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UNIVERSITY OF KENT AT CANTERBURY

UNIT FOR THE HISTORY, PHILOSOPHY AND SOCIAL RELATIONS OF SCIENCE

Physics in France circa 1850-1914; its national organisation, characteristics and content.

John L Davis

Thesis submitted for the award of Ph.D. in the History of Science.

June 1990

(originally presented April 1989)

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Abstract

The thesis begins with an examination of what was understood by the term 'physics' in France circa. 1850. The development of the centralised state educational system and the physics research which was produced within this system in Paris and the provinces, is then considered. Although all the relevant institutions, where some form of physics or physical science was taught, have been examined,the *Ecole Polytechnique*,and the *Ecole Normale Supérieure* have a particular importance in the early period of this study. As time passed and as a result of reforms put in hand by the republican regime which came out of the defeat of the Franco-Prussian war of 1870-71, the *université* system grew in importance, while the role of the *Polytechnique* declined. The *Ecole Normale*, the Paris Faculty and the provincial faculties form part of the *université system* and participated in its growth.

A knowledge of the objectives of the physics courses in these institutions helps in the understanding of the characteristics of physics in France in this period. The central objective was, largely, to produce either science teachers, or (in the case of the *Polytechnique)*, a type of elite 'technocrat', for the state, i.e. men who could communicate clearly, or technically utilise knowledge, which was already established on a firm theoretical basis. This is not to say that research had no place in the institutions of higher education, on the contrary, and this research, carried out by both teachers and students, is examined here to try to relate its form and content to the particular institution in which it was carried out.

The role of national organisations like the *Société de physique* and the *Association Française pour l'avancement des sciences* in the development of physics in France is also considered, as is the role of the *Académie des sciences*. The predominantly experimental nature of physics research in France is related to the interests of these organisations, to the requirements of the *licence* programme, and to the increasingly fierce competition for membership of the physics section of the *Académie*.

Abbreviations used for journals frequently quoted in footnotes and the bibliograhy.

Primary sources.

A.c.p.	*Annales de chimie et de physique.*
A.s.i.	*Année scientifique et industrielle.*
Bull.a.s.F.	*Bulletin d'association scientifique de France.*
C.r.	*Comptes rendus hebdomadaire de l'académie des sciences*
C.r.a.F.a.s.	*Comptes rendus de l'association Française pour l'avancement des sciences.*
J.d.p.	*Journal de physique*
R.g.s.p.a.	*Revue générale de science pure et appliquée.*
R.s.	*Revue scientifique.(originally Revue des cours scientifiques.)*
S.s.F.p.	*Séances de la société Française de physique.*

The journal *Le Radium* has not been abbreviated.

Secondary sources.

B.j.h.s.	*The British journal for the history of science.*
Bull.h.e.	*Bulletin de l'histoire d'électricité.*
H.s.p.s.	*Historical studies in the physical sciences.*
Hist. sc.	*History of science.*
J.h.i.	*Journal of the history of ideas.*
Min.	*Minerva.*
S.s.s.	*Social studies of science.*

Isis and *Annals of science* have not been abbreviated, nor have journals which occur very infrequently like *French historical studies* and *Catholic historical studies.*

The *Dictionnaire de biographie Francaise* has also been abbreviated to *D.b.F.* in footnote references.

CONTENTS

2.Physics teaching and research during the Second Empire.

4.The organisation of physics, and research in the subject, during the first two decades of the Third Republic .

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* Its full title was the *Ecole Normale Superieure*

5. Physics in the last decade of the century

6.From the *Congrès International de Physique* to the outbreak of the Great War.

1. INTRODUCTORY PERSPECTIVE; 'PHYSICS' IN ITS CONTEMPORARY FRENCH CONTEXT

When the period of this thesis opens, in the middle of the nineteenth century, the subject known as la physique* had already been established as an academic discipline in some Parisian institutions of higher education for half a century, and from as early as 1785 a separate section in the Academy of Sciences was devoted to it. This is not to say that each institution which taught the subject had precisely the same idea of the content of physics, for this would be modified by the concrete objectives of the educational programme of the particular institution, but there was general agreement as to what constituted the core of the subject. In order to understand what physics meant to the teachers and research workers of mid-nineteenth century France, and to appreciate the changing boundaries of the subject as new knowledge acquired later was accomodated within it, it is necessary to take a synoptic view of the evolution of physics, within France and outside it, in the previous century.

'Physics', (from the Greek physis), originally the study of nature and the natural world, had acquired a more specific meaning in Europe by the end of the eighteenth century. In the Encyclopédie of Denis Diderot, published between 1751 and 1772, la Physique was split into two separate branches; physique générale, which dealt with the properties, including movement, common to all bodies, and physique particulière which dealt with the properties which distinguished one body from another, like hardness, elasticity or fluidity.[1] Robert Silliman

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*. The noun, la physique is, throughout this thesis, translated as physics, as, for example when it appears in Société de Physique. Where the French word appears as an adjective, e.g. sciences physiques it is translated as 'physical'.

has made the point that physique générale was quantitative, coherent and mathematically sophisticated, and had at its core the mechanics of Newton as expounded in his Principia of 1687, while physique particulière was much more hypothetical and experimental and dealt with a range of topics, whose subject matter roughly corresponded with the 'Queries' of Newton's Opticks.[2] Within physique particulière was to be found the study of sound, heat, light, electricity and magnetism, as well as, originally chemistry and natural history. Thomas Kuhn, referring to earlier work by I.B. Cohen , also traces two separate strands of physical science in the eighteenth century, both influenced by Newtonian thought. He calls the two strands,'classical' and 'Baconian' respectively, commenting that while the Principia is squarely within the tradition of classical science ,the Opticks is much less unequivocally in the 'Baconian'.[3] By 'classical, Kuhn meant that tradition which came from the Greeks in such works as those of Archimedes, Ptolemy, and Euclid for example, which shared a mathematical tradition and were inaccessible to the layman, and by 'Baconian', he meant those sciences based predominantly on observation and experiment which lacked a theoretical base.

Thus the phrase 'Baconian' physical science corresponds quite closely to the term physique particulière, or to the term which tended to replace it, in France, as the eighteenth century drew to a close, physique expérimentale. This latter term had been explained by Jean D'Alembert in the Encyclopédie, who said that its object was to study;

> '..phenomena where reasoning can help us little.. where it is not possible to see the links between phenomena.'[4]

He placed in this category , the study of electricity and magnetism, as well as chemistry.

The emergence of physics as a separate autonomous discipline in the nineteenth century went through a process which involved both meticulous experimental work utilising precise measurement on the one hand, and the application, to the studies of heat, light , and electricity, of a powerful body of mathematical theory on the other. In this way was a bridge built between the two separate strands, the 'Baconian' and the 'classical', between *physique expérimentale* and *physique générale*. This is not to say that the process of unification only began in the nineteenth century, for by the time the *Encyclopédie* was published in the mid-eighteenth century, Dutch experimentalists like Boerhaave, Mussenchenbroek and s'Gravesande had already began to put *physique particulière* on a firmer empirical basis, and their work translated into French in mid-century, reinforced the native tradition as represented by such men as Dufay and Nollet, who were pursuing an experimental study of electricity. But their experiments, in common with many experiments of the time, as Hankins has pointed out, were designed to create rather than measure phenomena, for 'measurement had to wait until qualitative theory had specified what it was important to measure'.[5] Possibly as a result of this lack of an adequate theoretical base, interest in experimental physics, which had grown rapidly around the middle of the century, later began to show signs of decline in France.[6]

However, it might be argued that this decline owed more to institutional factors than theoretical ones within experimental physics. Antoine Lavoisier, writing in 1775, lamented that the reform of the Academy in 1716 had made a mistake in not setting up a section for *physique expérimentale* , which he claimed ; 'was making at that time, rapid progress in England, Holland and Italy'.[7] It was for this reason, according to Lavoisier that;

> '..experimental physics ,to which academicians had, at first, applied themselves with zeal, has been practically abandoned'.[8]

Lavoisier led the reform to bring about the establishment of a section devoted to physique expérimentale, but the new section when it was formed in 1785, was called the section of physique générale and was placed in the division of sciences mathématiques along with geometry, astronomy and mechanics, rather than in the division of the more manipulative sciences physiques, which contained anatomy, botany and agriculture, and natural history and mineralogy.[9] Chemistry, now associated with metallurgy, was also placed in this section, and this probably had the effect of accelerating its separation from physics.[10] After the revolution and the reconstitution of the Academy in 1795 as the First Class of the Institute, the section was renamed physique expérimentale but in 1803 reverted to physique générale, a title which it retained throughout the nineteenth century. The change in title to physique générale, the term used to denote the more mathematical, deductive, part of physical science, possibly signalled the intention of the Academy to strengthen this aspect of the science at the expense of the purely experimental.

This is certainly the point which R.W. Home made in his analysis of the circumstances surrounding Poisson's election to the physics section of the Academy in 1812.[11] Home stressed that Poisson was known to be hopelessly lacking in experimental skills, and his election on the basis of a mathematical analysis of the distribution of charge over the surface of a conductor was an acknowledgement by the leaders of the community (in particular, Laplace), that mathematics would henceforth play a central role in a discipline, which had hitherto been based almost wholly on experiment.[12]

Thus it would appear that in the first quarter of the nineteenth century, the Academy as the major regulatory body of French science, wished to strengthen the bonds between la physique and the other mathematical sciences; astronomy, mathematics, mechanics, and geography and navigation. It is no accident that this was the period in which French mathematicians, astronomers and engineers, began to provide a mathematical and theoretical basis to physique expérimentale, as apparently firm as that which Newton had provided for mechanics.[13] For example, the research programme of Laplace and his pupils began to bring some order to the study of light, heat and electricity, by applying the principle of action at a distance on a molecular scale. If this was not entirely successful, and explanations later moved away from the consideration of material particles, and the corpuscular theory of light and the caloric theory of heat gave way to theories based on the idea of the vibrations in an elastic solid ether, these theories too, were very much the production of French savants. Apart from the work of Fresnel on the wave theory of light, there was that of Poisson on elasticity, Fourier on heat conduction (using an analytical technique which ventured no hypothesis about the nature of heat), and Ampère's two fluid mechanical theory of electricity. Thus by 1830, French savants had done much to mathematise those subjects which had previously been categorised under the heading of physique expérimentale, and the gap between it and physique générale was, for practical purposes, eliminated. From this time on, the practitioner of physics, whatever his manipulative and experimental skills, would have to possess the basic mathematical skills which could normally only be acquired from academic study in higher education. There are one or two notable exceptions to this rule, which will be examined, but in general it can be said that the contribution of the mechanic or the instrument maker, so important to the development of physique expérimentale in the eighteenth century, would be minimal to the later development of physics.

This is not to say that the burst of mathematisation of physics, which owed so much to the influence of Laplace in the first quarter of the century, was sustained after his death in 1827. It would appear that the pendulum had partially swung back towards a greater emphasis on experimentation by mid-century, even though it was now experimentation based firmly on the theoretical foundations laid down in the era of Laplace, and carried out by men who were well versed in the advanced mathematical techniques taught at the grandes écoles. Perhaps the influence of Arago, the permanent secretary of the mathematical sciences section of the Academy, played some role in this, as did the authority of Gay-Lussac who was a member of the physics section from 1816 until his death in 1850. Gay-Lussac was predominantly an experimental physical scientist, whose research interests ranged widely on either side of the physique/chimie interface, while Arago was an astronomer, with an astronomer's interest in optical phenomena. The election to the physics section of A.C Becquerel in 1829, Pouillet in 1837, Babinet (meteorologist of the Paris Observatory)in 1840, and Despretz (once the laboratory assistant of Gay Lussac) in the following year, strengthened the experimentalists weight in the section. By 1850, the only mathematical physicist in the section was Duhamel, who had begun his career as an assistant to Fourier.

But we can also say that by 1850 all the members of the physics section were men trained in, and earning a living in the educational and research institutions of Paris. Such a training had become a virtual necessity for a career in science and the period under examination here is one which saw a considerable development of science courses in different institutions. Most of those who took science courses would themselves become teachers of the subject, communicating an already acquired body of knowledge and demonstrating accepted experimental techniques, to new generations of students some of whom, in turn, would enter the same profession. A growth in such courses went hand in hand with the growth of publication of text-books, and it is an examination of these books in the first half of the nineteenth century, which can give us an idea of what the term physique signified when this thesis begins its study. At least such a study can illuminate the content and boundaries of the subject as it was communicated to students, although as there is a necessary delay before new knowledge finds its way into the pages of a text-book, it may not be much help in deciding what precisely constituted physics in the area of research. This latter question can only be decided;

a. By reference to the classification the research work received in the scientific journals of the time.

b. The prizes it might have received.

c. The section of the Academy to which its originator might eventually be elected.

d. The chairs or positions in higher education he held (or was to hold later) on the basis of this research.

Physics had been an academic discipline at the *Ecole Polytechnique* since Hassenfratz had taught it (apparently very badly)[14] at the beginning of the century. Then it had been a two year course covering the general properties of bodies, heat, meteorology, electricity and magnetism in the first year, and light and sound and the 'system of the world' in the second.[15] When the Paris Faculty opened its doors in 1808, Gay-Lussac was made its professor of physics, although Biot, the professor of astronomy, taught certain sections of the syllabus of special interest to him, like light and electricity. Biot wrote several textbooks of physics while Gay-Lussac's physics lectures were collected and published in a pirated edition of *Leçons de Physique* in 1828.[16] To Gay-Lussac, physics was basically the study of what he called the agents of nature, of which the most powerful were, heat, electricity, magnetism, light and gravitation.[17] This Faculty physics course contained no mathematics and its level was only a little above that taught in the *lycées*.

By the time Gabriel Lamé was teaching the subject in the 1840's, the subject had acquired (using his Cours de physique de l'Ecole Polytechnique as a source) the following content;

Book 1. General properties of bodies; the physical theory of heat.

Book 2. Acoustics (vibrations of bodies, properties of sounds, musical instruments), optics (photometry, dispersion, polarisation, diffraction and interference.)

Book 3. Electricity (its effects and sources, laws of electric currents, electrochemistry), calorific and chemical radiation.[18]

Lamé explained in his forward that the objective of the Polytechnique physics course was not only to prepare the student for his specific engineering studies, but (like all the courses at the school) to elevate his capacity for reasoning. Physics was considered particularly important because a study of its different branches, in different stages of development, gave an example (which Lamé had possibly taken from Comte's classification of the sciences) of the successive states through which a science had to pass.[19] In his first lesson Lamé set out to explain to his students what could be understood by the term la physique. After giving an explanation of the origin of the term and its meaning in antiquity he went on to say;

> 'Finally, la physique, restricted to the study of inorganic and terrestrial phenomena, is again subdivided into three partial sciences; geology, including mineralogy, which is concerned with classifying the inert bodies of which the globe is composed; chemistry, in some ways a kind of inorganic anatomy, which decomposes (these inert bodies) and studies the laws of their combination; and properly called (proprement dite) physics, the particular science with which we are concerned, which deals especially with the general properties of bodies and phenomena that do not entail permanent changes in the inner composition (of these

> bodies) and appear to depend on several universal agents whose definition and laws must be investigated.'[20]

Thus in this scheme, geology and mineralogy are purely classificatory sciences, whereas chemistry is more than classification because it seeks the laws by which inorganic substances combine. Physics is here differentiated from chemistry in that, while the two sciences were investigating laws, those which fell within the domain of physics dealt with general properties of solid bodies, (density, hardness, and elasticity, for example), of liquids, (compressibility, capillary and osmotic action) and gases (compressibility, elasticity, diffusion, etc.), but not with actions which permanently altered the internal composition of the body. It cannot be assumed that all French teachers of the period employed exactly the same definition of physics as that which was used by Lamé and so (without claiming to have made an exhaustive study of all contemporary textbooks) some other sources have been consulted.

Appearing later in the 1840's than Lamé's textbook was that of J.C.E. Péclet. Péclet, a _normalien_ by training and an ex-_lycée_ teacher, held the chair of _physique générale_ from 1829 to 1836, and the chair of _physique industrielle_ from 1829 to his death in 1857, at the _Ecole Centrale des Arts et Manufactures_. Like the _Polytechnique_, the _Ecole Centrale_ (originally a private venture), was concerned with the training of engineers, but whereas the former trained young men as military engineers and for the prestigious state corps of civil engineers, the _Centrale_ aspired to train a new breed of engineer, who would be capable of transforming technologically, France's industry. Peclet's book, published in two volumes, had the following content;

Vol. 1. The general properties of solid bodies, liquids and gases. Acoustics.

Vol. 2. Properties of heat, magnetism, electricity and light.[21]

In defining the limits of physics and delineating the subject from chemistry, Peclet advances the following argument;

> 'The phenomena, which are the province of physics, are characterised by this circumstance; that they do not result from molecular actions which change the nature of bodies. It is in this respect that they differ from those (phenomena) which make up the domain of chemistry.'[22]

Thus both books deal with topics such as acoustics, heat, magnetism and electricity, optics and general properties of bodies, topics which in an earlier period would have been classified first under the heading *physique particulière* and later under *physique expérimentale*. They are different in their characterisation of physics in that Péclet resorts to molecular hypotheses and Lamé does not, but there seems to be a general agreement that the phenomena of physics 'do not result from actions which change the nature of the bodies' (Péclet), or which entail 'permanent changes in the inner composition' of the body (Lamé). It has not been possible to consult other physics textbooks of the period, like the Paris Faculty professor C. Despretz's *Traité élémentaire de physique* (Paris 1832), or the *Conservatoire National des Arts et Métiers* C.S.M. Pouillet's *Eléments de physique expérimentale et de météorologie*, Paris 1840. Pouillet held the chair in physics applied to the crafts at the *Conservatoire National des Arts et Métiers*. As there was a lively debate between Péclet and Pouillet in the Academy in 1845, when Péclet accused Pouillet of plagiarising his text book to write his own,[23] one can assume that the content of the two showed marked similarities.

Textbooks are not the only source of information about the contemporary view of physics, although they have a particular importance in that they give a record of ideas which were accepted by the scientific community and communicated to those who aspired to be members of this community. On the other hand a work such as Auguste Comte's *Cours de* *philosophie positive* published in 1838, which dealt with scientific questions, was probably not read by the majority of science teachers and researchers of the time. Comte's poor mathematics teaching at the *Ecole Polytechnique* and his attack on the administration of the school did his own scientific standing little good at all,[24] and therefore it should not be assumed that his classification of the sciences into a hierarchy in which physics is dealt with as separate from and superior to chemistry, found widespread support among savants. Comte saw the two sciences as being similar only in that they both sought knowledge of the general laws of the inorganic world, but his attempts to demarcate them would have seemed somewhat idiosyncratic, even to his colleagues and contemporaries at the *Ecole Polytechnique*.

But the period of this thesis opens some twenty years after the first appearance of the *Cours de philosophie positive*, as the newly proclaimed Second Empire began a period of expansion of secondary and higher education, and in which several textbooks for students preparing for the *baccalauréat* and the *licence* in the sciences was published. The most popular in the field of physics was the *Traite élémentaire de physique expérimentale et appliquée et de météorologie* by A. Ganot, which reached its fifteenth edition in 1872. The first task which Ganot set himself, on the first page of his book, was to differentiate physics from chemistry in the following way;

> '1. **The objective of physics.** Physics has for its aim, the study of phenomena to which bodies give rise while not undergoing a change in their composition.
>
> Chemistry, on the other hand, deals particularly with phenomena which modify, to a greater or lesser extent, the nature of the body.' [25]

Two pages later he attempted to define what is meant by 'physical phenomena' by saying;

'Anything which is accomplished by matter without an alteration

> in its composition is a physical phenomenon. A body which falls, a sound which is produced, some water which freezes, are such phenomena.'[26]

He also subdivided physics into four categories;

'1. The general properties of matter, including universal attraction, hydrostatics, pneumatics and the vibration of elastic bodies, or acoustics.
2. Heat.
3. Light.
4. Electricity, of which magnetism is only a particular case..'[27]

In relation to the concept of energy, Ganot (in the 1872 edition) discussed the dynamical theory of heat, giving a little historical resume of its development, and explaining that the phenomena of heat have a single cause, movement, and are thus submitted to the ordinary laws of mechanics,'of which the most general one is that of the conservation of _force vive_'.[28] In relation to the old _physique générale/physique particulière_ dichotomy, _physique générale_, the study of motion and of force, and the resolution of forces, occupied only some ten pages in a book of over nine hundred pages. But the text-book does claim to be a treatise of experimental physics, and while giving some simple mathematical treatment, obviously sees its function as showing to students, the apparatus, instruments and the experimental procedures of the science. Students would use the book to help them prepare for the science baccalaureat in the _lycées_, or for the _licence_ in _sciences physiques_ (physical science) at the faculties of science, neither of which required much mathematics.

But if we can use physics text-books of the period to give an idea of the content and limits of the science as it was taught, they cannot tell us where the new knowledge acquired by research would be accommodated as the frontier moved. Only by seeing how contemporary scientific journals classified research papers, how the researcher viewed his own work, and how (most important of all) the Academy of Sciences viewed it, will it be possible to include a piece of research work within the boundaries of physics.

What made the Academy of Sciences so important throughout the entire period of this thesis was its role as the central, state sponsored, regulatory body for all sciences in France. It could distribute funds in the form of prizes and grants, it could recommend appointments to the most prestigious teaching and research posts in the country, and it could direct (or at least attempt to direct), research effort in the direction which it considered to be important, by means of prize competitions.

Moreover, election to the Academy represented the peak of savant's career as it signified his acceptance into the ranks of the highest elite of France's scientific community. Although in the early years of the century it might have been possible to gain election, as Arago did, on the basis of brilliant promise, generally a body of scientific research which the Academy judged to be both weighty and sound, was later required of successful candidates. As the century wore on, the age of savants achieving membership increased, for the scientific community grew larger but the number of positions in the Academy remained static at around sixty. But for those who were fortunate or talented enough to attain membership, the rewards were many. The honorarium of 1500 fr. a year may have been very little, but membership made possible entry into the highest teaching and research posts in the most prestigious Parisian institutions; the _grandes écoles_, the Paris Faculty, the _Collège de France_, the _Muséum_, and the _Bureau des Longitudes_ .

But if we considered the Academy as simply a club for a scientific elite, which owed its membership to work already done, it would have had little influence on the development of science in France in the nineteenth century. It is true that the Academy itself had no research laboratories and its members were only expected to attend it for two hours each Monday afternoon, but in a number of ways it shaped, directed and stimulated French scientific research. Those savants who lived in the provinces could never become members if they were not prepared to move to Paris, for residence in the capital was a condition of membership, but they could aspire to become corresponding members. This position had none of the rights or privileges of a full member, but was a focus of ambition for the provincial faculty teacher, the director of a provincial observatory or a talented scientific amateur or state engineer content to stay outside the capital. We will encounter, in this thesis, several provincial researchers, whose work earned them the position of corresponding member of the physics section of the Academy.

The practice of the Academy to publish reports and memoirs in its weekly Comptes rendus, was a stimulus to scientific work at lower levels in the scientific community. A provincial lycée teacher for example, would derive enormous satisfaction from seeing his memoir or note published in the journal of the Academy and commented on - perhaps even commended - by one of the great figures of French science. Having said this, however, it is also true to say, as Fox points out, that scientists and teachers who were content to remain in the provinces, often published most of their research work in regional and provincial scientific journals,[29] but for those who aspired to move back to Paris research published in the Comptes rendus was an absolute necessity. As mentioned above, the Academy could shape and direct the French research effort by awarding prizes through competitions in areas and topics which it specified. This practice had a long history in the ancien régime and was revived in 1796. During the nineteenth century, the Academy had more and more funds from private bequests to distribute, and the method by which it did this, changed as time went on. It gradually passed from awarding money for work already done and subsequently judged by the Academy, to giving financial assistance to research projects of which it approved.[30] It should be emphasised again here that the research areas considered in this thesis (i.e. optics, electricity, heat, acoustics, etc.) were regarded by the Academy as mathematical sciences, and were eligible for prize competitions in this category. Later in the century there were other bodies which also awarded prizes and grants, but the Academy remained pre-eminent in this respect.

But, important though it was, the Academy was not the only influence shaping physics as the period of this thesis opens. There was also the national centralised system, the Université de France, established in the days of the First Empire. We will see that this system was expanded in both its lycée and its faculty sectors during the Imperial regime, improving career prospects and providing more research facilities for science teachers. In 1852 there were faculties of science in eleven French cities including Paris, and four more weere added in 1854. In addition to the faculties, there were the other prestigious teaching and reseach institutions in the capital, whose roots went back to the ancien régime or to the post-thermidorian revolutionary period. There was also the Ecole Centrale des Arts et Manufactures, set up in 1829 and taken into state ownership and put under the control of the Ministry of Commerce in 1858. The origin and histories of these institutions have already been fully described elsewhere,[31] and so for the moment they are simply listed in Table 1 (overleaf),[32] together with their founding dates, responsible ministries, and senior physics staff.

In most cases the physics chairs were founded when the institut-

Table 1. Physics posts in higher education in Paris in 1852, when the period of this thesis opens.

Institution	Founding date	Ministry	Senior physics staff, 1852
Ecole Normale Supérieure	1795/1808	Education	E. Verdet.
Ecole Polytechnique	1795	War	A. Bravais. J. Jamin.
Collège de France	1503/1793	Education	J.B. Biot (maths. phys.) H.V. Regnault.
Muséum d'histoire naturelle Formerly Jardin du Roi	1620/1793	Education	A.C. Becquerel.
Conservatoire des Arts et Metiers	1794	Commerce	E. Becquerel.
Ecole Centrale des Arts et Manufactures	1829	Commerce	E. Péclet.

ions themselves were established (or in the case of the *Collège*, refounded) except in the case of the *Conservatoire*, whose chair in 'physics applied to the crafts' was established in 1831, and for the *Muséum*, whose post 'physics applied to the natural sciences' dates from 1838. The *Collège* did not have a regular student body studying for an examination and for this reason its staff were freer to investigate subjects which did not form part of the national programme of study for the *licence*. There were no professorships in the *Ecole Normale Supérieure*, presumably because it was originally a type of teachers' training college, but its senior teaching position, the *maître de conférence* had the same prestige as a professorship by the middle of the century. There were also more junior posts in the Parisian institutions; the *répétiteur* or tutor, and (in the *Ecole Normale*) the *agrégé-préparateur*, a post which permitted a few of the most able students to work full-time at the school for their doctorates, while assisting the research of senior staff. In this way the previously obligatory spell teaching in a *lycée* while working for a doctorate was avoided. The institutions listed in this table remained throughout the period of this thesis, the pinnacle of the French educational system, giving their staff the best facilities for research.

Men trained in the mathematical sciences might also have found employment in the Paris Observatory in the period of this thesis, but after Arago's death in 1853, it no longer played the same role in optical research that it had in the past, although a post of *physicien*, (which translates as 'physicist') was established during the Second Empire. But the major source of employment for men holding the *licence* would have been the teaching posts in physical sciences in the expanding state *lycée* system. There there were 24 science posts and 41 in mathematics in Paris, and 116 science and 190 mathematics posts in the provinces by 1857.[33] Also, following the Falloux law of 1850, which broke the *Université* monopoly of secondary education and permitted the religious orders to establish their own schools, some Catholic *licenciés* found employment in this sector.[34] Higher education remained in the hands of the state, whose monopoly in this sector was only broken some twenty years later in the new republican regime, when Catholic institutes for higher education were set up to train young men for the *licence* without running the moral risks of attending the secular faculties. The physics research carried out in these institutions will also be considered later in this thesis.

So it was above all the growth of the secondary school system, both state and private, and the inclusion within its curriculum of physics as part of a more general physical science programme, which was to bring about the expansion of physics instruction in the faculties which prepared students for the teaching qualification, the *licence*. The French *licence* dates from 1808, when Napoleon founded the *Université de France*, and although, by the middle of the century all the faculties in the country were preparing students for this examination, in practice only the Paris Faculty had a regular and serious student body.[35]

Initially the same physics content appeared in the curriculum of the *licence* for mathematics which contained also calculus, astronomy, and mathematics and that of science, which embraced physics, chemistry, mineralogy, geology, botany, zoology and physiology.[36] By mid-century the situation had changed slightly. There was now the *licence es sciences mathématiques*, which, apart from its purely mathematical content, contained some Newtonian mechanics and astronomy, and the *licence es sciences physiques* containing some physics but being more heavily weighted towards chemistry and mineralogy.[37] However, the content of the *licence* was only a little higher than that which had been taught in the *lycées* to prepare students for the *baccalauréat*, and the teachers and research workers, who were to make the most successful careers in physics, were the ones who had not received the totality of their scientific education from the faculties, but who had attended either the *Ecole Polytechnique* or the *Ecole Normale*. Those who rose to the summits of the French academic establishment were normally the ones who had studied at the *Normale* and been highly placed in the national competitive *agrégation*, which *normaliens* dominated, although *polytechniciens* could often combine a successful career in one of the state corps with teaching at the *Polytechnique*, or the *Ecole des Mines*, or the *Ecole des Ponts et Chaussées*.

So in this study of physics in France, we will be looking at the work of teachers in the context of the institution in which they taught and possibly, but not necessarily, did their research. As a teacher at the top of his profession would often work in more than one Parisian institution (the French practice of *cumul*) it is often difficult to associate a particular piece of research with one particular institution. This in turn tends to blur any distinction which might otherwise be made between institutional research styles; what emerges much more clearly in such cases, is a personal research style, which asserts itself in whatever institution the individual is working. Nevertheless, the attempt is made in this thesis to identify, as far as is possible, what the term 'physics' meant to, respectively a *polytechnicien*, *normalien*, *centralien*, or to someone who had taken his *licence es sciences physiques* at the Paris Faculty, for example, and to see how the meaning of the term changed with time to encompass new discoveries.

Silliman and Kuhn have argued cogently that physics had already established itself, through the work of Laplace, Poisson, Fourier, Fresnel and others,(which had formed a bridge between the old categories of *physique générale* and *physique particulière*), as an autonomous well defined discipline by about the third decade of the century.[38] Certainly it had long been recognised as having a different content to chemistry, as testified by the title of the journal, *Annales de chimie et de physique*, established in 1816 with Arago appointed as the editor of the physics articles and Gay-Lussac the editor of the chemistry ones.

Although originally within the category *physique particulière*, chemistry had defined itself more precisely through the work of Lavoisier in the eighteenth century, while physics was slower to establish both its content boundaries and an internal coherence. Where the actual boundary lay between the two sciences, was interpreted differently by different institutions. The fact that the Academy placed physics in its mathematical sciences section, while the Faculty included the study of physics not within its mathematical sciences *licence* programme, but in the physical science *licence*, demonstrates that the definition of the discipline was still not as clear as Silliman and Kuhn suggest. By examining, a), the content of the teaching and the research done by men who held physics chairs, b), research submitted to the Academy for physics prizes, and c), research presented to the *Société de Physique* and the physics section of the *Association Francaise pour l'avancement des sciences*, throughout the period of this thesis, the process of continuing definition of the discipline will be charted.

Notes for chapter 1.

1. D. Diderot,'Explication detaillée du système des connoissances humaines', *Encyclopédie,ou Dictionnaire raisonné des sciences,des arts et des métiers,par une société de gens de lettres. Mis en ordre et publié par M. Diderot; et quant à la partie mathématique par M. D'Alembert*, 36 vols., Paris, 1751-1780, *1*, xlix.
2. R.H. Silliman, 'Fresnel and the emergence of physics as a discipline', *H.s.p.s.*, 1974, *4*, 137-62, (141).

3. T.S. Kuhn,'Mathematical versus experimental traditions in the development of physical science.', The esential tension. Chicago, 1977, pp.31-65, (p.50).
4. J. D'Alembert,'Expérimentale', in D. Diderot, op.cit., (note 1), 6, 301.
5. T.L. Hankins, Science and the enlightenment, Cambridge, 1985, p.50.
6. R.H.Silliman, op.cit., (note 2), p.142.
7. A. L. Lavoisier,'Notice relative à l'Académie des Sciences', Oeuvres de Lavoisier, ed. J.B. Dumas and E. Grimaux, 6 vols, (Paris 1862-1893), 4, 559.
8. Ibid.
9. Ibid.,560.
10. T.S. Kuhn, op.cit., (note 3), p.52.
11. R.W. Home,'Poisson's memoirs on electricity; Academic politics and a new style in physics', B.j.h.s., 1983, 16,239-260.
12. Ibid. (p. 255). But it should be stressed that Arago, writing after Poisson's death, stated that it was only because the majority of Academicians were impatient to see Poisson take his place among their ranks, that they elected him to the the physics section, at a time when there was no vacancy in the mathematics section. From;

    D.F.J. Arago,'Simon Denis Poisson', Oeuvres de Francois Arago, 2^{me} edition, (ed J. Barral), Tome ii, Paris 1865, p.603.
13. T.S. Kuhn, op.cit., (note 3), p.52.

    Apart from the Silliman article, op.cit., (note 2) see

    R.Fox,'The rise and fall of Laplacian physics', H.s.p.s,1974, 4, 89-136.

    T.S. Kuhn, op.cit., (note 3), pp.61-62.

14. D.F.J.Arago,'Histoire de ma jeunesse', *Oeuvres de François Arago*, 12 vols.,(Paris 1854-1859), *1*, 12-13.

15. R.H. Silliman, op.cit., (note 2), p.145.

16. J.L. Gay-Lussac, *Leçons de physique*, Paris, 1828.

17. J.L. Gay-Lussac, *Cours de physique*, 2 vols. Paris, 1828, tome i, p.7.

18. G. Lame, *Cours de physique de l'Ecole Polytechnique*, 3 vols., Paris, 1840.

19. Ibid.,*1*, ii-iii.

20. Ibid.,*1*, 2.

21. J.C.E. Péclet, *Traité élémentaire de physique*, 2 vols., 4th ed., Paris, 1847.

22. Ibid.,tome i., p.i.

23. J.C.E. Péclet, *C.r.*, 1845,*20*, 54.
    C.S.M. Pouillet,'Reponse', ibid., 199.

24. J.L.F. Bertrand, *Revue des deux mondes*,1896,*138*, 528-48.

24. A. Comte,*The positive philosophy*, Paris 1855, (reprint of the H. Martineau translation, New York, 1974), pp. 193-94.

25. A. Ganot.*Traité élémentaire de physique expérimentale et appliquée et de météorologie*, Paris,1872, 15th ed.,p.1.

26. Ibid., p.3.

27. Ibid., p.4.

28. Ibid., p.243.

29. R. Fox, 'The savant confronts his peers', in R. Fox and G. Weisz, *The organisation of science and technology in France*, 1808-1914, (Cambridge and Paris 1980), pp.241-282,(p.249) This book also gives a very full bibliography on French teaching and research institutions in the nineteenth century.

30. M.P.Crosland,'From prizes to grants in the support of scientific research in France in the nineteenth century: The Montyon legacy', Minerva, 1980, 17, 355-80.

    E. Crawford,'The prize system of the Academy of Sciences 1850-1914', in R. Fox and G. Weisz, (eds.) op.cit., (note 29), pp.283-307.

    A. Galvez, 'The reward system of the French Academy of Sciences in the nineteenth century'. Unpublished Ph.D. thesis, University of Kent, 1987.

31. R. Fox and G. Weisz,'The institutional basis of French science in the nineteenth century, in R. Fox and G. Weisz,(eds), op.cit., (note 29), pp 1-28.

32. Ibid.

33. Almanach Imperial, Paris, 1857. The figures were compiled by adding up all the lycée staff listed under the section on the Université Imperiale.

34. R.Fox, op.cit., (note 22), p. 268.

35. V. Karady,'Educational qualifications and university careers in science in mid-nineteenth century France', in R. Fox and G. Weisz,(eds), op.cit., (note 22), pp. 95-124, p.99.

36. M.P. Crosland, The society of Arcueil; a view of French science at the time of Napoleon I, London, 1967, p.216.

37. Almanach, Paris, 1875, p.1163.

38. R.H.Silliman, op.cit., (note 2), p.137.

    T.S. Kuhn, op. cit., (note 3). The emergence of physics as an autonomous discipline is dealt with in the section, 'The genesis of modern physics', pp.60-65.

2. PHYSICS TEACHING AND RESEARCH DURING THE SECOND EMPIRE.

a. Introduction.

The Second Empire, which was proclaimed in December 1852, was to be a period of relatively dynamic economic and industrial growth in France. In fact, not until the early twentieth century were the annual growth rates achieved in the best years of the Second Empire, surpassed by the republican regime which succeeded it. This economic success was due to several factors, both national and international. As Landes has shown, 1850 to 1873 was a period of unprecendented growth for continental Europe, especially Belgium, France and Germany, with railway mileage, coal consumption and production, and pig iron production all rising strongly.[1] That other index of industrial development, the consumption of raw cotton also rose sharply until textile manufacturers were hit hard by the American Civil War in the 1860's.

