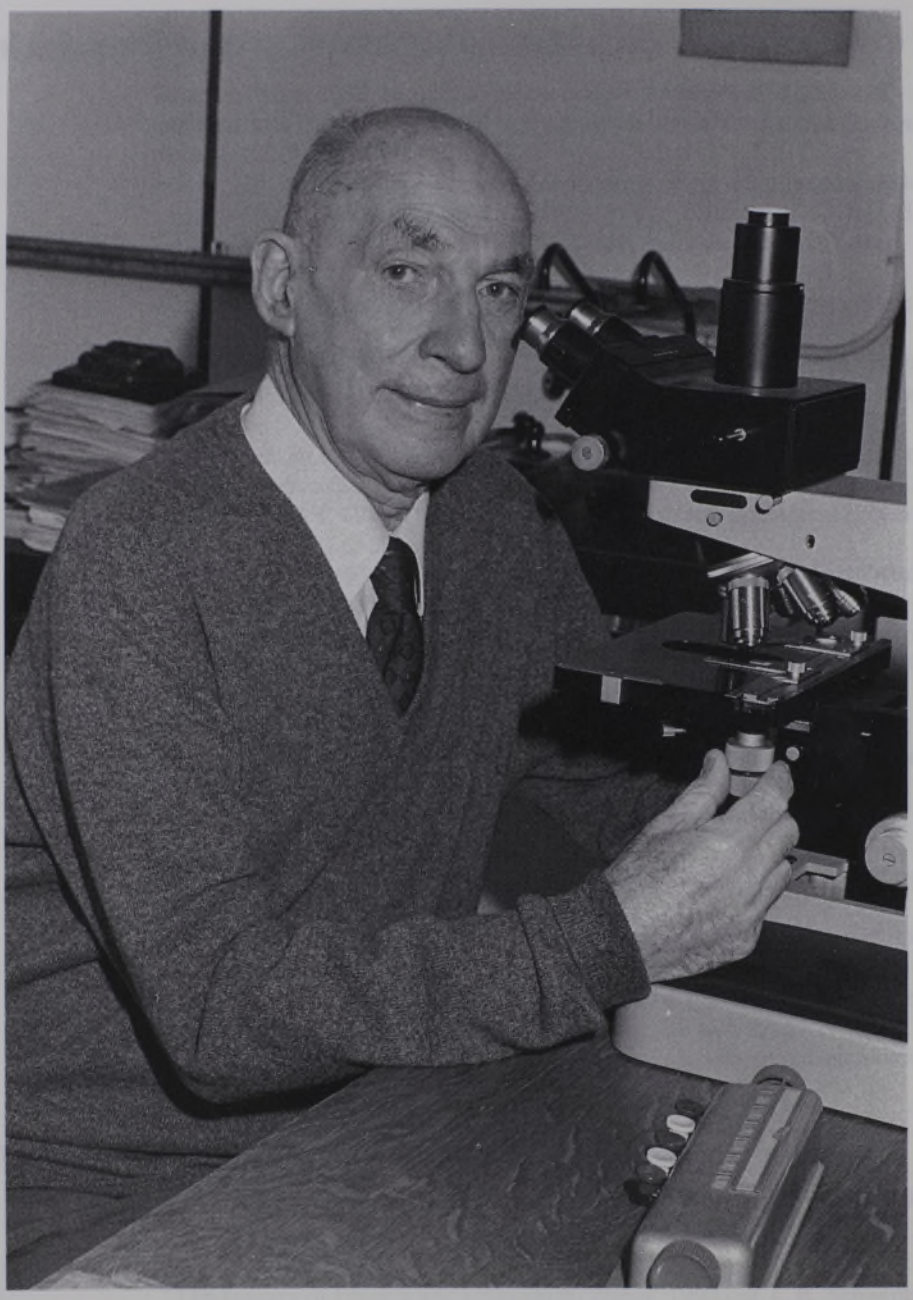
JEAN BRACHET
19 March 1909—10 August 1988



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Elected F.R.S. 1966

BY N.W. PIRIE

JEAN LOUIS AUGUSTE BRACHET was born in the Etterbeck district of Brussels. His father, Albert Brachet, was an experimental embryologist, Professor of Anatomy and Embryology at the *Université Libre de Bruxelles* (ULB), and ultimately Rector there. In August 1914, when Belgium was invaded, he was working in the marine biology laboratory at Roscoff in Brittany. The family therefore remained in France until the end of the war. Jean went to the Ecole Alsacienne in Paris and to the Athénée Royal d'Ixelles in Brussels. He said that he failed repeatedly in biology examinations because of his dislike for classification. While studying medicine at ULB he said (TV)* that he wondered at times whether he had made the right decision and whether archaeology, chemistry or philosophy would not be more interesting, but he graduated in Medicine with '*la plus grande distinction*'.

Experience in hospitals and clinics convinced him that he was temperamentally unsuited for medicine. Familiarity with his father's embryological research, and conversation with his father's friends, lured him into research. While a student he had heard that the anucleate half of a divided cell could often retain many capacities; even motility. That excited his imagination and made him decide to be an embryologist. When he learnt of the widely differing survival times of anucleate cells, 1–2 days with mammalian cytoplasts, ten days with amoebas and months with the alga *Acetabularia*, he was convinced he had made the right decision. The problem was: where should he work? Following closely in his father's footsteps, and working in his laboratory, was naturally '*chose dont je ne voulais sous aucun prétext*' (1987*a*). His father sympathized with Jean's difficulty and arranged that he should work in the laboratory of his colleague Albert Dalcq.

