Pyrenees at an altitude of more than 2,000 metres with flasks containing hay infusion. They opened four flasks at the village of Rencluse and four in a crevice of the glacier of Maladetta. In their paper they described how two flasks from each location after a few days contained an abundance of bacteria or other microorganisms. However, Pasteur made several objections

^{52.} F. A. Pouchet, 'De la nature et de la genèse de la levure dans la fermentation alcoolique,' C.r. Séanc. Acad. Sci., Paris, 1861, 52, 248-288.

^{53.} C.r. Séanc. Acad. Sci., Paris, 1862, 55, 785.

^{54. &#}x27;Prix Alhumbert pour l année 1862,' C.r. Séanc. Acad. Sci., Paris, 1862, 55, 977-979.

^{55.} Pouchet (n. 47).

^{56.} F. A. Pouchet, N. Joly, and Ch. Musset, 'Expériences sur l hétérogénie exécutées dans l'intérieur des glaciers de la Maladetta (Pyrénées d'Espagne),' C.r. Séanc. Acad. Sci., Paris, 1863, 57, 558-561.

to the experiments of Pouchet and his collaborators.⁵⁷ He pointed out that they had not followed his technical procedure precisely. After opening the flasks to admit the air they had shaken them before they were sealed.⁵⁸ Also they had used a file to break the tip of the sealed flasks instead of pincers as Pasteur did. In using a file the experimenter is liable to put his thumb against the rim of the glass opening, and the thumb could not be sterilised in a flame, Pasteur pointed out.⁵⁹ Both these departures from Pasteur's procedure increased the chances of contamination.

Secondly, Pouchet and his collaborators opened only four flasks at each location, and Pasteur considered this number insufficient to secure a statistically representative sample. He also pointed out a telling incompleteness in the report: only two flasks from each location were reported to contain microorganisms.⁶⁰ If the other flasks remained sterile the experiment would confirm rather than refute Pasteur's work. Joly and Musset later affirmed that all eight flasks had contained organisms, 'all our infusions were populated with microphytes and microzoa.'61 But the incompleteness of the original report nevertheless revealed a serious lack in their understanding of the experimental method that Pasteur had used. The incompleteness of the data was not a mere slip. When Pasteur asked Pouchet whether there had been growth in the remaining four flasks, Pouchet could not give an answer before consulting his collaborators.⁶² In his reply to Pouchet, Joly, and Musset, Pasteur repeated his challenge that they show, in any location, that a large number of flasks prepared precisely according to his directions would all contain microorganisms.

As long as Messrs. Pouchet, Joly and Musset cannot confirm that by opening in an arbitrary location a large number of flasks, prepared precisely according to the directions of my memoir, none will remain intact, all will be altered, they have done nothing but confirm the perfect correctness of the claim in my memoir which they pretend to refute. I challenge them to produce a result of this kind.⁶³

61. N. Joly and Ch. Musset, 'Réponse aux observations critiques de M. Pasteur, relatives aux expériences exécutées dans les glaciers de la Maladetta,' C.r. Séanc. Acad. Sci., Paris, 1863, 57, 842-845. 62. L. Pasteur, 'Remarques,' C.r. Séanc. Acad. Sci., Paris, 1863, 57, 846.

63. 'Tant que MM. Pouchet, Joly et Musset ne pourront pas affirmer qu'en ouvrant dans une localité quelconque un grand nombre de matras, préparés exactements selon les prescriptions de mon Mémoire, il n'y en a pas qui se conservent intacts, et que tous s'altèrent, ils ne feront que confirmer l'exactitude parfaite de l'assertion de mon Mémoire qu'ils prétendent réfuter. Or, je mets au défi que l'on produise un pareil résultat.' Pasteur (n. 57), p. 726.

^{57.} L. Pasteur, 'Note en réponse à des observations critiques présentées à l'Académie par MM. Pouchet, Joly et Musset, dans la séance du 21 Septembre dernier,' C.r. Séanc. Acad. Sci., Paris, 1863, 57, 724-726.

^{58.} Ibid., p. 725 fn.