In France, Louis Napoleon's regime offered industrialists and financiers the prospect of social and political stability, which is what they required above all else before embarking on long term projects involving large amounts of capital. Moreover, Napoleon and his Saint Simonian advisers, held firmly to the conviction that the state must play a pre-eminent role in liberating the productive power of industry. And there can be no doubt that the state in this period, facilitated the massive investment in railway building. In 1852 France possessed no more than 3,000 kilometres of track, discontinuous, fragmented and divided into many companies. In the Second Empire, lines which had been proposed nearly ten years previously but held up by local interests and the slowness of the parliamentary discussion in the July Monarchy, were put in hand. By the time the Empire collapsed in 1870, some 20,000 kilometres of track in a coherent national system with connections to foreign networks, had been constructed.[2]

The shortage of coal, and particularly certain grades of coking coal for metallurgical processes, remained a problem for France in this period, in spite of the discovery of new coal deposits in the Pas de Calais. In fact she would continue to have to import around 35% of her coal needs until the twentieth century, but the new deposits and the lowering of transport costs during the Second Empire, stimulated the growth of other sectors of the economy. The adoption of the Bessemer process in six great ironworks, including Le Creusot, almost halved the cost of steel railway lines between 1860 and 1867. Although, to keep this achievement in perspective, it must be said that costs fell in Britain during a similar period by a considerably greater amount. However, by 1869 the production of iron and steel, not all of it produced by the Bessemer process, peaked at 1.3 million metric tons a year.[3] This amount, slightly more than that produced by Germany, was not to be surpassed for more than ten years in a France, which lost her industrialised regions of Alsace/Lorraine to Germany as a result of the defeat of 1871.

The shortage of coal resources also hampered the utilisation of the steam engine in France, although it must not be forgotten that hydraulic machines continued to make a larger contribution to the total energy requirement than anywhere else in Europe, with some water turbines producing more than 200 H.P..[4] Although statistics on the use of steam power in Europe in the mid-nineteenth century are approximate and figures vary according to the sources consulted,[5] there is fair agreement on the growth rates if not on the absolute figures. It would seem that the power derived from steam engines, both fixed and mobile, increased by a factor of six between 1850 and 1870 in France, but again to keep this figure in perspective, it should also be said that in Germany the figure increased by a factor of nine in the same time. More significantly, by 1870, France had been ousted from the second place she held in Europe in 1850 in the production of steam power, to third place behind Germany. Britain of course was far in the lead, although the gap was beginning to close by 1870.

Electricity was not yet playing a significant role in the industrial process, although arc lighting and electro-plating had been introduced. However, the telegraph system which had a national extension and was joined by submarine cable to England in 1851 and to the developing colony of Algeria in 1857, was raising the demand for conductors and insulating materials, transmitting and receiving apparatus, and reliable electric cells. A new corps established in 1846, L'Administration des lignes télégraphiques, directed by polytechniciens, ensured that this new means of communication was firmly in the hands of the state.

All this economic and technical advance put new demands on the educational system, and successive administrations tried to find adequate responses to it. The point should be made that dissatisfaction had long been been expressed in France about the position of science in the school curriculum. As long ago as 1833, Guizot had attempted to introduce a more technical curriculum through the establishment of écoles primaires supérieures, but they had failed because the farmers, clerks, and teachers, who could afford to send their sons to post-elementary education, preferred the social status which the word 'secondary' brought with it and continued to patronise the lycées and collèges. Fourtoul's bifurcation reform of 1852 introduced a new scientific baccalauréat, which aimed to prepare its students 'for the battles of production' rather than the pacific contemplation of literature and the classics.[6] A decade later, Victor Duruy the Minister of Education from 1863 to 1869, had become convinced, while acting as an inspector of education, that while the classical education of the lycées was excellent for those who intended to become doctors or lawyers, it would be much better;

> '..to give to the future employees of industry, commerce and agriculture, the special knowledge demanded for their professions.'[7]

Duruy's law of 1865 attempted to graft this 'special' education onto the *lycée* curriculum. And as there were to be 'special' subjects taught in the secondary schools, there would have to be teachers for these subjects. In 1866 the *Ecole Normale Spéciale* was set up in Cluny for this purpose, and a special *agrégation* established for its teachers. As with the *Ecole Normale Supérieure* in Paris, the objective of the Cluny school was to train science and technologgy teachers, not to teach science for its own sake. In its first eleven years of existence it supplied 119 teachers to the *lycées* and 186 to the *collèges*.[8] But the teachers it produced met with some hostility in the schools in which they taught, from colleagues who were reluctant to accept them as equals, and the school lost its administrative autonomy in 1872 and was closed in 1891.[9]

In 1855 the Ministry of Education instituted a new type of school in some of those centres which did not have a faculty of science or letters. This type of school, the *Ecole préparatoire à l'enseignement supérieur des sciences* provided its students with a two year course, practical in orientation, suitable for young men entering industrial and commercial careers. Physics was taught as a discipline separate from chemistry, whereas in the *lycées* a single combined science, *sciences physique, chimiques et naturelles* was taught. Initially four of these new institutions, financed by the local authority, were set up; in Rouen, Nantes, Angers and Mulhouse. In the following year, one was formally established in Moulins but never staffed, and then came two more; in Chambery (1861) and in Algiers (1880).[10]

But although the increase in importance given to science in the schools, signified a greater demand for science teachers and therefore operated to the advantage of the science section of the Ecole Normale and the faculties of science, the political climate of the time put strict limits on what should be taught there. Fourtoul in 1851 had insisted on the need for the religious and political conformity of the Ecole Normale, insisting on its role as an institution for the training of teachers,not for the pursuit of original knowledge. Another contemporary commentator was to write that;

> 'The proper mission of the university is to teach the most undisputed parts of human knowledge,it is not to encourage the inventive spirit nor to propagate discoveries which are not fully verified.'[11]

So the Ecole Normale was to be a centre which prepared its students for the modest and laborious duty of teaching and the faculties would communicate knowledge which had already been established on a firm theoretical basis. Moreover, the provincial faculties of science in the Second Empire and early part of the Third Republic, as several modern historical analyses agree,[12] found few serious students to communicate knowledge to anyway, and taught at a level little higher than that of the lycées. Provincial faculties gave employment, as professors of physics, to those men who had gained a doctorate in a suitable area of research from the Paris Faculty, and it then afforded them a salary (less than they would have gained in Paris), time and very limited facilities for research.

In France, only at the Ecole Normale where teaching took place in something like seminars, the conférence, were teachers able to have any personal influence on their students. In all other institutions teaching took place in lectures, in which the highest attributes of the teacher were considered to be a fine oratorical style and clarity of explanation. This was particularly true of the provincial faculties, whose staff members saw few students for the licence and sometimes tried to find a role for themselves by giving popular and uplifting science lectures to the local bourgeoisie. It should be remembered that, although it was necessary to register at the faculty to take the licence, attendance at lectures was not compulsory and many students prepared for it by private study. The major function of faculty staff in the first half of the century was the grading of baccalauréat candidates, although some taught in local lycées, and a few found the enthusiasm and the necessary equipment to do research.

Although recent studies like those of Nye,[13] have tended to re-assess the traditional centre-periphery model in the analysis of French nineteenth century science, in mid-century at least it can be said that there were no geographically dispersed competing centres of excellence in France, as in Britain and Germany. Paris was virtually the only producer of new knowledge in physics, and it was not until the end of the century that it could be said that there was any challenge to this domination. Even then, and in the years leading up to the Great War, by far the most research in the revolutionary areas of physics, was being done in the capital.

The most important reform of higher education in the Second Empire was that of the establishment of the Ecole Pratique des Hautes Etudes in 1868. Coming after a conference between the Emperor and four of France's most eminent scientists, it responded to the growing anxiety over the relative strengths of German and French research facilities. Pasteur had been waging a campaign for increased funding for French science, and in an article published in February 1868, graphically described the hardships under which scientists laboured in their miserably ill-equipped laboratories.[14] Pasteur's campaign won the support, first of the liberal Minister of Education, Victor Duruy (1811-1894) and then of the Emperor himself. Duruy had come to recognise that it was particularly necessary in the provinces;

'.. to give to our professors, genuine students, instead of a floating, continuously changing audience.'[15]

But the reforms when they were made, benefitted only the Paris institutions; Pasteur was to get a new laboratory in the Ecole Normale and Claude Bernard one in the Muséum. Of more importance, certainly for the history of physics, was the establishment of the Ecole Pratique des Hautes Etudes. This was not, as the name might suggest, a new school of experimental studies, but a new mechanism of awarding grants to the existing Parisian institutions of higher education, permitting more students of high calibre to stay on to undertake research. While making no significant difference to the number of posts for physics teachers, it provided more money for teaching laboratories and also provided more agrégé préparateur (research assistant) posts. This would allow more normaliens to avoid the previous pattern of starting their career by teaching in lycées, giving them,instead, a grant and a place at one of the Paris institutions of higher education to work directly for their doctorates. Most of the physicists who came to prominence in the last two decades of the century and after, began their careers as research assistants in Paris, in this way. The measure also accentuated the centralisation of French science (one of its merits in the view of Duruy) by making this new grant structure only available to institutions of the capital.

As we shall see, most of the research in physics in the Second Empire was the work of members of staff from the higher education institutions of Paris, particularly the Ecole Polytechnique and the Ecole Normale. It must not be forgotten that these were institutions with specific vocational objectives; in the case of the Polytechnique, to train young men for both the military and the civil engineering corps of the state, and in the case of the Normale, to produce science teachers for the Université system, which in practice meant lycée teachers. These objectives determined both the science which was taught and the pedagogic methods employed, which in turn determined the special traits of Polytechnicien and Normalien scientists.

b. The Ecole Polytechnique.

Let us look first at the Ecole Polytechnique. The recent analysis of the school by Shinn, has brought out very clearly the elite and privileged character of the majority of polytechnique student.[16] High fees for tuition and residence, stiff entrance qualifications which required a year or more of extra tuition in mathematics after the baccalauréat, put the school well beyond the purses of all but the sons of the most wealthy of French citizens. Throughout the nineteenth century, the school accepted around 200 students each year, most of whom were destined to enter the army as engineers or artillery officers. But the ones who performed best in the school's final examination would go on to the Ecole des Mines, or the Ecole des Ponts et Chaussées in order to train for a further year to become engineers of these corps. Around 12% of the students passed to the state corps; 2% to the Corps des Mines, 8% to the Corps des Ponts et Chaussées, and the other 2% to Manufactures de L'Etat, or Télégraphes.[17] Usually the best individuals (no more than about five or six students) would go into the Corps des Mines, and the next twenty or so into Ponts et Chaussées.

The debt which science owes to the early nineteenth French engineers trained in the mathematical rigours of the Ecole Polytechnique has already been well documented. Almost all the great contributions to mathematical physics of the beginning of the century came from men associated with the Ecole Polytechnique either as teachers or students, or both. But it was an establishment under the control of the Ministry of War, with a highly disciplined regime, and one not suited to the formation of independent, creative scientific minds. Physics never had a particularly great weight in the syllabus, accounting for about 10%

in a time-table heavily biased towards the study of mathematics and mechanics,[18] and the lack of space in the new premises which the school moved into in the early nineteenth century, meant that practical instruction in physics was severely limited.[19] Certainly the students learnt highly advanced and abstract mathematics, but they learnt it through the memorising of theorems and the continued repetition of many similar examples.[20] Polytechniciens were formed with a sharp sense of social responsability, and a Saint-Simonian conception of the utility of science for the development of society. They acquired, by a rather backward form of pedagogy, knowledge of a complete, well organised, finished set of principles and laws. Starting as a social elite, the student body was formed by the school into a technological elite, with a reputation for objectivity and infallibility.[21]

The school had already come under attack before 1850 because of the mediocrity of its graduates and its declining contribution to the training of physical and mathematical scientists (in which it had been pre-eminent), but successive efforts of reform did little to improve it. Only at the end of the century, when the teachers of the faculties of science had grown in number and self confidence, did their criticism of the inability of polytechniciens to tackle the increasingly complex problems thrown up by technological advance, especially in electricity, begin to effect significant changes at the school. Members of the Corps des Mines, were cast as the villains in the story of the slow development of new iron ore deposits discovered in Longwy-Briey in the 1890's, and Polytechniciens were blamed for the general inefficiency of the French telephone system in the early 20th century, by which time the faculty professors were arguing that the general programme of study of the Ecole Polytechnique, could just as well, if not better, be taught in the courses of the faculties of science.[22] But apart from its deficiencies in technological matters, what interests us here is its role in the training of men who did research, which was independent of and separate from their function as a state engineer and which they and their peers would have called physics. In 1850, five of the six members of the physics section of the Academy were polytechniciens, but this number dropped to one in 1880, although recovering to three in 1900. After mid-century the most prominent polytechnicien physicists were Cornu, Potier and Henri Becquerel, and (even though he was chiefly a pure mathematician), Henri Poincaré. Nothing illustrates better the elite nature of the Corps des Mines than to say that Cornu, Potier and Poincaré were members of it. Becquerel was an engineer of Ponts et Chaussées. It is clear that even by the end of the century, polytechniciens occupied positions in the highest level of French physics, disproportionate to the number produced by the school. This might be better explained by the continued existence of networks of power and influence, rather than by the quality of instruction at the school.

In the first half of the nineteenth century, the Polytechnique had a strong tradition of physics teaching, even though (as has been said) the subject did not have the same prestige as astronomy, geodesy, mathematics or mechanics at the school. By 1850, when our period opens, the influence of the mathematical physicist Gabriel Lamé (1795-1870), who had held the physics chair in the 1830's was still strong, and his textbooks were still used and regarded as models by both teachers and students alike.

But the chair had passed to someone of a very different mould by 1850; Auguste Bravais (1811-1863). Bravais was something of a scientist in the 'Humboldtian' tradition; an ex-naval lieutenant and explorer, who had worked in no fewer than 13 different fields of study including oceanography, geology and botany, and he had been one of the founders of the French meteorological society. His doctorate, defended in 1837, was appropriately enough on the equilibrium of floating bodies, but he is best remembered today as a mineralogist through his work on crystal structure. After a short spell in the Faculty of Lyons, during which time he continued research in meteorology and the geology of the Alps, he was unanimously chosen to occupy the chair in physics at the *Ecole Polytechnique*. It is possible that his military background and his own *Polytechnique* origins compensated for his lack of specialisation and his interest in botany, geology and mineralogy, all of which had their own discipline groups in the *sciences physiques* section of the Academy. Physics, on the other hand, was in the *sciences mathématiques* section. Having accepted the post, Bravais declared his intention 'to devote myself exclusively to the study of physics',[23] although this did not prevent his competing successfully for a place in the geography and navigation section of the Academy in 1854, on the basis of his previous naval experience. His research in the 1850's, although now more specialised than before, continued to range over many fields; *physique du globe*, electricity, the velocity of sound, polarisation and double refraction. In 1851 he was responsible for the design of a new type of polariscope.[24] In the *notice* to further his application to the Academy, he stressed that most of his work had a practical motive and that he had unjustly been accused of being a mathematician.[24]

Bravais was the physics incumbent during the 1850 commission of enquiry, which was set up to investigate the school and its programmes of study. The commission, headed by the astronomer and parliamentary deputy Urbain Leverrier, who had resigned as *répétiteur* in geodesy at the school in order to lead the commision was very critical of the syllabus and of the director of studies, the mathematical physicist Duhamel, who was forced to resign. An historian of the *Ecole Polytechnique*, Mercadier,(professionally a telegraph engineer who later taught physics at the school), writing at the end of the century was not at all convinced of the usefulness of the reforms instituted by the commission, which led to the extension of an already overburdened curriculum. For apart from a recommendation that there should be practical work (*manipulations*), in physics the content of this course was left unchanged.[26] But the other recommendation relating to physics, the establishment of a second chair,was important, at the very least, for the career prospects of physics teachers.

Since succeeding to the physics chair, Bravais had instituted changes in the syllabus inherited from Lame. Lamé's teaching methods had been described as 'more profound than clear'[27] and Bravais had suppressed some of the most difficult parts, added some points of his own of clarification, and orientated the course towards a more experimental approach. He also instituted a study of natural phenomena. The enquiry commission had made it clear that it wanted an experimentalist to occupy the second chair and its choice fell on Jules Jamin (1818-1886). Jamin had left the *Ecole Normale* in the early 1840's, ranking first in the *agrégation*, and earned his doctorate with a thesis submitted to the Paris Faculty while teaching in Paris *lycees*, before being appointed to the new *Polytechnique* chair in 1852. His research interests, at least in this stage of his life, always related to optics and interferometry, and although it would be too strong to refer to him as a *protégé*, he undoubtedly received encouragement and material support in the early part of his career, from Arago who shared these interests.[28]

Jamin was to have an enormously successful career, adding the physics chair of the Paris Faculty to his *Polytechnique* one, as well as becoming permanent secretary of the mathematical sciences section of the Academy. There is,however,little evidence that he had much influence as a teacher. Mercadier,possibly not very sympathetic to Jamin,as a *normalien* intruder into the *Polytechnique*, described him as an 'orator who gave the illusion of clarity'.[29] Jamin taught at the school for over 30 years, pushing the mathematical approach of Lamé into the background. He maintained the content of the physics course virtually unchanged throughout this period, and in 1881, passed on to his successor, Potier, a teaching programme of some antiquity.

In the 1850's, Jamin continued his work on reflection from metallic surfaces (the subject of his doctoral thesis of 1847) and in 1852 published a memoir on the coloured rings produced between a curved lens and a reflective surface, employing polarised light.[30] The experiment provided verification for the mathematical treatment of the question which Cauchy had made in 1836. Four years later Jamin constructed the refractometer,[31] with which he carried out extremely precise experiments to determine the refractive index of water at different pressures, and the refractive indices of a number of different gases, finding surprisingly good agreement with the non-interferometric studies which Arago and Biot had made, half a century before.[32] He did not forget the astronomical applications of his work and in 1857 used the refractometer to measure the effect of water vapour on the refractive index of air.[33] The value used by astronomers in calculating the true positions of observed celestial bodies close to the horizon,was 1.000 292 and Jamin concluded that the presence of water vapour affected only the seventh decimal place of this number,and thus could be neglected.

A particularly precise experiment was carried out by Jamin in 1856, on the variation of the refractive index of water with temperature. This had originally been of interest to Fresnel who had constructed some apparatus to do the experiment,and on his death this apparatus had been bequeathed to Arago. In 1850 Arago passed it on to Jamin with the recommendation that he should try to to carry out the experiment, but Jamin after 'months of fruitless attempts'[34], gave up the work because the apparatus was unsuitable. Now, six years later, he had his own refractometer, an instrument precise enough to measure the very small changes in the refractive index of water expanding or contracting as the consequence of temperature changes. What made the experiment so interesting to Arago in 1850, was that it could be considered 'crucial' in the sense of refuting one of the last unrefuted consequence of the emission theory; the independence of the refractive power of a medium, of its density. Refractive power, given by the formula $(n^2-1)/d$, where n is the refractive index of the material and d its density, should remain constant as the material of the medium expands or contracts, according to the emission theory.[35] By observing the changes of refractive index of water around its maximum density at 4°C to a very high order of accuracy, Jamin established that the refractive power did not remain constant. Thus the emission theory of light was 'falsified' for the second time. One must presume that the result held considerably less interest in 1856, than it did earlier, for by this time the first 'crucial' experiment which had pronounced in favour of the wave theory, (Foucault's demonstration that light travelled faster in air than in water) was already six years in the past.

Jamin's work at the Paris Faculty, to which he was appointed in 1863, will be considered later in the appropriate section; here we will return to Bravais and the other _Polytechnique_ physics chair. Bravais was stricken with illness in the early 1850's and much of his teaching was done by _répétiteurs_, until in 1856 he was succeeded by another mineralogist, H.D. De Senarmont (1801-1862). De Senarmont had also been a student at the _Polytechnique_ and had gone into the elite _Corps des Mines_. From 1847 he held the chair in mineralogy at the _Ecole des Mines_ and had published some 35 research papers by the time he took up his post at the school but published little afterwards. An able experimentalist and mathematician, De Senarmont had built his career upon his work on double refraction of crystals and their thermal conductivities in different directions. Described as a brilliant teacher with an interest in 'lofty theoretical conceptions'[36] and much admired and respected by his pupils, his influence on future generations of French engineers and research workers might have been profound had he not died suddenly in 1862. His successor, Emile Verdet (1828-1866), held the chair for less than four years, and as his influence was more marked at the Ecole Normale where he worked for 18 years until his death,we will leave the consideration of his work to the section on that school.

Verdet's successor was the precocious M.A. Cornu (1841-1902) whose career spanned the remaining years of the century. An orbituary to him which appeared in the Révue Génerale of 1903 and which concentrated , not unnaturally on his original scientific research, had this to say about his teaching;

> 'The students admired the clarity of his teaching and the elegance of his experimental demonstrations.'[37]

Not given to theoretical speculation, Cornu also never showed any particular liking for the new theories which came to the fore in the final years of the century, although he did hold assumptions about the ultimate reality of the physical world being no more than matter in motion, and he did defend atomism against the attacks of Ostwald.

Cornu's earliest research papers were joint ones with the polytechnician telegraph engineer, E.J.Mercadier (1830-1911) on musical scales, but his career was built on the research into the velocity of light, using the toothed wheel apparatus of Hippolyte Fizeau (1819-1896) which he had presented to the school. Cornu's research, which occupied him through the 1870's, will be considered in a later chapter. Fizeau had not been a student at the Polytechnique, but he is said to have educated himself from the lecture notes of the school, and after being elected to the physics section of the Academy in 1860, he became an examiner there. His scientific education also came from the optics lectures of H.V. Regnault at the *Collège de France*, and above all from Arago's popular astronomy lectures at the Observatory.[38] He was also one of the many research workers whose early careers had been assisted by Arago, having been invited to work in the Observatory with the technician Bréguet on the experiment to compare the speed of light in air and water. Because of these connections, and because he undoubtedly had a profound influence on the type of research which Cornu was to undertake, we will briefly examine his work in this section on the *Polytechnique*.

The 1850's and 1860's were Fizeau's most productive period, and produced such experiments as his confirmation of the Fresnel ether drag coefficient through a column of flowing water, and his attempts, which he cautiously considered to have been successful, to do the same for a solid transparent body. These experiments were carried out in his own private laboratory and largely at his own expense. The experiment to confirm the hypothesis of ether drag in a moving liquid, was reported to the Academy in 1851.[39] Using two parallel glass tubes of about 1.5 metres in length and about 5mm diameter, he connected them together and passed water through them, so that it moved in opposite directions in the two tubes. Two coherent beams of light were passed down the centre of each tube and brought to a focus so that interference fringes could be observed. When the water was sent flowing through the apparatus, a small fringe shift was observed, and the shift increased as the velocity of the water flow was increased. The fringe shift showed that there was a change in wavelength (and hence of velocity) of the two beams of light; it was increased for the light travelling with the flow, and lowered for the light going against the flow. Fizeau made many observations at the maximum water flow which he could achieve, 7.06 metres per second, and found an average fringe shift of 0.23 fringes. Calculations from Fresnel's ether drag coefficient found a value of 0.2022 fringes which was in fairly good agreement with Fizeau's result. Furthermore, Fizeau was able to reason that the water in the centre of the tube was probably flowing faster, due to the effect of viscosity, than the mean value of 7.06 m/s., which would reduce the discrepancy between the two values still more. He also tried the experiment with air flowing through the tubes, but could obtain no visible fringe shift due to the low value of the absolute refractive index of air. His result in water was confirmed by Michelson and Morley in 1886.

In 1859, Fizeau carried out an experiment to try to verify the drag coefficient for a solid transparent medium, as an earlier polytechnicien, Babinet had tried, unsuccessfully, to do more than a decade before. Clearly the method used for fluids cannot be used in this case, and Fizeau's ingenious solution involved the use of polarisation techniques. The velocity of light through glass was to be found by measuring the rotation of the plane of polarisation of a beam of polarised light passing through a pile of glass plates, set at an angle to the incident ray. The measurement of this angle would yield information about the refractive index of the glass, which in turn could be related to the velocity of the light travelling through it. The rapid movement of the glass plates through space would be provided by the rotation and translation of the earth itself through space, a velocity which would vary with the period of the year annd the time of day. In the words of Fizeau himself;

> 'In the period of the solstice for example, the direction of movement is found to be horizontal and from west to east at midday: so that in these circumstances, a plate of glass, receiving a ray of light from the west must really be considered

to be moving with a speed of 31,000 m/s, in a sense contrary to that of the propagation of light.[40]

Fizeau was convinced that the observed effect was real and caused by the dragging along of the ether in the moving pile of plates, but he realised that the apparatus was crude. He announced in this paper that he was going to continue the experiment with more suitable equipment, but there is no record that he ever did, and towards the end of his life in 1894 he wrote that he no longer considered the experiment to be decisive. H.A. Lorentz (1853-1928) writing in the following year,[41] declared his suspicion that the data were the result of experimental error, or at least did not correspond to the theoretical considerations on which the experiment was based. Later in the century, as we shall see in a later chapter, E. Mascart (1837-1908) repeated Fizeau's experiment but could obtain no alteration in refractive index.

It can be said that these optical projects of Fizeau lie very much on the inter-face between physics and astronomy. An earlier insight of his linked the spectrum of a heavenly body with its motion. In a paper read to the Philomatic Society in 1848, Fizeau argued that the dark lines in the spectrum of the sun (lines which were by this time called after their discoverer, Fraunhofer) would be shifted towards the red end of the spectrum if the body was moving away from an observer on the earth and towards the blue if it was moving towards the observer.[42]

This was a similar, but more refined idea to the one put forward by the Prague mathematics professor Christian Doppler. Doppler who had argued from the analogy with sound, thought that the light of a body would appear red or blue depending on whether it was moving away from or towards the observer, neglecting the fact that the invisible rays at each extremity of the spectrum would be made visible by such movement, and the overall coloured appearance of the body would remain unchanged. Fizeau, who does not mention, and perhaps had not heard of Doppler's work, insisted that the continuous spectrum would remain unchanged but that there would be a displacement of the spectral lines. Fizeau's conclusions were not published until 1870, after a communication to the Academy by the director of the Observatory of the Roman College, Secchi, brought attention to the fact that the characteristic sodium C line in the solar spectrum was very slightly displaced towards the violet end of the spectrum at one extremity of the solar equator, and displaced towards the red at the other extremity. The observed affect was very small and Fizeau cautiously concluded, in a memoir read to the Academy in 1870, that it was probably caused by a rotation of the sun.[43]

So there is evidence to show that the staff of the Polytechnique continued in the tradition, which had been established from the time when Arago viewed the school as the pépinière, the nursery, of young talent destined to make their career in the Observatory.

It is not easy to find students of the Polytechnique, who without going on to make an academic career, used their scientific and mathematical training acquired at the school, to do, (independent of and separate from their professional duties) research which could be described as physics. Clearly, without a laboratory at one's disposal, physics research is virtually impossible, and in any case the Polytechnique graduate was likely to be fully employed (perhaps even peripatically) in his professional work as a state engineer. But there are two interesting cases, which could be said to meet this description, which we will consider here.

The first is that of the mining engineer H.F. L. Peslin (b. 1836) who was able to use published data from the Paris Observatory and from scientific journals, to deduce mathematical relations between physical variables. Peslin's first piece of research related to meteorology, and combined the pressure gradient force and the deflecting force caused by the rotation of the earth, to derive what was later called the geostrophic wind equation. The deflecting force due to the earth's rotation had been demonstrated by Foucault with his pendulum and gyroscope experiments and the phenomenon had been much discussed in the late 1850's in the Academy with Babinet demonstrating that it operated on air and water masses moving in any direction on the earth's surface. Peslin's paper was submitted to the Academy in 1869 and considered by a commission containing the astronomers Leverrier and Faye, and the geologist of the _Muséum_, Daubrée. Daubrée presented the paper to the Academy but it was published in the _Comptes rendus_ only in a very abbreviated form without any of the mathematical reasoning.[44]

It should be remembered that Leverrier was head of the meteorological service, and probably did not take kindly to a mining engineer intruding into his scientific sphere of responsability. However, the paper was published fully, three years later in the _Bulletin météorologique international_, a journal published by the meteorological Observatory of Montsouris, and edited by its director, Marié-Davy. Peslin's later paper on spectroscopy will be left for consideration until later, but it can be seen that his research interests continued in those topics which were related to astronomy and the work of the Observatory.

The other ex-student of the _Polytechnique_, who carried out research in physics in the middle years of the century, is a rather different case; J. Gaugain (1810-1880). Information on him is sparse but it would seem that after some initial employment he had no paid post and only very limited personal means. All his research was in the area of electricity, and it seems that the laboratory study of the factors affecting the velocity of telegraph signals, was, during this period, exclusively in his hands. Gaugain, who was assisted by his son in the construction of experimental apparatus, received some financial help from Leverrier's *Association Scientifique de France*, and (in the form of the Tremont prize) by the Academy.

Gaugain was one of the most prolific contributors to the *Comptes rendus* on the subject of electricity in this period. He carried out experiments to verify whether Ohm's law held good for poor conductors, including liquids and from about 1859 he began to concentrate his attention on the study of the ability of certain arrangements of conductors and insulators to store electricity in a manner similar to a Leyden jar. The contemporary term was the ability of the system to 'condense' electricity, from which came the word condenser, for the device which today is called a capacitor. Beginning with a consideration of the condensing properties of telegraph cables under water,[45] he later developed theories for the storage of charge in plane and spherical condensers.[46] In the early 1860's Gaugain worked on the problem relating to the time it took for a cable to reach a permanent state of charge. He did not work with telegraph cables but with lengths of poorly conducting materials, like cotton threads, and then later extended these ideas to telegraph cables.[47] He realised that a submarine cable with a central conductor, insulated by gutta-percha, AND surrounded by the conducting sea water, acted as a giant capacitor, and that therefore the time to charge it up (or reach a permanent state of tension to use his terminology) would be increased and the velocity of the signal along it decreased.[48] Gaugain experimentally derived a formula for the time of charging and discharging a cable, which showed it depended on the square of the length of the cable. A similar expression had been put forward in the late 1850's in Britain by William Thomson in the discussion over the transatlantic cable, but Gaugain appears to have reached his conclusion independently of Thomson. In fact Thomson's work on telegraph cables seems to have been unknown in France in the Second Empire, despite its being published in

British scientific journals of national importance. In the obituary to Guagain in the Année scientifique et industrielle it does say of him that he;

> '..could not triumph over the indifference of the public and the community of savants.'[49]

Thus it would seem that Gaugain's electrical research evinced little interest in the scientific community of the time, even though the telegraph system, linking France with her North African colonies by 1857 and with the New World by 1869, was the technological wonder of the period. As the Polytechnique provided the scientific training for the members of the new state corps of telegraph engineers, which was established in 1846, it is not surprising that those who did research in this field, were products of the school. Apart from Gaugain's laboratory work, polytechniciens like Gounelle, Burnouf and Gullemin, as well as Fizeau, measured the speed of of telegraph signals over long distances along the line, finding that it travelled faster along copper than along iron and that the applied voltage made no difference to the speed.[50] Telegraphy, and then telephony and wireless telegraphy were particular areas of interest of the Polytechnique in the later years of the century.

c.The Ecole Normale

The student intake to the Ecole Normale, as to the Polytechnique was selected by means of a national examination, and only about twelve to twenty students entered the school each year for the science course after special preparation classes in certain elite lycées. It was overshadowed in the first half of the century by the Polytechnique, but in the second half, benefitting from the directorship of Pasteur, the inauguration of the publication of the Annales de L'Ecole Normale, and the founding of the position of research assistant (agrégé préparateur),it became the premier institution for the training of scientific research worker.[51]

The school did not award degrees, but its students took the licence awarded by the Paris Faculty, and the national agrégation, an award which opened to its holders the very best positions in lycée teaching. In 1850, the maître de conférence at the school was Emile Verdet, and he held the position until his tragically early death. In the last years of his life he also held positions at the Paris Faculty, and (as we have seen),at the Polytechnique. He also participated in the publication of the Annales de chimie et de physique, translating the latest papers from German and English and following, with particular interest, the developments which were taking place in thermodynamics, a subject which was somewhat neglected by French researchers. His own research began with the examination of effects in electrical induction and went on to investigate magneto-optical rotation. His heavy teaching duties, the consequence of the French practice of cumul, probably prevented his making a more significant con tribution to the production of new knowledge in physics, but there is little doubt that he was an excellent teacher. A later normalien teacher, Violle, writing in the commemorative volume to mark the 100th anniversary of the school, said of his lectures that;

> 'Each was a harmonious whole,nothing needed to be added and nothing taken away'.[52]

After his death, some of his students, Violle among them, collected his lectures and published them. But although Violle expresses an enormous respect for his teacher, there is little in his article to indicate the precise way in which Verdet influenced Violle's own work. He says of Verdet that he had a preference for philosophical questions but does not elaborate further. The dominant strand in Verdet's own research, appears to be a generally positivistic one, but one cannot identify a common philosophical standpoint among his students.

Verdet was twenty eight when the Second Empire was proclaimed, already with some research published, highly ranked in the agrégation, his doctorate awarded, and already teaching in the capital. Because of his interest in so many aspects of science, his work of scientific journalism (every volume of the Annales de chimie et de physique between 1852 and 1864 contained at least one article, synopsis, or translation by Verdet), his extensive academic duties, his short life and poor eyesight, Verdet was not responsible for a large quantity of research work. His earlier interest had been on electrical research but in the early 1850's he turned towards optical questions.

Verdet's most orginal work was on the influence of magnetism on the optical properties of transparent bodies, published in four memoirs in 1854, 1855, 1858, and 1863.[53] This work, continued the research of Faraday, who had already shown in 1845 that the plane of polarisation of a beam of polarised light is rotated as it travels parallel to the lines of magnetic flux, in a powerful magnetic field. Verdet's contribution was to deduce two quantitative laws for this effect. The first stated that the magnetic rotative power of the field varies proportionally to its intensity, and secondly that the rotation was proportional to the cosine of the angle between the direction of the rays and the lines of magnetic flux. In his third memoir he dealt with the effect of dissolved salts in the water through which the light passed, and in the fourth he sought to link the phenomenon of magnetic rotatory polarisation with the nature of the light rays themselves. Biot had shown that in substances which naturally rotated the plane of polarisation, the rotation was inversely proportional to the square of the wavelength, and Verdet wished to see if the same was true for materials which produced magnetic rotation. In experiments with carbon disulphide he showed that this relationship was only approximately true. Verdet died in 1866 at the age of forty two. He had not sought election to the Academy, although positions in the physics section became vacant in 1860 and 1863, because according to his biographer, De La Rive,[54] he did not want to apply while his friend Foucault was excluded. Foucault, in spite of the opposition of Leverrier, did gain admission to the mechanics section of the Academy in 1865.

Verdet's succesor was P.A. Bertin-Mourot* (1818-1884), whose teaching career at the *Normale* had more influence in the Third Republic, and so we will leave consideration of it to a later chapter. Also his own research was mainly carried out while he was at the Faculty of Science at Strasbourg and will be examined in the section on the provincial faculties.

As many of the graduates of the *Normale* went on to make succesful academic careers, their research will be examined when the institution in which they held the physics chair is considered, but it would be appropriate here to look at the doctoral research carried out by E.E.N. Mascart when he was working as an *agrégé-préparateur* at the school in the 1860's. Mascart entered the school after having worked for a time as an assistant teacher at the lycee in Lille, and after his *agrégation* worked for his doctorate in the chemistry laboratory of St. Claire Deville. This research was in the field of spectroscopy, and although of course, spectroscopy was being used as a valuable tool of analysis by chemists,(and presumably this is why he found the necessary equipment there) Mascart's spectroscopic research related more to the astronomical application of the technique.

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* Bertin-Mourot is frequently called Bertin in contemporary literature and so will be referred to by this shortened form in this thesis.

Mascart's work began in 1863 with the determination of the wavelength of the 'A' ray in the near infra-red portion of the solar spectrum, for which, in the following year, he was awarded his doctorate. Working with potassium chloride in a gas and oxygen flame, and using a diffraction grating, he obtained a value of 7.68 x 10^{-7} m., as against the previously accepted value of 7.5 x 10^{-7} m.[55] Later in the same year, using a photographic plate as a detector, and a spectrometer with quartz lenses, he determined the wavelength of a number of principal lines in the solar spectrum. In 1867 he received the Bordin prize for the 'determination in a very precise way of the wavelength of some rays of light'.[56] He had determined the wavelengths of a number of Fraunhofer lines going from the ultra-violet region to the long, already mentioned, 'A' ray. He employed six different diffraction gratings (as a check on the accuracy of the wavelengths obtained) which had been made for the Ecole Normale by the intrument maker, Norbert. In 1869 Mascart continued his research into the UV region of the spectrum, using a spectrometer with optical components of Iceland Spar, purchased with funds provided by Leverrier's Association Scientifique.