When Brachet worked in the Biochemistry Laboratory in Cambridge in 1934 he took a sympathetic interest in the political activities of many of those working there. As a foreigner with a keen social sense he naturally did not participate in these activities: furthermore, there is no record of his involvement in politics in Belgium at that time. The Spanish civil war, and then the German occupation of Belgium, affected him profoundly. His brother, Pierre, went to Spain as a reporter, joined the government army, and was killed near Madrid. His

* This refers to a television interview in October 1971.

mother was very active in collecting money and sending medical supplies to Spain and, before the 1938–45 war, in several antifascist associations. After the war she was secretary to the communist Minister of Health in the Belgian Government.

In June 1940 the Germans occupied Belgium and set about 'reforming' the universities, especially ULB which the military administration regarded as a 'haunt of liberals and free-masons'. All Jews and members of the staff who had shown hostility to Germany were to be dismissed. After a period of turmoil, the military government made three new appointments (one of those to be appointed had been condemned as a collaborator in the 1914–18 war) on 22 September 1941. In September the University was closed by the Administrative Council: it was occupied by the army and did not function again as a university until the Liberation. Having failed to persuade the University to reopen, the Germans spitefully took Brachet and some other members of the staff as hostages on 9 December and imprisoned them in the citadel of Huy for three months.

When released, Brachet had little money and no laboratory. He was supported for a time by a brewery which wanted him to write a report on the nutritional value of alcohol. His report cited evidence that alcohol harms people in the half-starved state that many Belgians were in at that time. The support ended. Some friends of the University organized a clandestine fund to support dismissed members of the staff. That enabled Brachet to write *Embryologie chimique*, which was published in Paris in 1944. During this difficult period he also managed to write 15 papers, mainly on themes connected with nucleic acids.

After the end of the war, he joined the Communist Party. That move was prompted more by recognition of its effective role in the Resistance than by ideology. He was a materialist who defined his philosophical outlook as 'operational certitude' and found the hypothesis that there is a god, superfluous. When interviewers (TV) pointed out that some other scientists were not atheists, he replied that he had friends like that in Israel and Louvain who, unlike him, had compartmentalized minds and '*les deux cases ne se mélangent pas*'. Because of this self-reliance he could not tolerate Party discipline and Marxist ideology. Having rejected traditional forms of religion, he was disinclined to adopt a novel one, even if it seemed more up-to-date. While still in the Communist Party, he was asked to lecture on Lysenko's ideas about genetics and therefore visited the U.S.S.R. to find out what they were. After meeting Lysenko he was convinced '*absolument de l'erreur dans laquelle il se trouvait*'. This was his final reason for quietly leaving the Communist Party. He remained a socialist and was optimistic about the future because we have '*une capacité d'adaptation prodigieuse*'. His only regret over the whole episode was that it made getting a U.S. visa tedious (TV).

During the International Biochemical Congress in Brussels in 1955, Brachet gathered together for lunch all those he knew of who were actively concerned with peace movements. Soon after, International Physicians for the Prevention of Nuclear War was founded in the U.S.A., Professor M. Errera established, with Brachet's active encouragement, its francophone branch, *Association Médicale pour la Prévention de la Guerre Nucléiare*. Brachet took little part in running the organization, but gave advice on activities and talked occasionally at meetings. His continued enthusiasm for this subject is clearly shown by the last words of *Molecular cytology* (1985), 'What we must also do is to admire, respect, and

love life – we must protect it and not destroy it. We biologists must work more than anyone else for peace.'

In 1934 he married Françoise de Baray, a pianist and daughter of a doctor. One son is a doctor in the University Hospital in Brussels, their daughter was a dentist and is now a painter, another son studies slime moulds.

RESEARCH

Assay and distribution of nucleic acids

During the first half of this century research on nucleic acids had become unfashionable. This was partly because increasing evidence for the versatility of proteins and polysaccharides directed attention towards them, but also because the ridiculous tetranucleotide hypothesis, which made nucleic acids seem very simple, was widely accepted. This is a classic example of what Sir Harold Hartley, F.R.S., called 'Lavoisier's fallacy' (the assumption that logic can be relied on to fill gaps in the chemical evidence). It was assumed that, because four different nucleotides were found in every nucleic acid, just one of each is always present. That may have been logical, but there was no evidence for the assumption although it got into most text books. Levene, the originator of the hypothesis was more cautious. When, in 1936, I teased him about it, he smiled and said, 'After all, I only called it a hypothesis'.

Those familiar with what analytical evidence there was, and with the physical properties of nucleic acid solutions, thought there was as much scope for specificity in nucleic acids as in any other type of macromolecule. Nevertheless, the false assumption that nucleic acids were trivial was so widely made that Brachet, at the very beginning of his research career, showed unusual independence and prescience in accepting Dalcq's advice that he should study thymonucleic acid (now deoxyribonucleic acid or DNA). His first paper (1929) was on its behaviour during oogenesis in various animal species. Almost all his publications are to some extent connected with nucleic acids; about half have them as their main theme. He originated or reinforced, either by making a suggestion or by supplying evidence, many of the fundamental ideas, e.g. that genes contain DNA, and that RNA (ribonucleic acid) controls protein synthesis. Although he said (TV) that he preferred working with a microscope to making chemical analyses, he was as influential as anyone in redirecting biochemistry and establishing the dominant position of nucleic acids in biochemical research.

Embryologists and biochemists made little contact in ULB. As Brachet (1987*b*) put it: 'It was quite an event when I went through the 'tunnel' to the Biochemistry library to read Feulgen's original paper: nearly an act of treason to my friends of the Anatomy Institute.' That enterprising act, and the related one, when he read Unna's paper on staining with pyronine and methyl green, had important consequences. DNA gives Feulgen's reaction (restoration of colour to sulphite-treated fuchsin) after brief acid hydrolysis, but the specificity of the action was widely doubted. He improved its specificity by careful attention to detail and used the reaction throughout his life although he remained critical of it because the intensity of colour did not follow Beer's Law closely, and substances other than DNA

gave the reaction. He had, for example, to retract (1952) his early suggestion that RNA was converted to DNA in frog embryos. That mistake arose from interference by furfural derived from the jelly coat. He later (1967*a*, 1975*a*) worked on genuine examples of ribonucleotide reductase action. Until fertilization, the enzyme is not detectable in sea urchin eggs and is feeble in amphibian eggs. It then increases to a maximum at about the four cell stage and later becomes undetectable.