^{59.} Ibid., p. 725.

^{60.} Ibid., pp. 725-726.

Joly and Musset in answer rejected Pasteur's criticism of their experiment. But they accepted his challenge to repeat the experiment precisely according to his directions.

We pick up the gauntlet which has been thrown to us by our scientific opponent, and we promise him to conform even more scrupulously than before to all the minute details which he points to as absolutely indispensable. If a single one of our flasks remain unaltered in contact with air taken at Toulouse, we will loyally concede our defeat. If all are populated with infusoria or moulds, what will Mr. Pasteur answer and do?⁶⁴

Joly and Musset proposed that the Academy of Sciences name a commission to judge the conflicting claims. Two weeks later Pouchet also subscribed to the declaration of his two colleagues.⁶⁵ He asserted that a cubic decimetre of air taken anywhere on earth will always generate living organisms. But Pouchet did make a small reservation concerning Pasteur's methods which he found often to 'paralyze' the 'biological phenomena.' Pouchet preferred flasks of a somewhat different form and a different fluid nutrient medium.

Pouchet, Joly, and Musset had now publicly committed themselves to disprove Pasteur's experiment. And they had themselves proposed that a commission named by the Academy of Sciences judge the experimental evidence of the two parties to the conflict. This was a situation which suited Pasteur ideally because the controversy had been pinned down to a crucial experiment of his choosing. On 4 January 1864 the commission was nominated. But the start of its work was postponed until the summer because Pouchet and his collaborators claimed that winter was a poor time for heterogenesis. When the commission and the contestants met in June it turned out that Pouchet, Joly, and Musset were not interested in merely repeating Pasteur's experiment; they wanted also to do quite different investigations. At the commission's request they produced a program of investigations ranging in order of importance. They placed Pasteur's experiment, which the commission considered crucial, in the last rank.⁶⁶

^{64. &#}x27;... nous relevons le gant qui nous est jeté par notre savant antagoniste, et nous lui promettons de nous conformer, plus scrupuleusement encore que nous ne l'avons fait, à toutes les plus minutieuses précautions qu'il indique comme étant rigoureusement indispensables. Si un seul de nos matras demeure inaltéré au contact de l'air pris à Toulouse, nous avouerons loyalement notre défaite; si tous se peuplent d'Infusoires ou de Mucédinées, que répondra et que fera M. Pasteur? Joly and Musset (n. 61), p. 845.

^{65.} F. A. Pouchet, 'Adhesion à la protestation contenue dans une Note récente de MM. Joly et Musset,' C.r. Séanc. Acad. Sci., Paris, 1863, 57, 902-903.

^{66. &#}x27;Rapport sur les expériences relatives à la génération spontanée (commissaires MM. Flourens, Dumas, Brongniart, Milne-Edwards, Balard rapporteur),' C.r. Séanc. Acad. Sci., Paris, 1865, 60, 384-397, p. 387.

The commission wanted both Pasteur and Pouchet and his associates to make parallel experiments with yeast extract and hay infusions respectively. Each party was to open a series of about twenty flasks at each of three different locations in Paris and its vicinity. But Pouchet, Joly, and Musset demanded a broad program of investigations: 'microscopic analysis of the air of the amphitheatre where we operated, microscopic analysis of a litre of beer, etc.'67 The commission did not accept their program because they did not believe such investigations could lead to any clear result. Pouchet, Joly, and Musset then withdrew and Pasteur proceeded with his own series of experiments, which produced results in perfect accord with his claims. Afterwards the commission also conducted some preliminary experiments with hay infusions with results that pointed in the same direction. But since the best season for heterogenesis was already past, the commission's experiments were discontinued without reaching any definite conclusion.⁶⁸ The judgment of the commission, presented 20 February 1865, was that Pasteur was correct in his factual claims and that Pouchet and his collaborators had produced no good reason to reject them.⁶⁹

Various authors have suggested that the positive results which Pouchet, Joly, and Musset obtained with hay infusions were due to heat-resistant spores, in particular to spores of the hay bacterium *Bacillus subtilis*, which are not killed by boiling at 100°C. but which will germinate only in the presence of oxygen. If Pouchet, Joly, and Musset had not lost their nerve and withdrawn from the contest, Pasteur would have been put in a very embarrassing position because their flasks would have shown a regular generation of bacteria on the admission of air.⁷⁰ *Bacillus subtilis* may have been the source of some of the positive results, although the description of the contents of the flasks from the Pyrenees indicate clearly that there were also organisms other than *Bacillus subtilis* present. Joly's and Musset's claim that all their infusions contained both microphytes and microzoa suggests that contaminants were introduced during the experiment.