In his next paper, later in the same year, Mascart permitted himself some theoretical speculation about the relationship between the observed spectral lines, in the following question:

> 'Is it not natural to admit that these groups of similar rays are the harmonics which the gas has, through its molecular constitution? Without doubt it will be necessary to make a many more analagous observations to discover the law which governs the harmonics.[57]

It was this statement, pointing towards an analogy between the observed frequencies of light in the spectrum of a material, and the number of related frequencies in the note emitted by a vibrating body, which influenced the work of later spectroscopists.

d. The Paris Faculty

The Second Empire saw an expansion of the Université. In 1854, new faculties of science were established in Marseille, Clermont, Lille, Nancy and Potiers, giving the basic structure of French secondary and higher education, which was to last into the twentieth century. The number of lycée positions for a man trained in the physical sciences, doubled during the period of the Empire, and a similar growth took place in the lower status collèges communaux. Thus the employment market for holders of the licence grew, and that in turn swelled the numbers taking that examination. Moreover, as the best positions, i.e. the lycées rather than the collèges communaux, or the Parisian rather than provincial lycées, were open only to the holders of the agrégation, the importance of this examination grew as well. Although this was originally, essentially an exam to further the internal advancement of already serving teachers, [58] it had long been dominated by students of the Ecole Normale and would continue to be so.

As was mentioned in the previous chapter, physics was not taught as a separate subject in the lycées, but as a part of a combined physical, chemical and natural sciences programme. Faculties of science employed physics professors, who taught the subject as a component of the licence in the physical sciences. The level of the licence was little higher than that of the baccaleauréat taught in the lycées , and both the teaching programme for it, and the position for which it prepared the holder (ie. teaching science in a lycée or collège) tended to make it a non-mathematical, descriptive, and experimental science, rather than a mathematical one. Thus the Paris Faculty, (as on a much more reduced scale the provincial faculties), produced secondary school teachers in the physical sciences. So did the Ecole Normale, but gave its students a higher level of scientific preparation, for the agrégation. Only if students went on to do a doctorate, with the subject matter unequivocally physics, and if they went on to gain a physics chair in higher education, will they be described as physicists here.

When the period of this study opens, the two professors of physics at the Paris Faculty were C.M. Despretz (1791-1863) and J.M.C. Duhamel (1797-1872). Both men were then in their fifties, both had a substantial amounts of published research behind them, and both were members of the physics section of the Academy. We have already encountered Duhamel when the Polytechnique was considered, and where, in spite of the criticisms of the Leverrier commission which resulted in his being removed as director of studies, he continued to be the professor of analysis. Although his principal interest was in the mathematical, rather than the experimental aspects of physics, he did experimental work on the analysis of sounds, and his 'vibrascope', a rotating cylinder on which was traced a line produced by a thin bristle attached to the prong of a tuning fork, is to be found in physics textbooks of the later nineteenth century.

By the 1850's, Duhamel's experimental research in physics had come to an end but he continued to publish on mathematical and theoretical aspects. Continuing the work of Fourier, with whom he had worked earlier in the century, he had also published treatments of the conduction of heat through solids, but in the 1860's he turned his attention to a re-examination of the formula for the velocity of sound in air. In the first of these memoirs, published in the Comptes rendus of the summer of 1862,[59] Duhamel, starting from the equations given by Poisson for waves in solids, derived an expression for the velocity of sound in air, almost numerically identical to the one normally in use (containing Laplace's correction), but not supposing any elevation in the temperature of the gas. In Duhamel's isothermal treatment it was necessary to multiply Newton's value by the constant $\sqrt{7/5}$

while in the Poisson/Laplace adiabatic treatment, Newton's value must be multiplied by the square root of the ratio of the principal specific heats of the gas. As the ratio C_p/C_v for air is very close to 1.40, one sees that the two answers gave the same result, although starting from very different theoretical bases.

Unfortunately for Duhamel, and to his great embarrassment, he had to report to another meeting of the Aademy later in the year, that he had made a fundemental error in his calculations because he had used the wrong formula of Poisson.[60] This error had already been spotted by a number of foreign savants, including Clausius, and they communicated their corrections to the Academy. While accepting the correction, Duhamel returned to the question, again using Poisson's solid body treatment, but this time accepting adiabatic changes in the gas, and arrived at an expression for the velocity of sound, different from the one in general use.[61]

Taking the velocity of sound as 330 m/s at 0°C, Duhamel's formula gave a value of C_v/C_p of 1.684, as against Laplace's value of 1.42. Thus there had to be a flaw in the reasoning of Duhamel because the value of C_v/C_p had been calculated as being close to 1.4 in the studies of Regnault as well as in the much earlier ones of Clement and Desormes. Duhamel was, of course, aware of this discrepancy, but he had confidence in his own method and left it to later physicists to, 'decide which (value) is closer to the truth'.[62] This work by one of France's most eminent mathematical physicists gives the impression of a certain eccentricity, a certain obstinacy in the face of experimental refutation. Certainly it contributed little to contemporary understanding of the problem, and may even have damaged the reputation of French science abroad.

The other physics professor, Despretz, who had once been the assistant of Gay-Lussac, was almost at the end of his creative life by the 1850's, with an extensive body of published work behind him. He had worked on the thermal conductivity of water and had determined in a very precise way, the variation of density of water with temperature, finding that it had its maximum density at 4°C. But in the later 1840's, he turned his attention to electrical research. Around the middle of the century he thoroughly investigated the performance of the two liquid cell, and his results were published in nine papers in the Comptes rendus. Much of this work appears to verify, confirm, or supply extra experimental evidence for earlier ideas. It is true, however, that there was still considerable confusion about electric cells, their electro-motive force (a term introduced by Ohm earlier in the century), and their internal resistance, and the new two liquid cells, of Daniel, Grove, Bunsen and others, had never been systematically studied before.

Despretz, in the sixth memoir on the cells, examined the spectra of electric sparks obtained by putting together a very large number (100 to 600) of electric cell elements, and compared these spectra with those produced by electro-static and electro-magnetic machines.[63] In the final one,[64] he sought to verify more rigorously than had been done previously, the earlier laws of Ohm and Pouillet, which were being contested by various researchers. The formula which he set out to confirm, was the following:

$$I=\frac{nE}{nR+L}$$

n was the number of cells
R the internal resistance
L the external load resistance
E the electromotive force of the cells

From his research, Despretz was able to say that the internal resistance was not a constant, but varied with the length of wire in the external circuit. He gave the reason for this as being the build up of a paste of zinc sulphate on the zinc plate.

Although it might be said that the electrical work of Despretz did not contribute anything particularly new to the science of electricity, it established, by the rigour of its experimental method and the authority of the Academy, certain norms and imposed a certain order in a sector of physics still beset by vague, imprecise conceptions and lacking a uniformly agreed set of standards and units. For it must be stressed that research into different aspects of electricity was well established by the middle years of the century. Taking the index of the *Comptes rendus*, 1851-65, as a guide to the popularity of this field, it can be seen that it consistently, year after year, attracted more contributions than any other branch of physics research. It should be said, of course, that electricity presented a broad field, offering many research topics, in electro-chemistry, electrolysis, electro-magnetism as well as in the design of instruments,electric motors,and generators etc. It is also possible that electricity improved the possibilities of research for the amateur, the instrument maker, and the *lycée* teacher with only limited means, because even though the cost of producing electricity for industrial purposes, was prohibitively high using cells, they could cheaply provide the small amounts of current needed for many types of laboratory experiment. Equipped with a cell, various lengths of wire of different material and diameter (which he would probably have to insulate himself with varnish or cotton), a compass needle, a few magnets, and some chemical solutions, a whole field of electrical, thermo-electric, electro-magnetic, and electro-chemical research was open to the enterprising man with a modest laboratory and a smattering of theory. This would go some way to explain, not only the very large number of electrical papers submitted to the *Comptes rendus*, but also the very high proportion of papers, whose reception was noted by the Academy, but which never appeared in print. Members of the Academy may well have felt that they had to guard, jealously, the integrity of science against a flood of papers, many of which may have been inexact experimentally, excessively speculative, or based on incorrect theoretical foundations.

Electrical research work rested firmly on old established bases. In France these foundations were provided by the much earlier research of Coulomb in electrostatics, Ampère's explanation of magnetism as the result of molecular electric currents, and Biot and Savart's research on the magnetic effect of a current flowing in a straight wire; work which was over 30 years old by mid-century. Other French savants had made contributions to the field in the intervening period; Arago's chance discovery of eddy currents, Pouillet's invention of the tangent galvanometer, or A.C. Becquerel's work on cells, are a few examples. The most prolific researchers by far in the field, were Th. DuMoncel (1821-1884), who had no formal education in the subject, and the previously considered polytechnicien, Gaugain, who between them, contributed nearly 15% of the 500 or so papers on electricity which appeared in the index of the Comptes rendus in this period, although not all of them were published.

In the early 1850's Duhamel was replaced by Q.P. Desains (1817-1882), while the chair of Despretz passed on his death in 1863, first to Verdet and then in 1866, to Jamin. Desains and Jamin saw the Faculty through the upheaval of the Franco-Prussian war and the Commune into the Third Republic. The loss to French science caused by the death of Verdet must again be stressed here. He was one of the few French physicists who was keeping abreast of foreign developments in the new areas of physics, particularly thermodynamics, and he incorporated this new knowledge into his lectures at the Faculty.

Desains, on the other hand, showed no such interest in the new ideas relating to the dynamic theory of heat. His own research had been centred around the study of 'radiant heat'; what would now be called infra-red radiation. This work was carried out in collaboration with a fellow lycée teacher, F.H.de la Provostaye (1817-1864), during the 1840's. By the end of the decade, both men had gained their doctorates after leaving the Ecole Normale, and after considerable work together had been able to conclude that 'The laws of radiated heat are precisely those of light'.[65] In 1851 they continued their work together with an investigation of the degree of polarisation of heat and light emitted from incandescent platinum using a method similar to that which had been developed many years previously by Arago.[66] Polarisation was achieved by the refraction of the beam by a pile of mica plates set at an angle to the incident radiation. In the following year they published a memoir on the diffuse reflection of heat from a number of matt and granular surfaces.[67] Later in the same year they published a note on the quality of the heat emitted by different bodies at the same temperature, concluding that different bodies emit very different kinds of'obscure heat' at the same temperature.[68]

This work was in the tradition of the research instituted in France in the 1830's by the Italian emigré Macedonio Melloni, although in the 1850's certain differences of interpretation surfaced between the older savant, now with a state scientific post in Naples, and the French researchers. The argument between them continued with increasing acerbity throughout 1853, and only ended in the following year with Melloni's death.

The following year also saw the end of the period of fruitful collaboration between Provostaye and Desains which had lasted more than a decade. Provostaye, in the hope of improving his failing health, left Paris to become Inspector General of higher education in Algeria, and died there in 1864.[69] Desains did no original research in the latter part of this decade, probably as a result of taking up employment in the Paris Observatory, as well as teaching at the Paris Faculty. He only began to publish the results of research again after he had resigned from the Observatory in 1862, returning to his study of radiant heat. Now he investigated the rotation of the plane of polarised heat when it was passed through quartz, using the observations to determine the wavelength of the heat.

In the same memoir Desains reported that using a diffraction grating and a very sensitive thermopile developed by the Paris instrument maker, Ruhmkorff, he had found no heat in the dark Fraunhofer lines of the solar spectrum. In the following year, 1867, he carried out experiments on the absorption of obscure heat by chloroform, benzene, and glycerine, finding the first two of these substances, very permeable.[70] In the last few years of the Second Empire, Desains, with his assistant Edouard Branly (whose career in the Catholic Institute of Paris in the Third Republic will be considered later) began a different line of research, measuring and comparing the intensity of solar radiation at different altitudes, and investigating the effect of water vapour in the air.[71]

We have already looked at Jamin's extensive optical research when we considered the _Ecole Polytechnique._ In the 1860's his interests began to turn towards other areas, but his research continued to be extensive, and the papers he published in the _Comptes rendus,_ prolific. Even in the late 1870's, when he was in his sixties, he published some 30 papers on a variety of topics; magnetism predominated, but there were also some on liquefaction of gases and critical temperature phenomena. Jamin was elected to the physics section of the Academy in 1868, taking the place left vacant by Pouillet, and later served as the Permanent Secretary of the Mathematical Sciences section of the Academy. Desains had to wait until the death of Babinet in 1872 before he found a seat in the physics section. Desains published more slowly than Jamin and never strayed from his first love, radiant heat, and on his death in 1885 it was said of him that 'there was not a corner of the relation between heat and light which he had not explored with success'.[72]

Unfortunately for his students and for French science, Desains' love of radiant heat appeared to make him blind to all the other developments in physics. For example, Fox in a recent study, reported that Desains expressed amazement on hearing of Kirchhoff's two laws of electrical circuits, even though they had been published in the Annalen de physik thirty five years before.[73] Students who attended Jamin's and Desains' lectures for the licence criticised both their form and content, and it is clear that during the 15 or more years that both men remained at the Faculty, the theoretical content of their lectures remained virtually unchanged.

On the other hand they have been praised for the work which they did in organising the laboratories of the Paris Faculty following the establishment of the Ecole Pratique des Hautes Etudes. Desains was responsible for the teaching laboratory which was in operation by December 1868. A French writer of the early twentieth century says of the laboratory that;

> ' Under the remarkable direction of Desains, the laboratory quickly acquired a great fame. Its reputation can only be compared to that which Regnault's enjoyed thirty years before at the Collège de France.[74]

Desains supervised some forty students, who watched certain experiments being carried out, repeated them to familiarise themselves with equipment and its use, but did no specialised research.[75] As we have seen, Branly did research in Desains laboratory, but he was not a student, but chef de travaux. Post licence students working for their doctorate, of which there were very few, would have done their research in Jamin's laboratory.

It is interesting to note that the laboratories of Jamin and Desains came into being within the period 1866-1873, a period during which it has been pointed out, eight laboratories for teaching and research were established in British scientific education.[76] Thus the French reforms may perhaps best be seen as part of a more general European process, where the importance of the practical, manipulative, aspect of physics for teachers of science, was beginning to be recognised.

But it cannot be said that Jamin played much of a role in stimulating and guiding young research talent; there is nothing which remotely resembles a research school associated with him. One can find some joint papers which both of these teachers published with co-authors, who presumably were their students. In the 1870's, Jamin published papers with five collaborators, none of whom attained any distinction in physics and only one of them, Gustave Roger (b.1843)- presented research for a doctorate. Roger was Jamin's most productive collaborator and together they published five papers on magneto-electric machines and electrical induction. Roger's doctoral thesis, defended in 1872 'The study of interrupted currents' was clearly related to the work he was doing with Jamin, and so we can assume some kind of supervision, guidance and communication from the teacher to the student here. Paul makes the point that the Paris 'Faculty was turning out an average of one physicist a year in the 1870's'[77], presumably meaning that by this that one doctoral candidate in a subject area which was described as physics, successfully defended his thesis each year.

We will leave the consideration of doctorates at the Paris Faculty in the 1870's to a later chapter, below are two tables 2(i) and 2 (ii) which relate to the Second Empire. They are taken from the catalogue of theses of the period,[78] and their classification into categories has been done by the writer of this thesis. The categories signify; light, heat, electricity and magnetism, vibrations and waves, and general physics. These categories are then combined in the physics total column. The other columns are for ; astronomy, mathematics, meteorology and *physique du globe*, natural, chemical and medical science theses. The final two columns show the total number of theses presented in that year and the total number of candidates, the two figures not always being the same as some candidates would present two theses and some (increasingly as time went on) only one. The purpose of the tables is to show the popularity of physics topics relative to other branches of science, and to indicate what were the most popular topics within the physics discipline. Physics has the content it had in the textbooks of Lamé and Péclet in the 1840's and also in the more modern (i.e. 1860's textbook of Ganot).[79] It is true that some theses in the 'meteorology and *physique du globe*' section might happily be accommodated in the 'physics' total, but others were more descriptive; as of geological strata, for example.

It will be seen that the percentage of physics theses fell slightly from about 26% in the first decade, to about 21% in the second. The second period also shows the growing specialisation in the optical and electrical area, these areas increasing from 56% of the total physics contribution, to 73%. It would seem that the possibility of original research in acoustics, was considered to have been exahausted by the latter period.

There were, however, two *normaliens* who made successful careers as physics professors in provincial faculties on the basis of doctorates

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Tables 2(i) and 2 (ii) overleaf.

Doctoral theses submitted to the Paris Faculty of Sciences

Table 2 (i).

Doctoral theses submitted in the decade 1850-1859.

	Category										Total	
Year	Lght	Ht	El mag	Vibs. and Waves	Gen. Phys.	Phys Total	Ast	Maths	Met Phs Globe	Nat Chem Med	Theses	Men
1850	1		1	1		3				5	8	4
1851						0		2		4	6	3
1852	4		2			6	3	4	1	6	20	10
1853	1	1	2		1	5	2	2		4	13	8
1854	2	1		1	3	7	1	5	1	2	16	10
1855		1	1		1	3	4	4	1	6	18	11
1856			1		1	2	1	5		2	10	7
1857	1					1	2	2	2	4	11	8
1858	1		1		1	3		5	1	3	12	8
1859				1	1	2	1	4		3	10	7
Total	10	3	8	3	8	32	14	33	6	39	124	76

Physics papers as percentage of total 25.8%

Of the Physics theses,31% light, 25% electricity,
25% general physics, 9% heat,
9% vibrations and waves.

Table 2 (ii). Theses submitted in the eleven year period 1860-1870

	Category										Total	
Year	Lght	Ht	El mag	Vibs. and Waves	Gen. Phys.	Phys Total	Ast	Maths	Met Phs Globe	Nat Chem Med	Theses	Men
1860		1	2			3			1	3	7	7
1861	2		2			4		4	1	3	12	9
1862					1	1	2	2		4	9	7
1863			1			1		1		3	5	5
1864	3					3		5	2	7	17	13
1865						0	1	3		9	13	12
1866	1				1	2		2	1	3	8	7
1867	3	1	1		2	7		2	2	1	12	11
1868						0	2	5	1	5	13	11
1869	1		1			2	1		2	8	13	11
1870		1	2			3		3		10	16	11
Total	10	3	9	0	4	26	6	27	10	56	125	104

Physics papers as percentage of total; 20.8%

Of the physics papers; 38% light, 35% electricity,
15% general physics, 11% heat,
0% vibrations and waves.

and later research on acoustics; J.A. Lissajous (1822-1880), who defended his thesis on the position of nodes in a transversally vibrating plate at the Paris Faculty in 1850, and Alfred Terquem (1831-1887), whose thesis on longitudinal waves in plates was defended nine years later in Paris. Most of the career of Lissajous was spent in Besançon, and that of Terquem in Lille, and both eventually became corresponding members of the Academy.

The work for which Lissajous is remembered, the displacement pattern traced out by a body impressed by two vibrations acting at right angles to each other,'Lissajous figures', was completed in 1857.[80] Lissajous obtained these patterns by attaching small mirrors to the prongs of two tuning forks whose axes were perpendicular, and by directing a narrow beam of light onto one mirror, so that it was reflected from there to the mirror of the second fork. The beam, having undergone two reflections, was then displayed as a spot of light on a screen, and the spot described circular, or elliptical or more complex patterns, depending upon the frequency of one fork relative to the other. It is an excellent demonstration technique for a science teacher, for the resulting light pattern, displayed on a screen, can clarify to students a phenomenon which otherwise would be difficult to explain.

It is obviously not possible to examine in detail all the work done for the physics doctorate during the Second Empire. Mascart's thesis has already been considered; his line of spectroscopic research using diffraction gratings was to win him Academy prizes and lead to his becoming Regnault's assistant at the Collège de France in 1868, and to the chair of physics there when Regnault retired in 1872. Another example of a doctoral thesis which was considered of sufficient merit to be published in the Comptes rendus, was that on the measurement of electro-motive forces (*forces électromotrices*) of cells by Jules Regnauld (1820-1895), who later became a professor at the *Ecole de Pharmacie* in Paris.[81] This work, for which he was to earn his doctorate in 1855, compared the EMFs of cells by a method which today would be called a potentiometer null deflection method. He explained that two cells connected in opposition would give a current I given by the formula

$$\frac{e-e'}{r+r'}$$

Thus if e was equal to e' no current would flow whatever the value of r and r'.

With a number of cells this expression would become

$$\frac{\Sigma e-\Sigma e'}{\Sigma r+\Sigma r'}$$

Using a sensitive galvanometer, Regnauld could easily see when the EMFs of the cells he was employing balanced each other; the galvanometer would read zero. Regnauld compared the EMF's of several cells, balancing each one separately against a thermopile of 60 thermocouples in boiling water, and by adding or subtracting a thermocouple he could vary the comparison EMF by a small amount. So as not to have to employ an excessively large number of thermocouples, Regnauld made himself a zinc-cadmium cell which was equivalent to 55 bismuth and copper thermocouples with their hot junction in boiling water. Using this method Regnauld found that;

A Daniel cell was equivalent to 165 couples.

A Grove cell was equivalent to 310 couples.

A Zinc amalgam/lead peroxide cell was equivalent to 466 couples.

It must be remembered that there was as yet no unit for electromotive force, but Regnauld's method allowed for the first time, the precise measurement of EMF in the necessary condition that no current is drawn from the cell.

The last doctoral thesis of this period which we shall consider is the one submitted by Jules Violle (1841-1923) in 1870. Violle was a normalien, who passed his agrégation in 1868, and after his doctorate, worked in the faculties of Grenoble and Lyons before returning to Paris in the 1880's to become a maître de conférence at the Normale, and a member of the physics section of the Academy. It is interesting because it was the first doctoral thesis to be concerned with the mechanical equivalent of heat, the precise numerical value of which, had been disputed by some French scientists in the 1850's, but which by 1870 was settling at a value close to 425 kilogrammetres per calorie. The kilogrammetre was defined as the work done when one kilogram falls through one metre.

Violle rotated a copper disc between the poles of an electromagnet, and measured calorimetrically the heat generated by eddy currents ('Foucault currents' as they were known in France) set up in the disc.[82] The disc is rapidly rotated using a hand operated system working through a train of gears. The copper disc is attached to its steel axle by a rubber mounting for thermal insulation. When an observation of the heat gained by the copper disc was to be taken, it was removed quickly from its mounting and plunged into a calorimeter of water, and the temperature rise of the mixture read with a thermometer which could read to 1/200 of a celsius degree. Correction was made for the cooling which would inevitably take pace as the disc spun at its high speed. To give an example of Violle's data; turning the handle at 8 revolutions per minute, which produced a rotation of 1224.2 revs. per minute of the copper disc, gave an uncorrected temperature rise of 1.22°C in 15 minutes, and after applying the cooling correction this rose to 1.269°C. Then, Violle wrapped a fine thread around the axle of the machine and rotated the disc using a falling weight. Thus the amount of mechanical power needed to achieve different constant rates of rotation of the disc was determined. From the description of the experiment, one feels a little surprised at the accuracy of the final result, for Violle obtained 435.2 kilogrammetres per calorie.

It is a good example of the doctoral thesis in physics of this period; those charged with judging its accuracy could do so by reference to the value of the equivalence, which by 1870 has been determined by many experimenters. The thesis contained nothing speculative, and its originality lay in its linking of two phenomena, eddy currents and temperature rise, which hitherto had not been investigated.

e. Physics in the provincial faculties.

From what has already been said about the role of the provincial faculties in the first half of the century, it would not be expected that they would make any significant contribution to new knowledge in physics during the Second Empire. In general this is true; the picture of the *lycée* teacher who does some research for his doctorate in order to win the prize of a faculty chair, and then spends the rest of his career grading *baccalauréat* candidates, still applied in the 1850's and 1860's as it did thirty years before. But it was no longer universally true and it would become progressively less true as time went on. Although there would be no other geographical centre which would seriously challenge Paris, no competing centres of scientific excellence as in Germany or Britain, the faculties in certain provincial cities would begin to make significant contributions to physics, and their professors would attain the limited distinction of becoming corresponding members of the Academy of Sciences.

An examination of the list of provincial faculties in the *Almanach Imperial* of 1857, shows that there were fifteen professors of physics in that year. Identifying the scientific work which they did up to the end of the Second Empire from the index volumes of the *Comptes rendus*, we find that five never published anything (at least in this journal , they might have published in local scientific journals), five submitted less than five papers, while the remaining five submitted between five and nineteen papers. Of these professors, one, J.J.B. Abria (1811-1892) of Bordeaux, published twelve papers on meteorology and optics up to 1865, and four afterwards, before being elected as a corresponding member of the physics section of the Academy in 1880. F.Bernard (1816-1866) of Clermont, published seven papers on optical and spectroscopic questions and became a corresponding member of the mechanics section in 1866. Bertin-Mourot , who we have already encountered as a teacher in the *Ecole Normale*, published eight papers on optics and magnetic optical rotation while he was at the Faculty of Strasbourg. The fourth academic, J.F.A. Morren (1804-1870), at Marseille submitted nineteen papers to the Academy on a variety of topics ranging from astronomy, through phosphorescence, and pneumatic machines to the 'absorption of azote by animalcules and algae'.[83] The fifth individual, H.S. Viard (1821-1858) from Montpellier, contributed five papers.

We can also see from this survey that the conception of what constituted physics research was broader in the provinces, the work less specialised in general. Certainly a provincial physics teacher would be unlikely to have the expensive, precise, apparatus which Parisian savants might be employing for optical and magneto-optical research, for example, and he would therefore have to find other, cheaper fields of research. Moreover, a physics teacher who was content to stay in the provinces, would often prefer to publish locally, and because of this would tend to write on accessible topics. This was certainly the case with Abria who had many more papers published in the *Mémoires de L'Académie des Sciences de Bordeaux* than in Parisian journals.

An indicator of the role which the provincial faculties played in the production of new knowledge, is the number of doctorates they awarded. It is a very imperfect indicator to be sure, because it may well be that many of the doctoral theses had little real value, and even if they had, the role of the staff of the local faculty could have been minimal in their production. But nevertheless, if a faculty has been awarding doctorates regularly, year after year, even on a modest scale, it gives the impression that there is, in the faculty, staff who are encouraging a research spirit among the local lycée teachers, state engineers, pharmacists, and others who have both the time and the money to spend on research. In the table below, the number of doctorates awarded from the date of establishment of the faculty, to 1851, is compared with the numbers awarded within the life of the Second Empire (1852-1870). As we are here concerned particularly with physics, the table on the next page,(table 2 iii), shows both the total number of doctorates and those which fall (sometimes not very easily) within the area of physics.

The first point to be made from the information of the table relates to the small proportion of physics doctorates awarded. This is to be expected when the growing expense and complexity of physics apparatus is considered; a mathematician or mathematical astronomer would not have this problem and nor would a scientist researching local geological strata, or distribution of flora and fauna. Nevertheless, the three most productive provincial faculties in all doctorates up to 1851, Strasbourg, Montpellier,and Toulouse, are also the most productive in physics, although Montpellier shows a much higher proportion of physics doctorates. In the Second Empire there is no upsurge of productivity; Strasbourg and Montpellier continue in a way similar to before, while Toulouse declines quite markedly. Lyons becomes considerably more productive, but the most striking aspect

Table 2(iii).

Table of doctoral theses presented at provincial faculties of science up to and including 1870.[84]

Faculty	Foundation date	Doctoral candidates Until 1851		Doctoral candidates 1852-1870	
		Total	Physics	Total	Physics
Besançon	1808 and 1845*	2	1	4	1
Bordeaux	1838	1	0	3	1
Caen	1808	3	1	2	1
Clermont	1854	-	-	1	1
Dijon	1808	7	2	2	2
Grenoble	1811	9	2	0	0
Lille	1854	-	-	2	0
Lyons	1808 and 1833*	3	1	6	0
Marseille	1854	-	-	2	0
Montpellier	1808	20	6	8	2
Nancy	1854	-	-	7	2
Poitiers	1854	-	-	1	0
Rennes	1840	1	0	0	0
Strasbourg	1809	37	7	15	1
Toulouse	1808	10	3	3	0

*These two faculties were closed in 1815 and later reopened.

The figures refer to doctoral candidates not doctoral theses. Some of the candidates would submit two theses.

is the vitality of one of the new faculties, Nancy, while the other four, Marseille, Poitiers, Lille, and Clermont Ferrand, show very little activity. It is not possible to draw firm conclusions about any rise or decline in productivity from these figures because they are so small anyway.

Few of those who took their doctorates in provincial faculties rose to positions of great eminence in French science; such positions were the prerogative of those who had studied in the grandes écoles and submitted their doctoral theses to the Paris Faculty. Even fewer were those, who pursuing academic careers, ever succeeded in obtaining a position in higher education in Paris on the strength of their provincial doctorates: Bravais the polytechnicien, was awarded his doctorate by the Lyons faculty, and returned to Paris, as we have seen, to teach physics at his old school. A normalien, E.H. Marié-Davy (1820-1897) who gained both doctorates in medicine and in physics in Montpellier, left a faculty position there to go to teach in a Paris lycée, and went on to organise the French storm warning system in the Paris Observatory under Leverrier. Marié-Davy carried out research on electric motors in 1861, deriving a mathematical expression for the self-induction of a motor coil moving in a magnetic field,[85] as well as producing a type of two-liquid cell which was employed in the French telegraph service for many years before it was replaced by the Leclanché cell. But a man who gained a doctorate from a provincial faculty , was unlikely to gain a chair in Paris, the best he could hope for was to become a professor at that faculty, like A.P.P. Crova (1833-1907) at Montpellier, whose work we will examine later, but even these positions were usually filled by those whose doctorates had been awarded in Paris.

We will pay more attention to the provincial faculties when we go on to the Third Republic, as, benefitting from the founding of the Société de Physique and the Association Francais pour l'avancement des *sciences*, in addition to the increased encouragement of the state and the generosity of local industries, they began to make a bigger contribution to scientific research in the country.

f. The Collège de France and the Muséum d'Histoire Naturelle

These two institutions, whose origins could be traced far back into the *ancien régime*, had very high prestige in the French academic system. The *Collège* awarded no degrees or diplomas and its lectures were free and open to the general public, although, at the beginning of the century, this did not prevent these lectures being more advanced than those given at the Paris Faculty. The *Collège* had both a chair in experimental physics and one in mathematical physics, the former being held by H.V. Regnault (1810-1878) and the latter by J.B. Biot (1774-1862), when the period under consideration here, opens.

Regnault was a *polytechnicien* and a member of the elite *Corps des Mines*. After some professional work with the corps and study abroad with Liebig at Giessen, and then a junior post at the faculty of Lyons, he succeeded Gay-Lussac to the chemistry chair at the P*olytechnique* in 1840. In the following year, he also became professor of experimental physics at the *Collège*, where the precision of his experimental research achieved such distinction that foreign scientists like the young William Thomson

(contd. overleaf)

came to his laboratory to learn experimental physics in this decade. By the time the Second Empire came into being, much of the physics work for which Regnault is remembered, the precise determination of the expansion coefficient of gases, the deviations from Boyle's law, and the demonstration of the approximate nature of Dulong and Petit's law of 1819 on the constancy of the product of the specific heat and atomic weight of a substance, had already been completed. As the Imperial regime appointed him director of the porcelain factory at Sèvres in 1854, on top of his other duties, it is not surprising that his research activity slackened in the late 1850's and 60's.

But we can take one research project of his, later in the period as a kind of case study to show the type of research at which Regnault excelled. This was the experiment to determine the velocity of sound in air whose results were published in 1868 and for which Regnault had the benefit of the use of new gas conduits and sewage pipes being laid under a Paris undergoing major reconstruction to make it worthy of its status as an Imperial capital.[86] Regnault's objective in carrying out the experiments, was to check the validity of the mathematical theory, which as he pointed out, applied only to a perfect gas. In fact a number of important conclusions emerged from the experiments. Pipes of three different diameters were used; the gas conduit of Ivry with a diameter of 0.108 metres, a conduit presumably for water, with a diameter of 0.30 metres, and the St Michel sewer, whose diameter was 1.10 metres. Regnault first of all found the distance, in each of the tubes, over which it was possible to detect a sound produced by the same source, a pistol charged with 1 gram of powder. He sealed up the distant extremity of each tube with pieces of sheet iron, and detected with a membrane, the return of the wave after a number of reflections up and down the pipe. He found that a wave produced by 1 gram of powder impressed its last mark on the membrane when it had run;

4056 metres in the 0.108 m. conduit
11430 metres in the 0.300 m. conduit
19851 metres in the 1.100 m. conduit.[87]

Beginning with the smallest pipe, Regnault found that the speed of the wave diminished steadily as it was timed over a longer and longer distance. For example over 566.74 metres it was 330.99 m/s, falling to 327.52 m/s over a distance of 2,833.70 metres, and the same was true with the larger pipes but to a lesser extent. A greater charge of powder produced a wave which, over the initial stages at least, travelled faster than that produced by a smaller charge. Thus Regnault could conclude firstly, that the walls of the pipe exerted an influence on the wave inside it, which slowed down due to the friction with the wall, and secondly, that a strong wave (what would now be called a shock wave) has a higher velocity of propagation, at least in the first part of its passage. The mean value of propagation, taken in the largest diameter tube, gave a value of 330.6 m/s which agreed well with that which Le Roux of the Conservatoire had obtained in the previous year with a much smaller pipe.

But Regnault had not yet exhausted the possibilities of the underground conduits of Paris. Using the pipe of 0.108 metres diameter he filled its length (around 600 metres) with, successively, hydrogen, carbon dioxide, and the gas used for lighting (town gas). In a second series of experiments he used another pipe of the same section but only about 70 metres in length, which was in the Collège de France, filling this pipe with, in turn, hydrogen, carbon dioxide, ammonia, and nitrous oxide. He determined the velocity of sound through these different gases in order to verify another theoretical formula relating to perfect gases; that the velocity of sound in a gas is inversely proportional to the square root of its density.

Regnault was justifiably pleased with the agreement between the velocities and the values of the density. The coincidence he said was;

> 'Quite remarkable; the difference would have been smaller if it had been possible to operate with very pure gases, but this is difficult with conduits of such large capacity'.[88]

This series of experiments still remain today the model of experimental technique for the velocity of sound. It is very much in the tradition of Regnault's work on the specific heat capacity of materials; precise, painstaking and rigorous, but firmly grounded on earlier established theory. In fact Regnault claimed that his principal interest in taking up the study, was because of its relation to the mechanical theory of heat. Although he believed he would be able to derive some important consequences for the theory from this work, he had no space to develop the ideas in his memoir, and does not seem to have come back to it later.[89]

Regnault was assisted by Mascart from 1868,who succeeded to his chair when Regnault retired in 1872. Mascart's research has already been referred to and will be examined again in the chapter on the Third Republic. Towards the end of the century The Collège was to become much more important in the formation of new research workers in physics as the reforms of the Ecole Pratique des hautes etudes, allowed more young normaliens to take up positions of agrégés-préparateurs and do reseach for their doctorate.

In relation to the research work of the other chairholder of the Collège, Biot, we can say that it was virtually at an end by the beginning of our period. Biot had had an enormously productive research career, which it is not possible to give justice to in a few lines. As a protégé of Laplace in the First Empire he had been a partisan of the emission theory of light and had later adopted an agnostic position in relation to the wave theory. In 1820 he experimentally determined (with his assistant at the Collège, F.Savart) the factors affecting the magnetic field around a current carrying conductor. His later work investigated the ability of certain organic solutions to rotate the plane of polarised light, and in this he became the mentor of Pasteur. Nearly eighty years of age by 1850 he intervened in discussions in the Academy on the work of other scientists and served on Academy commissions to advise the government on scientific matters, as in the discussion over the establishment of meteorological stations in Algeria . Biot's successor, J. Bertrand (1822-1900) gained a reputation during hs lifetime as a teacher and a writer of textbooks on electromagnetism, thermodynamics and mathematics which were published later in the century.