Two dogmas were quickly overthrown. DNA, hitherto thought of as a peculiarly animal product, was found universally in plants, not only in nuclei but also in chloroplasts. It was generally accepted as a chromosome component during part of the mitotic cycle, but it was assumed that it was not an essential gene component because it seemed to disappear during part of the cycle. According to received opinion, heredity should not have such a fickle foundation. Brachet demonstrated that DNA did not disappear as he (1929) and others had thought, but became much more finely dispersed (1937*b*).

Both RNA and DNA contain purines, it was assumed that there was no RNA in animal cells, it should therefore be possible to follow the behaviour of DNA during embryonic development by measuring the amount of purine present. Brachet found more purine than corresponded with the amount of DNA as measured by Feulgen staining. He identified the reason for the discrepancy: the extra purine came from RNA in animal cells. Many objections were raised against this conclusion. He rebutted them patiently and often humorously. The best evidence came from comparing, by Unna staining under the microscope, two slides of the same material, one of which had been incubated, before staining, with highly purified ribonuclease. During this period he worked with Joseph Needham, F.R.S., in Cambridge. Needham was at first one of those who doubted Brachet's conclusions, so he discussed the matter with Professor Sir Frederick Hopkins, P.R.S., and got the characteristic reply; 'Tell the young man to trust his experiments and not to believe all that is in textbooks: they are full of errors'. This response delighted the iconoclastic Brachet so much that he tells the story in several articles.

As centrifuges improved in performance during the 1930s, interest increased in the properties of the fractions into which tissue extracts can be separated. In several institutes, particles containing RNA, which were later called ribosomes, were found in animals and plants. Using a primitive air-driven centrifuge made by Henriot, Brachet & Jeener prepared ribosomes in 1942. Brachet was extremely excited by the RNA gradient from the animal to the vegetal pole in amphibian oocytes. These experiments were discussed in two review articles (1947, 1952). He and his colleagues were among the original observers of the correlation between protein synthesis, rather than other forms of vigorous metabolism, and the presence of RNA (1941). For example: the silk gland of silk worms, which is not known to do anything except synthesize protein, has an exceptional RNA content, and ribosomes from nucleated erythrocytes and pancreas, in spite of repeated washing, carry with them some haemoglobin and insulin respectively (1942, 1947).

Ribosomes resemble some viruses so closely in size and chemical composition that several scientists considered the possibility that they might be the end product of a virus infection. This suggestion is an extension of Altmann's old hypothesis which, with the title 'endosymbiosis', is gaining some respectability after a period of oblivion, and of the related

'plasmagene' hypothesis which is less respectable. Brachet (1949) could not get evidence that ribosomes, either free or bound to membranes (microsomes), behaved as if infectious. But, after considering many lines of evidence, he (1952) concluded:

> To summarize: it is not yet possible to state with certainty if the microsomes, like viruses, are capable of multiplying and of passing from cell to cell; however, if the proof of this hypothesis still remains to be found, it nevertheless rests on a large number of facts which are otherwise difficult to explain; moreover, no experimental argument against it is known.

A later review (1971) discussed cytoplasmic self-reproducing organelles without including ribosomes, and still later (1985) he dismissed the idea because of evidence about the relationship between ribosomes and the reticulo-endothelium. Nevertheless, he dealt sympathetically with the suggested invasive origin of chloroplasts and mitochondria and with the suggested escape of some mitochondrial DNA into the nucleus in the pre-Cambrian. But he commented that 'this is not easy to prove experimentally' (1985).

Among the many techniques used by Brachet and his colleagues for studying the behaviour of RNA, that are described in detail in *Biochemical cytology* (1957), exposure to purified ribonuclease was particularly informative. Amoebae, onion root tips and oocytes, but not algae, moulds and yeasts, readily absorb many basic proteins. When members of the first group were exposed to a solution of the enzyme, growth and protein synthesis were inhibited and the treated cells no longer gave staining reactions characteristic of RNA. By washing the treated materials and then soaking in solutions of yeast nucleic acid, the changes could be, to a great extent, reversed. Results with onion roots were particularly interesting because only 1–2 h were needed to demonstrate inhibition and recovery. There was some specificity in the process: onion RNA was more effective than yeast RNA in reversing the effect of ribonuclease on onions.

Nuclear functions

Brachet records in several places that he was drawn towards Embryology less by his father's influence than by learning of the survival of many cellular activities after enucleation. Till then he had thought of enucleation as equivalent to decapitating an animal. He was not alone in making the understandable, if illogical, assumption that the most noticeable component of a cell would be metabolically very important. Others had argued, with little or no evidence, that the nucleus was the site of protein synthesis, energy production and enzyme storage. It would not be unfair to call Brachet's refutation of these ideas an obsession. More than a page in *Molecular cytology* (1985) deals with contamination as the origin of what energy production and protein synthesis there is in nuclei. He also argued that, although RNA is synthesized in nuclei and thymidine is incorporated into DNA, DNA synthesis is not initiated there. He thus paved the way to the now generally accepted idea that nuclei guide rather than drive the activities of cells.

It was well known that *Acetabularia*, the largest single-celled organism, survived enucleation and was still able to make its characteristic cap or umbrella. Brachet *et al.* (1955*d*) found that, for a few weeks, energy production and photosynthesis continued as before, the rate of protein synthesis was at first increased, and RNA was still synthesized. These observations greatly diminished the range of activities for which the presence for a

nucleus was essential. They were puzzling until DNA was found in its chloroplasts (1963, 1965*a*). Until maturity, the nucleus of *Acetabularia* is confined to the rhizoid at one end of a 30–100 mm stalk, containing chloroplasts. By careful dissection, chloroplasts uncontaminated by nuclear fragments can be isolated. With this organism therefore, the claim that DNA is present in chloroplasts was more convincing than the claim which was being made with other plants at about the same time. Brachet started work on *Acetabularia* to find out how fragments could function so well and for so long without nuclei: he found instead that ostensibly anucleate fragments contained many pseudo-nuclei in the chloroplasts.