However, if Pouchet, Joly, and Musset had produced positive results with their hay infusions before the commission in 1864, there is little doubt that Pasteur would have checked the possibility of heat-resistant spores in the medium. He had already shown the presence in milk of forms that resisted boiling temperatures. It is, therefore, quite unlikely, that Pouchet's

^{67.} Ibid., p. 389.

^{68.} Ibid., p. 397.

^{69.} Ibid.

^{70.} For instance, in Duclaux, Pasteur (n. 7), p. 141; Dubos, Pasteur (n. 8), p. 175; Farley and Geison (n. 1), p. 193.

flasks of boiled hay infusions might have lent 'crucial support to spontaneous generation during the 1860s,' as stated by Farley and Geison.⁷¹

Pasteur has been criticised for not repeating Pouchet's disputed experiment from the Pyrenees. But such criticisms overlook the fact that Pouchet's experiment was designed to disprove Pasteur's hypothesis about germs in the air. The main conclusion that Pouchet, Joly, and Musset drew from the experiment was a refutation of Pasteur's claims. Pasteur in his reply pointed out serious weaknesses in the experiment and its interpretation. If the experiment could not be repeated under more carefully controlled conditions, Pasteur was quite justified in neglecting it. Repetition of the experiment was just what Pouchet and his associates failed to carry out before the commission in 1864.

Pasteur did not break any rules of experimentation in his unwillingness to repeat the experiment that Pouchet and his collaborators conducted in the Pyrenees. Quite the contrary, it was Pouchet, Joly, and Musset who did not appreciate the importance of meticulous care about details, the need for precise repeatability of experiments, and for control experiments. Pasteur pointed out crucial weaknesses in their experiment. They were unable to produce a properly revised experiment to support their case and withdrew from the contest. The decisions of the Paris Academy of Sciences in 1864, in the second phase of the controversy, had a sound scientific basis. On the basis of accepted rules of inquiry and the evidence provided, they could hardly avoid a scientific verdict in favour of Pasteur's claims.

Doubts about the scientific propriety of Pasteur's behaviour in the 1863-64 controversy is part of the classical accounts of his work written by Duclaux, Dubos, and Dagognet. Farley and Geison have developed such doubts into strong criticism. Both doubt and criticism are based on an excessively fact oriented, 'inductivist,' view of scientific method. On a standard hypothetico-deductive view Pasteur was not to be reproached. At the time the hypothetico-deductive view was well developed by leading members of the Academy of Sciences, for instance by Claude Bernard, and was professed by Pasteur himself. Pouchet was more fact oriented and had little understanding of the necessary role of theories in scientific research.

The Pasteur-Pouchet debate is not a suitable case to demonstrate the influence of external factors on the conclusions of basic science because Pasteur's scientific and technical superiority is so clear throughout the controversy. There is, consequently, little room for an interesting interference from external factors.

71. Farley and Geison (n. 1), p. 193.

The Pasteur-Pouchet controversy is better suited to support the traditional internalist view that the most beneficial basic science is a methodologically pure science which avoids external considerations and concentrates on establishing factual truths in its field. Pasteur, in contrast to Pouchet, was careful not to appeal to general beliefs of religious or other nature in support of his scientific claims. Similarly the commissions of the Paris Academy of Sciences focused on central factual discoveries and claims and avoided general discussions in which it would have been hard to draw a line between science on the one hand and politics and religion on the other.

Institute for Studies in Research and Higher Education The Norwegian Research Council for Science and the Humanities Oslo