The Muséum d'Histoire Naturelle was, in the early part of the nineteenth century, the 'internationally recognised centre for research in natural history'.[90] In 1832 it had thirteen chairs, including one in general chemistry, one in applied chemistry and one in mineralogy, and in 1838 another chair was added, that of physics applied to the natural sciences. This chair was held by A. C. Becquerel (1788-1878), and remained in the Becquerel family throughout the period of this study. A.C. Becquerel had received his scientific training in the Ecole Polytechnique, had been elected to the physics section of the Academy in 1829, and by mid-century had published close to one hundred research papers. The founding of the chair was part of a process in the period 1837-38, in which five new chairs were founded, each in an experimental field rather than in descriptive natural history.[91]

A brief examination of two reports which Becquerel made to the Academy in 1851 and 1860 will give some impression of the particular type of research he carried out for the Muséum. Not unnaturally he was interested in meteorology and the phenomena associated with atmospheric electricity, because of their influence on the growing conditions of plants, and he developed instruments for measuring atmospheric variables. Becquerel's 1851 paper classified by the editors as electro-physiologie described experiments employing a very sensitive current measuring device, which he had constructed himself and which he called an electro-magnetic balance, to measure the current flowing between two platinum needles inserted in the root, tuber and fruit tissue of various plants. Such experiments had considerable interest at the time because they seemed to point the way to clarifying the mysteries of life and growth, linking electrical activity to living processes. Becquerel would have none of this, making the point that;

> '..;the electrical effects appear to be due, at least in the majority of cases, to simple chemical reactions'.[92]

The chemical reactions take place between the different fluids which are to be found in the tissues and the alterations they undergo when coming into contact with the air or with the platinum needle, and this 'is the principal cause for the liberation of electricity'.[93]

His paper of 1860, classified as physique végétale, examined the temperature inside the trunk of a thick pine tree compared to the outside temperature. The experimental work had been done by others and Becquerel worked on the published results. The fact that the temperature in the pine, lagged behind the outside temperature was not simply a question of the thermal conductivity of the wood, argued Becquerel, because a dead pine gave a lower mean temperature than a living one. It could be explained by the roots drawing up water from the subsoil where the mean temperature is higher than the air in winter, and because of chemical processes going on in vegetable tissues.[94]

Becquerel's research for the Muséum must be viewed in the context of the attempts to raise the status of botany, seen as a low status science with a basis in classification and description, to one which was dedicated to experiment.[95] Thus the work of the physics chair-holder at the Muséum was peripheral to the central preoccupations of the other academic physicists of the capital. The Muséum, offering no degrees, giving lectures to a lay public and therefore having no regular student body, never imprinted any particular traits on a later generation of research workers. It will figure again in this study at the end of the century when Henri Becquerel (1852-1908) using the mineral collection of the Muséum with which he had been working on the study of phosphorescence, made the chance discovery of radioactivity.

g. The institutions of the Ministry of Commerce.

There is no doubt that the need was felt in France to try to bridge the gap between theory and practice, to allow the intelligent artisan the opportunity to learn, without the pressure of examinations or high tutition costs. An institution, under the control of the Ministry of Commerce, the Conservatoire National des Arts et Métiers, which had begun life as a museum of industrial techniques was reorganised in 1817 to provide a type of technical instruction. In 1831 a chair in 'Physics applied to the crafts' was established with C.S.M Pouillet (1790-1868) as professor. When he was dismissed on refusing to take the oath of allegiance to Napoleon III, his place was taken by A.E. Becquerel (1820-1891), the son of the Muséum professor, who held the position until the 1880's. Examination of the lectures on electricity given by Becquerel during the Second Empire,[96] shows that he gave his students a grounding in static electricity, different types of voltaic cell, quantity of, and tension of electricity, and instruments to measure these quantities, resistance of conductors and the effect on intensity, and the chemical effect of a current. As the lectures were related to industry (arts et métiers) Becquerel spent a lot of time on electro-plating and on galvanoplastie, a process in which metal was deposited on a non-metallic base. It is interesting that he compared the cost of obtaining heat from decomposing zinc in a voltaic pile,and burning coal, finding the zinc about 150 times more expensive. This is an example of the general interest at the time, of energy conversion processes, as well as a preoccupation with economy. The classes were non-mathematical, but did touch on theoretical problems; electricity was referred to in Amperean terms as a fluid, and water was said to be decomposed by passing electricity through it, into;

> 'two molecules of hydrogen which gain the negative pole, and one molecule of oxygen which goes to the positive pole.[97]

At this time Edmond Becquerel was working on the mechanism of cells and their electrolytes, and the results of his research frequently appeared in the Comptes rendus. One piece of research published in 1855, showed that it was possible to obtain a 'voltaic couple' from a cell using electrodes of the same material, provided that they were in motion relative to the electrolyte, (or what amounts to the same thing), the electrolyte in motion relative to the electrodes.[98] A metallic cylinder, functioning as the negative electrode, augmented the value of the current when it was set rotating about its axis. Becquerel did not think that he had produced a practical cell in this way, he merely used it as an illustration to show that it was necessary to invoke other principles than those usually employed, when describing the mechanism of the cell.

In another paper the following year, Becquerel went on to make a study of 'polarisation',the build up of a gaseous layer at one of the electrodes, which reduces the ability of the cell to supply a current.[99] As a part of this research, Becquerel set out to measure accurately the electromotive force of the cell. The method which he used for this, involved using what we would now call a current balance. This device, the electro-magnetic balance invented by his father, depended upon the magnetic effect of the current, and allowed the current to be measured in units of gravitational force, weight. Thus it shows that the terminology of electricity was still far from standardised, for as we have already examined with the work of Regnauld, electromotive forces can only be measured when no current is being drawn from the cell, a condition satisfied by Regnauld's 'null-deflection' method, but not by Becquerel's. Certainly, as Becquerel pointed out, his method is very precise over a wide range; it 'can compare directly the action exerted by a thermopile, and that produced by a Bunsen battery with 50 to 60 elements'.[100] But nonethless, it is a sensitive current measuring device, not an instrument to measure E.M.F.

Edmond Becquerel continued to be the foremost research worker in the more theoretical, less applied aspects of electricity. For example, in 1853 he examined the conducting properties of different gases in conditions of high temperature and reduced pressure.[101] At low temperatures he found that;

> 'there was no appreciable difference between a rarified gas and one at ordinary pressure; neither one nor the other, conduct electric currents'.[102]

At high temperature, however, the low pressure gas always conducted better. Becquerel considered that his results were very strange, considered in the terms of contemporary molecular physics. He wondered how it was, that even though electricity was regarded as being conducted by material particles, the conductivity of the hot gas attained its maximum values at the lowest pressures which the vacuum pumps of the time could achieve. The possibility that charge carriers were being emitted from the red hot electrodes did not occur to him at this stage. Six years later, however, in another paper on conduction through gases, using much higher voltages, he posed the question that perhaps the electricity was being transmitted by particles which detached themselves from the electrodes.[103] But he seems to have given little importance to this hypothesis, it does not appear to have guided his later research.

Edmond Becquerel's préparateur at the Conservatoire , R.L.G. Planté (1834-1889) who was working on the action of depolarising agents in cells, published a number of papers on this subject, and in 1860 presented to the Academy, a cell which had the characteristics of what was later, with the advent of practical generators, to become the rechargeable accumulator. At this time, however,the principal importance of Planté's cell, was its very low internal resistance.

One can see at the Conservatoire that there is a link between some of the research carried out there by Becquerel, and the objectives of the course being taught. Electricity was being used in industry in the main for electroplating, and the efficiency of cells and the cost of the electrical power derived from them was of the outmost interest to the artisans and small workshop owners who would be attending the Conservatoire lectures.

The Conservatoire was the only institution in Paris in which research into the mechanical equivalent of heat was being carried out. Perhaps it is not surprising that interest was more marked here than elsewhere, because the Conservatoire had something of a tradition in this area, with its association with Nicholas Clement (d. 1841) and through him, with Sadi Carnot (1796-1832).[104] The work was carried out by two members of staff, C. Laboulaye (1813-1886) editor of the Annales of the Conservatoire, and H. Tresca (1814-1885), who held the mechanics chair there and was later to become a member of the mechanics section of the Academy in 1872. Their work, which they reported to the Academy in 1863,[105] and which was reviewed in another longer memoir by the Academician and director of the Conservatoire, A.J.Morin (1795- 1880),[106] seems to be very much in the style of French physics of the period; thorough, precise, painstaking, but theoretically unadventurous. It has its counterpart in the laboratory determinations of specific heat by Regnault or the optical work of Fizeau and Foucault.

The two research workers, obviously with considerable funds to support their efforts, used a much bigger version of the apparatus first used by Clement and Desormes in their unsuccessful attempt to win the Academy's 1812 competition on the determination of the specific heat of gases. Such apparatus, later in the hands of Gay-Lussac and Welter, was used to determine the ratio of the specific heats of a gas; C_p/C_v. The vessel used by Clement and Desormes in 1812 had a capacity of ten litres, while the one used by the Conservatoire team was no less than 3,000 litres, but apart from this, and the fact that the pressure was recorded automatically on smoked glass, the two experiments were the same. Air was first compressed in the containing vessel and left to take up the temperature of the surroundings. Then, some of the air was allowed to escape through a tap; the tap was then closed and the air was left to again return to ambient temperature. Three pressures were taken; P_0 at the start of the experiment, P_1 at the end of the expansion (while the tap was open), and finally P_2, when the gas had come back to room temperature.

The mechanical equivalent of heat was then calculated in the same way as in Meyer's original experiment. After a large number of trials, repeating the same operation each time with the gas escaping for intervals of between three and five seconds, pressure finally fell to atmospheric. A mean of all their results, still however, only gave approximate agreement with the value of Joule. Laboulaye and Tresca quoted a value of 433 kilogrammetres per calorie as against Joule's value of 425. The reason for this 2% error, in the view of the two experimenters, must lie in the imprecision of the constants used. There is no mention of the fact that some internal work is done in expansion, work against the internal attractive forces of the molecules, as Joule and Thomson had demonstrated.

It might be argued that as this research was done by a professor of mechanics, while the physics professor of the same institution, A.E. Becquerel never showed any interest in it, that it was not even considered at the time, to be physics research. This seems to be confirmed by the fact that the few people around France who worked in this area, were engineers, like Ferdinand Reech of the Naval school or the Alsatian, Gustave Hirn , mathematicians like A.Dupré of the Faculty of Rennes, or chemists like P.A. Favre of the Marseille Faculty. But on the other hand it was Verdet, the physics professor who translated the foreign papers on the dynamic theory of heat for the Annales de chimie et de physique and incorporated these new ideas into his physics lectures at the Faculty. Moreover, Favre put himself forward, albeit unsuccessfully, for a vacant place in the physics section in 1868, presenting his work as falling within the domain of physics.[107] Lastly it must be said that papers to the Comptes rendus, were classified as 'physique' until the more specific title 'thermodynamique' began to appear in the late 1860's.

The other institution in Paris under the responsability of the Ministry of Commerce, which it acquired in 1857, was the Ecole Centrale des Arts et Manufactures. This school, set up as a private venture in 1829, sought to give its students a solid basis of theoretical science and its application to industry. The school's curriculum integrated theory and practice; twelve hours a week were spent in lectures and the rest of the time in the laboratory, making engineering drawings, discussing with the professors and laboratory staff and studying individually. The course was a three year one, and both physique générale and physique industrielle were taught. The school's aim was to train directors of workshops and industries, civil and construction engineers, not scientists. Hence the course of study of the first year gave the students a theoretical knowledge of the four sciences the founder of the school, Theodore Olivier, thought important (geometry, mechanics, physics and chemistry), but with the theory taught with regard to its industrial applications.[108] It has been argued, however, that as the school directed itself towards the sons of the most affluent families, it tended to stress those elements of the curriculum which in France were closely linked to social status; i.e. mathematics.[109] As a result of this, the curriculum began to resemble that of the *Ecole Polytechnique* so closely that an applicant needed two years of extra study after the *lycée* in order to pass the entrance exam. The quality of its graduates was very high, many of them winning design awards in International exhibitions. In 1868, the *Centralien*, G. Leclanché working as an engineer for the Eastern railway produced his two liquid cell which used manganese dioxide as a depolarising agent, and became the standard cell in several national telegraph systems. Later examples of its graduates were Gustave Eiffel and Louis Bleriot.

J.C.E. Péclet (1793-1857), one of the founders of the school, held both physics chairs at first but relinquished the general physics one in 1836, continuing to teach industrial physics until his death in 1857. Regnault held the general physics chair for two years from 1839 tO 1841, but when our period opens the incumbent was A. Masson (1806-1860). Masson was a *normalien*, who completed his physics thesis for the doctorate, then went to work as an assistant professor of physics in a *lycée* in Caen, returning to Paris to complete his chemistry thesis under Dumas at the *Ecole Centrale*. After Masson's death in 1860, the chair passed to the *centralien*, M. Daniel who held it until 1881.

Masson's research work ranged over a number of topics, but about half of his 30 contributions to the *Comptes rendus* related to electric light, heating effect of a current, electrical induction, and electrical photometry. He carried out a series of experiments between 1850 and 1855, observing the spectra of iron, copper, tin, lead, antimony, bismuth, zinc, cadmium and carbon, and showing that some lines were common to several metals. The results were published in the Annales de chimie et de physique, and included drawings of the spectra obtained. Using the metals under test as the electrodes of a spark gap, Masson did not realise that the spectra obtained were the result of the combination of the spectra of the metal and the gas through which the spark passed. This was subsequently pointed out by Angstrom.[110] It would appear that the scientific community did not judge Masson's research to be of the highest importance; his two attempts to gain election to the physics section of the Academy failed in 1851 and 1859.

Masson's successor, Daniel, did not produce much research, but there is a paper of his in the Comptes rendus of 1870, which continued a line of research of Masson. Masson had discharged a Ruhmkorff coil, (an induction coil producing a unidirectional voltage) through a gas at very low pressure in a glass tube, (called, after its original constructor, a Geissler tube). Daniel sent two separate currents through the Geissler tube ; first in the same direction, then in opposite directions and finally at right angles to each other, and observing the effect which an externally applied magnetic field had on the luminous columns of gas.[111] Daniel observed that when the currents were going in opposite directions, the magnet attracted one and repelled the other, and if they passed in the same sense the effect of the magnet on them, was the same. It was not a question of the magnetic or diamagnetic properties of the gas in the tube because oxygen and hydrogen responded in exactly the same way. There seems to be an observational error on Daniel's part because the magnets would have deflected the luminous columns in different senses, but would not have attracted or repelled them. But the Ecole Centrale did not give much importance to research in 'pure' physics, and Daniel does not appear to have continued with this work.

h. The Paris Observatory and the Association Scientifique de France.

The Paris Observatory had never solely occupied itself with routine positional astronomy, and its premises were always used for research in the days when Arago was director. This tradition was to continue under Leverrier. In 1854 E. Liais (1826-1900) was recruited specifically to carry on the collection of geomagnetic data, and he designed three continuously recording instruments which used an optical system in which a light mirror carried on a compass needle or magnetic balance lever, deflected a spot of light across a photographic plate.[112] Liais left in 1858 to become director of the Observatory of Rio-de-Janeiro, and his place was taken first by Desains and then by Marié-Davy. Three years earlier, in the decree of February 1855, the Imperial regime established the post of physicien with a very specific job description. The tasks of the physicist would be to supervise the construction of large lenses, supervise the telegraph apparatus, develop photographic methods for the observation of the sun and the stars, develop remotely and automatically operated instruments, measure the velocity of light, demonstrate the rotation of the earth, and develop ultra sensitive apparatus for the measuring of force.[113] As this list of tasks reads like the curriculum vitae of J.B.L. Foucault (1820-1868), it comes as no surprise that the decree ends with the recommendation to the Emperor that Foucault should be appointed to the post.

Considering the work which Foucault had already accomplished, the contribution which he made to the Observatory was disappointingly small, and to Leverrier, annoyingly so. He did, however, improve his rotating mirror apparatus so that its angular velocity could be held at a known constant value, and used it in 1862 to determine the velocity of light avoiding the very long path lengths which Fizeau had been obliged to use.[114] Foucault obtained a rather lower value than Fizeau had done, but with a higher accuracy and his figure was used by astronomers to revise the value of the solar distance, diminishing it by some four million miles.

Other research, which might be described as lying on the inter-face between physics and astronomy also went on in the Observatory. The work of C.J.A. Wolf (1827-1918) and G.A.P. Rayet (1839-1906) on the spectral analysis of starlight is in this category. Observing a newly discovered star in 1866 through a direct viewing spectroscope attached to the eyepiece of the telescope, they observed;

> '..a complete and very pale spectrum , on which a number of brilliant lines stood out'.[115]

The observed spectrum was seen to have its brightest band in the yellow/green region, but there were also less bright bands present and Wolf and Rayet concluded that the star owed its brightness to the incandescent vapours surrounding it. This type of star, of which some 300 have been observed to the present time, has a temperature of 80,000 K and its spectra is due mainly to hydrogen and helium. They are still known today as Wolf-Rayet stars.

The Observatory will not be considered in later chapters because under Admiral Mouchez, Leverrier's successor in 1878, no research which could be described as physics went on there.

Apart from his role as director of the Paris Observatory, Leverrier was president of the Association Scientifique de France . From the foundation of the Association in 1864, to Leverrier's dismissal from the Observatory in January 1870, the Association could call upon the material, the personnel, and the premises of the Observatory. Indeed, one of the criticisms made of Leverrier was that too much of the Observatory's budget was going to sustain the Association. At its first annual general meeting held in April 1865 it could already claim a membership of 3,500 people.

The aim of the Association was to encourage what it called the physical sciences, although its major preoccupations would have come under the Academy classification of mathematical sciences,i.e. physics, astronomy and meteorology. From 1867 it extended its field of interest to encompass all pure and applied science, and anybody who agreed to pay the modest subscription of 10 francs annually, was entitled to join. Considerable emphasis was placed on the benefits it would bring to the development of science in the provinces, although it must be said that 80% of its founding membership came from the Paris region. Foreign scientists were also welcome to join.

The organising and ruling body of the Association was the conseil, made up of sixty members, of which, one third was renewed each year. The day to day administration was in the hands of the eleven man bureau, whose president was Urbain Leverrier, while the vice-president was Belgrand, a chief engineer in the corps of Pont et Chaussées. The bureau also contained a distinguished mathematician, Serret, another member of the Paris Observatory, Gaillot,the secretary of the French meteorological society, Renou, and Barral the director of the Journal d'agriculture pratique. There were five non-scientists on the bureau, bankers, senators and deputies of the National Assembly. Of the six scientists, four at least were products of the Polytechnique, and only one, Serret, came from the Normale. Normaliens were better represented in the first elected conseil, for there were several professors from provincial faculties, Abria of Bordeaux, Lereboullet of Strasbourg, Morren of Marseille as well as other Parisian savants like Marié-Davy and Wolf, meteorologist and astronomer respectively at the Paris Observatory, and Puiseux, mathematics professor at the Paris Faculty. Leon Foucault also served on the conseil, as did three members of the Academy, including its permanent secretary, Elie de Beaumont. There was also a number of high ranking military and civilian state employees, the director of the state telegraph administration, as well as a representitive of the Education Ministry. Clearly the Association found favour at the highest levels of both the academic establishment and the state.

In March 1865, the publication began, of a small bulletin entitled the Bulletin de L'Association scientifique de France , which carried scientific news and reports of the monthly meetings. It is this Bulletin which has been the major source of information about the work and membership of the Association. The Association planned to distribute 21,000 francs each year, divided equally between meteorology, astronomy and physics, the money coming from its annual subscription income of around 35,000 francs.[116] In fact, in its first year it did not distribute the full amount but kept back 7,000 francs towards the cost of the construction of a large astronomical instrument to be set up in a provincial town. For meteorology, a large sum was put aside for a prize competition calling for an explanation of the mechanism of the general movement of the atmosphere, and various smaller sums were distributed to sea captains, both French and foreign ,for their observation of weather at sea. Admiral E.B. Mouchez (1821-1892), who had been a collaborator in meteorological work with the English Admiral, Fitzroy in the early years of the decade before being sent on a hydrographic surveying voyage to the coast of Brazil, won the Association's gold medal and 300 francs for his weather observations. Mouchez was to succeed Leverrier as director of the Paris Observatory after the latter's death in 1877.

The Versailles *lycee* teacher, A. Cazin (1832-1877), was one of the first to benefit from the fund, receiving 1,000 francs (a not inconsiderable sum compared to the *lycée* teacher's salary of around 4,000 fr), to help in his work on the properties of vapours. Some of his research will be examined later. D. Gernez (1834-1910), at this time a *préparateur* at the *Normale* received 300 francs for a spectroscope and 200 francs for a collection of prisms and crystals,[117] setting him along the road of a successful teaching and research career which, passing through stages which took him to positions in the Paris Observatory, prestigious Paris *lycées*, a chair at the *Ecole Centrale*, and *maître de conférence* at the *Normale* , culminated in his succeeding to Pierre Curie's place in the Academy in the twentieth century. Most of his research was in the area of optics. Another very successful physicist of the later nineteenth century, Mascart (whose doctoral work we have already considered), at this time working in a *lycée* in Metz, also benefitted from the *Association*, receiving 500 francs,which allowed him to buy a spectroscope, some prisms and lenses made of Iceland spar, and a diffraction grating with 600 lines per mm. Among others researchers who benefitted from the generosity of the *Association* were Terquem of the faculty of Lille, and the poverty stricken electrical researcher, Gaugain.

Later in 1865, meetings were held in Marseille and Strasbourg, as well as in the Paris Observatory. In that year there were reports from the recipients of funds from the *Association*, Terquem and Cazin, on the work they were doing. There was a also a report of some important foreign research, the spectral analysis of the sun which Bunsen and Kirchhoff had just completed [118], and a report by Pasteur on the deposits which form in wine.[119] Important technological advances like the laying of the trans-Atlantic telegraph cable, or the one from Sicily to Algeria, also found a place in the meetings. In the following year, 1866, the Association developed rapidly. Its regular monthly meetings were now held in a bigger auditorium in the Conservatoire des Arts et Métiers, while in the provinces, meetings were held in Metz, Bordeaux and Marseille, while Elbeuf, Mulhouse and Cherbourg announced their intention to organise meetings at a later date.[120] Cazin continued to be the most active reporter to the Paris meetings, and he and Terquem continued to receive financial support. Cazin also reported on the joint research on superheated steam which he undertook with the Alsatian engineer, Gustave Hirn. In 1867 two more physics teachers, Bertin who we have already encountered as the teacher of physics at the Normale, and A.P.P. Crova of the faculty of Montpellier, together with three others, also benefitted from its research grants. Its major prize competition in meteorology 'on the general movement of the atmosphere' attracted nine entries but none was deemed to have sufficient merit to earn the prize.[121] In the middle of 1867 it was decided to publish the bulletin with at least eight pages every week, and the following year it was appearing regularly each week with sixteen pages. But now the publishing capacity of the Association exceeded the scientific material it was generating, and in spite of reports from recipients of grants, meteorological reports from the Paris Observatory, and numerous reports of the sighting of new minor planets, much of the bulletin material consisted of extracts and summaries from other scientific journals. The number of articles devoted to medical, agricultural, geological and natural history topics increased faster than those devoted to physics. The funds distributed to physics projects were still, however, quite generous; J.B. Baille (b.1841) of the Polytechnique received 500 francs to aid his proposed work on induction currents, and J.F.L.St. Loup (b.1831) of Strasbourg, 500 francs for his magnetic research.[122] The principal beneficiary of the Assoociation continued to be Cazin, who in this year was awarded 1200 francs to assist his work on the compression and expansion of vapours.[123]

The *Association* appeared, at least from the bulletin, to continue in good health in 1869, although as it did not publish either information about its membership, or a detailed breakdown of its finances, it is difficult to measure its real development. Physicists like Bertin and Terquem, who had benefitted from its generosity, continued to play an active part in its affairs. These two, and J.A. Lissajous , were elected to the directing council in 1869. The sixteen page bulletin continuing to appear each week, published very little original work in physics now. There was a report of the work of L.P. Cailletet (1832-1913) on the compressibilty of gases at high temperatures,[124] and a summary of the contribution of the Peslin to the debate on the laws of the general movement of the atmosphere.[125]

But whatever the state of the *Association* in 1869, it is certainly true to say that criticism of its president, Leverrier and his administration of the Paris Observatory, was growing and the *Association* was involved in this criticism. The astronomer Charles Delaunay (1816-1872), went so far as to accuse Leverrier of neglecting observational astronomy, and of only spending 10% of the Observatory's budget on astronomy, while squandering the rest on his 'parasite' organisations, the meteorological service and the *Association Scientifique*.[126] After the mass resignation of Observatory staff in December 1869, and an attack in the Senate on the Minister of Education, by Leverrier, the ministry had no other alternative but to remove Leverrier from office. Early in 1870 he was replaced as director by his arch-enemy, Delaunay, and although the meteorological service continued to function from the Observatory, the administration and finances of the *Association* were removed.

Although the *Association* continued to function after the Franco-Prussian, war we will not trace its fortunes in the Third Republic. The roots of the organisation were in the Second Empire, its leaders Catholic and politically conservative, and it would make little more contribution to the development of science in France. Besides, there would be other organisations like the Association Française pour l'avancement des sciences and the Société de physique which would play a much more important role.

i.Conclusions on physics research in the Second Empire.

Can it be argued that the different institutions were teaching, and its staff pursuing research in, different types and styles of physics, in the period of the Second Empire ? There can be little doubt that the vocational objectives of the institution played some role here. For example the Polytechnique communicated abstruse truths to engineers who acquired authority by their facility in handling them, and much of the research carried out by its staff continued along the well-trodden path of research based on well established theoretical foundations, more often than not,in optics. The Ecole Normale, conscious of its duty to produce good science teachers, stressed the ability to explain and to make clear to students, by the facile use of apparatus, effects which otherwise would remain complicated and obscure. Of the faculties of science both in the capital and in the provinces,in this period, little can be said; the level of science was low, and outside Paris, the number of serious students few. As for the institutions of the Ministry of Commerce, it was their function to communicate the industrial applications of physics, not to train a new generation of research workers, but one can see in the work of A.E. Becquerel at the Conservatoire, a concern with the efficient application of electricity to industrial processes.

For those who went on to do research, and these were usually the graduates of the Normale or Polytechnique, there were potent factors inhibiting them from straying too far from the well trodden path of non-controversial physics. Wanting to attract the attention of some established member of the community who could serve as a patron to help their career advancement ,(although it is probably true to say that the role of the patron was no longer as important as it was earlier in the century), they would tend to adopt lines of research, which were of interest to their prospective patrons and even a continuation of the work which they had been doing. And, of course, a considerable body of published research was required for successful competition in the contest for the most prestigious posts in the educational system. Such a competition had become especially fierce, as a larger physics community competed for a number of top positions which had remained virtually unchanged over three or four decades.

In the immediately previous period, the third and fourth decades of the century, many young physicists were set on the way to a successful career by Arago, the director of the Paris Observatory. The best graduates of the Ecole Polytechnique, were taken into the Observatory as pupil astronomers, but others like Jamin, Foucault, Fizeau, and Edmond Becquerel were helped with ideas, equipment, and the use of the Observatory as a laboratory. Their choice of research project, therefore naturally tended to reflect the interests of Arago himself. This tended to mean that those people reaching the top positions, and being elected to the physics section of the Academy in the Second Empire, had specialised in the optical tradition of Arago and the Paris Observatory. Such people as Fizeau, Jamin, and (at least in the Second Empire) the astronomer Leverrier, ensured as patrons that this optical tradition would continue. Leverrier, as president of the Association Scientifique de France and as director of the Paris Observatory, could distribute funds and make the Observatory available for research purposes to those who lacked facilities. Thus young research workers like, Mascart, Cazin, Gaugain, and Gernez, received funds, as did the rather older Hirn. More posts in the Observatory, in geomagnetism, meteorology and physical astronomy were founded. Fizeau, too, was to provide apparatus to the next generation of research workers, providing the toothed wheel apparatus on which Cornu was to build his career, thus assuring the continuity of interest in optical research.

It might also be said that the particular political traits of the Second Empire had some effects on the sciences practiced in its institutions. It has been argued that the early authoritarianism of the Second Empire and its close association with the Catholic Church (modified, certainly in its later, more liberal phase) gave it a definite aversion to scientific ideas which could open the doors to a godless materialism which in turn could be subversive of public order.[126] Although this argument refers to the Pasteur-Pouchet debate over the question of spontaneous generation and is therefore within the realm of biology, the argument can perhaps be extended to physics as well. Achille Cazin, writing the preface to his succesful popular treatise on heat, <u>La Chaleur</u> in 1866, is at pains to point out that the study of the mechanical theory of heat does not lead to materialism. Quoting from his collaborator and fellow protégé of Leverrier, Gustave Hirn, he says;

> 'One of our most distinguished savants, Gustave Hirn , has even demonstrated in his <u>Exposition de la théorie mécanique de la chaleur</u>, that the experimental principles on which it is based, have as a rational consequence, neither materialism nor pantheism, but the purest religious feeling (spiritualisme).[127]

Why the mechanical theory of heat could lead to materialism is not explained by Cazin, but it could have been related to the use of the model of randomly moving atoms, a model used by the Greek philosopher Lucretius and some of his predecessors which had been widely discussed and often vilified since the mid-seventeenth century, as being atheistic in all but name.

An example of an accusation of atheism in science came in the journal *Revue Européene* of 1860, where Alfred Sudre had attacked Pouchet as being the unwitting promoter of atheism, and had attacked scientists in general on the grounds that ;

> '..nearly always their observations and experiments are guided by preconceived opinions, systems or philosphical tendencies'.[128]

Thus scientists of the Second Empire felt the need to defend themselves against such accusations of godlessness by insisting that their scientific work was independent of, and separate from their beliefs. The astronomer Faye, presenting to the Academy the book of Gustave Hirn on the mechanical theory of heat in 1868, stressed that scientists;

> '..left metaphysics outside the door of the laboratory when they experimented.'[129]

Pasteur, even though defending the conventional Catholic position on the origin of life, also was at pains to stress that his experiments on spontaneous generation, were executed without any preconcieved notion as to their outcome.

This very defensiveness shows that there were some areas of research in which the savant might feel uncomfortable, and an able and ambitious young physicist, seeking to make his career in a centralised educational system controlled by the state, clearly took this into account in his choice of research topics. We have already noted that thermodynamics was a neglected area in France inspite of Verdet's attempts to educate the scientific community with his continuous stream of translations on the subject which appeared in the Annales de chimie et de physique. Electricity and above all optics remained the surest path to success in physics research.

It might also be said that the Second Empire saw the growth of positivistic tendencies in French science which affected physics. By positivism is meant (put very briefly) the rejection of metaphysics, the eschewing of speculation about underlying causes or mechanisms, and the limiting of scientific activity to the establishment of mathematical relations between phenomena. Positivism also contended that science was the ideal form of knowledge, and this strand of positivism later grew stronger in the secular, anti-clerical Third Republic.

But in the Second Empire, the scientific community still needed to present itself to society as a group which dealt with problems different from those which were rightly the preserve of the church or the state. It also wanted to demonstrate that it dealt with these problems with techniques and methods which were specific to the scientist and hard won through study and practice. Its methods were first and foremost experimental ones as it wrested information from nature in the laboratory, but also mathematical in its treatment of experimental data, seeking to find mathematical relations between phenomena. The scientist claimed to separate values and beliefs, and by eschewing metaphysics, delineating his area of activity, he sought to avoid clashes with the church and state. By emphasing the primacy of his methodology he ensured that those not trained in scientific methods of measurement, experiment and mathematical analysis, were effectively excluded from membership of the scientific community.

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3.DEFEAT AND REVOLUTION 1870-1871

The period from the end of September 1870 to the end of January 1871, a period of extreme hardship, hunger, and cold for the people of Paris, did not bring an end to physics research in the capital or the provinces. Naturally in Paris, scientific attention was mainly directed towards finding the means to overcome the siege of the Prussians, but the Academy of Sciences continued to hold its weekly meetings and publications like the *Annales de chimie et de physique* continued to appear. The role of science in the defence of Paris has already been well chronicled and very little of originality can be added here.[1]

Sub-committees of the Academy vetted the flood of ideas, most of them impractical, which the people of Paris contributed for the defence of the city. Some physicists turned away from their long term research projects and tackled problems which could bring immediate benefit to the war effort. Desains and his assistant Bourbouze, worked to establish communication with the outside world using the Seine as a conductor. The *lycée* teacher D'Almeida (1822-1880) escaped from the capital in a balloon, as did the physical astronomer, Janssen. Charles Delaunay, Leverrier's successor at the Paris Observatory, continued with a reduced staff to maintain the work there, although Marie-Davy, the head of the meteorological service, left Paris with the government which established itself first at Tours and then in Bordeaux.

From Bordeaux, Marié-Davy maintained the International Meteorological service, and each day received dispatches from Sweden, Norway, the Netherlands, Belgium, England, Spain, Portugal, Italy and Austria, and each day produced a bulletin which contained an isobaric chart on which a forecast was based. The regularity of this bulletin, in conditions so difficult for France, earned considerable respect abroad.[2]

But France was to suffer the humiliation of defeat, the loss of her most industrialised provinces of Alsace and Lorraine, and the loss of one of her most active provincial faculties, Strasbourg. After defeat came revolution in Paris followed by the sanguinary repression of the Commune by government forces. The birth and early days of the Third Republic were far from auspicious.

But before all these events, before the siege and capitulation, when the news of the French defeat at Sedan arrived in Paris, the writer and philospher Ernest Renan, dining with a group of friends which included the chemist Marcelin Berthelot and the novelist Edmond de Goncourt, expounded in despair, on the reasons for the victory of Prussia. It was an expression, Renan asserted intemperately, of the superiority of the Germans whose Protestantism had developed their mental qualities, over the French 'cretinised' by Catholicism.[3] Although twenty years later, when part of Goncourt's journal on the events of 1870 was published, an embarrassed Renan was to declare that' he had never given voice to a single unpatriotic opinion'[4], his view that Germany had won the war through superior organisation based on superior education, was one which many scientists and academics shared in France. Perhaps it would be more true to say that, whether they shared this view or not, they used the supposed fact of the superiority of German education, particularly at the university level, to press forward a discussion on questions of greater government support to science, and on improvements in the quality of scientific and technical education. It would be wrong of course, to suppose that there had never been any disquiet expressed before about the state of French science. Even such a supporter of the Imperial regime as Pasteur, had made his worries public about the poverty of research facilities, and some limited reforms like the establishment of the Ecole Rratique des Hautes Etudes in 1868, had been made.[5] But now the defeat at the hands of the Prussians, gave the critics powerful arguments in their campaign for more fundamental reforms.

Renan himself was to put forward criticisms more considered than his original outburst, in a book published in 1871, La Reforme intellectuelle et morale de la France.[6] While making a critical review of all aspect of French education, his most urgent appeals for reform related to higher education.

> 'The special schools set up by the revolution,the puny faculties created by the Empire in no way replace the great system of autonomous rival universities; a system which Paris created in the middle ages and which Europe, except for France, has kept'[7].

France, said Renan, must return to the old system and create five or six universities, independent of each other other, of Paris, of the town in which they were established, and of course independent of the clergy. Schools such as the Ecole Normale or the Ecole Polytechnique, which creamed off the best students, would have no place, if a good university system existed, and should be abolished. And to the conservatives who were still haunted by the memories of the commune, to those who feared that university reform would be a dangerously democratic exercise, Renan addressed himself reassuringly, saying ;

> 'Young men educated to a sense of their own superiority will revolt if they count for no more than anyone else. The universities will therefore be be nurseries of aristocrats'.[8]

He also attacked the whole concept of centralised authority for public education, as well as reasserting his anti-clericalism by declaring that 'Catholic nations which do not reform themselves, will be beaten by Protestant nations'.[9]

In these few lines of Renan, who was to become one of the ideologues of the Third Republic, one can see some of the determining features of the state which emerged from the events of 1870-71.The

Republic not only had to respond to the deep desire for revenge and the recovery of the lost provinces, but it also had to combat the implacable hostility of the legitimist and clerical opposition, and reform the educational system both to satisfy its own political allies and to prepare the conditions for a later struggle against Germany. Shinn has made the point that the new political elites of the Republic required political allies, and the pre-1870 power structure was associated with the *grandes écoles*, particularly the *Ecole Polytechnique*. Thus the call to strengthen the university system at the expense of the *grandes écoles*, was one which struck a chord as a means of neutralising the old elites and creating new ones.[10] Genuinely interested in social reform and material advancement, successive republican governments developed primary schools (primary education was free,compulsory and secular by 1882) and the secondary school system, as well as expanding and modernising the faculties. On the other hand, during this period, the *Ecole Polytechnique* whose 'courses and their content remained relatively constant from 1830 to 1880'[11], found it difficult to adapt to new scientific programmes and technologies and came increasingly under attack from teachers in the university sector. And this sector expanded under reforms made in 1877 and 1885, which doubled the teaching personnel and awarded scholarships and grants to students.[12]

Also in the 1880's and 1890's new technical institutions like the *Ecole Municipale de physique et de chimie industrielle* in which Pierre Curie was to work for twenty years, was set up by the city of Paris in the old buildings of the *College Rollin* in 1882, and the *Ecole Supérieure d'Electricité* established by the *Société Internationales des Electriciens* in 1894. The first of these institutions was to acquire a national importance through the work of the Curies on radioactivity, but in its early days at least it had little social status in the hierarchy of French educational establishments, by virtue of both the type of practical and technological knowledge it disseminated and its position outside the state system.