Caps are not formed while *Acetabularia* are kept dark. Brachet *et al.* (1955*d*) found that caps formed on nucleated fragments illuminated after long periods of darkness, but not on enucleated fragments which had been kept dark for four weeks. From this, and from similar experiments in which cap formation had been reversibly inhibited by pharmacological agents, Brachet *et al.* (1964) deduced that the morphogenic agent was made in the nucleus, passed into the cytoplasm and had a half-life there of about ten days. Before DNA had been discovered in chloroplasts, he had evidence that morphogens and the agents responsible for the synthesis of many different enzymes in enucleated fragments were RNAs. He outlined his hypothesis in the sixth Weizmann Memorial Lecture in 1959 (1960*b*): 'specific DNA molecules (or parts of molecules), corresponding to each gene, would act as a template for RNA synthesis; there would be as many specific RNA molecules as there were genes. Finally, each specific RNA molecule would act as a template for a specific protein'. That foreshadowed the concept of messenger RNA (mRNA). The main difference is that Brachet's morphogens are more durable *in vivo* than bacterial mRNAs, which were originally postulated as controllers of protein synthesis. As he put it (1987*c*); 'Variations in the life span of anucleate cells result primarily from differences in the stability of their mRNAs'.

Much of Brachet's work was concerned with the extent of nuclear influence on activities in the cytoplasm. Work on *Acetabularia* suggested that cap formation was promoted by a steady supply of mRNA from the nucleus, whereas the removal of nuclear influence was held responsible for the brief increase in RNA synthesis in enucleated fragments. But he commented; 'Nothing is known about the molecular mechanisms involved in negative nuclear control in eggs and in *Acetabularia*: this is another promising field for future research' (1985).

Amoeba proteus does not give such a striking sign of developmental competence as cap formation and its cells are inconveniently small for analytical work: 1200 of them had to be cut into nucleate and anucleate fragments in some experiments. Nevertheless, Brachet and his colleagues used them in many early experiments. He wrote of them (1957) 'It is only in amoeba that enough is known about the 'general metabolism' of anucleate fragments to allow fruitful discussion'. That phrase was repeated in 1961(*b*) although he had already (1960*a*) begun to have qualms about the validity of some of his conclusions. He made little more use of amoebae because (1961*b*) they often contain undigested and still living bacteria, or even endosymbionts (1976*b*). Work on them was not discussed in his last three reviews (1987*a*, *b*, *c*).

In spite of the probability that amoebae contained contaminants, some of the results probably remain valid. Anuclear fragments quickly become spherical and lose mobility and the ability to stick to surfaces. Intact amoebae lose mobility and become spherical when soaked in solutions containing RNAase; they partially recover when washed and recover more completely if soaked in yeast nucleic acid solution. They lose RNA and the ability to synthesize protein (measured by phenylalanine incorporation) when soaked in RNAase (1955*a*). Because amoebal movements resemble a muscular activity, it was logical to try the effect of amoebae on adenosine triphosphate. Intact cells and fragments destroyed it at similar rates, after five days the non-motile anucleate fragments had lost much of this enzymic activity (1961*a*). Anuclear fragments survived nearly as long as nuclear fragments although no longer able to feed. Both respire at similar rates, but in the latter it is the substances responsible for basiphilia, and in the former it is glycogen and glycerides, that are being destroyed (1955*a, c*). These differences are shown vividly in photographs of stained preparations. Because of the possibility of microbial contamination, less reliance can be put on evidence (1961*a*) that protein synthesis by anucleate fragments, and differences in the rates at which enzymic activity is lost (1955*c*), are genuine properties of the amoebae.

Sea urchin eggs

Before Brachet started research, sea urchin eggs had been split into nucleate and anucleate fragments by centrifuging in a sucrose gradient. Each fragment can then go through the first stages of development if parthenogenetically activated by exposure to concentrated sea water. Brachet was well aware that centrifuging eggs, without enucleation, deranged their development; he said that the 'eggs of many species are a mosaic of germinal localisations' (1985) and went on to say 'if the egg from which one is derived had been centrifuged soon after fertilization, the result, perhaps, would be a microcephalic idiot instead of an intelligent molecular biologist cloning and sequencing genes; yet the idiot and the scientist would have exactly the same genes'. Centrifugation alone, without separation into nuclear and anuclear parts, could therefore partly explain anarchic development. Brachet pointed out in several reviews that mitochondria are more abundant in anuclear than in nuclear fragments and that this could explain their greater respiratory rates.

There is little protein synthesis in unfertilized sea urchin eggs or in anucleate and nucleate fragments made from them. After parthenogenic activation, protein synthesis starts in all three; the last is less active than the first two (1965*b, c*). This was further evidence that the nucleus is not necessary for protein synthesis. Brachet and his colleagues suggested that synthesis depended on a reserve of mRNA which had been synthesized during oogenesis and was activated by parthenogenesis. He suggested (1967*b*) that a protein coat prevented ribosomes from synthesizing protein and that they were activated when this coat was removed by a protease.

Embryos usually get equal amounts of nucleic acid from each parent when these are of the same species. In hybrids there is preferential loss of paternal DNA, but preferential transcription of paternal RNA (1969, 1970, 1971). This nucleo-cytoplasmic incompatibility was suggested as a reason for the failure of these hybrids to survive.