The 1870's was also to see the establishment of another sector of French higher education which came to compete with the state university system and contribute to the diversity of the picture of science education in late nineteenth century France. In the bill of December 1874, the minister of Education, Cumont, proposed to allow any social or religious group with the means to do so (in practice this only meant the Catholic Church) to set up universities. The Republican opposition directed its fire on the right of these institutions to award degrees; they argued that the examinations should be organised by the professors of the local state faculties.[13] After a compromise in which it was agreed that the examining bodies would be mixed, i.e. personnel from both the state and the Catholic universities, the bill became law in 1875. The church quickly set up five universities; in Paris, Lille, Angers, Lyons, and Toulouse, and although the increasing anti-clericalism of the last years of the decade meant that their right to call themselves 'universities', was revoked, the Catholic 'Institutes' (as they became) continued as a competitor of the state system. Probably the most significant names in physics to be associated with the Catholic Institutes in the late 19th century are those of Edouard Branly, (1844-1940) and Emile Amagat (1841-1915), whose work we will examine later. In those towns where state university and Catholic Institute existed side by side, it was rare for there to be much contact. It is reported for example, that Duhem in Lille shocked many people by his organisation of meetings and discussions between the staff of the two institutions,[14] and many ambitious academics in the state system would not have wanted to jeopardize their career by publicly fraternising with clerics and lay catholic teachers. The mutual suspicion between church and state was an essential feature of the first twenty years of the Republic. Relations became better after 1892 when Pope Leo X111 called on French Catholics to give generous support to the Republican government. This policy of ralliement, a policy which the devout Duhem found it impossible to accept,[15] marked the end of the Church's support for Legitimism, and permitted her in the following years to establish many religious schools. One can say that such a policy came in part at least as a result of the growth of anarchism, which appeared to threaten the stability of the French State, whose downfall would inevitably also entrain the destruction of the Church.

Just as the official ideology of the Second Empire was Catholic, so the Third Republic espoused a philosophy which was militantly anti-Catholic, or at least anti-clerical. This philosophy which came to be called 'scientism' was underpinned by the positivist notion that science constituted the ideal form of knowledge, that value free scientific concepts can be applied to all aspects of humanity and society. As one of the Third Republic's favourite scientists, Berthelot, put it ;

> 'in modern civilisation every social utility derives from science because modern science embraces the entire domain of the human mind: the intellectualll, moral, political, and artistic domain as well as the practical and industrial'.[16]

Thus the Republic saw science as a weapon in its struggle against the church; ultimately science would explain everything and organise everything, even as its chief ideologue, Renan was to say 'Science would organise God.'[17] This 'scientism' was an essential part of the Republic's educational system, and to make a successful university career a young research worker would have to ensure that his republican virtues and his republican philosophy were apparent to his superiors and to the functionaries of the Ministry of Education. This is not to say that the researchers who made their name during the Third Republic, men such as Perrin, Curie, or Langevin, shared its ideology simply for careerist motives, for 'scientism' had a powerful and seductively optimistic message, and it afforded to scientists a position of high esteem. They were not to know that 'scientism's' essentially nineteenth-century view of constant progress, of science and technology overcoming all abscurantist obstacles and leading mankind to a better and more rational future, would crumbled to dust in the carnage of the First World War.

Scientists were able to take advantage from the defeat at the hands of the Prussians by arguing that the French had failed to employ science either adequately or in time. The first few years of the Third Republic saw not only government intervention to improve science facilities in the universities and grandes ecoles, but the process of self organisation of the scientists themselves. In 1872, the Association Francaise pour l'avancement des sciences, AFas) modelled on the British Association was set up, and proposed to hold its annual meeting in a different provincial city every year. There was already a national scientific association in the form of Leverrier's Association scientifique, and this would continue to function for a time, but it was dominated by Parisians and its early enthusiasm for provincial meetings had already evaporated by 1870. The AFas was to offer the members of the older organisation, free and complete participation in the new. However, this was not to mean the immediate demise of the Association scientifique which continued in separate existence until well after Leverrier's death in 1877, but the roots of the organisation, as the poltical allegiance of its founder, were firmly in the Second Empire and it never thrived in the republican regime.

It is not surprising, given the circumstances, that the model for the organisation of science following the disasters of 1870-71, should have been a thoroughly military one. The zoologist, Quatrefages, the president of the first congress of the AFas in Bordeaux in 1872, presented the scientist in the role of a soldier who 'knows the ardours of struggle, the intoxication of victory.'[18] The greatness of states was not to be measured solely by territorial size or the number of inhabitants, and struggles between states did not take place only on the battle field, argued Quatrefages, but in the domain of intelligence and of science, and it was on this terrain that France should first find her revenge.[19]

But apart from this patriotic appeal for France to 'assemble under the banner of militant science', Quatrefages had something critical to say about the general level of science in the country. Rejecting German assertions that the role of France in the realm of scientific knowledge was at an end, Quatrefages admitted that even though France could match any of the great names of foreign science with some of her own sons, who were no in no way inferior;

> 'We must recognise on the other hand, that the general scientific level is much more elevated among several of our neighbours, than among ourselves'[20]

Thus the prime task of the AFas was seen by Quatrefages and other leading members, as being that of bringing science to a much wider stratum of the population, above all in the provinces, to whom it was still quite foreign. This according to Quatrefages, was what the BAAS had been able to do with startling success in Britain;

> 'Thanks to it,(the BAAS) a part of the population has been transformed. The sons of fox-hunters,who, to refresh themselves after their rough past-times, only enjoyed other equally violent and material pleasures, are today botanists, geologists, physicists, and archeologists. A banker directs the Institute of Anthropology and a brewer presides over the section of astronomy, while towns vie with

each other for the honour of staging its meetings'[20]

Following the speech of welcome by the mayor of Bordeaux, the General Secretary of the Association, Cornu, welcomed foreign delegates and explained why he had not, although animated 'by a spirit of conciliation and appeasement'[21] invited any scientists from across the Rhine. The most bitter thing about the war said Cornu, had been the anti-French statements 'coldly elaborated by profesors of German Universities'[23], when the least they could have done,in his view, was to have remained impartial. So no Germans were invited, and this was 'not prompted by any sentiment of hatred or wounded self-pride; we simply thought that in this state of spirit, our national work could not expect any support from German science'.[23]

Thus the war was to have profound consequences for French science. Science became a national patriotic task, a form of warfare in itself, and the scientist would regard himself as a soldier in this situation. New scientific organisations were formed, including the _Societe de Physique_ (which we shall consider in the next chapter), while the Faculty system was enlarged and improved to meet the competition of the _grandes ecoles_ and the Catholic Institutes. Scientists enjoyed the highest prestige since the days of their defence of the First Republic, and as in that time, some were called to the highest offices of the state.

Notes for chapter 3.

1. M.P.Crosland,'Science and the Franco-Prussian War', _S.s.s._, 1976, _6_, 185-216.

M.Gorman, 'Electric illumination in the Franco-Prussian War', _S.s.s._, 1977, _7_, 525-29.

2. C.Delaunay, 'Note sur le service météorologique de l'Observatoire de Paris, *C.r.*, 1871,*72*, 178-79.
   In this note Delaunay brings to the attention of the Academy, the letter of thanks to the meteorological staff, from Piazzi-Smyth of the Royal Observatory of Edinburgh.
3. Edmond and Jules Goncourt, *Pages from the Goncourt Journal*, (trans. by R. Baldick), Oxford, 1962, pp.1-4.
4. Ibid.,p.357.
5. The concern with the continuing rise in the German universities as centres of research, is briefly dealt with in R. Fox, op.cit.,(note 12 chapter 2). p.102.
6. Taken from an extract in David Thomson, *France,Empire and Republic, 1850-1940*. London, 1968, p.232.
7. Ibid., p.232.
8. Ibid., p.233.
9. Ibid., p.230.
10. T. Shinn,'The French science faculty system', *H.s.p.s.*, 1979, *10*, 271-328, (302).
11. T. Shinn, op.cit.,(note 16 chapter 2), p.47.
12. Ibid., p.123.
13. G. Chapman, *The Third Republic of France.The first phase,1871-94.* London,1962, p.157.
14. S.L. Jaki, *Uneasy genius: The life and work of Pierre Duhem*, The Hague, 1984, p.86.
15. Ibid., p.92.
16. Ibid., p.53.
17. Ibid., p.228.

18. Quatrefages de Breau, 'La Science et la Patrie', C.r.a.F.a.s., 1872, pp. 36-41, (p.38).

19. Ibid.

20. Ibid., p.37.

21. Ibid., p.41.

22. M.A. Cornu,'L'Histoire de L'Association Française', C.r.a.F.a.s., 1872, pp.44-49, (p.48).

23. Ibid.

24. Ibid.

4. THE ORGANISATION OF, AND RESEARCH IN PHYSICS, DURING THE FIRST TWO DECADES OF THE THIRD REPUBLIC.

a. Introduction

The opening years of the Third Republic saw the implementation of measures which the national humiliation at the hands of Germany had raised in a particularly acute way. Whether or not the real reasons for defeat lay in the superiority of German education or German science was really immaterial, it was stated to be so and the statement found support. University reform, the encouragement of science in the provinces, the application of science in industry, the formation of disciplinary societies, were seen to be patriotic duties which no true Frenchman could oppose. The reforms carried out to the university system, the formation of the Société de Physique and the AFas will be considered in this chapter, together with the development of physics until the last decade of the century.

The growth of the physics community and the quantity of research being undertaken, raises more and more sharply the question of the criteria adopted for the inclusion of material in this thesis. That research which was, as far as can be determined, considered to be of some importance at the time, has been included. However, much of the work done by the growing army of scientific instrument makers and electriciens, who were involved particularly in the development of electrotechnology in the remaining years of the century, a work which effectively was the application of the principles of physics, will be excluded as engineering. Some, however, will be considered in the appropriate section because engineers and instrument makers made a powerful contribution to the early years of the Société de physique. The 1881 Congrès International des Electriciens, in which the units of electricity were standarised and agreed upon by the international scientific community, and the establishment of both a society and a journal (edited by Du Moncel) for electrical engineers in the same year, marked the separation of this discipline from physics. Thus, after 1881, the work of the électriciens will figure less and less prominently in this survey.

But French researchers still found plenty of work to do within the confines of the discipline of physics. Thermodynamics, a term which was first used in the subject index of the Comptes rendus in 1866, generated a considerable amount of experimental work. After being a little slow in accepting the mechanical view of heat, French savants began to consider it as almost a French creation by the 1870's, recalling the work of Carnot, Marc Seguin and Babinet, for example. But most of the workers in this area in the 1870's onwards, people like the Alsatian engineer Hirn, or Ledieu, professor of hydrography at the naval school of Brest were not associated with the institutions studied in this survey and therefore their research is not examined here. On the other hand there was no shortage of scientists in the university system,who found that the optical paradigms of Fresnel and Cauchy could still provide them with experimental research programmes on which to base a successful career. There is little evidence that the conceptual revolution which had already been signalled in the study of light, by James Clerk Maxwell's model of the ether, which was able to unite static electricity, current electricity and electro-magnetic induction, was widely known in France before the 1880's. It is true that after Hertz's successful demonstration of the existence of electro-magnetic waves in 1886, Maxwell's ideas did enter France but even then only in the particular form which Helmholtz had given them. Pierre Duhem, writing at the end of the century, stressed that Maxwell's work provoked among French savants a certain stupefaction on encountering;

'..the same absence of order and method, the same lack of concern for logic, not only in the collection of mechanical models but in a series of algebraic theories'.[1] Henri Poincaré too, commented that Maxwell's work provoked both admiration and distrust among French scientists.[2] So in the two decades covered by this chapter we will not encounter many scientists who show evidence of being influenced by Maxwell's ideas, although there were some.

From now on there will be certain institutions which will play an ever increasing role in the production of new knowledge in *La physique*, while others will decline. In part this is due to the greater emphasis placed on research following the reforms of the *Ecole Pratique des Hautes Etudes*, partly to the development and better funding of the provincial faculties, and partly to the development of such national organisations as the *Société Francaise de physique* and the *Association Française pour l'Avancement des Sciences* (*AFas*). *Lycée* teachers will from now on produce much less research than in the past, not only because it has become much more expensive, but because the most able *normaliens* (who in the past had frequently worked for their doctorates while teaching in secondary schools) would now become *agrégé préparateurs* in Paris institutions of higher education, complete their doctoral research there and go on to teach in the faculties. The teachers of physics at the institutions of the Ministry of Commerce, the *Conservatoire* and the *Ecole Centrale*, will also play a much smaller role in research than previously and so these institutions will not be considered further. For the same reason Leverrier's *Association Scientifique de France* plays no further part in this thesis. It continued to exist, continued to hold meetings in Paris where a passive audience came to admire the scholarship of some of the great names of French science, and it continued to distribute funds, but by the time it was amalgamated into the *AFas* in 1886 it had long ceased to play a role of organising or stimulating the practice of science.

On the other hand, the staff of the provincial faculties will now begin to produce much more research than hitherto, even though much of it will be by young lecturers whose ambitions are centred on Paris and who will remain in the provinces for only a short period. Some physics professors will, however, be content to stay in the provinces and they and their faculties will acquire national reputations. In this category will be, among others, Blondlot of the Faculty of Nancy, Macé de Lepinay of Marseilles, and Gouy of Lyons.

b. *The founding of the Société Française de Physique*

Although founded in the second year of the Third republic, the *Société Francaise de Physique* had its origins in the informal meetings, organised by a group of physicists from the teaching institutions of the capital, held in the *Ecole Normale* in the last year of the Empire.[3] Prominent in this group were six individuals; Bertin, deputy director of the *Ecole Normale*, Cornu, professor of physics at the *Ecole Polytechnique*, Mascart who had succeeded to Regnault's physics chair at the *Collège de France*, and the physical science teachers in Paris *lycées*, D'Almeida, Gernez and Lissajous. We have encountered all these individuals in previous chapters as they began their careers in the Second Empire. This group, with the exception of Lissajous, who was appointed to a provincial chair in 1874, formed the intellectual and organisational nucleus of the society for its first five years of existence, and all, with the exception of Cornu, were products of the *Ecole Normale.*

Although there was no formal connection between it and the *Société de Physique*, the founding of the *Journal de physique théorique et appliquée* in 1872 was a part of the same process of professionalisation of the physics community. Its founder and editor was Charles

D'Almeida, one of the founders and the first general secretary of the Société de physique, and in the first edition of the journal, he outlined its objectives. It was to give a new impetus to the study of physics, to expound its most recent and least known theories and the experiments on which they are based, and to explain the easiest way of repeating these experiments. It addressed itself particularly to the teachers of physics, isolated in the provinces, deprived of resources, who did not know where they could most profitably devote their energies. But, at the same time its addressed itself to all those who had a 'scientific profession', industrialists, engineers, doctors and others, above all the young, who wanted to contribute to the intellectual development of France.[4] The journal and the society, born at the same time and out of the same conditions, sharing the same ideals and directing personnel, would continue to maintain close relations throughout the rest of the period of this thesis.

The founding conference of the society took place on 20 December 1872, in the Ecole Normale, with the attendance of around 70 people. The academician Fizeau, by this time probably the most eminent of French physicists and with an international reputation, was elected to be its first president. By the end of its first year of existence the membership had grown to about 200 (see table 4.i. overleaf) of whom 120 came from the Paris region, 77 from the provinces and four from abroad.

The names of ten Academicians appear on the membership list and although most of them played only a minor and chiefly ornamental role, one, Jamin was to contribute a number of papers in the early years. No formal qualification in physics was necessary for membership, only an interest in the subject and the recommendation of two existing members. Those who lived in the Paris region paid an annual subscription of 20 francs, those from the provinces or abroad, 10 table 4.i.

MEMBERSHIP OF THE SOCIÉTÉ DE PHYSIQUE
1873 - 1900

	PARIS							PROVINCES						
	Academy	Faculty & Grnd.Ecs.	Lycées	Muséum & Collège	Observ. & R.I.	Other	Total	Facul-ties	Lycées	Obsvs. & R.I.S	Other	Total	Foreign	Grand Total
1873	10	28	32	6	5	39	120	16	47	0	14	77	4	201
1874	11	30	31	6	4	51	133	15	51	0	13	79	4	216
1875	12	28	33	7	5	58	143	19	49	0	19	97	17	257
1876	13	29	32	6	3	77	160	20	56	0	29	105	22	287
1877	12	30	36	5	4	95	182	24	53	0	44	121	35	338
1878	15	29	32	4	5	122	207	31	60	0	60	151	46	404
1879	16	29	31	4	7	140	226	34	59	0	68	151	59	436
1880	17	34	32	2	8	158	251	36	65	1	88	190	67	508
1885	16	59	49	7	11	227	369	42	81	2	85	210	94	673
1890	18	70	43	6	10	248	395	68	81	1	82	232	131	758
1895	21	65	48	4	8	219	365	72	119	1	67	259	129	753
1900	22	57	54	7	11	219	370	97	112	0	81	290	161	821

Source: S.s.F.P., 1873-1900

francs. Life membership could be achieved by paying a single sum of 200 francs, or four payments of 50 francs. This inclusivity, neither excluding aspirants on the basis of academic achievement in the subject, nor by a high subscription, probably made it less attractive to the already successful physicist.[5] He could see little advantage in explaining his latest research to an audience, who on the one hand could probably not understand it, and on the other could not offer any financial and social patronage to the speaker. But to the young researcher and above all to the unqualified instrument maker or electrician,it was a useful and valuable forum.

The meetings of the society were held every two weeks, except for a long summer break,in the *Salle Gerson*, a hall conveniently close to the *Ecole Normale*, the Faculty of Sciences and several *lycées* and workshops. It was also large enough to accomodate a large audience and permit demonstrations and experimental work.In the early years, experiments and demonstrations took place at most meetings, while the Easter meeting was used to repeat the most successful and interesting ones for the benefit of provincial members visiting Paris during the holidays. As we shall see later, the contribution of the provincial members to the intellectual life of the society through the submission of papers, was always meagre, never exceeding more than about 10% of the papers given at a meeting. After a period of steady growth during the 1870's the society was recognised as an *établissement d'utilité publique* in an official decree of 1881.

Although it has been said that the founding nucleus of the society was made up of science teachers and research workers from the Paris institutions, and although these sectors were always well represented, it must be stressed that a large proportion of the membership, particularly in Paris, were not teachers of science either in *lycées* or the faculties. There were in Paris some research workers from the observatories,[6] a few from the Bureau Centrale Météorologique (after its foundation in 1878) and some from the International Bureau of weights and measures at Sèvres. But the majority of members whose occupations were listed, were engineeers of the State Corps (mainly Ponts et Chaussées and Mines with the occasional one from Manufactures d'Etat and Administration des Lignes Télégraphiques, and electrical engineers and electricians associated with the growing electrical industry, constructors of precision instruments and apparatus, or military engineers. In fact the dynamic growth of the society in its early years in Paris, came from the expansion of these technological sectors, while the number of lycée and higher education teachers remained nearly constant up to 1880. The situation in the provinces was a little different, for there the number and the proportion of engineer members was much smaller. Given that it was the Parisian members who gave most of the papers and, given the weight of the engineering sector of the capital,the technological character of much of the early proceedings of the society is not so surprising as it might first appear. It is to be expected that many of the electrical engineers and instruments makers would have used the meetings to demonstrate their new optical and electrical apparatus to academics with funds to spend on apparatus, while others would have been looking for new avenues for the commercial exploitation of their inventions. Moreover, there was a certain social status to be acquired by belonging to the society and associating, at least for two hours every fortnight, with the most eminent figures of French science, and such status was very desirable to self-made engineering workshop owners wishing to differentiate themselves from the workmen they employed. But when the rate of growth of the Société de Physique slowed down in the late 1880's,it was probably due to the founding of the Société International des Electriciens in 1883, as the electrical engineers began to see that their professional interests were better served by the new organisation.

In fact this near stagnation in growth began to cause a certain disquiet in the society, so much so that in the 1888 conference, the outgoing president, the astronomer Charles Wolf, appealed to every person present to recruit a new member before the next conference. He also commented upon the changing character of the meetings, which was becoming more 'academic and formal', and the lively debates which followed the papers of earlier years, were now more and more replaced by the silence of members 'who were afraid of saying something stupid'.[7]

The growing reserve of members might simply be explained by the fact that the meetings were becoming larger, but is more likely an indication of the more theoretical and mathematical content of the contributions in the later years. Certainly, by 1890, the days had long gone when meetings were enlivened by demonstrations of elastic powered aeroplanes and birds, or voice recordings on primitive phonographs. Such an evolution was clearly not to everybody's liking, for the number of non-renewed subscriptions increased, and if by 1890 there was still a small rate of growth, it was largely the result of the continuing interest of foreign scientists in joining the society. See table (4.i.) page 122 .

Table (4.i.) also shows the occupations of the members of the society from its foundation until 1890. The analysis has been made yearly until 1880, the period in which the growth was most rapid, sometimes reaching 15% a year,and then for the years 1885 and 1890.The annual growth rate between the last two dates is only 2.5%. The table also illustrates the continuing interest in the society by foreign scientists, who, by the year 1890, made up about 15% of the membership.

Table (4.ii.) p.126, makes a finer grained analysis of the year 1890, to try to give more information about the engineer members who

MEMBERSHIP OF THE SOCIÉTÉ DE PHYSIQUE 1890

Table 4.ii.

a)

	Academy	Fac+ Grnd. Ecs.	Post Lycée	Lycée	Collège + Muséum	Obs + R.I.	Télé-graph	State Corps	Inst. Makers	Elec. Engs.	Mili-tary	Others *	Total
Paris	18	47	23	43	6	10	18	25	38	63	23	81	395
Provinces	-	48	20	81	-	1	7	5	2	9	11	48	232
Total	18	95	43	124	6	11	25	30	40	72	34	129	627

Foreign	131
Total	758

* No occupation given, or retired.

b)

	Paris		Provinces		Total
Total No. Pure Science	147	23	150	24	297
Total No. Technology	167	27	34	5	201
Total No. Unknown	81	13	48	8	129
	395	63	232	37	100 627

Red figures are
percentages of
the French membership

Source: S.s.F.p. 1890

appear undifferentiated in the 'other' category of table (4.i.). There were also, by 1890, more post lycée educational establishments, which previously were included in the faculty and grandes écoles category, and these too have been separated in table (4.ii.). In the post lycée category, are included the Faculty of Medicine, the School of Pharmacy and the Ecole de Physique et de Chimie Industrielles de la Ville de Paris, in which Pierre Curie worked for many years.

Table (4.ii.) shows that the society in 1890, even after a certain expansion of higher education in the capital and the competing attractions of the Société Internationale des Electriciens, was still numerically at least, still dominated by the technological sector. On the other hand , in the provinces, the society was very much an organisation of teachers. In the first years of the society, the engineers of Paris were well represented among the people giving papers at the meetings, but increasingly after 1880, and particularly in the years from 1885 to 1890, contributions came increasingly from the staff of the Paris institutions of higher education and research. This is well demonstrated in table (4.iii.) p.128, which makes a comparison between an earlier year chosen at random (1879) and 1890. These two years show an increasing predominance of Parisian academics, and therefore normaliens in the intellectual life of the society.

Table (4.iv) p.129, shows an analysis of the content of the papers presented to the society in the years from 1873-1885. This has been taken from an analysis published in the 1885 edition of the proceedings of the society, so it therefore uses the categorisation of topics made by the society itself. It can be seen that the two areas, 'electricity and magnetism', and 'optics' make up the bulk (more than 75%) of the papers. Most of the optical papers were 'pure' science contributions made by such academics as Cornu and Bertin, although the instrument maker Laurent has ten papers to his credit, mainly of a practical

TABLES 4.iii

ANALYSIS OF THE ORIGIN OF SCIENTIFIC PAPERS

PRESENTED TO THE SOCIETY IN 1879 AND 1890.

a)

	1879		1890	
Paris	33	77	38	79
Provinces	2	4	4	8
Non-Members	3	7	0	0
Foreign	5	12	6	13
Total	43	100	48	100

Red figures are percentages

b)

ORIGIN OF THE SCIENTIFIC PAPERS FROM PARISIAN MEMBERS

	Academy	Gr. Ecs. & Collège	Faculty	Lycée	Sèvres	Engineers	Inst. Makers Telegraph Engs. Military Engs.	No Occupation Listed	Total
1879	2	4	3	4	1	8	3	8	33
1890	1	18 *	6	3	3	2	2	3	38

* 5 From Ecole Normale, 6 From Ecole Polytechnique, 7, From Collège de France

SOURCE . S.S.F.P. , 1879 AND 1890.

Table 4.iv.

Papers presented to the Société de Physique 1873-1885

Year	Mech/-anics	Heat	Acoust/ics	Electricity Magnetism	Optics	Meteor/.ology.	Total
1873	2	2	2	7	4	0	17
1874	3	0	2	5	7	0	17
1875	5	2	1	14	8	1	31
1876	4	7	2	11	12	0	36
1877	4	3	1	9	12	1	30
1878	1	8	5	22	9	2	47
1879	3	1	3	21	17	1	46
1880	4	1	1	20	11	0	37
1881	8	3	0	24	11	3	49
1882	2	3	0	15	13	0	33
1883	3	2	0	10	11	1	27
1884	2	5	1	14	6	0	28
1885	1	7	1	26	10	1	46
Total	42	44	19	198	131	10	444
%	9	10	4	45	30	2	100

Table 4.V.

The most productive members of the Société de Physique 1873-1885.

Name	Career in period 1873-1885	No. of papers	Topic areas
M.A. Cornu 1841-1902	Chief Eng. Mines Phys.prof.E.P. Phys.Academy 1878	19	Optical 16 papers Waves 1 " " Mechanics 1 " "
E.E.N. Mascart 1837-1908	Phys.prof.Collège Dir.B.C.M. 1878 Phys.Academy 1884	18	Electrical 10 papers Optical 3 " " Met. 2 " "
G.Lippmann 1845-1921	Phys.prof. Paris Fac. Phys.Academy 1886	17	Electrical 16 papers Optical 1 " "
D.Gernez 1834-1910	Lycée Descartes 1873 M.de C. E.N. 1885 Phys.Academy 1906	16	Heat 9 papers Optical 1 " " Electrical 1 " " Phys/chem 4 " "
E.M.L. Bouty 1846-1922	Lycée Louis-Le-Grand Prof.Phys. Paris Fac. 1885 Phys. Academy 1908	11	Electrical 11 papers
L.Laurent	Instrument maker	10	Optical 10 papers
E.Mercadier 1830-1911	Inspector telegr. Prof.E.P.	10	Vibrations 5 papers Electrical 5 " "
P.A.Bertin 1818-1884	Deputy Dir. Ec.Normale	10	Optical 5 papers Electrical 5 " "
E.J.Marey	Collège de France	10	Mech and physiology 10 papers

Source : S.s.F.P. 1885.

instrumental character; e.g. the projection lantern, the polarimeter, the saccharimeter. On the other hand, of the large number of electrical papers, a high proportion come from engineers, on such problems as the generation of electricity and its transmission, the efficiency of electric motors and their control, the electric light, the telephone and phonograph,etc. For example, in 1881, a report on the functioning of Paris's first telephone exchange was given, as well as an account of the experiment to produce electrical amplification of speech and song at the Paris Opera house. Several of the engineer members of the society won themselves considerable reputations ; Marcel Deprez for his work on electrical generation and transmission, Antoine Breguet for his work on telephones, Dubosq for his arc-lamps, and Jablochkoff for similar work on electrical lighting.

In relation to the more 'pure science' type of paper given by the teaching staff of the Paris institutions, these were almost invariably restatements or *resumés* of work already presented to the Academy, and already published in the *Comptes rendus*. Contemporary foreign research was frequently considered to be important enough to be presented to the society's meetings by their authors. Andrews' work on the isothermals of carbon dioxide and Crookes' research on the discharge effects through gases at very low pressures, were discussed during this period, as was Crookes' radiometer which provoked several papers from French members, attempting to give an explanation of its rotation. There were no papers from Germany before 1885 as the result of the lingering chauvinism produced by the defeat of 1870-71.

The most productive members of the society up to 1885 are listed in table (5.v) p.129. Apart from well established people like Cornu, Mascart and Bertin, who we have already encountered in the Second Empire, there appear now the names of men whose careers are going to span the whole period until the outbreak of the First World War. Of the nine individuals listed, Gabriel Lippmann (1845-1921), and E.M.L. Bouty (1846-1922), who were both professors of physics at the Paris Faculty, probably played the most influential role in French physics, more as a result of their teaching than through their own research.

There were also a few meteorological papers in this period both from abroad and from the staff of the *Bureau Centrale Météorologique* (hereafter referred to as the BCM). The opening of the Eiffel tower in 1889 gave the opportunity for collecting data at different altitudes and this was reported to the society in that year by a member of the BCM, Angot. The BCM was well represented in the *Société de Physique*; apart from its director, Mascart, there was E.Fron (1836-1911), a veteran from the early days of the storm warning service in the Paris Observatory, and the younger members Angot, Teisserenc du Bort, and Perier. These men, all highly trained in the Paris institutions of higher education, helped change meteorology from a descriptive science carried out by devoted amateurs, into a mathematical science with a basis in hydrodynamics and thermodynamics.

c.The Association Française pour l'Avancement des Sciences from its foundation until 1890.

The establishment of the *AFas* has already been referred to in an earlier chapter. It was one of the responses to the crushing defeat suffered at the hands of the Prussians, and the conclusion drawn that such a defeat owed much to the inferior organisation of science and technology in France. In particular the need was felt to raise the general level of science in the country, bringing up the provinces to the level of Paris, whose illustrious savants were considered, of course,the peers of any in the world. Given the situation in France in 1872, when the *AFas* was founded, it is not surprising that the emphasis in the association should have been on science as an element in the moral regeneration of France. Science was both a patriotic duty, as well as a prerequisite for the rebuilding of national prestige, and for the construction of a more dynamic industrial and military machine. Thus, in the general sessions which opened each annual meeting and set their tone, nationally respected scientists would share the platform with local industrialists, politicians and military men, to expound on such topics as 'The services which modern science can render to the art of war'[8], or the 'Relation between science and industry'.[9] The AFas received large sums of money from industrial interests, particularly the railways, and its lavish annual meetings with excursions to local beauty spots, cultural events, museums, schools, hospitals and factories, attracted a large provincial following. For example 712 people attended its 1880 meeting at Rheims out of a total membership of 3,156.[10] This represents about 22% of the national membership.

What the contribution of the AFas was to the development of physics in France generally and in the provinces in particular, is very difficult to assess, but one is inclined to think it was not very great. Certainly, if the journal of the annual meetings is a reliable guide, physics did not figure very prominently in the association. The number of pages devoted to the fifth section (the section for physics) of the mathematical sciences group of the association, was only a small fraction of that devoted to sections within the natural sciences group; anthropology or medicine, for example. Moreover, sometimes the fifth group was amalgamated with the seventh (meteorology and earth sciences) because the contributions to both were so meagre. Perhaps the paucity of the physics contribution is hardly surprising considering the increasing difficulty of physics concepts and theories on the one hand, and its dependence on more and more expensive equipment on the other. Specialised knowledge and specialised equipment, made physics more and more the preserve of the trained professional, and the faculty professional at that. There might still be a place for the gifted amateur in astronomy or meteorology, making observations with fairly modest equipment, or even, if he had a thorough grounding in the subject, in mathematics, but both intellectual and material factors militated against the continuing role of the amateur in physics. On the other hand, in the natural sciences group, collectors, classifiers, and country doctors, would still find much they could comprehend and discuss and plenty to interest them in meetings of the association.

Physics professors from the capital always played some role in the meetings, and gave a large proportion of the papers (table (4.vi) p. 134 but they were always summaries and popularisations for a lay audience, of work which had been published before. This was also true of the foreign contributions, and even some of the provincial faculty members had published their contributions previously in national or local journals. Again this is to be expected; no scientist would report original research to a meeting of people he did not consider to be his peers, especially as he would have to wait many months before he saw his work in print. Those savants who did continue to contribute, at least the Parisian ones like Cornu or Janssen, were perhaps motivated, at least partly, by a sense of duty, fired by the patriotic ideals of the association.

It would be wrong to think that young ambitious scientists played a role in the Association merely for crude careerist motives, attempting to win the approval of the Ministry of Education by performing public patriotic actions. They genuinely shared the Association's patriotic and republican ideology. Although this allegiance to the Association tended to cut across the boundaries of the different educational institutions, it was Polytechniciens like Cornu, or J.B. Baille (b.1841), or the Parisian telegraph engineer, E.J.Mercadier (1830-1911) and Marcel Deprez (1843-1910) the electrical engineer, who table 4.vi.

Contributors to the Conferences of the AFAS 1872-1890

Year Place	Cornu	Merc- -adier	Gariel	Deprez	Jans- -sen	Baille	Other Parisians	Total
1872 Bordeaux	2	1	1				3	7
1873 Lyons	2	1					3	6
1874 Lille	2	1	1	2			0	6
1875 Nantes	2			1			4	7
1876 C'Ferrand	2				1		3	6
1877 Le Havre	1	2	1	1	1		8	14
1878 Paris	1	1		1			5	8
1879 M'pellier	2	2	1				2	7
1880 Rheims	1	1		2	1		6	11
1881 Algiers			1		2		3	6
1882 La R'chle	2			1			3	6
1883 Rouen							3	3
1884 Blois	3					2	4	9
1885 G'oble						2	1	3
1886 Nancy						1	7	8
1887 Toulouse						1	5	6
1888 Oran						1	2	3
1889 Paris						1	10	11
1890 Limoges	2				1	1	4	8
Total	22	9	5	8	6	10	76	135

In this period Crova from Montpellier submitted 11 papers
Lallemand from Poitiers submitted 8 papers
Merget from Lyons submitted 9 papers

Total no. of papers from Paris = 135
Total no. of papers from provinces = 78
Total no. of papers from foreigners= 49
Total no. of unknown source = 33
Grand total = 295

Source: C.r.a.F.a.s

Papers from Parisians as % of total= 46
Papers from Provincials as % of total= 26
Papers from Foreigners as % of total= 17
Papers of unknown source as % of total= 11

were the most regular Parisian contributors. On the other hand, established Academicians of the physics section like Desains, Jamin, or Edmond Becquerel, for example, played little or no part in its affairs. The three most regular contributors from outside Paris, were the provincial faculty physics professors, A.P.P.Crova (1833-1907) of Montpellier, E.A. Lallemand (1816-1886) at Poitiers at this time, and A. Merget (b.1819) of Lyons. Lallemand was elected a corresponding member of the physics section of the Academy in 1882, and when he died in 1886, Crova was elected to his place. Their participation in the Association probably helped them to gain a national reputation and brought them to the attention of physicists from the capital. Crova, whose research work tended to concentrate in the area of solar radiation and its absorption by the atmosphere, submitted six papers to the five meetings of the AFas, after he was elected to the Academy, so his participation cannot be seen merely as a careerist manoeuvre.

But although it might be expected that most papers came from the faculty sector,it is also true to say that there were some contributions from local members working in lycées,in industrial enterprises and other areas. In the 1873 meeting in Lyons for example, most of the local contributions came from the previously mentioned Merget, but there were others. Frequently, the local contributions, at least from outside the faculty sector, would probably have appeared to the Parisian savant as being peripheral,if not actually eccentric. For example, at the Lyons meeting, a certain Beekensteiner expounded on the problem of the build up of static electricity on pens, a build up which resulted in the premature fatigue of the user.[11] This type of paper, lacking any mathematical basis or even a systematic experimental component, would probably have elicited considerable interest in an audience lacking formal training in physics, but it was also the type of rather eccentric contribution which the organisers would have preferred to exclude. But it would be wrong to imagine that the local contributions were always so eccentric. A more common characteristic was the rather utilitarian nature of the local contribution relating to the scientific and technical problems of the industry of the region. For example when the Association met in the shipbulding city of Nantes, two local teachers spoke on the problem of the preservation of iron ships,[12] and later when the meeting was in the champagne region of Rheims, a mechanic explained his system of taps and valves for a wine storage system.[13]

The quality of the local intervention depended very largely on the quality of the personnel of the local faculty, and if the meeting was held in a town or region where there was no faculty, as for example in Le Havre or Algiers, then it would not be unusual for there to be no physics papers from inhabitants of the host region. On the other hand where a faculty existed, it was natural that most of the local papers would come from there. Merget gave four of the six papers which came from Lyons in 1872, Crova two of the three from Montpellier in 1879, and of the five local papers which were given in Lille in 1874, three came came from men associated with the faculty; Terquem, Boussinesq, and Trannin. The first two were professors in the faculty, the third, Trannin is listed simply as being a Licencié of Lille Faculty. Terquem had already won prizes and achieved a national reputation for his acoustical work, while Boussinesq was to become professor of mathematical physics at the Paris Faculty, and a member of the mechanics section of the Academy in 1886.