Gradients and evocation

Gradients in the concentrations of hypothetical agents were an aspect of cytoplasmic organization that had often been postulated by embryologists to explain some features of morphogenesis. Brachet (1940) demonstrated a real gradient in protein–SH groups in amphibians, birds and fish; in many later papers (1952, 1955*b* etc.) he discussed the gradient in RNA-rich granules in amphibian eggs. Briefly, the gradient is initially from the animal to the vegetal pole and then, as cell division gives the embryo more form, head to tail and dorsal to ventral gradients develop. There are similar gradients in other species.

Like many other embryologists, Brachet had been interested in induction by the dorsal lip of the blastopore and in the chemical nature of the evocator or organizer which seemed to emanate from it. He had studied the rapid disappearance of glycogen from it (1934) and had worked on it in Cambridge with Needham and Waddington (1936). They concluded that the inducing substance was already present as an inactive complex at the site where it acted, and that it was released by the evocator. That was produced when there was enhanced respiration in the dorsal lip of the blastopore. Specificity of action did not therefore reside in the evocator, but was already present on site. The subject remained confused: it was therefore natural for Brachet to wonder whether the evocator could be RNA. At first (1944; 1950) there seemed to be evidence for this idea, but it later (1955*b*) became doubtful. In *Biochemical cytology* (1957) several pages were devoted to the factors that make it difficult to reach a definite conclusion, and he wrote; 'it might be a mistake to reject completely the idea that RNA itself is an essential component of the organizer'. By 1985 (*Molecular cytology*) he dismissed the subject as of only historical interest. The story of the decline and fall of *the* organizer merits thorough study as an example of the illusions that can arise from the study of even slightly artificial systems. Brachet was well aware of this and quotes with approval the comment; 'embryology will ultimately have to be studied in embryos'.

Oocytes

Various aspects of the development and metabolism of eggs were the main themes of Brachet's first 40 papers. His interest in germinal vesicles and oocytes started when he accidentally slit a frog oocyte; 'A translucent spherule popped out of the wound; it was the oocyte nucleus, the germinal vesicle' (1987*c*). He said that was in 1938, but he had already (1937*a*) studied the respiration of frog and newt germinal vesicles. His interest then veered towards the functions of the nucleus and the behaviour of nucleic acids. This phase of his work established his reputation and it is relatively easy to distinguish his contributions from those of others. That is also true of his work on differences between normal and parthenogenic organisms and on sea urchin and amphibian hybrids. Brachet's role in the recent growth of knowledge about oocyte maturation and the effects of pharmacological agents at various stages of the reproductive cycle is not so easily identified. He seems to have recognized this difficulty himself. Reviews and autobiographical notes written towards the end of his life (e.g. 1987*c*) deal copiously with the subject as a whole, but tersely, if at all, with his own work.

In *Molecular cytology* (1985) he wrote: 'The oocyte is an old cell, since it has the same age as the mother (eventually more than 40 years in the human species). The unfertilized

egg is a very young cell; it can be considered as zero time for embryogenesis.' Many, at first sight, unrelated substances initiate breakdown of the germinal vesicle and the maturation of the oocyte into an unfertilized egg. Brachet *et al.* agreed with others (1977*a*) in finding that they acted on the oocyte surface and not when injected into it, and that a unifying feature in their mode of action was their interference with the balance if K^+, Mg^{2+} and Ca^{2+}, and their tendency to change the intracellular distribution of Ca^{2+}. He pointed out that Dalcq had got near to this generalization in 1928.

Progesterone, well known as a controller of mammalian reproduction, is synthesized in follicle cells surrounding toad oocytes. It was known to be a natural inducer of oocyte maturation in amphibians, insects and sea urchins. Brachet found that, although protein synthesis is a normal and essential precursor of maturation in toad oocytes and that it is increased by exposing young oocytes to progesterone, exposure does not cause maturation until oocytes are nearly full-grown (1976*a*). A 'maturation promoting factor' in the cytoplasm of mature oocytes will, however, promote maturation in oocytes too young to respond to progesterone (1974*a*, 1976*a*). This factor is probably a protein. Various agents, unrelated to any substances known to occur naturally, e.g. three organomercurials (1975*b*), slowly induce maturation without apparently entering the cell. There is room to mention only two more of his many papers connected with this theme.

While toad oocytes are maturing, there is no chromosomal DNA synthesis (1974*b*, 1977*b*). Nucleic acid synthesis in mitochondria and elsewhere, unlike protein synthesis, is inessential. Oocytes can mature after exposure to various inhibitors of nucleic acid synthesis. Nucleoli break up, during maturation, into particles containing DNA that is the complement of ribosomal RNA: these then migrate to the vegetal pole (1976*c*). The effects of this process are still obscure.

Personality and attitude to research

When Brachet started work it was unusual for a paper on embryology or development to include chemical information. He was as responsible as anyone else for the metamorphosis of biology. At first glance, it is now hard to distinguish a biological from a biochemical laboratory. Nevertheless, he retained a fundamentally biological outlook. He said (TV) that he preferred using a microscope to doing chemical analyses, and wrote (1987*a*) of being '*toujours laissé entraîné par ma curiosité du moment, sautillant sans cesse d'un oeuf à une amibe, une algue ou une racine d'oignon*'. The list could have gone on to axolotls, fibroblasts, HeLa cells, planarians, starfish and toads. He urged others to adopt a similar broad-minded approach to biology, and derided the dictum 'What is true for *E. coli* is true for elephants'. Though versatile in choosing research tools, he was consistent in his objective: unravelling the chemical bases of embryonic development. To some extent, choice depended on aesthetics. As he put it in both *Biochemical cytology* and *Molecular cytology* (1957 and 1985)

> Nothing is more beautiful than an embryo developing in its harmonious way; nothing is uglier than a cancer growing in its malignant way. A cell has gone mad, dividing without differentiating, the multiplication of its descendents ultimately killing the whole organism. Cancers are not under the control of organizers, gradients, and fields of differentiation, as are developing embryos. In some

respects, they resemble these ectoderm cells, which, having been acted upon by abnormal evocators (killed organizers, implanted chemicals), may react by forming complex structures that look more like teratomas than well-differentiated embryos.