Although the AFas is associated with the Third Republic and the anti-clericalism of its early years, this did not exclude the participation of lay or clerical Catholic scientists. Presumably it was for them to judge whether participation would give more authority and prestige to their faith, or to the scientific institution of a state which showed itself hostile to their religion. In the 1876 conference which took place in Clermont Ferrand, there were seven local papers, one from a faculty member, one from a chemist of the town, and five from a certain Abbé Lavaud de Lestrade, professor of physics at the seminary of Clermont. These five papers ranged over the whole spectrum of physics, but the editor of the journal saw fit to publish no more than a summary of a few lines of each one. This may have been an objective contemporary judgement on their value, but it may also have been simply the action of an anticlerical functionary who perhaps was interested in minimising the importance of the Catholic contribution.

The 1878 meeting was given in Paris, but even this did nothing to increase the participation of the highest echelons of French physics. It is true that the 'regulars' like Cornu, Mercadier,and Deprez gave one paper each, and there were five others which could be identified as coming from Paris. But the Parisian contribution made up only about a quarter of the total number of papers presented,and tends to show that not only the famous were reluctant to present their work at its meetings, but also many young researchers employed in the institutions of the capital, saw little point in contributing. One must also say that this was not a particularly fruitful period in French physics anyway; the kinetic theory of gases and Maxwell's electro- magnetic theory of light, were only slowly being assimilated, while the wave theory of light which had provided such a rich research programme for French physicists since the time of Fresnel, and was still being relentlessly pursued by Cornu, was no longer attracting new research workers.

Clearly the importance of the AFas cannot be sought merely in the quality of the papers presented at its meetings;its purpose was not to be an organisation in which the country's foremost research workers exchanged ideas. Its objective was to stimulate local initiatives,and it appears to have succeeded in this. It distributed considerable quantities of money which helped both private and faculty based research, particularly in the purchase of equipment.[14] The membership of the AFas peaked at around 3,300 in 1883 and this gave it an income of 66,000 francs, of which it distributed as grants some 11,000 francs. Even though its membership and income continued to decrease up to the First World War (35,820 francs in 1914) it continued to disburse at least 15,000 francs each year, and occasionally gave out twice this amount.[15] Physicists saw very little of this money, although Crova received 1,500 francs in both 1886 and 1887, for his work on actinometry, but the sum of a few hundred francs was a more normal order of grant for a physics project. In addition to its role as a distributor of funds, the Afas was important because its decisions were listened to by successive ministries of education and because it was supported by scientists as a vehicle for those reforms in science which they considered necessary. The reform of the French meteorological service and its separation from astronomy was one of the causes which the Afas campaigned for in the mid-1870's. The 1876 meeting set up a commission to look into the problem more deeply and make representation to the government.[16] In the following year, Leverrier, the director of the Observatory, died, and the way was opened, not without opposition from some members of the Academy, for the establishment of an independent meteorological service. The Bureau Centrale Météorologique under the directorship of Mascart, came into being in 1878. The AFas could thus claim the credit for first giving a national platform to the discussion of reform of the organisation of this particular branch of the sciences.

In 1881 the AFas held its annual meeting in Algiers but only a very few physicists from metropolitan France contributed. Janssen gave two papers on solar photography, and the young Marcel Brillouin (1854-1948) from the Faculty of Nancy spoke on electrical induction.

Foreign scientists, including three from Britain, the most eminent of whom was J.H.Gladstone F.R.S., gave five papers, but the programme was a very limited one and there was no contribution from French Algerian residents. It should be remembered that there was as yet no faculty of sciences in Algeria (it was not established until 1909) and the most advanced science education took place in the _Ecole préparatoire à l'enseignemente supérieure des sciences_, established in the previous year. Brillouin was to contribute another paper in the following year at La Rochelle, but then several years went by before he intervened again. The Rouen meeting of 1883 was exceptional in that it was the first to hear a contribution by a woman, Clemence Royer, who discussed theoretical questions about the molecular structure of materials.[17] Two local men, a colour-chemist and a teacher, presented the results of their trials on the effect of sunlight and electric light on a wide range of dyestuffs.[18] The problem of a search for fast dyes which would not fade under the effect of light, was one which was of particular importance to the textile industry of Rouen. The following years saw little of interest from a physics point of view, and the meeting of 1885 in Lyons, saw only ten papers in the physics section.

The meeting in Nancy in 1886 stands out in a generally desolate period. The number of physics papers nearly doubled to nineteen, they tended to be more original, and the weighty intervention of the local faculty gave an indication of the vitality of the scientific life of the region. The two major local contributors were the faculty members Ernest Bichat and René Blondlot (1849-1930). It should be emphasised that the position of Nancy in the French faculty system was an exceptionally privileged one. It had profited from the transfer of faculty members from Strasbourg after the loss of the region in the Franco-Prussian war. The Ministry of Education wanted to build it up as a competitor of the German Institutes on the other side of the

Rhine, it was a centre of new technological industry, and it benefitted from the generosity of local industrialsts and municipal councils which poured money into its faculties.[19] Bichat and Blondlot's speciality, electrical physics, was expressed in the content of their papers to the meetings. Four years later Bichat was made director of the Institut Electrotechnique financed largely by the Belgian industrialist Ernest Solvay. Blondlot was, after a distinguished research career, to earn national and international notoriety some twenty years later with his 'discovery',of the spurious N rays.

The meeting in the following year in Toulouse was of no particular interest; no foreign members gave papers, and there were no contributions from the local faculty members. Crova spoke on the solar radiation work for which he had received funds from the Association, at this meeting and at the following year's meeting in Oran. Brillouin , now promoted to a post in the Ecole Normale, also contributed.

The year 1889, being the centenary of the French Revolution, gave a pretext for the meeting to be held, for the second time in the short life of the AFas, in the capital. Not surprisingly as a result, the number of physics papers was a little higher than normal, but even here the great names of French physics were absent. Less important figures from the capital like Baille, or Charles Fery (d.1935) of the Ecole Centrale , contributed, as did Crova from the provinces. Madame Royer, after an absence of six years, tried out her ideas on the molecular constitution of water on a Parisian audience.The only Parisian Academician to contribute, and he seems to have strayed into the wrong section, was the explorer and geographer Antoine Abbadie, who reported on a meteorological phenomenon known as the 'Quobar', a dark overcast atmosphere which sometimes occurs in desert regions in certain conditions of temperature and calm winds.[20]

The following year's meeting, while otherwise unremarkable, was interesting in hearing a paper from a member of the administrative staff of the Ecole Polytechnique, Rochas, on what had come to be known as "odic forces".[21] Odic forces were a part of that area of research which became popular in the late nineteenth century ,in which telepathy, spiritualism, and psychic phenomena became candidates for physical experimentation and research. Odic forces were thought to be emitted by the body and could be transmitted along iron wires,and can probably be classified along with Lebon's 'black light' and Blondlot's 'N' rays, as some species of 'auto-suggestive' phenomenon. It is an indication of the rather popular character of the AFaS meetings, that 'odic' forces could be discussed here, and presented by a man only obliquely connection to physics. There were no such papers presented to the meetings of the Société de Physique.

Thus by the time the last decade of the century opened, the Afas had become a well established organisation with a membership of around 3000. It never was to play a role as important as that played by the British Association, but it did give encouragement, once a year, to the provincial doctor, teacher, pharmacist, to the worthy republicans who saw themselves as fighters of the good fight to vanquish obscurantism and to make science and rationalism triumph in every distant corner of France.

d.The Ecole Polytechnique.

The two physics professors of the Ecole Polytechnique who had been appointed in the Second Empire, Jamin and Cornu, continued in their posts in the republican regime. Jamin's reputation was already made, and with other responsabilities in the Paris Faculty and the Academy, his research ouput slackened, but Cornu's career was still in its early stages. From 1871 onwards he was to concentrate on a research project which would continue the optical research traditions of the Polytechnique, would strike a patriotic chord in a country which had suffered a national humiliation at the hands of Germany, and would gain him powerful patrons. This optical research project, which continued throughout the 1870's, was the re-determination of the velocity of light using Fizeau's toothed wheel apparatus. Cornu had begun this work in 1871, sending the light over a comparatively short distance (2.5 Km) from the school to the tower of the state telegraph building, but it was in the following year that the work won the backing of the leaders of the Paris physics and astronomy community.

In July 1872, Leverrier discussed in the Academy the question of determining with greater precision than hitherto, the value for solar parallax; the angle subtended by the earth's diameter at the centre of the sun.[22] If this angle is known with precision, the earth-sun distance can be calculated, and then from Keplers 2nd law, all the other distances of the solar system can be found. Laplace had obtained an angle of 8.813 seconds from observations on the transit of Venus, Encke 8.578 seconds, while in 1862, Foucault's terrestrial determination of the speed of light, was used to find the diameter of the earth's orbit from Roemer's method, and yielded a result of 8.86 seconds, a result which also agreed with the astronomical determination of Struve.

Leverrier had no official position in an observatory at this time, although he was to succeed to the directorship of the Paris Observatory for a second term after the death of Delaunay in August 1872, so he was in no position to institute a new programme, he could only appeal to those astronomers who had observing facilities. Following his appeal, Fizeau intervened to suggest that it was now time to make another terrestrial determination of the velocity of light, considering his own toothed wheel experiment of 1849 and that of Foucault with a revolving mirror in 1862, to be simply the first trials which could easily be improved upon.[23] Another contributor to the discussion was the geographer, Abbadie, who expressed the hope that the method used to find the velocity of light would be the toothed wheel method and not the revolving mirror, because the first, having a much greater path, permittted a greater accuracy.[24] This was something of a nonsense, as subsequent experiments by Michelson using a type of rotating mirror, were to show, for the toothed wheel method itself had some very severe weaknesses. Still, the architect of the toothed wheel experiment, Fizeau, was present at the meeting, while Foucault, never a happy collaborator of Leverrier when he was physicist at the Observatory, had been dead for some six years. He was defended , however, by his friend Lissajous who wrote from Besançon to say that the error in Foucault's apparatus was only one part in 500,(not indeterminate as Cornu had claimed), and that Foucault had been working to improve this figure at the time of his death.[25]

But as Cornu had already begun experiments with the toothed wheel, which Fizeau had donated to the Ecole Polytechnique, it was almost inevitable in the circumstances that this piece of apparatus should have been preferred to the revolving mirror, and employing it, Cornu knew he would have the full support of the Observatory and the Academy. He continued with Fizeau's original apparatus, made by the instrument maker Froment, but introduced some modifications. The velocity of rotation was measured by connecting the toothed wheel electrically to a chronograph which recorded every 100 revolutions, while a clock mechanism measured seconds and tenths of seconds. He made the speed increase and decrease in a slow regular way whose period was known, and the observer noted the appearance and disappearance of the reflected light beam in the telescope. This enabled him to calculate the exact velocity of rotation and tended to overcome the main problem which Fizeau had encountered with the apparatus; the uncertainty in deciding when the reflected beam was eclipsed. The intensity of the reflected ray is very reduced, and it is rendered less distinct by extraneous light reflected back from the toothed wheel. Thus it was very difficult to say exactly when the image had reached its maximum or its minimum brightness.

In 1873 Cornu reported that he had carried out a series of experiments over a distance of 10.31 km. and that a mean of a large number of observations gave a value of 298,400 km/sec., which meant a velocity in vacuo (taking the refractive index of air as being 1.0003) of 298,500 Km/sec. This was a value rather lower than Fizeau had found, but quite similar to that of Foucault's. It gave a value for the solar parallax of 8.86 sec.[26]

In the following year the distance was increased to around 23 kms.; the wheel was now capable of turning in excess of 1600 turns per second, and the chronograph and electric recorder constructed by Breguet could measure to an accuracy of a thousandth of a second.[27] The path was now from the terrace of the Observatory to the tower of Montlhery, the two stations which had been used by the <u>Bureau des Longitudes</u> for the determination of the velocity of sound at the beginning of the century and was thus 'linked to the most glorious memories of the history of French science'.[28] The mean result obtained from this series of experiments was rather higher than the previous one; 300,400Km/sec. in vacuo, with an accuracy probably less than one part in a thousand . It yielded a value for solar parallax of 8.878 seconds and was considered the definitive value until Michelson's determination, some ten years later.

Cornu's work won him powerful patrons like Fizeau and Leverrier, and allowed him to be one of the youngest men elected to the physics section of the Academy for many years. When he took up the experiment at the begining of the decade he was just about thirty years of age, with litle published work. By 1878, when he was elected to the Academy at the age of thirty seven, his corpus of work was principally the velocity of light experiments, which with technical improvements and modifications continued from 1871 to 1874, and won him the Lacaze prize in 1877.

Cornu's competitor for the Academy place in 1878, was the *normalien*, Mascart, also a recipient of the Lacaze prize two years previously for his work in optics and electricity, as well as the *Grand Prix des sciences mathématiques* for his ether drift experiments, and some four years older than Cornu. It is therefore clear that there was some peculiarity of Cornu's work or training which particularly favoured his career. Perhaps it was his position as a *polytechnicien* which worked to his advantage, at a time when *normaliens* were beginning to occupy most of the leading positions in the mathematical sciences, but where many of the older Academicians were products of the *Ecole Polytechnique*. But more probably it was due to the fact that Cornu's work struck a patriotic chord at a time when France felt humiliated after the defeat by Germany, and felt, moreover that scientific leadership was passing out of her hands. Her educated public, as well as her savants, could see in the work of Cornu, the continuation of a research programme, in which the Paris Observatory had traditionally been the leader; they could still feel that the velocity of light was still, exclusively, the province of France. Cornu presented his work to the Academy as a heroic national triumph of central importance to the progress of physics and astronomy; and the Academy responded to this patriotic sentiment. Cornu introduced his 1873 memoir to the Academy in the following manner;

> 'It is to be desired, **for the honour of French Science,** that the great work relating to the velocity of light, begun by Roemer at the Paris Observatory, simplified and continued by French savants, should be achieved in France with all the necessary precision which its importance demands.[29]

Given his interest in the question of the propagation of light, it is not surprising that at the end of the 1880's the discovery of electro-magnetic waves by Hertz, should have provoked some response from Cornu. Hertz's experiments, confirming the theoretical ideas of Maxwell, were carried out in 1887 and 1888 as part of a prize competition set by the Berlin Academy of Sciences at the suggestion of Helmholtz. The French Academy reacted promptly and generously to Hertz's discovery, awarding him the La Caze prize in 1889, after his results had been confirmed before both the *Société de Physique* and the *Congrès des Electriciens*, by a young French researcher, Joubert. But doubts remained in France, about the soundness of Hertz's experiment as much as about the rigour of Maxwell's theory. Henri Poincaré pointed out that Hertz had made an error in his calculation of the period of the transmitting spark, employing the formula for a plane capacitor when in fact he had used not parallel plates, but two spheres. Using the value of periodic time so derived, the velocity of the waves would have to be increased by a factor of root two. Poincaré commented out that Hertz's results were so grossly approximate anyway, that there were insufficient grounds to say whether or not light and electro-magnetic waves travelled at different velocities.[30]

The conclusions of Hertz's experiment rested on the fundamental hypothesis that the spark of the transmitter oscillated at a fixed frequency, determined only by the components of the exciter circuit, and that this spark induced electrical oscillations of the same frequency in the receiving wire. Likening the receiving wire to a vibrating elastic column, Hertz found the position of nodes and antinodes in it by means of an auxiliary piece of apparatus, an electric resonator. The period T of the exciter being known, the wavelength of the received wave could be found by measuring the distance between antinodes or nodes, and the velocity calculated from $V = \lambda/T$.

Two Swiss research workers, Sarasin and De la Rive, had found , however, that the period of the receiving wire was not invariable but was affected by the electric resonator which explored it. They did not find this surprising, but simply put it down to the fact that the spark transmitted multiple frequencies and the resonator simply selected the one which corresponded to its own frequency.[31] A note by Cornu, following the communication of the Swiss, took rather too enthusiastically the view that their results were;

> 'Extremely serious for the theory of M. Hertz; in effect the only fixed and incontestable experimental element appeared to be the value of the wavelength of the electrical propagation compared to a well defined period of the exciter.
>
> We now learn today that this wavelength varies with the apparatus of observation; the theory of Hertz is thus caught in the dilemma in which two terms are equally troublesome (facheuse); the experiment shows that $\lambda = VT$ is variable so that either it is the period T which is not fixed and unique, or it is the factor V which varies with the explorer, an absurd consequence as V represents the velocity of propagation of the electrical induction,..[32]

Cornu concluded that the 'curious experimental method' thought up by Hertz should be studied much more carefully before one could consider it as a demonstration of the identity of electricity and light.[33]

Whether any other French research workers took the doubts of Cornu very seriously it is impossible to say, although there does not seem to have been much work in this field in the years immediately after Hertz's discovery. The notable exception, of course, was the professor at the Catholic Institute in Paris, Edouard Branly (1844-1940), whose work will be considered in the appropriate section.

It was the Ecole Polytechnique which could claim the credit for introducing Maxwell's electro-magntic ideas into France, even if it was to give them, as Paul has noted, 'a quasi-Gallic guise'.[34] The school's professors, Cornu, Potier and Sarrau and its graduate Seligman-Lui, were responsible for the translation and publication of the second edition (1881) of Maxwell's treatise on magnetism and electricity which appeared in two volumes, in 1885 and 1889. The translation was made by a young telegraph engineer, Seligman-Lui, and there were annotations to the text by the others. Presenting the translation to the Academy, the mathematician Sarrau, while praising Maxwell's treatise with its many original insights, judged it to be less than perfect as a text book. Cornu therefore annotated the French version to make it clearer for teaching, and Potier clarified some of the particularly difficult concepts. Sarrau himself contributed the appendix on quarternions. Almost apologising for being associated with the book, for he began by asserting that France already had an excellent treatise on electricity by Mascart and Joubert, Sarrau nevertheless thought it useful for physicists and electrical engineers who needed to study the original work of the 'Illustrious Englishman'.[35] The Mascart and Joubert treatise on electricity,(which contained some ideas from the first edition (1873) of Maxwell's work) was compared favourably by Sarrau with Maxwell's, but it is also the one which was so roundly condemned later in the century by Duhem's student, Marchis, when he said it consisted of quantities of material taken without comprehension and assembled without any order, from contemporary foreign textbooks.[36]

In referring to the translation of Maxwell's treatise we have introduced the name of another physics professor of the Ecole Polytechnique, Alfred Potier (1840-1905). Potier, who succeeded to Jamin's chair in 1881, was like Cornu, an engineer of the Corps des Mines. His theoretical interests were concerned with electricity and with ether drift, although he maintained a more practical concern for mining and geology. For example, he gave a speech to the 1877 meeting of the AFas on the geological problems associated with the construction of the channel tunnel.[37] In his view the enterprise presented no major geological difficulties. Potier was elected to the physics section of the Academy in 1891 and continued to teach at the Polytechnique until 1895 when he was succeeded by Henri Becquerel (1852-1908). Following an exhaustive series of experiments by Mascart to detect ether drift in the early 1870's, Potier applied Fresnel's ether drag hypothesis in conjunction with Fermat's principle of least time, to demonstrate that no experimental arrangement of the type employed by Mascart (which we will look at in the section on Mascart's work), could ever reveal the existence of an ether wind. Moreover he reasoned that the wind can never manifest itself optically except by an effect of the order given by u^2/c^2, where u is the velocity of translation of the system and c is the velocity of the light.[38] All the types of experiment employed up to that time, had measured what is called the first order effect: u/c. Thus the result of the efforts of Potier (and others including the Frenchman Boussinesq and the German Veltmann) was to bring about an abandonment of the search for first order effects and to redirect effort towards second order determinations, of which the celebrated Michelson-Morley experiment was the unique example in the nineteenth century.

Michelson and Morley's experiment in 1886, once more drew the attention of the French scientific community back to the ether drift problem, and the results of their experiment were discussed in the Academy. Cornu reported on the experiment in a rather patronising fashion, saying that it was 'executed with the powerful experimental means which the savants of the USA like to deploy in great scientific questions'.[39] It also gave him the opportunity to express, as befitted someone whose meteoric career had owed so much to the patronage of Fizeau, his admiration for the great experimenter who was present at the meeting. Fizeau also spoke briefly, expressing his own continuing interest in the nature and properties of the ether, and announcing that he had plans to use the variations in the force of magnets,(a variation which seemed to him to have a relation to the direction of motion of the earth in space), to yield information on 'the immobility of the ether and the its relation with ponderable matter'.[40] There is however, no record that Fizeau, in his late 60's by now, was ever able to put his ideas into experimental practice.

Although Fizeau was never able to accomplish his ether drift experiment using magnetic techniques, at least he was to report the Academy in 1887 on some ideas he had formulated on the possibiltiy of employing a telescope, an inclined mirror, and a terrestrial light source to detect motion through the ether. The method depended on the proposition that the angle of incidence and angle of reflection of the light on the mirror, passing from source to telescope, would not be equal as they would be for a stationary mirror. In the words of Fizeau;

> 'When the mirror retreats before the incident ray, the reflected ray is closer to the surface (of the mirror) and when the mirror advances towards the ray, the ray goes further away from the surface after reflection'.[41]

It is unlikely that Fizeau's proposals evinced much interest, even in France. Mascart's experiments of fifteen years before had convincd many scientists interested in this problem that it was impossible to measure absolute velocities, and Potier had given a theoretical explanation for the impossibility of measuring it with the type of apparatus Mascart had employed. Although Fizeau was still connected with the Polytechnique as an examiner, the fact that he was not a teacher there had probably distanced him from the theoretical developments in the ether drift problem. On the other hand, a teacher at the school like Potier, confronting an elite student body which expected to receive from its teachers a complete and coherent body of knowledge, had felt the need to try to find a convincing explanation for the failure to detect an ether wind.

But ether-drift and the velocity of light were not the only research projects which interested Polytechnique staff and graduates. Cornu was interested in the role which spectroscopy could play in astronomy and clashed with the physical astronomer, Janssen over the interpretation of certain laboratory experiments. Cornu viewed very intense sparks through a number of different metallic vapours, thus producing their absorption spectra, and believed that he had created in this way, a 'veritable reproduction of the constitution of the sun',[42] an analysis which was soon firmly rejected by the astronomer, Janssen, as being too simplistic.[43] Later in the period, as we shall consider, Cornu supervised research, which Deslandres carried out at the Polytechnique.

Two Polytechnique graduates, both members of the corps des mines , L.P. Cailletet (1832-1913) and Peslin, who we encountered in the Second Empire, also worked in spectroscopy. Cailletet, having his own laboratory was able to take an experimental approach to the subject, while Peslin applied his mathematical skills to finding relationships between spectral lines whose wavelengths had been published in the scientifiic literature. Cailletet examined the spectrum of a gas at very high pressures, and saw that as the pressure was increased, the brightness of the spark which passed between the platinum electrodes through the compressed gas, rose to a great intensity, and then was extinguished.[44] But he made no attempt to explain either the process of conduction through the gas, nor the observed spectral lines.

Apart from the amateur, Lecoq de Boisbaudran, no French spectroscopist hazarded any hypothesis about the mechanism which caused a heated gas to emit specific frequencies of radiation. Some mechanism of vibration, rotation or translation of particles was involved, and this in turn would set the ether in vibration to produce the observed frequencies; this much was generally agreed. However, the method of analogy was widely used in spectroscopic research; the rapid movement of the particles of gas being compared in some way to the acoustical vibration of a string or some other more complicated geometrical shape, and the frequency of the lines emitted were considered to be harmonics of some basic frequency. But such analogies usually broke down when systematically compared with actual observations.

But the other Polytechnique graduate, Peslin, tried an apparently more successful approach when he compared the squares of the wavelengths of the principal rays of the solar spectrum. For example, using the sodium D line as his starting point, Peslin proposed the following relationships;[45]

1. $(L_D)^2 = 3/5(L_A)^2$
2. $(L_E)^2 = 4/5(L_A)^2$
3. $(L_G)^2 = 2/3(L_E)^2$

L is the wavelength of the observed ray.

Taking a mean value of 589.6×10^{-9} metres for the length of the sodium D ray, Peslin obtained values for the A, E, and G rays, which were very close to the observed values, although the difference was becoming larger for the G ray.

Peslin used the analogy of sound to justify his relation between the different wavelengths but it was not the simple picture of a stretched string which had led earlier researchers to look for harmonics i.e. integral multiples of a fundemental frequency. Such a situation, argued Peslin, only held good for transverse or longitudinal waves in strings or in the air columns of wind instruments.

> 'But (elastic theory) shows that this law ceases to be true when the molecular displacements do not have a constant direction in the vibrating body; and what replaces it for the general case, for example the vibration of an elastic prism submitted to any internal displacement whatever,is that it is the square of the elementary vibrations...which are in a simple relationship.'[46]

In this work of Peslin, completed in 1872, he uses a mechanical analogy to justify the mathematical relationship he has found. It is also interesting to note that he was an amateur in this field, just as was Balmer the Swiss shoolteacher, who in 1884 was to formulate the spectral series formula which bears his name. Balmer, however, did not feel the need to make any mechanical justification for the vibrations; to him it was purely a problem of mathematical proportions, not of describing physical reality.[47] Spectral analysis lent itself easily to a study by mathematically trained amateurs; the results were published and anyone could apply themselves to solving the puzzle. Peslin, a Polytechnicien, had already (as we examined in the chapter on the Second Empire) beaten the professional meteorologists of the Paris Observatory in developing a mathematical relationship, incorporating the Coriolis force, between isobar spacing and wind velocity.

Not much work in spectroscopy was done in the remainder of the 1870's and it was only in the following decade that researchers of the Ecole Polytechnique began to add anything to the body of knowledge on the subject. In 1886, Cornu announced that the American physicist, Henry Rowlands had presented the School with an example of his concave diffraction grating. This instrument which could bring a spectrum of very high resolution to a sharp focus without additional lenses, was to be put to very good use by the research assistant working under Cornu, Henri Deslandres (1853-1948). But first we must consider the warning which Cornu was to give in 1885, against carrying the analogy of acoustical vibrations too far in spectroscopic research. He agreed that the analogy was a very seductive one, but that any numerical relationship based on successive terms of a series of whole numbers, always broke down when compared with actual observational data. Appararently unaware that Peslin had come to the same conclusion more than ten years before, Cornu appealed to investigators to get rid of the preconceived idea that there existed a simple law like that of musical harmonics.

> '..This law of whole numbers only applies to a very particular form of sounding body whose type is the cylindrical column whose length is very great compared to its section. If the form of the vibrating body differs from this special type,the relation between the numbers becomes very complex'.[48]

From this we can see that Cornu is not against the acoustical analogy as such, he simply considered it 'puerile'to look for very simple laws, for this would be to attribute ;

> 'to the structure of incandescent molecules,a mechanical constitution which the whole of chemical and physical phenomena hardly justify'.[49]

So this is not a positivist rejection of molecules and atoms but simply a caution against the imposition of preconceived mathematical ideas, and an exhortation to investigators to find some more complex law to connect the regularities of spectral lines. And as Cornu was to supervise the research of Deslandres in spectroscopy in the following years of the decade, it is not surprising that his student tried to utilise a more sophisticated model to explain the relationship between spectral lines.

Deslandres made his mark with two papers published in the Comptes rendus in 1886 and 1887 on the investigation of the band spectra of molecules. In his first paper he examined the relationship between the successive lines in the bands emitted by nitrogen. A discharge was passed through the gas at nearly normal pressures and the glow around the negative terminal examined spectroscopically. The first general conclusion which Deslandres arrived at was expressed as a simple mathematical law;

> 'The intervals from one ray to the next, expressed in numbers of vibrations or as the inverse of the the wavelength, are practically in an arithmetical progression'. [50]

While he continued to use the acoustical analogy to give some physical meaning to his mathematical law, he consigned this to a long footnote. He compared the intervals between the rays in a band and the edge ray, which he said were in the ratio of the square of whole numbers, to the modes of vibrations of a rod which is vibrating transversally, while the relationship between successive edge rays was compared with successive harmonics of the rod vibrating in longitudinal mode. It was prudent to separate the mathematical relationship from the hypothetical physical explanation, so that the former would stand even if the the latter was rejected as being metaphysical or unverifiable.

In the following year he extended his work to include the relationship between bands, and his doctoral thesis of 1888 enunciated a general law relating both to successive bands and to lines in a band. These laws were found to be generally valid, were useful in the classification of molecular spectra, and came to find an explanation in quantum theory in the twentieth century. After being awarded his doctorate in 1888, Deslandres left the Polytechnique to go to the Paris Observatory where the director, Admiral Mouchez wished to strengthen the astrophysical work. Around the turn of the century Deslandres moved to Meudon to work with Janssen and after the latter's death, became the director of that establishment. Thus Deslandres was very much in the Polytechnique astronomer/physicist tradition, only concerning himself with different aspects of optical research.

But it would be wrong to think that the graduates of the Ecole Polytechnique only worked in the field of optics even though this was certainly the predominant one. We have already encountered Gaugain in a previous chapter working on the 'condensing' properties of various materials, and there would be several other polytechniciens producing research in other aspects of electricity in the remaining years of the century. Most of these would be engineers and some of the work was of an applied electrical engineering nature, by men responsible for the state's telegraph and telephone system, for example. It should be emphasised that there were few polytechniciens working in the université system, either in the faculties, where of course most research went on, or in the lycées. Thus we would not expect them to be responsible for much 'pure' research, and in general this is the case, although there is the unusual example of Jules Moutier, the teacher of Pierre Duhem at the College Stanislas.

Moutier published research in the Comptes rendus in the 1870's in which he dealt with the force between two current carrying conductors from a mechanical point of view, regarding electricity as being caused by the vibratory motion of the ether. Taking the ether as a fluid to which it is possible to apply the laws of Bernouilli, and making assumptions about the pressure and velocity of the ether, Moutier succeeded in arriving at known formulae.[51] This is a continuation of work begun in the latter part of the Second Empire when Moutier put forward the hypothesis that the electrostatic charge on a body is a measure of the velocity of ether in that body. In a neutral body the ether has a certain velocity, and accordingly as to whether the body is electrified positively or negatively, this velocity of the ether is either increased or decreased. Moutier used this hypothesis to explain electrification by friction.[52]

Duhem, who as a result of Moutier's teaching left the College Stanislas as a 'convinced partisan of mechanism', said of him that;

> ' He saw the ideal of physics in an explanation of the material universe constructed in the manner of the atomists and the Cartesians'.[53]

It is, however, probably an indication that Moutier's ideas did not find much favour in the highest levels of French science, that although a prolific producer of scientific papers , some 150 in all, most of them were published, not in the _Comptes rendus_ but the _Bulletin de la Société Philomatique_.

But, as we have said, few graduates of the school were teachers, the best directed themselves towards careers as state engineers, and probably the most successful of these in this period was Marcel Deprez (1843-1918). After graduating from the _Ecole des Mines_ in the 1860's he had worked on steam engine design, improving locomotive valve gear, but during the 1870's he turned to the design and manufacture of current meters and small electric motors. In 1886 in an experiment financed by the Rothschilds, he transmitted electrical power from Creil to Paris, a distance of some 60 kilometres.[54] A steam locomotive in Creil, developing about 110 H.P., operated a gramme dynamo, and the power generated was transmitted to Paris at about 6000 volts through a bronze, partially insulated wire of 5mm diameter. At the Paris end another gramme machine acting as a motor, converted the electrical energy to mechanical work. The overall efficiency of the system was about 44%. The trials took place under the eyes of a commission of the Academy (all of whom were products of the *Ecole Polytechnique*) presided over by the mathematician Bertrand, and with the participation of Becquerel,Cornu and Levy. In spite of some windy weather conditions, which caused the suspended transmission cable to come into contact with some telegraph lines, burning out two telegraph machines in the office at St Denis, the trials were considered to be something of a success, although an overall efficiency of 50% had been hoped for.[55] Six years later Deprez reported to the Academy on his experiments transporting power from a hydroelectric station to the town of Bourganeuf,14 kilometres away,employing, as he had before,a D.C. generating system.[56] Deprez, who was elected to the mechanics section of the Academy in 1886, became an early partisan of high voltage AC power transmission.

As *Polytechniciens* were responsible for the state's telegraph and telephone systems, it is to be expected that they would make some contribution to the discussion of the technical problems which emerged in the 1880's in relation to the transmission of the human voice over telephone lines. The requirements of the telephone line, (its need to reproduce intelligible speech) were quite different from those of the telegraph line which needed only to maintain the separation of a succession of simple pulses. Papers on this subject came mainly from two telegraph engineers working together, Mercadier and Vaschy, and it is clear that at least by 1889 they were aware of the need to increase the self inductance of the wire, a question which had split the practical men and the Maxwellian theorists in Britain.[57] Earlier work by Vaschy in the decade had referred to Maxwell's *Treatise on electricity and magnetism* and he too, like Maxwell, turned his attention to the role of the medium in the transmission of ' electrostatic action'. Vaschy used a different, much simpler treatment than Maxwell's and arrived at identical expressions for the tension and pressure in the medium between two electrified conductors.[58]

This work of Vaschy and Deprez would probably not have been considered physics at all. Both men were state engineers and Vaschy was very much associated with the Société Internationale des electriciens, and the majority of his research papers were published in the journal, Lumière Electrique. But their research is worth mentioning here because underpinning it was the physics training of the Ecole Polytechnique, where it was recognised that the progress of electricity at the end of the century made the study of physics more important for engineers and officers.[59] So, whereas in the first half of the century the school was the pépinière of the Paris Observatory, and the study of optics and its application to astronomy ranked high in prestige within the physics programme, the unification of light and electricity by Maxwell (whose ideas entered France through the medium of Polytechnicien translators and annotators), together with the growth of electrical technology which the state engineers needed to dominate, put a similar importance on electricity from the 1870's onwards.

e.The Ecole Normale.

The Ecole Normale would continue in the Third Republic to train science teachers for the secondary school system. To be able to clarify difficult concepts and to illustrate them confidently by practical demonstration, was considered the highest attribute of the teacher, by Bertin, by this time the deputy director of the school. Bertin was, at least by the account of one of his students, Brillouin, a warm, lively man with a passion for whist, beer and operettas.[60] He saw his role at the school as being that of an educator of science teachers, not as someone who should encourage a research spirit among his students. This approach did not entirely satisfy Brillouin, although he did not criticise his teacher for it, because to him, teaching was simply the means of undertaking scientific research. Brillouin, in a way similar to Duhem, recalls with affection, Bertin's 'jesting scepticism'[61] towards mechanical theories. Bertin disliked works of mathematical physics and although, Brillouin tells us, he bought them for the school's library, he did not encourage his students to read them. In this way he obtained Maxwell's Traité de l'électricité and his other works on kinetic theory of gases, but would warn his students in a joking manner, not to let themselves be carried away by their study. Bertin preferred his students to read books ;

> '..of easy theory and difficult experimentation,which he would point out to us in each lecture'[62]

When students left the school they had obtained virtually all their knowledge of physics from Bertin. Brillouin does explain that they attended some lectures at the Paris Faculty, but found that Jamin dealt with questions they had mostly covered in the lycée, while Desains commented on, rather than explained',[63] the optical experiments he had undertaken. Students also attended some lectures of Briot and Bertrand on theoretical physics at the Paris faculty, and some by Levy and Mascart at the Collège de France, but Bertin was by far the greatest formative influence.

On the sudden death of Bertin in 1884, his place was taken by Jules Violle (1841-1923), and another physics post was created and held briefly for about a year by E.M.L. Bouty, who in turn was succeeded by M.L. Brillouin (1854-1948). The research of Violle and Brillouin will be considered in a later chapter, here we will simply make the point that in the period around the turn of the century, when physics underwent rapid transformations in its theoretical foundations, the Ecole Normale had, as its maîtres de conférences two very able physicists, who unlike Verdet and Bertin before them, were still actively engaged in original work. Of the two, Brillouin was the more influential, both as a research worker himself, and as a teacher who comes closer to creating something of a research school in physics. Interested in the history and philosophy of science, and a partisan of the kinetic and atomic theories, he played an important role in the formation of a new generation of physicists who came to maturity in the years around the end the century. This new generation, prominent among which were Perrin and Langevin, was to play a significant part in the new work which was done in radioactivity, ionisation, cathode rays and X rays, etc., around 1900. Much of this work was to be carried out in the laboratories of the *Ecole Normale*, testimony to the fact that by the end of the century the school was not simply training science teachers, but forming 'savants' with a spirit of original research.

But in the 1870's and early 1880's, while Bertin was responsible for physics at the *Ecole Normale* , there was little research done at the school, although Bertin himself, an active member of the *Société de physique* , contributed ten research papers to it in the period 1873 - 1885. His research tended to continue earlier work on polarisation and magnetic polarisation, and some of his contributions were joint papers with the instrument maker Dubosq. Most *normalien* students on the other hand still had to wait until they had left the school before they began their research. The research of these *normaliens*, in all levels of the *université* system, will be considered when their particular institutions are examined.