As several of the quoted passages show, Brachet developed considerable skill in the use of English. It is not just the intrinsic interest of the subject which has made his two big books (1957, 1985) extremely influential. His early papers carried notes thanking others for linguistic help: these disappeared before he was half way through his list of more than 400 publications. He often used humorous phrases and had strong idiosyncratic opinions about usage: for example, he said that 'dramatic' was over-used and should be reserved for episodes resulting in death. I hope that 'artefictual' in *Molecular cytology* (1985) is a happy innovation and not a printing error. Whenever it seemed relevant, he went back to the work of Roux, Driesch, A. Brachet, etc. and to the still older arguments about 'epigenesis', 'evolution' and 'preformation'. He said (TV) that his favourite reading was Saint-Simon: characteristically, the chronicler of behaviour at the court of Louis XIV and not his christian-socialist descendent.

His books show his vast knowledge of the literature: one review article contains more than 500 references (1987*c*). These books and reviews survey not only what was generally agreed when they were written but also suggest points where ignorance is nearly complete, or where conclusions are only probable. For example: a brief semi-popular article (1979) makes 11 suggestions about important novel topics for research, and rates five widely accepted conclusions as only probable. This knowledge and awareness of themes awaiting research, naturally drew students and collaborators from all over the world.

Early in his career, Brachet felt isolated in the Department of Anatomy and had little equipment suited to the work he was doing. He and Jeener therefore migrated to a hut in the Botanic Gardens near the Rouge Cloître à Auderghem ponds. After the interruption caused by the German occupation, the University added a prefabricated shed to accommodate the increasing number of visitors. (Until getting letters from Belgian informants, I was unaware that French and English were equally rich in synonyms for shack or hovel.) In 1963, Brachet's group was chosen by Euratom to be one of its four centres for work on the biological effects of ionizing radiation. As a result, a proper laboratory was built at Rhode St. Genèse; this was supported by grants from many sources.

Students and collaborators were not drawn to Brachet solely because of his knowledge and intuition. All informants, and my own experience, testify to his easy and helpful manner. He was not inclined to direct research in a formal manner, but was always ready to be interested, helpful and encouraging. Nevertheless, he could be as brusque as was necessary to stop elaborate argumentation and get the participants back to experimentation.

In spite of a reiterated preference for small research groups, working without control, and of a dislike of patents, secrecy and commercialization, Brachet was active in helping to establish molecular biology. Perhaps it was his misgivings which led him, during the argument about who was the originator of that title, to point out mischievously that, soon after the death of the Belgian botanist Léo Errera in 1905, his *Cours de physiologie moléculaire* had been published. He was a member of the Council of the European molecular biology Organisation from 1963 to 1966, and nearly managed to arrange a site in Belgium

for its laboratory. For several years, his own laboratory was its address. In spite of this early enthusiasm, he feared that the physicists and pure chemists, who dominate the subject, would not realise that '*Biologie est un substantif et moléculaire n'est qu'un adjectif qualificatif*' (1987*a*). The passage of time did not dissipate his qualms. He predicted that sequencing the human genome would divert scientific funds from more useful projects; he quoted Jean Dausser approvingly: '*Le patrimoine génétique humain ne peut être la propriété de firmes commerciales*!'.

Work on oocyte maturation revived his early interest in the synthesis and distribution of nucleic acids. He had been largely responsible for disproving the idea that RNA was a plant and DNA an animal substance, and for replacing it with the idea that RNA was predominantly ribosomal and DNA nuclear. With his iconoclastic outlook, he must have enjoyed helping others to replace that idea with something much more confused and complex. Unlike John Donne, who regretted the Copernican overthrow of the easily understood Ptolemaic system of astronomy and wrote 'Tis all in peeces, all cohaerence gone;'. Brachet, in a brief article surveying the position (1977*b*), seemed unperturbed by the unfolding complexity. A new systematization will presumably evolve slowly; all the slower without Brachet.

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SCIENTIFIC TITLES

Belgian

1960	Member of the Académie royale des Sciences
1962	Member of the Académie royale de Médecine

Foreign

1966	Foreign Member of the Royal Society
1956	Royal Danish Academy of Sciences (Foreign Associate)
1959	American Academy of Arts and Sciences (Honorary Member)
1960	Honorary Fellow Royal Society of Edinburgh
1962	Deutsche Akademie der Naturforscher Leopoldina (Halle)
1964	Instituto Lombardo – Accademia di Scienze e Lettere (Milan) (Foreign Member)
1965	National Academy of Sciences of Washington (Foreign Associate)
1965	Serb Academy of Sciences (Foreign Member)
1967	Institut grand ducal du Lexembourg (Foreign Member)
1971	Corresponding Academician of the Accademia delle Scienze dell' Istituto di Bologna
1974	Académie des Sciences de Paris (Foreign Associate Member)
1978	Foreign Member of the Accademia dei Lincei (Roma)
1983	Foreign Member of the Accademia mediterranea (Catania)

SCIENTIFIC PRIZES

Belgian

1936	Prix P.J. & Ed. Van Beneden (Académie des Sciences de Belgique)
1940	Prix Agathon de Potter (Académie des Sciences de Belgique)
1943	Prix Leo Errera (Académie des Sciences de Belgique)
1948	Prix Francqui de la Foundation Francqui
1950	Prix A. Slosse de l'Académie Royale de Médecine de Belgique
1953	Prix A. Brachet (Académie des Sciences de Belgique)
1971	Prix des Sciences médicales fondamentales de l'Académie royale de Médecine de Belgique
1972	Prix quinquennal des Sciences médicales, de l'Académie Royale de Médecine de Belgique

Foreign

1957	Charles Leopold Mayer Prize (Société de Chimie biologique de Paris)
1961	Schleiden Medal (Académia Leopoldina – Halle)
1966	Heineken Prize (Academie néerlandaise des Sciences)
1969	Charles Leopold Mayer Prize (Académie des Sciences de Paris)

HONORARY DEGREES

1954	University of Poitiers, Sciences Faculty
1959	Honorary Fellow of the Weizmann Institute (Rehovoth, Israel)
1960	University of Torino, Medical School
1962	University of Palermo, Medical School
1962	University of Edinburgh, Sciences Faculty
1964	University of Strasbourg, Sciences Faculty
1966	Institut agronomique de Gembloux (Belgium)
1968	University of Lille, Sciences Faculty
1975	University of Liège Medical School

Distinctions honorifique

1940–45	Croix civique lère classe
1942	Chevalier Ordre de Léopold
1948	Officier Ordre de la Couronne
1949	Doyen d'Honneur du Travail, puis Doyen Honoraire du Travail
1954	Officier Ordre de Léopold
1954	Rayures d'Or
1960	Médaille civique de lère classe
1961	Commandeur Ordre de la Couronne
1971	Officier de la Légion d'Honneur
1972	Grand Officier de l'Ordre de la Couronne

ACADEMIC POSTS

1933	Laureate of the Concours universitaire
1934	M.D. with 'la plus grande distinction'
1934	Laureate of the Concours des Bourses de Voyage.
1934–1935	Research at the University of Cambridge (U.K.) and the marine laboratories of Roscoff, Sete and Naples

1937	Rockefeller travelling Fellow: Research at Princeton University (N.J.) and the Marine Biological University at Woods Hole (Mass.)
1945–1946	Missions for the Fonds National de la Recherche Scientifique in Great Britain (1945) and the United States (1946)
1946	Visiting Professor at the Institut Pasteur de Paris
1947	Visiting Professor at the University of Pennsylvania (Philadelphia) Senior Lalor Fellow at Woods Hole
1948–1949	Francqui Professor at the Medical School of the University of Liège
1956	Visiting Professor at the Indian Cancer Research Centre
1957–1958	Francqui Professor at the Sciences Faculty, University of Gent
1958	Visiting Professor at the Rockefeller Institute
1959	Weizmann Lecturer at the Weizmann Institute (Israel)
1959–1960	Francqui Professor at the University of Louvain (Faculty of Medicine)
1964	Robert Hooke Lecturer at the University of Texas (Austin)

PAPERS BY J. BRACHET REFERRED TO IN THE TEXT*

1929	Recherches sur le comportment de l'acide thymonucleique au cours de l'oogénèse chez les diverse espèces animales. *Arch. Biol.* **39**, 677–697.
1934	Etude du métabolisme de l'oeuf de grenouille (*Rana fusca*) au cours du développement.1. La respiration et la glycolyse de la segmentation à l'éclosion. *Arch. Biol.* **45**, 611–627.
	Métabolisme respiratoire et 'centre organisateur' de la gastrula. *Arch. Biol.* **46**, 25–45.
1936	(With C.H. WADDINGTON & J. NEEDHAM) Studies on the nature of the amphibian organizer centre. III. The activation of the evocator. IV. Further experiments on the chemistry of the evocator. *Proc. R. Soc. Lond.* B **120**, 173–198.
1937(*a*)	Some oxidative properties of isolated amphibian germinal vesicles. *Science Wash.* **86**, 225.
(*b*)	Remarques sur la formation de l'acide thymonucléique pendant le développement des oeufs a synthèse partielle. *Arch. Biol.* **48**, 529–548.
1940	Étude histochimique des protéines au cours du développement embryonnaire des poissons, des amphibiens et des oiseaux. *Arch. Biol.* **51**, 167–202.
1941	La détection histochimique et la microdosage des acides pentose-nucléique. *Enzymologia* **10**, 87–96.
1942	(With R. JEENER) Sur la présence d'hormones protéines et d'hémoglobine dans les granules à pentosenucléoproteides. *Acta biol. Belg.* **2**, 447–450.
1944	Le rôle des acides nucléiques dans l'induction neurale. *Bull. Acad. Sci. Belg.* **29**, 707–718.
1947	Nucleic acids in the cell and the embryo. *Symp. Soc. exp. Biol.* **1**, 207–224.
1949	(With J.R. SHAVER) The injection of embryonic microsomes into early amphibian embryos. *Experientia* **5**, 204.
1950	Quelques observations sur le mode d'action de l'organisateur chez les amphibiens. *Experientia* **6**, 56–57.
1952	The role of the nucleus and the cytoplasm in synthesis and morphogenesis. *Symp. Soc. exp. Biol.* **6**, 173–200.
1955(*a*)	Action of ribonuclease and ribonucleic acid on living amoebas. *Nature, Lond.* **175**, 851–853.

* The complete bibliography appears on the accompanying microfiche.

(*b*) The biological role of the pentose nucleic acids. In *Nucleic acids* (ed. E. Chargaff & J.N. Davidson) vol. 2, 475–519. Academic Press.

(*c*) Recherches sur les interactions biochimiques entre le noyau et le cytoplasme chez les organismes unicellulaires. 1. *Amoeba proteus*. *Biochim. biophys. Acta* **18**, 247–268.

(*d*) (With H. CHANTRENNE & F. VANDERHAEGHE) Recherches sur les interactions biochimiques entre le noyau et le cytoplasme chez les organismes unicellulaires II. *Acetabularia mediterranea*. *Biochim. biophys. Acta* **18**, 544–563.

1957 *Biochemical cytology*. Academic Press.

1960(*a*) Ribonucleic acids and the synthesis of cellular proteins. *Nature, Lond.* **186**, 194–199.

b) The biological role of ribonucleic acids. *Sixth Weizmann Memorial Lecture Series*, April 1959. Elsevier.