When Violle came to the school in 1884 from the Faculty of Grenoble, to replace Bertin, he had been working on the determination of the velocity of sound, using the water pipes of that town. Although now based in Paris, he continued this work in collaboration with Theodore Vautier (b. 1852), of the Grenoble Faculty and in 1890 a brief note from the two experimenters was presented to the Academy by

Mascart. The principal conclusion of their paper was that there were no dispersion effects in the motion of sound through air; i.e. all frequencies travel at the same speed. It also concluded that the sharp pulse of sound produced by a pistol shot, although initially travelling faster, progressively slowed down until it travelled at the normal speed of a sound wave.[64] This research field was to occupy Violle for the next twenty years and he was to report on it at the 1900 International Congress of physics.

Although in this period there were young agrégé préparateurs working for their doctorates using the laboratory facilities of the school, their contribution to new knowledge in physics would only become influential in the last years of the century, and will be considered later.

f.The Paris Faculty

The two physics professors, Jamin and Desains, continued in their posts until the 1880's, Jamin also directing the physics research laboratory and Desains the physics teaching laboratory. In the decade up to 1880, it can be seen from table 4.vii,(overleaf) that the number of doctoral theses in topics which could be described as physics and were defended at the Paris Faculty, showed no significant increase over previous decades, despite the establishment of the physics research laboratory. In the following decade,(1881-1890), however, the increase is quite marked, particularly after Jamin's death in 1886 when the chair was taken over by Gabriel Lippmann (1845-1921). Lippmann, born in Luxembourg, spoke fluent German and had worked with Bertin on the Annales de chimie et de physique abstracting German articles for the journal. He worked for a period in the laboratory of Kirchhoff on the subject of electrocapillarity (the variation of surface tension at the meniscus between two liquids when a potential difference is applied across the interface), research for which he gained his doctorate at

Doctoral theses submitted to the Paris Faculty of Sciences.

Table 4 vii.

Doctoral theses submitted in the decade 1871-1880.

	Category										Total	
year	lght	ht	El mag	Vibs. and waves	Gen. Phys.	Phys Total	Ast	Maths	Met Phs	Nat Chem	The-ses	Men
1871	1			1		2	1			1	4	4
1872	1		1		2	4			1	8	13	12
1873	1	1	2			4				6	10	8
1874	1		2		1	4		1		6	11	10
1875			2			2				5	7	7
1876	1		2			3	1	5	1	8	18	15
1877	2					2	1	5	1	10	19	15
1878		1				1	1	4	2	8	17*	14
1879	2		1	1		4	2	6	1	11	24	22
1880			2	1	1	4	1	6		12	23	22
Total	9	2	12	3	4	30	7	27	6	75	146	129

Physics papers as percentage of total 20.5%
Of the physics theses; 30% light,40% electricity,
13% general physics, 7% heat,
10% vibrations and waves.

Theses submitted in the decade 1881-1890

	Category										Total	
year	lght	ht	El mag	Vibs. and Waves	Gen. Phys	Phys Total	Ast	Maths	Met Phs Globe	Nat Chem Med	The-ses	Men
1881	1		2			3		1	2	11	17	16
1882	1	1	1		1	4	1	5	1	13	24	24
1883						0		3	1	6	10	9
1884	1	1				2	1	1	1	15	20	20
1885	1		1			2		5	2	18	28*	28
1886	1	2	3			6	2	4	1	19	32	31
1887		2				2	2	3	2	17	26	26
1888	3	1	4		2	10		3	2	22	37	37
1889	1	1	3		1	6		2	2	23	33	33
1890	2	1	3		2	8		4	4	23	39	38
Total	11	9	17		6	43	6	31	18	167	266	262

Physics papers as percentage of total 16.2%
Of the physics papers; 26% light, 40% electricity,
14% general physics, 21% heat,
0% vibrations and waves.

* Indicates that a thesis on a proposition given by the faculty was submitted.

Source. Maire (note 78, ch. 2).

the Paris Faculty in 1875.[65] From the 1890's onwards he tended to concentrate on work employing interferometric methods in the production of colour photographs, work for which he was to win the Nobel prize in the twentieth century. Both from Bertin and from Kirchhoff he assimilated a generally positivist approach towards science, and he was above all an experimentalist who appeared to be unmoved by the ferment of ideas generated in physics at the end of the century.

The other chairholder, Desains, as we saw in a previous chapter, worked with a number of collaborators, including Edouard Branly and Pierre Curie. Branly was first Desain's chef de travaux , and then his deputy director in the teaching laboratory until he left to make his career in the Catholic Institute in Paris, after a personal disagreement. Branly's place was taken by another normalien, J.L Mouton (1844--1895) who had been awarded his doctorate in 1876 for his research on oscillatory circuits.

In 1885, E.M.L Bouty (1846-1922) succeeded to Desain's chair. Bouty's career follows the old-fashioned pattern of the normalien who has to spend his early years as a lycée teacher, begining in the provinces. It had already become rather unusual for someone to spend some 15 or more years of his career in secondary education before gaining a faculty post. Bouty taught first in Montauban and then in Rheims, and during this time he worked for his doctorate which he gained in 1874. From 1876 to 1883 he taught at the prestigious Parisian lycée of St. Louis and continued to do research there in magnetism and various different aspects associated with electrolysis, and was a frequent contributor to the meetings of the Société de physique. Electrolysis was possibly his favoured area because it was fairly inexpensive and because, being on the borders between physics and chemistry, it fitted best his role as a teacher of physical science. All of Bouty's research was in the fields of magnetism, electricity and electro- magnetism, and he was to gain the Lacaze prize in 1895 and was elected to the physics section of the Academy in 1908.

In the early part of this period, Jamin and some of his students worked on electrical calorimetry, constant flow methods for finding the specific heat of gases and liquids, and electrical methods to determine latent heat. Jamin's interest in electricity; (it was he who introduced the ring armature generator of Zenobe Gramme to the Academy in 1871[66]), obviously influenced the research direction of doctoral candidates in this period, and electricity became the most popular field of investigation. We can say that in most of the very extensive research into the various aspects of electricity and magnetism, the French were looking for laws not causes, and this is particularly true of doctoral theses. The examiners of the Faculty expected to judge theses which were well executed experimentally, and firmly based on existing paradigms, not ones which speculated and hypothesised excessively about underlying causes. Doctoral theses measured relationships between observable phenomena, and if possible linked them by mathematical laws; the causes of these phenomena were not their concern. This positivist tradition is very clearly illustrated in the work of J.M.R. Benoit (1844-1922) who presented his doctoral thesis in 1873, and who later went on to become the director of the Bureau Interational des Poids et Mesures. The title of Benoit's doctoral thesis was 'Etudes experimentales sur la resistance electriques des metaux et sa variation sous l'influence de la temperature', and a summary of the conclusions of the thesis was published in the Comptes rendus in the same year.[67] Benoit found the resistance of various metals over a temperature range zero Celsius to 860 C, using as his fixed points;

1.Boiling point of water 100°C

2.' ' ' ' ' ' mercury 360°C

3.' ' ' ' ' ' sulphur 440°C

4.' ' ' ' ' ' cadmium 860°C

Representing the curve of resistance against temperature by the formula,

$$R_t=R_0(1+ at + bt^2)$$

Benoit plotted a curve for each metal using the method of least squares, and found the value of the coefficients a and b. It was work which, without being particularly imaginative, helped the establishment of electrical science on a solid empirical basis. Benoit, carrying on the French tradition of precise measurement, determined the resistance/temperature relationship to the highest levels of accuracy possible at the time.

Doctoral candidates did not necessarily use the laboratories of the Paris Faculty but used the facilities of the insitution in which they were employed. L. M. Brillouin an _agrégé_ _préparateur_ at the _Collège de France_, having found in Maxwell's treatise on electricity and magnetism;

> '..a succint indication of a method of comparison of the coefficient of induction of coils, I undertook to study the conditions in which these methods are correct and exact.'[68]

This research, carried out in the _Collège_, was probably more influenced by its physics professor, Mascart, who had included some of Maxwell's ideas in his textbook on electricity, rather than by the Faculty staff. Brillouin successfully defended his thesis in 1882, and reported on this research to the _AFas_ meeting of the same year.

But there was research going on throughout this period in the teaching laboratory of the Faculty. Pierre Curie (1859-1906) had gained the _licence_ in physical science in 1877 and in the following year was appointed Desains' assistant in the teaching laboratory. After some collaborative papers with Desains, Curie embarked on a programme of research on the physics of crystals, carried out with his elder brother Paul-Jacques (b. 1856), under the direction of the mineralogist Friedel. Paul was to gain his doctorate in 1888 and become professor of mineralogy at the Faculty of Montpellier.

The first joint publications of the brothers in the Comptes rendus, had appeared in August 1880, and was followed in early 1881 by a paper which summarised all their findings, and gave general laws for what was to become known as the piezo-electric effect. This effect, the exhibiting by certain unsymmetrical crystals, of a potential difference across their faces when subject to pressure, was discovered by the brothers when they were investigating at Friedel's suggestion, an older known effect of crystals. This other effect, known as pyro-electricity, relates to the appearance of electric charges across the faces of the crystal when it is heated. In a paper published in 1881,the laws governing piezo-electric E.M.F. appear for the first time.[69] These laws can be summarised by saying that the E.M.F. produced when a tourmaline crystal is compressed is proportional to the variation in pressure and independent of the dimensions of the crystal. This work was presented to a meeting of the Société de Physique later in the decade.

In 1882 Pierre Curie moved to the Ecole Municipale de Physique et de Chimie Industrielles where he was first appointed to be the director of laboratory work and then in 1894, professor of physics. His later researches, first on magnetism and then on radioactivity, which will be considered in the next chapter, were carried out at the school.

But it was not only in Friedel's laboroatory in the Faculty, that research was undertaken. A young Russian, Stoletow, working in the physics teaching laboratory began,in the late 1880's to work in a new research field which had been opened up by Hertz's experiments on electro-mgnetic waves. In the course of his experiments Hertz had found that a spark crossed a gap more readily if the gap was illuminated with ultra-violet light. While Hertz and others had used induction coils to produce potental differences across the gap of many thousands of volts, Stoletow investigated the effect using only low voltages. He connected one electrode of a cell to a metal disc which was separated by a thin layer of air from a metal grid which was connected to the other electrode of the cell through a Thomson galvanometer. If the metal disc was connected to the negative pole of the cell and illuminated with ultra violet light, a current was seen to flow through the galvanometer. If the disc was made positive there was only a very small deviation in the galvanometer. Stoletow concluded that the 'illuminated air acquires a sort of unipolar conductivity'.[70] Cleaning the face of the metal disc increased the effect, as did increasing the intensity of the source of ultra-violet. Stoletow tried a number of different metals for his illuminated disc; but aluminium was the most effective, then zinc and lead, which made the researcher think that the those metals whose spectrum was richest in ultra-violet gave rise to bigger currents. He also observed that they were the most positive metals in Volta's (i.e the electrochemical) series.

It is an index of a new receptivity to foreign research in France that a doctoral student could adopt for a research project, work which was very much on the moving frontier of science, although of course the majority of doctoral research tended to be, like that of Benoit, on more firmly established theoretical bases. We will see later that research students of the Paris Faculty were to be very prominent in the new revolutionary areas of physics which opened up at the end of the century.

Before leaving the Paris Faculty,the research of the <u>préparateur</u>

Louis-George Gouy (1854-1926),who worked under both Jamin and Desains, will be examined. Gouy obtained his doctorate in 1879, with a thesis presented to the Paris Faculty on the photometric analysis of coloured flames. After a period of work under Jamin, which terminated after a disagreement over the interpretation of the 'black spot' observed in a soap film just before it breaks, Gouy became a préparateur in the Paris Faculty laboratory of Paul Desains.

Gouy was both a theoretician and an able experimentalist with an interest in some of the more obscure and neglected aspects of optical research, and in 1880 he addressed himself to the question of the velocity of light through a dispersive medium. In a dispersive medium, and air must be considered as feebly dispersive, the different wavelengths which constitute white light will travel at different velocities. Thus there is the question of what really is observed, when one talks of the 'velocity of light'. Roemer's observations on the moons of Jupiter, and the much more recent velocity determinations first of Fizeau and then Cornu, observed intermittent pulses of light. Gouy argued that;

> '..if the milieu is endowed with dispersive properties the amplitude is transported with a velocity which is not that of the waves...(this would not take place)..if the medium lacked dispersive properties, but this example is enough to show the necessity of not being restricted to such media, and to treat the problem from a more general point of view'.[71]

This treatment first of all encountered the opposition of Cornu, who perhaps felt that his own determination of the velocity of light was under attack, and then a few years later its originality was questioned by the English physicist, Lord Rayleigh. In a note to the Academy in 1883, Rayeigh claimed that his 1877 book Theory of sound had first carried the mathematical expression for the velocity of a pulse, which Rayleigh called the 'group velocity' to distinguish it from the wave velocity. But in his book, Rayleigh, although applying his analysis to a number of cases, had not applied it to light, because, as he explained; 'nobody had supposed that the velocity varied with wavelength'.[72] Later, the experiment of Young and Forbes in England, in 1882, showed that it did so vary. Not surprisingly, Gouy rejected Rayleigh's priority claim, insisting that he knew nothing of his work on sound, and that in any case the treatment applied there could be applied to light only after a considerable amount of research and investigation into the properties of a light pulse, had been made. The problem was quite a different one from that dealt with by Rayleigh originally, and he had only extended it to light after the Young and Forbes experiment, which of course, appeared after Gouy's 1880 paper.[73]

Gouy continued his work in this area for the next few years, extending his treatment to deal with the method employed by Foucault to find the velocity of light; the turning mirror. He showed that his 'group velocity' conclusion was valid here as well, and then used a spinning mirror type of apparatus to show the difference between the velocity of red and blue light in carbon disulphide. This substance was used because of its known high dispersive power. Michelson had already shown with white light travelling through carbon disulphide, that Gouy's expression for the velocity of a pulse in a dispersive medium gave better agreement with experimental results, than the previous, more simple treatment of wave velocity.[74]

Gouy moved to the Faculty of Lyons in 1888 and remained there for the rest of his life. In the same year he published in the Journal de Physique, a short article on Brownian motion, the random motion of pollen particles in water viewed through a microscope, which had first been observed in 1827 by the English botanist Robert Brown.[75] Although it cannot be said that this insight of Gouy owed its genesis to the Paris Faculty, and is independent of any particular institution, it appears to have been formulated while he was still working there and is therefore included in this section. Gouy was of the opinion that the phenomenon of Brownian motion had not been sufficiently studied by physicists, and that it raised many important and interesting questions in thermodynamics. Gouy realised that the observed motion was not caused by convection currents or vibrations of the particles themselves, but that they were being agitated, jostled one might say, by the internal agitation of the liquid. Not that such an idea was new, for Maxwell, addressing a BAAS meeting in 1873, pointed out to his audience that Lucretius explained that the random motion of the motes in a sunbeam;

> '..is but a result of the far more complicated motion of the the invisible atoms which knock the motes about.'[76]

Gouy's conclusion on the movement of the pollen particles, echoes that of Maxwell fifteen years previously;

> 'Brownian motion shows us, therefore, certainly not the movements of the molecules, but something which is very close to this, and furnishes us with direct and visible proof of the correctness of present hypotheses on the nature of heat. If one adopts these views, then this phenomenon, the study of which is far from being finished, most certainly takes on an importance of the first order for molecular physics.'[77]

Whether he had heard of Maxwell's speech, or knew the specific reference to Lucretius, ones does not know, but it is certain that it was Gouy's article which was to influence later French experiments. It would not be, however, until more than fifteen years later that an experimentalist (apart from Gouy himself) took up this idea, when Jean

Perrin began his series of experiments which were to provide direct evidence for the real existence of molecules.

9.The provincial faculties.

The striking feature of this period is the growth of research from the provincial faculties and although some of it was done by young 'birds of passage' whose ambitions were focussed on Paris, there is much which is the work of men who were content to remain in the provinces. Indeed, in this period, some provincial faculties were begining to attain national recognition of their supremacy in particular areas of expertise. On the other hand it must be said that the prestige of the provincial doctorate remained low, and those who wanted to make a career in the Faculty system, still required a doctorate from the Paris Faculty. For this reason the number of doctorates awarded by the provincial faculties remained very low (35 in the period from 1871 to 1890 of which only three were in the area of physics).[78]

If we were to judge the importance of a faculty within the system, by the number of doctorates it awarded in this period, Montpellier with ten, would be the leader. From the point of view of research published in national journals and presented to national meetings we have to look elsewhere, and the Faculty of Nancy, already referred to in the section on the AFas, would rank very highly. It was in the 1880's that its professor of physics, Blondlot, began the research which was to enhance the prestige of Nancy and lead him to be elected a corresponding member of the physics section of the Academy in 1894, and to win the prestigious Lacaze prize in the early twentieth century. In 1888, Blondlot and his colleague Bichat, following research by Hertz in Germany, began research on the effect of light on the conductivity of the air gap between two charged conductors. However, instead of irradiating a metal plate they used a vertical layer or 'curtain' of water. They found that whether the water was connected to the positive or the negative terminal of a cell, whether the water was in motion or stationary, there was no effect. If they used a metal disc and employed their curtain of water simply as a screen between the source and the disc, a current was seen to flow.[79] The conclusion drawn was that ;

> "The transparency of water for the effective rays is perfect'.[80]

The further conclusion that, therefore, the rays which caused the effect were not 'calorific rays' (infra-red), contributed little new to a discussion in which Hertz, Wiedermann, Ebert and Hallwachs in Germany had already published that it was the other extreme of the spectrum which affected the metal surface.

Blondlot was a fairly prolific research worker at this time with many of his papers finding their way into the Comptes rendus, and some into the Annales de chimie et de physique. But if it must be said that his experimental arrangements were often ingenious, his choice of subject was sometimes conservative, simply submitting to new experimental test, questions which had long been settled. His experiment to show the conductivity of heated air for example, was concerned with defending an accepted corpus of knowledge; the conclusions which Edmond Becquerel had arrived at (see chapter 2) some twenty five years before. When Blondlot makes the claim that his experiment put the work of Becquerel beyond all doubt, one cannot avoid the feeling that this was directed more towards securing the good-will of a figure already well established in the French scientific community, rather than advancing new knowledge. Certainly Blondlot made no new conjectures concerning the hypothesis of conduction in the heated gas, not even re-examining the suggestion which Becquerel had advanced (and rejected) that the heated cathode itself emits charge carriers.[81]

In this decade Blondlot also did some work on dielectric double refraction; the rotation of the plane of polarisation of polarised light when passing though certain dielectric media in a powerful electric field. Blondlot's experiment, to see if the optical rotation took place simultaneously with the applied field, added little to the knowledge of the phenomenon which had first been discovered by Dr. Kerr in 1875.[82] Working with his colleague, Bichat, and using the skills of a Nancy instrument maker he developed a new type of electrometer which could measure very high voltages, but it does not seem to have gained much popularity. It was in the following decade that Blondlot made the Nancy Faculty the leading centre in France for the study of 'Hertzian' radiation.

In this period another provincial faculty, that of Marseilles, became the centre for the precise measurement of length, based on interferometric methods, through the work of its physics professor Mace de Lepinay (b.1851). The polytechnicien, Charles Fabry (1867-1945), was also to work there from 1894, devising with Alfred Perot, the interferometer which bears their names and which was used for the determination of a series of wavelengths of light which were used as international standards.[83] De Lepinay, having successfully defended his doctoral thesis on double refraction in 1879, began to work, using inteferometric techniques which were already known but which he applied in new ways, to measure very small lengths with greater precision than ever before. For example in 1885 he measured the thickness of a thin sheet of quartz using a method based on the observation of Talbot's fringes. Some half century before, the Englishman H.F. Talbot had observed that if a spectrum is viewed though a hole , one half of which is covered by an extremely thin sheet of glass or mica, the spectrum will appear covered along its entire length with fine parallel dark bands. The wavelength corresponding to the centre of each of these dark bands is related to the thickness of the sheet and the refractive index of the material for the corresponding wavelength. Using this technique, de Lepinay measured the thickness of a piece of quartz, of about 4 mm thickness to an accuracy of plus or minus 0.000 001 cm.[84] These results were presented to the Academy in the form of a short note, by Mascart.

Mascart presented another note to the Academy on a similar line of research, for de Lepinay in the following year. Having shown that he could measure thickness to an extraordinary degree of accuracy, de Lepinay went on to use a modification of the same method to measure the wavelength of a particular wavelength of light. A number of researchers, including Mascart, had measured the wavelength of the sodium D_2 line using diffraction gratings, but there was a small but significant difference between the results obtained by different experimenters. The discrepancies came from the difficulty in constructing diffraction gratings, and so de Lepinay devised a method which could dispense with this piece of apparatus. His previous paper had shown how he had used Talbot's fringes to measure length and he now used the same method to construct, or rather to employ a precision glass worker to construct for him, a cube of glass of about $1cm^3$, the dimensions of whose sides he knew exactly in terms of an integral number of wavelengths of sodium D_2 light.[85] Thus the volume of the cube could be found in terms of the wavelength. Next, de Lepinay weighed the cube in air and then in distilled water, the loss of weight being the weight of the displaced water and hence the volume of the cube could be found, by the relationship between the milli-litre and the gramme. In this way the wavelength of sodium D_2 light was found to an accuracy of five significant figures. De Lepinay had the cooperation of members of the International Bureau of Weights and Measures in measuring this weight loss very accurately.

While the importance of precise measurement in the development of physics should not be underestimated, and the French were the masters of this science, it does seem to be work which lacks imagination. It is true that the work of De Lepinay , and later Fabry and Perot, was very ingenious, but its seems to indicate a feeling , on their part, that physics was complete and that all that remained was to increase the accuracy of already measured parameters.

Other faculties also began to establish for themselves, special areas of research and expertise. Among these we could mention Lille, which through the work of its physics professor Terquem, specialised in acoustics research, or Montpellier where Crova specialised on work on solar radiation. The prize competitions set by the Academy had a certain influence on the direction of the research effort of phsyics staff in the provinces. We have already seen that one of the Academy's prize competitions in the Second Empire directed attention towards the mechanical theory of heat. Another prize, the Bordin prize for the mathematical sciences, was set in 1874 for a problem relating to heat which was closer to the traditions and interests of French physicists, for it dealt with radiant heat. This competition required of the contestants that they carry out new calorimetric observations and discuss and possibly incorporate old ones, in order to try to find the true temperature of the sun's surface.[86] There were two separate problems involved in the solution of the question; the first was to measure the quantity of heat falling each second on unit area of the earth's surface, and the second was to relate this quantity to the temperature of the sun's surface.

In 1838 Pouillet had submitted a memoir to the Academy, in which he described a new experiment to measure the rate at which heat arrives at the earth's surface (known as the solar constant), using an instrument which he had devised and called a 'pyrheliometer'. Pouillet computed the amount of heat arriving at the earth's surface, made some correction for the amount which had been lost traversing the earth's atmosphere, and then by simple geometry, calculated the rate at which heat was being lost per unit area of the sun's surface.[87] Similar measurements had been made in Britain in the first half of the century by Forbes and John Herschel.[88] To relate the rate of heat lost from the sun's surface to its temperature, Pouillet had two empirical formulas to choose from; the older was Newton's law of cooling, which connected the rate of heat lost to the excess temperature of the body,and the second was the exponential law of Dulong and Petit which they had submitted to an Academy competition in the early years of the century. Choosing the law of Dulong and Petit, because they had shown in their research that Newton's law does not hold good for large temperature differences, Pouillet arrived at a temperature for the sun's surface of between, 1471 C and 1761 C. But later researchers had insisted that this must be too low because a burning glass was known to be able to melt platinum, and the sun surface could not be at a temperature lower than it produced on the surface of the earth. Thus, in the second part of the century, the problem still remained to be solved, and in addition the validity of Dulong and Petit's exponential law of cooling was also at stake.

In fact there was only one candidate for the competition; Jules Violle, at this stage of his career teaching at the Grenoble Faculty, and his work, while not meriting the whole prize, won 2,000 francs. Two other provincial research workers, who had not entered the competition but had made contributions in the field, Crova, of Montpellier and a regular participant in the AFas and J.M.Vicaire (b.1839), at this period professor of chemistry and metallurgy at the Ecoles des Mines in St. Etienne, were both given prizes of 1,000 Francs each.[89] Violle and Crova had both done some new experimental work on measuring the solar constant; Violle with a colleague had made simultaneous measurements using blackened thermometers, at different altitudes on Mt. Blanc, while Crova, working at a single level, had employed almost identical apparatus to that used by Pouillet, thirty years previously. Both arrived at empirical formulae for relating the thickness of the air layer to the absorption of the incoming heat. Crova did not attempt to link his value of two calories per minute per square metre arriving at the limits of the atmosphere, to the sun's temperature, and the report on Violle's work, said little about this question except that Violle 'could not escape the uncertainties inherent in the very principles on which he had to be based'.[90] The chemistry professor, Vicaire, did no experimental work on the subject, but made a mathematical treatment employing Dulong and Petit's law. Starting with Pouillet's value of 1.75 calories per min. per square metre, he arrived at a value of 1300°C, for the sun surface temperature, a value which he knew to be too low.

Although it might be said that none of the three provincials had been able to contribute anything new to the discussion, the Academy had directed their attention to a problem which had been neglected over the previous thirty years,(and still seemed to evince no interest in the capital) and which it considered was important to solve. Both Violle and Crova were able to take advantage of their geographical locations and collect data with comparitively inexpensive apparatus, but both lacked an adequate theoretical relationship between the temperature of an emitting surface and the rate at which its radiates energy. One can infer that it was this lack of a relationship between the rate of energy radiated and temperature (or at least one which could arrive at a value for the sun's surface temperature which was not clearly in error) which made the subject unattractive for most research scientists. Even after the enunciation of the fourth power law by Stefan in 1879, which was shown to give better agreement with the earlier data of Provostaye, Desains and Despretz than could be achieved using Dulong and Petit's law, little interest in this field of research was shown in France.

h. The Collège de France and the Muséum

As we considered in a previous chapter, Regnault at the College had carried out a series of extremely precise experiments on the velocity of sound. His successor, Mascart, began in the last years of the Second Empire an equally painstaking programme of research to try to detect the presence of the 'ether-wind'. This question of the measurement of ether wind, was of the highest importance to the French scientific community. It was of astronomical interest, affecting the question of stellar aberration, it allowed the French to employ their considerable skill in precise measurement using interferometric techniques, and it could furnish further corroboration for the Fresnel wave programme. Experiments by Arago and Babinet, earlier in the century failed to detect any ether wind, and the experiment by Fizeau was inconclusive. In 1870, the Academy was to direct the attention of its physics community towards the solution of this problem by the setting of one of its most prestigious prize competions; the Grand prix des sciences mathématiques. To be judged by the mathematicians, Liouville and Bertrand, and the physicists, Jamin, Fizeau and Edmond Becquerel, the competition sought to examine experimentally how the motion of source and observer modified the properties and mode of propagation of light. [91]

No prize was awarded during the period of the Second Empire, although Mascart was given an encouragement of 2500 francs. Two years later,there being no other entries, and Mascart's researches being of a very high order of precision, he was awarded the prize. The results of his work were published ,in full in two issues of the Annales scientifiques de L'Ecole Normale Supérieure, those of 1872 and 1874.[92] Mascart's articles began with an historical treatment of all the the past unsuccéssful attempts to measure an ether wind, and some of these were repeated and new ones attempted. Fizeau's experiment on the rotation of the plane of polarised light through a pile of plates, measured at different times of the day and the year (see chapter 3.) was repeated using a quartz crystal, with negative results. Mascart also repeated the experiment of the Dutchman, Martin Hoek, using a very precise piece of interferential apparatus; the refractometer developed by Jamin. Again the results were negative.

In this experiment a light beam was split into two, and one beam was sent through a tube of stationary water in one direction, and then after two internal reflections by a prism, the beam of light came back through air on a parallel path to the observing telescope. The other beam traversed exactly the same path but in the opposite direction before entering the telescope. So if there is an ether wind carried along by the water, one beam traverses it in the same sense as the 'wind' and the other beam against it. If the ether wind produces a path difference, rotating the whole instrument will produce a fringe shift, but Mascart could never observe any movement even as small as one tenth of a fringe.[93] The results of all his painstaking experimental work led Mascart to what was a rather disturbing conclusion for the theory of the luminiferous ether;

> 'The general conclusion of this memoir will therefore be (if one leaves out the experiment of Fizeau on the rotation of the plane of polarisation by a stack of glass plates) that the movement of the earth does not

> have any appreciable influence on optical phenomena produced with a terrestrial source or with solar light. These phenomena do not give us the way to demonstrate the *absolute* movement of a body, and the only ones that we can make evident, are *relative* movements.[94]

The tradition of research at the *Collège* , the tradition of Biot and Regnault, was thus ably continued by Mascart. However, his research activity slackened after he was appointed director of the *Bureau Centrale Meteorologique* in 1878, and the role of the *Collège* as a generator of new knowledge declined in this period. As for an explanation of Mascart's results we have seen in the section on the *Ecole Polytechnique* that Potier gave a theoretical explanation for the failure of this type of experiment to detect an ether wind, as did the Lille Faculty professor V.J. Boussinesq (1842-1929) using different reasoning.

Research at the *Muséum* continued to be in the hands of the Becquerels. Although the work of Antoine-Cesar was at an end by this time, his son Alexandre-Edmond was still active, and the third generation Becquerel, Henri (1852-1908), began research around 1875. Henri had been trained at the *Ecole Polytechnique* and entered the *Corps des Ponts et Chaussées* and worked very much in the tradition of research established by his father and grandfather; rotation of the plane of polarisation in magnetic fields, and phosphorescence and luminescence. He obtained his doctorate in 1888 and in the following year was elected to the physics section of the Academy. By the end of this decade, with teaching posts at the *Ecole Polytechnique* and the *Muséum*, (*aide naturaliste*), and as an *ingénieur de première classe* in the *Corps de Ponts et Chaussées*, and with a body of research behind him which appeared important in the judgement of the leaders of the French scientific community, Becquerel effectively abandoned research. By the early years of the 1890's Becquerel had succeeded to his father's physics chairs at the Muséum and at the Conservatoire, and by 1895 had succeeded Potier to the physics teaching chair at the Polytechnique . Thus his heavy administrative and teaching duties seemed to preclude any further original research until his chance discovery of radioactivity in 1895 stimulated a new burst of creative activity.

i.The Catholic Institutes.

In a number of works, Harry Paul has studied the Catholic Institutes,[95] and all that will be added in this thesis is a closer examination of some of the reseacrh in physics which was carried out in them. Established in 1875, at the cost of great sacrifice by the Catholic population of France, the primary aim of the Catholic Institutes was to produce licenciés to provide teaching staff for the Catholic secondary schools. Their importance should not be over- emphasised; the overwhelming majority of parents and even many devout Catholic ones, continued to prefer to send their sons to the State faculties. In the first ten years of their existence the Catholic Institutes produced only 128 licenciés in the sciences, and more than half of these came from the Paris Institute.[96]

What concerns us here is that the Institutes gave employment and restricted research facilities to a small number of talented scientists who work in the field of physics. At the Lille Institute the physics professor was the A. Witz, a centralien engineer by training, who did some work on steam and gas engines and wrote an influential treatise on thermodynamics which went through four editions between 1872 and 1924.[97] Lille was the most important Institute after Paris and its dean of the sciences Faculty was J. Chautard (1826-1901) who had spent many years in the state system, most of them in Nancy. By the time he went to Lille he had a respectable body of research behind him, mostly in electricity, including some work on the conduction of electricity through low pressure gases. But the two most important physicists associated with the Catholic Institutes were without doubt, Branly working in Paris and Amagat in Lyons.

Both of these men had already been working in the state sector, and both, for differennt resons were pleased to have the opportunity to leave it. Branly had been working under Desains at the Paris Faculty , but left after a personal disagreement and was offered 80,000 francs to establish a physics laboratory and organise the teaching in the Catholic Institute of Paris. He worked there for nearly fifteen years , doing fairly undistinguished research before the discovery of electro-magnetic waves by Hertz redirected his research activity and led in 1890 to the discovery of the 'coherer' on which his reputation rests. This device consisted of a glass tube containing metal filings whose resistance fell from some high value (perhaps millions of ohms) to a few thousand ohms or less when affected by a nearby spark.[98] In his first paper on the subject, Branly reported that the effect occurred when the spark was 20m. away and when spark and 'coherer' were in different rooms, although the full significance of the device as a sensitive detector of electro-magnetic waves was not at this moment appreciated. All of Branly's subsequent career was spent at the Paris Institute working in an under-funded laboratory which had been declared temporary when he joined it in 1875. In competition with Marie Curie for a place in the physics section of the Academy in 1911, his scientific credentials were considered superior and he won the election.

The other distinguished physicist in the Catholic Institutes, Amagat, had been working at the normal school at Cluny, but he accepted the offer of a physics post at the newly established Institute of Lyons which brought him 50,000 francs to establish the physics laboratory and the services of the Lyons crafstman, Benevolo as chef de travaux.[99] It was Benevolo who was responsible for the precision engineering of Amagat's apparatus, which allowed him to extend the investigation into the behaviour of gases up to extremely high pressures.

Before arriving at Lyons, Amagat had maintained a constant stream of research papers to the Comptes rendus examining the gas laws in regions of ever higher pressure and temperature. He had redetermined the value of the ratio of the specific heats of a gas using Clement and Desormes method, employing some corrections which Cazin had proposed. With this method he arrived at a value of 1.397, from which he calculated the mechanical equivalent of heat to be 434 kilogram-meters per calorie.[100] He achieved high pressures by screwing a piston into a water filled cylinder, the method used by Andrews in 1860, and measured these pressures by means of mercury manometers of enormous length.

While at Lyons, he mounted apparatus made by Benevolo in a mine-shaft, 300 m. deep, and after an accident there transferred it to a cliff side where the military fort of St. Just was situated above the river Saone.[101] The apparatus, made up of many jointed sections, was able to withstand pressures of up to 550 atmospheres without leaking. Amagat also investigated departures from Boyle's law at very low pressures (6.5 mm. to 10.5 mm.) and at different temperatures.

In none of his communications to the Academy did Amagat ever hazard any explanation of a mechanism for the behaviour of the gas under test; he was simply content to observe the phenomena over as wide a range as possible and with the greatest possible precision. In this respect he displays a positivist attitude; for him, precise experiment leading to mathematical expressions linking the observations, represented the limit of legitimate scientific enquiry. Amagat left the Lyons Institute in 1892, and went to teach in the Ecole Polytechnique, and although Branly continued with some work on the propagation of radio waves, the modest role of the Catholic Institutes in the production of new knowledge in physics came virtually to an end.

It would be tempting to believe that those men who had opted to teach in the Catholic Institutes , and whose chance of ever making the transition back into the state sector of higher education was remote, might have felt less constraint in their choice of research topics, and might have gone outside the well trodden paths so beloved by those intent on making their career inside it. There is hardly enough evidence to justify this idea, however, because while both Branly and Amagat pursued research which was somewhat peripheral to the principal interests of France's community of physicists, Amagat had begun his within the state sector, and the elctro-magnetic research of Branly was also being carried out by other French scientists, notably by Blondlot of the Faculty of Nancy.

j. Physics at the end of the 1880's

Before going on to examine the work in physics during the last decade of the century, a decade which would be both enormously fruitful and, at the same time, destructive of much of the accumulated body of physics knowledge, it would be useful to pause and take stock of the situation in the science, and the state of mind of its practictioners at the end of the 1880's. Is the feeling that physics was running out of steam, that its paradigms were reaching exhaustion, simply an imposition by a researcher coming to the field a century later and making a judgement based on contemporary knowledge ? Was there a feeling, in the French scientific community, that there was little new to discover, that physics was now to be simply an activity of improving the accuracy of constants and relations already found ? It has been argued by Laurence Badash,[102] that the 1880's saw the growth of a pessimistic sentiment of 'completeness', among, not only physicists, but astronomers and other natural scientists, in Britain and the USA, although this position has been disputed by others,particularly Stephen Brush. [103]

There is very little evidence of this sentiment in France, at least in relation to any feeling of gloom or pessimism. Much of the physics community had always found it more congenial or prudent to describe phenomena and to measure relations between them, rather than to seek causes or explanations. For these physicists, the ever increasing precision of this description, without discussion of mechanisms, was the very essence of scientific activity. Thus one would not expect any gloom in France at the prospect of 'completeness' especially as science as a 'next decimal place' activity, could never be repugnant to a community which revered the exquisite precision of its Regnaults, Foucaults, and Jamins for example. What else but a 'next decimal place' activity, was the continuing work of Cornu on the velocity of light, or Violle on the velocity of sound, or de Lepinay's interferometric measurement of length ?