1961(*a*) (With B.H. SELLS & N. SIX) The influence of the nucleus upon adenosine triphosphatase activity in *Amoeba proteus*. *Exp. Cell Res.* **22**, 246–256.

(*b*) Nucleocytoplasmic interactions in unicellular organisms. In *The cell* (ed. J. Brachet & A.E. Mirsky). vol. 11, 771–841. Academic Press.

1963 (With E. BALTUS) Presence of deoxyribonucleic acid in the chloroplasts of *Acetabularia mediterranea*. *Biochim. biophys. Acta* **76**, 490–492.

1964 (With H. DENIS & F. DEVITRY) The effects of actinomycin D and puromycin on morphogenesis in amphibian eggs and *Acetabularia mediterranea*. *Dev. Biol.* **9**, 398–434.

1965(*a*) (With A. GOFFEAU) Deoxyribonucleic acid-dependent incorporation of amino acids into the proteins of chloroplasts isolated from anucleate *Acetabularia* fragments. *Biochim. biophys. Acta* **95**, 302–313.

(*b*) (With E. BALTUS, J. QUERTIER & A. FICQ) Biochemical studies of nucleate and anucleate fragments isolated from sea-urchin eggs. A comparison between fertilization and parthenogenetic activation. *Biochim. biophys. Acta* **95**, 408–417.

(*c*) (With A. BURNY, G. MARBAIX & J. QUERTIER) Demonstration of functional polyribosomes in nucleate and anucleate fragments of sea-urchin eggs following parthenogenetic activation. *Biochim. biophys. Acta* **103**, 526–528.

1967 (*a*) Effects of hydroxyurea on development and regeneration. *Nature, Lond.* **214**, 1132–1133.

(*b*) Protein synthesis in the absence of the nucleus. *Nature, Lond.* **213**, 650–655.

1969 (With H. DENNIS) Gene expression in interspecific hybrids. 1. DNA synthesis in the lethal cross *Arbacia lixula* × *Paracentrotus lividus*. *Proc. natn. Acad. Sci. U.S.A.* **62**, 194–201.

1970 (With N. HULIN) Observations sur les acides désoxyribonucléiques des hybrides létaux entre oursins. *Expl Cell Res.* **60**, 393–400.

1971 Nucleocytoplasmic interactions in morphogenesis. *Proc. R. Soc. Lond.* B **178**, 227–243.

1974(*a*) (With G. STEINERT, E. BALTUS & J. HANOCQ-QUERTIER) Ultrastructure of *Xenopus laevis* oocytes after injection of an extract from progesterone-treated oocytes. *J. Ultrastruct. Res.* **49**, 188–210.

(*b*) (With F. HANOCQ, A. DESCHUTTER & E. HUBERT) Cytochemical and biochemical studies on progesterone-induced maturation in amphibian oocytes. 2. DNA synthesis. *Differentiation* **2**, 75–89.

1975(*a*) (With N. TONDEUR-SIX & B. TENCER) Ribonucleotide reductase activity during amphibian development. *Biochim. biophys. Acta* **395**, 41–47.

(*b*) (With E. BALTUS, A. DESCHUTTER-PAYS, J. HANOCQ-QUERTIER, E. HUBERT & G. STEINER) Induction of maturation (meiosis) in *Xenopus laevis* oocytes by three organomercurials. *Proc. natn. Acad. Sci. U.S.A.* **72**, 1574–1578.

1976(*a*) (With J. HANOCQ-QUERTIER & E. BALTUS) Induction of maturation (meiosis) in small *Xenopus laevis* oocytes by injection of maturation promoting factor. *Proc. natn. Acad. Sci. U.S.A.* **73**, 2028–2032.

(*b*) Interactions between nucleus and cytoplasm. In *Encyclopedia of plant physiology* vol. 3, 53–84. Springer Verlag.

(*c*) (With G. STEINERT & C. THOMAS) Localization by *in situ* hybridization of amplified ribosomal DNA during *Xenopus laevis* oocyte maturation (a light and electron microscopy study). *Proc. natn. Acad. Sci. U.S.A.* **73**, 833–836.

1977(*a*) (With E. BALTUS, J. HANOCQ-QUERTIER & A. PAYS) Ionic requirements for induction of maturation (meiosis) in full-grown and medium-sized *Xenopus laevis* oocytes. *Proc. natn. Acad. Sci. U.S.A.* **74**, 3461–3465.

(*b*) Deoxyribonucleic acid synthesis during early embryogenesis. *Biochem. Soc. Trans.* **5**, 1184–1190.

1979 Oogenesis and maturation in amphibian oocytes. *Endeavour* **3**, 144–149.

1985 *Molecular cytology*. Academic Press.

1987(*a*) Souvenirs sur les origines de la biologie moléculaire. *Bull. Acad. roy. Belg. Cl. Sc.* **7**, 441–449.

(*b*) Reminiscences about nucleic acid cytochemistry and biochemistry. *Trends Biochem. Sci.* **12**, 244–246.

(*c*) Nucleocytoplasmic interactions in morphogenesis. *Rev. Cytol.* **100**, 249–318.

Books

1944 *Embryologie chimique*. Desoer and Masson.

1950 *Chemical embryology*. Interscience.

1957 *Biochemical cytology*. Academic Press.

1960 *The biochemistry of development*. Pergamon Press.
The biological role of ribonucleic acids. Elsevier.

1974 *Introduction to molecular embryology*. Springer.

1985 *Molecular cytology*. 2 vols. Academic Press.

Books edited.

1961 (With A.E. MIRSKY)*The cell*, 6 vols. Academic Press.

1970 (With S. BONOTTO) *Biology of Acetabularia*. Academic Press.
(With M. ABERCROMBIE & T.J. KING) *Advances in morphogensis*, vol. 10. Academic Press.

1980 (With G. DELRIO) *Steroids and their mechanism of action in nonmammalian vertebrates*. Raven Press.