That most French physicists expressed themselves with a positivist-sounding rhetoric is not in doubt, but this is not to say that they lacked a scientific world view, lacked any beliefs about the ultimate nature of the physical world, it is just that they did not usually discuss them or even admit them. As Lucien Poincare was to say of his compatriot physicists in the early years of the twentieth century,'they did admit certain axioms which they did not discuss but which are, properly speaking, metaphysical conceptions',they believed that 'physics must someday re-enter the domain of mechanics,and mechanics was accepted without discussing its legitimacy.'[104] Poincare quoted statements of Verdet, Jamin, Cornu and Violle over a span of half a century to show that they believed that physics was coming under the laws of rational mechanics. In the 1870's there was also the spirited defence of the reality of atoms by Yvon Villarceau (1813-1883), an astronomer at the Paris Observatory, whose career had begaun in the days of Arago. Villarceau had ventured from his normal sphere of astronomy into thermodynamics and had derived expressions for Clausius's 'virial' theorem, and for the ratio of the principal specific heats of a gasin the most simple possible case, that is for the situation;

> 'In which each molecule contains only a single atom'[105]

His value of 5/3 did not correspond to any of the experimental determinations which had been made, but two years later, the Germans Kundt and Warburg measured the velocity of sound in mercury vapour, and from this value found the ratio of the specific heats to be 1.67; Villarceau's 5/3.[106] Villarceau reported this German work to the Academy and ended his note by denouncing the scepticism towards atoms, manifested by Berthelot, and claiming that it would indefinitely retard the progress of science if scientists waited until they could study atoms under the microscope like bacilli before accepting their reality.[107] He also concluded;

> 'The generalisation and extension of hypotheses,.. provide us with a powerful means of investigation, on condition that we compare these with observation'.[108]

But there was also the belief among physicists that they had banished metaphysics from their work, that their experiments were the only way to arrive at the truth, and that their methods were the model for the search for truth in other branches of human activity. They admired themselves and fondly imagined that other sectors of society admired them too.

The speech of Romilly, the outgoing president of the Société de Physique in 1889 gives us some idea of the self-image of physicists at the time, and is worth quoting at some length.

> 'We find devoted friends on all sides because they know that the physicist is bent upon an arduous labour which will not bring him any monetary reward. They know that the measurement of weight, the analysis of the sun's rays, the researches on the Hall effect, have never enriched anyone.. At this time when we see the crowd throwing itself into the struggle for position and fortune, the physicist, calm and alone in his laboratory, meditates, experiments and calculates. For the furiously ambitious there is the struggle to live, one against the other, for the physicist it is the struggle against nature for the truth. Truth is the only objective of our society, and it is this disinterested,this sublime goal, which inspires everyone around us to sympathise with us and respect us.[109]

Clearly, Romilly, an instrument maker rather than a savant , felt enormous pride at being associated with the physics community, a community endowed with a high status in the secular republic,and moreover a community which was beginning to regard itself as the cultural, intellectual and even moral leadership of society. It felt itself superior because it believed that science could explain everything, could give the basis for a moral existence without God and believed it could supplant the clergy whose authority stemmed from a set of unproven, unprovable, beliefs.

Certainly it is true to say that those who were carrying out research in physics gave no sign of being dissatisfied with their work, and there was little sign that students were turning away from the natural sciences and electing courses in new areas like social science for example. Nevertheless when one examines the research in the decade from 1880, one cannot escape the feeling that much of it is a sort of 'mopping-up' operation. The work in sound for example, firmly based on mechanical reasoning, gave only the possibility of more and more accurate determination of parameters whose values had been approximately known for half a century. Similarly the optical problems, so beloved by the French, seemed to be coming to an 'impasse'. It is true that optical instrumentation based on interferometry found practical applications in the field of metrology ,the precise measurement of length in which the French always excelled, and Cornu was able to build a career based on the determination of the speed of light using a modified version of Fizeau's apparatus, but no important theoretical conclusions emerged from any of the work. Spectroscopy as a subdivision of optics, was of course proving to be a powerful tool in giving information about the basic constitution of matter, but this does not seem to have been such a favoured area of research as it was in Germany and Britain.

It has been observed by Fox,[110] that the early meetings of the Société de Physique were dominated by demonstrations of technical and electrical gadgetry, and only at the end of the century did it play much of a role in the development of theory in physics. This was due to the large proportion of engineers and instrument makers in the society, it is true, but also because, at this stage, the most exciting developments were in the practical applications of physics, principally in electrical engineering; the telegraph and telephone, the phonograph, the generator and systems for the distribution of electrical energy. later was to come the upheaval in many research areas, but the late 1870's and the following decade was a barren period for physics in France.

It is also true to say that in this period a new generation of research workers were maturing, who were to be active participants in the revolutionary transformations in science around the end of the century. But in the 1880's, French physics seemed in several areas, backward in comparison with Germany and Britain. The experimental demonstration of the existence of Maxwell's electro-magnetic waves by Hertz in 1887, did not stimulate new research programmes in the Paris institutions of research. Significant early advances in this field were to be made by Branly in the relatively poorly funded Catholic Institute in Paris, and by Blondlot in Nancy, while the eminent Cornu showed himself to be decidely unimpressed by Hertz's experiment. There seemed to be a certain reluctance to embrace new theoretical ideas, or to admit that a revolutionary change in theory had occurred and a tendency to continue to try to organise new phenomena using well established French theories.

Notes for Chapter 4.

1.P. Duhem, The aim and structure of physical theory, (trans.P. Weiner, N.Y.,1977), 2nd ed., Paris, 1914, p.85.

2.H. Poincaré,'Optics and electricity', Science and hypotheses, (trans. W.J.G., London, 1905), Paris, 1905, p.213.

3.The principal source for the Société de Physique, has been the Comptes rendus of its meetings. For this reason this thesis does not intend to make and will not be able to make, an analysis of the political and social role of its leadership as was done for the British Association by Morrell and Thackray. We will try to say something about the professional composition of its membership and about the research presented to its meetings.

4.J.C. D'Almeida. 'Introduction', Journal de physique théorique et appliquée, Paris, 1872,

5. R. Fox makes the point that the very openness of the Société Française de physique, which allowed its rapid initial growth, was also the source of its principal weakness; its neglect of advanced theory. In 'Science the University and the state', Professions and the French state,1700-1900.(op.cit. note 12 ch.2), pp. 66-145. (p.106)

6. Wolf of the Paris Observatory, Marié-Davy of Montsouris, and Janssen who later in the decade had his own physical astronomy observatory at Meudon.

7. C.Wolf,S.S.F.p.,Paris,1888,p.12.

8. Col.Laussedet,Professor of the Conservatoire Nationale, and officer of the engineers,C.r.A.F.a.s,1872, 1, Bordeaux, 57-67.

9. D'Eichthal, President of the administrative council of the Midi Railway. C.r.A.F.a.s., 1875, 4, Nantes, 10-20.

10. R.Fox,'The Savant confronts his peers'in,The organisation of science and technology in France 1808-1914. (op.cit.ch.1 note 29), pp.241-82,(p.274).

11. Beekensteiner, C.r.A.F.a.s., 1873,2,Lyons,204.

12. Demance and Bertin, C.r.A.F.a.s., 1875,4,Nantes,392-401.

13. A.Tricout,C.r.A.F.a.s, 1880,9,Rheims, 369-72.

14. R.Fox,op.cit.,(note10),p.274.

15. For example in 1907 it gave out 32,250 Fr. Financial report,C.r.A.F.a.s.,1907,36,Rheims,

16. Hebert,' Conditions actuelles de la météorologie en France', C.r.A.F.a.s.,1876,5, Clermont Ferrand, .245-55.

A. Piche,'De l'état de météorologie en France au point de vue de l'organisation du travail,ibid, pp.255-66, (p.263).

17. Mme. C.Royer,'Le constitution moléculaire de l'eau..', C.r.A.F.a.s.,1883,12,Rouen, 318.

18.Depierre et Clouet,'Essais sur l'action de la lumière..', ibid, 301.

19.The information on Nancy and on the career of Blondlot comes from; M.J. Nye,' N rays:an episode in the history and psychology of science',*H.s.p.s.*,1980,*11*,125-56.

20. A.Abbadie,'Le Quobar',*C.r.A.F.a.s.*,1889,18, 262.

21. De Rochas,'L'od, en quoi il diffère de la chaleur, de l'électricité et du magnétisme,*C.r.A.F.a.s.*, 1890,*19*, Limoges, 170.

22. U.J.J. Leverrier,'Sur les masses des planètes et la parallaxe du soleil', *C.r.*, 1872,*75*,163-72.

23. H. Fizeau, in an addition to Leverrier's report, ibid, 172.

24. A. Abbadie. 'Ajoute', ibid.,172.

25. 'Lettre de Lissajous', *C.r.*, 1874, *79*, 1477.

26. A. Cornu, 'Détermination nouvelle de la vitesse de la lumière', *C.r.*, 1873,*76*, 338-42, (341).

27. A. Cornu,'Determination de la vitesse de la lumière et de la parallaxe du soleil', *C.r.*, 1874,*79*, 1361-65,(1362)

28. Ibid.,

29. A. Cornu,op.cit.,(note 26), 342.

30 H. Poincaré, 'Contributions à la théorie des expériences de M. Hertz',*C.r.*, 1890,*111*, 322.

31 Sarasin et De La Rive,'Résonance multiple des ondulations.." *C.r.*, 1890,*110*, 72-75.

32 A. Cornu, ibid. Note following the preceding communication.

33 Ibid.

34. H. Paul,*From knowledge to power. The rise of the science Empire in France,1860-1939*,Cambridge, 1985, p.276.

35. Sarrau,'Traité d'électricité et magnétisme de Maxwell', C.r., 1889,108, 1029-30.

36. S.L. Jaki,The uneasy genius: the life and work of Pierre Duhem. The Hague,1984, p.81.

37. A. Potier, 'Le tunnel du Pas-de-Calais au point de vue géologique', C.r.A.F.a.s., 1877, pp.241-43.

38. A. Potier,' De l'entrainement des ondes lumineuses par la matière pondérable en mouvement',J.d.p., 1874, 3, 201-04, (204)

39. A. Cornu,'Sur des expériences récentes faites par MM. A. Michelson et E. Morley, pour reconnaître l'influence du mouvement du milieu sur la vitesse de la lumière', C.r., 1886,102, 1207-08.

40. H. Fizeau, ibid.,1209.

41. H. Fizeau,'Recherches sur certains phénomènes relatifs à l'aberration de la lumière', C.r., 1887, 104, 935-40, (939).

42. A. Cornu,'Sur la renversement des raies spectrales', C.r., 1871, 73, 332-37.

43. J. Janssen, ibid.

44. L.P. Cailletet,'De l'influence de la pression sur les raies du spectre',C.r.,1872,74, 1285-89.

45. F.L. Peslin,'Sur les raies du spectre solaire',C.r.,1872,74,325-27

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47. There is an interesting account of Balmer's purely mathematical approach in S.L. Jaki, The relevance of physics, Chicago, 1966, p.106. Jaki makes the point that for almost 30 years the formula was not physics; 'it had no connection with physical reasoning of any sort'.

48. A. Cornu,'Sur les raies spectrales spontanément renversables et leurs lois de répartition ..', C.r.,1885,100, 1182-91,(1182).

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50. H. Deslandres,'Spectres du pôle négatif de l'azote. Loi générale de répartition des raies dans les spectres de bandes', C.r., 1886, 103,375-79, (376).

51. J. Moutier,'Sur la loi élémentaire des actions électro-dynamiques', C.r., 1874,78, 1221-24.

52. J. Moutier,'recherches sur la théorie des phénomènes électro-statiques',C.r., 1866,63, 299-300.

53. P. Duhem, op.cit.,(note 1), p.275.

54. M. Levy,'Sur es expériences de M. Deprez relatives au transport de la force entre Creil et Paris',C.r.,1886,103, 314-37.

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56. M. Deprez, 'Sur une application de la transmission électrique de la force faite à Bourganeuf', C.r., 1889, 109, 455-59.

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60. M.L. Brillouin,'Bertin-Mourot', in op.cit, (note 52, ch. 2) pp.401-06, (p.401).

61. P. Duhem , op. cit., (note 1), p.276.

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63. Ibid.

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66. J.Jamin,'Sur une machine magnéto-électrique produisant des courants continus de Gramme',*C.r.*, 1871,*73*, 175-78.

67. J.M.R. Benoit, 'Sur la résistance électrique des metaux', *C.r.*, 1873, *76*, 343-46.

68. M.L. Brillouin,' Sur les méthodes de comparaison des coefficients d'induction', *C.r.*, 1881,*93*, 1010-14, (1011).

69. J. Curie et P. Curie,'Lois de degagement de l'électricité par pression, dans la tourmaline, *C.r.*, 1881,*92*, 186-88.

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71. L. Gouy, 'Sur la propagation de la lumière',*C.r.*,1880,*91*, 877-80.

72. J. W. Rayleigh, 'Sur une formule relative à la vitesse des ondes', *C.r.*,1883,*97*, 567.

73. L.Gouy,'Réponse', *C.r.*,1883,*97*,1476.

74. L Gouy, 'Sur la théorie des miroirs tournants', *C.r.*,1885, *101*, 502-03.

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76. J.C. Maxwell,'Molecules', an address given to the British Association for the Advancement of Science,1873. Printed in *Nature*, 1873, *8*, pp.437-41, (p.440).

77. L.Gouy, op.cit., (note 75), p.563.

78. A.Maire op.cit (note 78 Ch. 2).

79. E. Bichat et R. Blondlot,'Actions des radiations U.V. sur la passage de l'électricité à faible tension au travers de l'air', *C.r.*, 1888,*106*, 1349-51.

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81. R. Blondlot,'Sur la conductibilité voltaïques des gaz echauffés'.

C.r., 1881, *92*, 870-02.

C.r., 1887, *104*,283-86.

82. R. Blondlot,' Sur la double réfraction diélectrique; simultanéité des phénomènes électriques et optiques',*C.r.*,1888,*106*, 349-52

83. C. Fabry et Perot, 'Mesure de petits épaisseurs en valeur absolue', *C.r.*, 1896, *123*, 802-05.

84. J. Macé de Lepinay,'Méthode optique pour la mesure absolue des petites longueurs',*C.r.*, 1885,*100*, 1377-78.

85. J. Macé de Lepinay,'Détermination de la valeur absolue de la longueur d'onde de la raie D_2',*C.r.*, 1886,*102*, 1153-55.

86. Title of the Academy question was ;'Rechecher, par de nouvelles expériences calorimétriques et par le discussion des observations antérieures, quelle est la véritable température à la surface du soleil', *C.r.*, 1876, *79*, 149.

87. C.S.M. Pouillet,'Mémoire sur la chaleur solaire..', *C.r.*,1838, *7*, 24-65.

88. The research on the solar constant in the first half of the nineteenth century is dealt with in the article by P.A. Kidwell in 'Pouillet, Herschel, Forbes and the solar constant', *Annals of science*, 1981,*38*, 457-76.

89. Commission report, *C.r.*, 1876,*84*, 813-16.

90. Ibid., 816.

91. The complete title is. 'rechercher expérimentalement les modifications qu'éprouve la lumière dans sa mode de propagation, et ses propriétés, par suite du mouvement de la source lumineuse et du mouvement de l'observateur', *C.r.*, 1870, *70*,

92. E.E.N. Mascart,' Modifications qu'éprouve la lumière par suite du mouvement de la source lumineuse etc.',*A.s.E.N.S.*, 1872, (2^{nd} ser), *3*, 363-420.

93. Ibid., 416.

94. Ibid., 420.

95. See the bibliography for the list of Paul's articles and books.

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97. Ibid, p. 236.

98. E. Branly,'Variations de conductibilité sous diverses influences électriques',*C.r.*, 1890, *111* , 785-87.

99. H. Paul, op. cit., (note 34), p.231.

100. H.E. Amagat,'Détermination du rapport des deux chaleurs spécifiques, par la compression d'une masse limitée de gaz', *C.r.*, 1873,*77*, 1325-27.

101. H.E. Amagat,'Sur la compressibilité des gaz a des pressions élevées',*C.r.*, 1878, *87*, 432-34.

102. L. Badash,'The completeness of nineteenth century science',*Isis*, 1972,*63*,48-58.

103. S.G. Brush,'Thermodynamics and history',*The Graduate Journal*, 1967,*7*, 477-565,(522-23).

104. This quotation is from Lucien Poincaré's, *The new physics and its evolution*, (trans. unknown), London,1907, p.10.

105. Y. Villarceau,'Suite aux applications du nouveau théorème de mécanique générale à l'équilibre des gaz , présenté dans la seance du 29 juillet', *C.r.*, 1872, *75*, 377-80.

106. Y. Villarceau,' Seconde note sur les déterminations théorique et expérimentale du rapports des deux chaleurs spécifiques, dans les gas parfaits dont les molécules seraient monoatomiques', *C.r.*,1876,*82*, 1175-78.

107. Ibid., 1178.

108. Ibid.

109. A. Romilly, *S.s.F.p.*,1889, Paris, p.

110. R. Fox,'Science the university and the state', in G. Geison, op.cit. (ch.2 note),p.106.

5. PHYSICS IN THE LAST DECADE OF THE CENTURY

a. Introduction

In examining the physics research of the last ten years of the Nineteenth Century, it might be considered appropriate to divide the decade about the year 1895. 1895, the year of Röntgen's discovery of X rays, seems to be something of a watershed between the gradual accumulation of knowledge on the basis of old well established paradigms, and the avalanche of new discoveries whose interpretation was to prove corrosive of those very paradigms. In fact, although such a simplification has some truth in it, it is basically misleading, because prior to 1895, the work on Hertzian waves with its attendant research into certain predictions of Maxwell's theory, and the research into photo-electric emission and cathode rays, prepared the conditions for the revolution in physics as surely as did Rontgen's discovery and the radioactivity research which followed it. For this reason, the whole of the decade will be considered in this chapter.

If there were physicists in the previous decade who thought that their science was reaching completion, the 1890's was going to surprise them. Lucien Poincaré, making in 1897 a review of physics, stressed that the discovery of X rays had made physicists more modest, less confident in the mechanical hypotheses which had seemed so easily to explain many physical phenomena.[1] The last decade of the century, together with the early years of the next, was a period which saw 'a work more of demolition than of definitive building'[2] in physics, a period of anarchy with no theory universally accepted by researchers. As we shall see, not only did the mechanical view of nature come under attack, but the very legitimacy of science was called into question. Scientists in general, and physicists in particular (for they were the ones who raised the most fundamental questions about nature and the physical world), had both to respond to the destruction of their old theories, and to defend their methods and the legitimacy of the knowledge they gained by these methods. In this chapter we will look, first of all, at the two national scientific organisations and the institutions where physicists taught, and did their research, and lastly look at what repercussions (if any) the upheaval in physics and the attack on science in general, provoked in the self confidence of physicists.

b. The Société de Physique in the last decade of the century.

In the ten years from 1891 to 1900, the society continued to increase its membership at the rate of around 2% a year, considerably less than in its early years. But as there was no great expansion of the faculty or lycée system in this period (although it did continue to expand), and no great development of physics based industry, the population of those who had a professional interest in physics would have remained fairly static. It should be added that the industrial and economic development of France in the last years of the century was considerably slower than in the days of the Second Empire, but the loss of the most industrialised provinces to Germany after the Franco-Prussian war, the burden of war reparation payments and the decimation by execution and exile of the skilled working class of Paris after the Commune, hampered the industrial recovery of the Third Republic. Economic development only began to quicken after 1900, based partly on new technology and partly on the exploitation of the newly discovered iron ore deposits of Lorraine.[3] In spite of the early French work in electrical transmission, generation, and power, a substantial electrical industry did not develop in France, and there was no great expansion in employment opportunities for physicists in industry. So the society appeared to accept its slow growth rate as inevitable, regarding the high recruitment rate of the past as something which belonged to its immature youth, and now it measured its health and vigour by the soundness of its finances, the attendance at its meetings and the scientific reputation it was acquiring at home and abroad. An indication of the last was the continued and growing membership (normally of a fairly passive nature) of distinguished foreign scientists and members of the French Academy of Sciences. A recruitment drive in the mid-1890's did, however, bring in 83 new members.[4]

In 1899 the Society realised the first part of a project which it had put in hand some ten years previously;it published the first of three volumes of physical constants which it was expected would become, both through their accuracy and their completeness, the definitive standards for industry, research and teaching. The second volume was published in the following year. 1900 also saw the organisation by the Society, of the Congrès international de Physique, with the participation of some thousand scientists from France and abroad. Because of the importance of this meeting it will be considered separately in the next chapter.

There was not a great change in the composition of the membership over this period. Members from the Paris region continued to constitute just less than 50% of the membership, although not surprisingly they played a disproportionate role in the presentation of papers to the fortnightly meetings. Also as to be expected, most of the papers came from the teachers of the higher educational and research establishments of the capital, while the contributions by the lycée teachers was much less than in the past, and also the contribution by engineers and instrument makers fell away. But an interesting change appears in the authorship of papers around 1896 with the explosive development of the new 'ray physics',the study of cathode rays, X rays and radio-activity. With the exception of Becquerel, none of the established leaders of the Parisian community, men like Lippmann or Bouty at the Faculty, Cornu at the Polytechnique, Violle or Brillouin at the Ecole Normale, or Mascart at the Collège de France, worked in the new field and research papers began to appear from the more junior levels of the academic institutions. The papers came from such people as Jean Perrin, agrégé préparateur at the Ecole Normale , Paul Langevin and Georges Sagnac, préparateurs at the Paris Faculty, Benoist (b.1856) teacher at the lycée Henri IV, and Raveau répetiteur at the Institut Agronomique Nationale. Perhaps it does say something about a certain intellectual 'liberalism' in the French higher education system,that junior members could undertake research in areas in which the senior staff of their laboratories did not participate. This would appear to be quite different from the situation which obtained in Germany, where students were, at least according to Heilbron, et al., obliged to follow lines of research which continued the interest of their superiors.[5] The work of Langevin was, in fact, presented in the Academy by Violle, and that of Benoist and Sagnac by Lippmann, before the authors themselves presented their work to the meeting of the Physics Society. One of the factors which probably increased the popularity of 'ray physics' was that it was comparatively cheap, at least in the early stages. Some of the necessary equipment like the meters, the induction coils, and switches etc. though expensive, would already have been found in the laboratories, and only the special Crookes tube would have entailed extra and fairly minor expense for the laboratory director.[6] And if it can be said that most of the important work in radioactivity and other fields was presented to the Academy before it came to the Society, it is also true to say that the Society heard none of LeBon's chimerical papers on 'black light'. LeBon was a member of the Society but probably felt less at home here than in the Academy where he had a number of personal friends. Certainly he never presented any papers to the Society.

In the early part of the decade, research papers on magnetism and electricity were most numerous in the meetings, followed by optics, but technological topics still figured prominently.[7] For example, members came to hear of the Laval steam turbine, the Panhard and Levassor petrol engine, new refinements to an ever more complex telegraph and telephone system, and a new type of meter to measure the quantity of electrical energy consumed, during this period. New vacuum pumps and high voltage equipment, essential to the advance of experimental physics also figured in the meetings. Work on cathode rays was presented to the meetings in 1894 and 1895 but after Röntgen's discovery of X rays, the number of papers devoted to 'ray physics' increased sharply to make up about a third of all contributions in 1896 and then declined steadily to less than 20% in 1900. To ensure priority in relation to discoveries, and to ensure rapid publication and wide distribution of research results, as well as to submit new work to the consideration of the leaders of the scientific community, it is not surprising that most important research was first submitted to the Academy before it came to the Society. For this reason, most of the research presented to the meetings of the Society will be considered when the institution in which it was generated is dealt with, and so will be only be referred to here in relation to the discussions which took place around it, or to the conclusions drawn from it. The discussions, controversies and resumés of the research being undertaken were frequently the most interesting and illuminating aspect of the Society's meetings.

Before considering the new 'ray physics' work in the decade, it would be interesting to take a synoptic view of the research into what Bouty called in 1897,'our old and dear areas of study'[8] which of course still made up the majority of the work. Violle returned to his study of the rays from incandescent bodies, and at the end of the decade found himself with a competitor in the shape of the polytechnicien, Henri Le Chatelier. Henri Becquerel continued the family study of phosphorescence, presenting his work on the laws of the decay of emitted light from phosphorescent bodies to the Society in 1891. Branly reported on the invention of the radioconducteur, his device to detect electro-magnetic or Hertzian waves, and Blondlot on his experiments to measure the velocity of these waves.

Hertzian waves were being transmitted and received over a distance of some 5 Km by the Russian, Popoff, in the early years of the decade, and by the mid 1890's Marconi was sending and receiving them at a distance of around 20km. In 1898 the Paris instrument maker, Ducretet demonstrated his automatic wireless telegraph to the Society. This apparatus needed no operator at the receiver but punched out the received message in the dots and dashes of the Morse code, onto a paper tape. The changes in resistance in the receiving circuit caused abrupt changes in current, which operated a type of electro-magnetic relay, which in turn controlled the current to the paper tape punch. Ducretet proposed to show the Society at a later date, other examples of the operation of apparatus at a distance by Hertzian waves; the switching on and off of an electric motor, the operation of an electromagnet and the switching on and off of an electric light. He ended his paper by expressing his pleasure at being able to show that French scientific industry was in no way lagging behind industrial developments abroad, and drew attention to the fact that he had excluded all foreign expressions from his paper, saying that 'our French language is rich enough to find in it all that we need'.[9] One can only conclude from this excessive defensiveness, that along with the new electrical technology being imported from Germany and the USA, German and English terms associated with it, were finding their way into the French language.[10] But one French physicist, Blondel, of the Ecole des Ponts et Chaussées, considered that Branly and Ducretet were playing down the contribution of foreigners to the development of wireless telegraphy, citing the work of Popoff, Lodge and Marconi, thus showing that some

French physicists were following much closer than in the past, progress being made outside France.[11] Branly continued his research on Hertzian waves, and in 1900 reported his experiments on the absorption of the waves through different thicknesses of different liquids, to the society.[12]

Although in the second half of the decade, many young physicists turned their attention to the new 'ray physics', this area of work seemed to remain outside the range of interest of the most established and eminent researchers. As for the work which had led up to the discovery of cathode rays, the conduction of electricity through gases at low pressure,this was a neglected area in France, apart from some work in the 1870's by Chautard of Nancy a d Daniel of the Ecole Centrale. In the early 1890's, the principal worker iin the field was the German, Lenard, and his work had been communicated to the Societe de Physique by Charles Guilaume (1861-1938) of the International Bureau of Weights and Measures at Sevres.[13]

Lenard's research and later work on what had came to be known as 'cathode rays' raised the question of whether the rays 'were a sort of current or a sort of light'[14], and Curie speculated that they were basically a type of light, although possessing a certain dissymmetry which allowed them to be affected by magnetic fields. Curie often used the method of analogy in his scientific work (see his comparison of magnetisation curves with the pressure against volume graph of a gas near its critical temperature later in his chapter), and here he is presumably thinking of his earlier work in piezo-electricity. The work on cathode rays, by Jean Perrin, which showed that they conveyed a negative charge, was not presented to the society by him, for Perrin was not a member in 1895, but in the following year he did join and reported on his work on X rays.

As was said earlier, the most important function of the meetings of the Societe, was probably the discussions of the new work, the summaries of the current theoretical positions in new areas of physics, and the consideration of foreign work. One should not make too much of the idea of a struggle between generations, but inevitably in this period of rapid advance, it was through the contribution of the younger researchers in the meetings of the Society, that something of the excitement and intellectual ferment of the new physics was expressed. Intellectual debate was less restrained here than in the Academy. In the meetings of the Society, the younger members were not submitting research for the approval of their elders, in areas in which the elders were already authorities; there were no authorities in ray physics in France. Thus the revolution in physics served, in a sense, to 'democratise' the science, and the meetings of the Société expressed this democratisation.

Those who had the facilities to do so, plunged into the new research; Perrin, Langevin, Benoist, Hurmuzescu, Sagnac and Villard were prominent among the young research workers of the capital. Inevitably, research would overlap, and priority disputes would break out, as in the meeting of the Society in 1896, when Perrin and Benoist argued over the discovery of certain X ray properties. Even those who had not done any experimental work could still participate in the great debate. For example, Raveau reviewed the experiments done by others and speculated that X rays were very short ultra-violet rays. This was fairly orthodox but he went further, arguing that as they were diffused by passing through solids, their wavelengths must be similar to the distances between the molecules of those solids.[15] At the large Easter meeting of 1896, Perrin made a summary of all the work on cathode rays and X rays done in France and abroad. France lagged a little behind Germany and Britain on the question of the ultimate nature of the rays. This lag may have been due to a positivistic reluctance in France to speculate about the reality behind the phenomena, but it might simply have been that France had taken up the experimental study of cathode rays (and to a lesser extent X rays) later than Germany and Britain. Certainly Perrin was in the position of surveying and judging between two different hypotheses on the nature of cathode rays. It would not be extravangant to call them national hypotheses, for the one which saw cathode rays as a modification of the ether, a kind of light of very short wavelength was held by the Germans Goldstein, Weidermann, Ebert and Lenard, and it had been Hertz's position until his death, while the one which saw them as material particles had the adherence of the British, Crookes, Varley, J.J. Thomson and Kelvin. Perrin tended towards the material hypothesis of the British but asserted that it made the phenomenon of fluorescence more difficult to explain. X rays were less controversial, everyone agreed that they were light of very short wavelength, although Röntgen also though that they were probably longitudinal waves.[16] In the following year, 1897, Sagnac showed that some materials bombarded by X rays emitted other X rays of a lower frequency. In that same year, J.J. Thomson in Cambridge succeeded in measuring the ratio of the mass to the charge of cathode rays, which seemed to put their material character beyond doubt, but reports of this experiment were not discussed in a meeting of the society until two years later.

Other research, which, though revolutionary in implication, seemed to be closer to the French tradition,gave the opportunity for the intervention of more experienced members of the community. For example, in 1897, Cornu reported on Peter Zeeman's experiment in Holland, in which the light from a discharge tube was passed through a powerful magnetic field causing the spectral lines to broaden and become circularly polarised. This work provoked much interest in France because it was in the tradition of optical-magnetic polarisation carried out earlier in the century by Verdet, Bertin and Cornu himself.[17] It did not however unleash a flood of research, probably because of its enormous expense; the high resolution diffraction gratings, spectrometers and powerful electro-magnets were the most expensive items of research of the time. Research by Rubens and Nichols in Germany on very long wave infra-red radiation, also came to the notice of the _Société_ in 1897, when Guillaume, the assiduous reader of foreign scientific publications, gave a report on it.[18] So the _Société de physique_ was playing an important role in educating French scientists on research going on abroad.

The society also followed very closely the discovery, at the end of February 1896, that certain uranium salts emitted invisible rays. In March 1896 Henri Becquerel was asked by the society's president to give an account of his first experiments, and he explained how the rays could penetrate black paper, and a copper or an aluminium sheet to discharge an electroscope.[19] In the next meeting he gave an experimental demonstration of this. In a meeting in November he wondered about the source of the energy of the radioactive uranium, commenting that though it had been kept in darkness since May it had lost none of its activity. After a short intervention in early 1897, Becquerel gave up his research in this field (see later in the chapter when Becquerel's work is considered in a little more detail) and did not return to it until 1899 when he reported to the society on his research on the rays from the radium sample which the Curie's had given him. At this meeting Pierre Curie commented that the rays appeared to have some of the properties of X rays and some of cathode rays.[20] In the following year, 1900, there were more radioactivity papers presented to meetings of the society; from Becquerel, from Villard, and from the Curies (although Marie was not yet a member), who outlined their physico-chemical techniques which were already being put into commercial operation by a Paris chemical company, _La Société centrale de produits chimiques_ to produce radium, polonium and a third, as yet unnamed highly radioactive element.[21] Paul Villard of the Ecole Normale reported that radium emitted some rays which could not be deflected by electrical and magnetic fields and which he thought were a kind of X ray.[22] Thus it can be said that the society was acting as an instrument for the rapid dissemination of the latest research into radioactivity, which was and remained, essentially an activity of the Paris institutions.

Paul Langevin gave his first paper to the Société in the Easter meeting of 1900, and one can see in it the continuing interest in the research on atomic phenomena which was going on abroad. Referring to the work of C.T.R. Wilson and J.J. Thomson, (under whom he had worked in Cambridge) Langevin compared the mass of a negative ion as observed in cathode ray experiments, or by the deflection in magnetic fields of some of the rays of radium, with the mass of the hydrogen ion found in electrolysis. This work was beginning to give insights into the structure of the atom, for as Langevin asserted;

> 'The mass of the negative ion is only a thousandth part of that of the hydrogen atom. The much heavier positive ion would constitute, according to Thomson, the rest of the atom'.[23]

c.Physics in the AFas

The first year of the decade saw the annual meeting of the AFas in Marseille, a city with a long established Faculty of Science and an Observatory with a long history and considerable prestige. Thirty two papers, the highest ever, were presented to this meeting, and as with Nancy five years previously, the local contribution made up about one third of the total. Contributions came not only from the Faculty of Science, but from the Medical School, the Faculty of Medicine in Nice, and from the Observatory. Lucien Poincaré who was to make a succesful career as an inspector of higher education and as a commentator and reviewer of contemporary physics, and who at this time was teaching in a *lycée* in the city, submitted a paper on the potential difference developed between electrodes in an electrolyte.[24] Charles Fabry gave a paper on the speciality of the Marseille faculty, interferometry, reporting on research which had already been published the previous autumn, in the *Comptes rendus* of the Academy.

In the following three years, the meetings were held successively in Pau, Besançon and Caen. At the first of these meetings there was a high proportion of Parisian savants present (perhaps because it was an attractive holiday area), and one of them,the Parisian engineer Marcel Deprez gave a paper on the transmission of electrical power using A.C.. In the field of electro-technology, a controversy over the relative merits of A.C. as opposed to D.C. had broken out among the engineering community and Deprez was an early partisan of the former method. At the Besançon meeting, Cornu gave two papers on optical topics, Janssen reported on the work of the Observatory of Mont-Blanc and the meteorologist of the *Bureau Centrale*, Teisserenc de Bort, reported on new research into the relationship between wind speed and atmospheric pressure gradient. There were other papers but this was probably the weakest of the meetings considered. The following year, in Caen, the meeting heard a number of papers on optical and acoustical questions which could only have been of interest to *lycée* teachers, as experiments for pedagogical purposes, for they went over ground which had already been thoroughly worked.

The *Association's* meeting of 1895, was held for the second time in its history, in Bordeaux. Thirty three papers were presented, including one from Pierre Duhem who had been working in the Faculty since 1893, after periods in Lille and Rennes. It is possible that Duhem regarded the *AFas* with somewhat mixed feelings; while supporting its patriotic aims and seeing it as a vehicle for bringing about needed reforms in the organisation of science in France, he would also have seen it as a part of the anti-clerical, anti-monarchist, ideological superstructure of the Third Republic. It is clear too, that the organisers of the AFas regarded Duhem with a similar suspicion, and in the publication of the proceedings of the Bordeaux meeting they dealt him two studied slights. The first was to neglect to print that he was a member of the Faculty, printing simply 'P. Duhem of Bordeaux', and the second was to publish no more than the title of his paper 'On the theoretical interpretation of Hertzian wave experiments.'[25] This must have been the most original and theoretical paper of the whole meeting; possibly it was considered by the organisers to be inappropriately theoretical. As if to underline the insult to Duhem, the officers of the Association published in full a quite unremarkable paper by the now obscure Morisot, on a new type of electric cell. Moreover they did not neglect to give him his title as 'professor of the Bordeaux Faculty'. A paper of more popular, if rather morbid interest from the Bordeaux meeting, was the one, published in full, by the Parisian doctor, Darin, on the effects of electricity on the human body.[26] In this paper, Darin, compared the effect of A.C. and D.C. on the body, his interest in the subject kindled by the adoption of electrocution by the State of New York.

In the winter of 1897, at a meeting organised in Paris, Gariel gave the first communication to a meeting of the AFas, on X rays. This was not a research paper, but a fairly popular exposition of the development of X ray photography, accentuating its usefulness in many applications. Gariel finished by making a statement on the importance of funding theoretical research which would later bring material advantages, and he also expressed his regret that French scientific establishments were so poorly endowed with X ray apparatus.[27]

This first report on X rays was followed by several more in the 1898 meeting at Nantes, together with others on cathode rays and

Hertzian waves. The meeting was more fully endowed than usual with savants from the capital, who were making, or were soon to make, their reputation through work in these new areas. It is true to say, however, that the majority of papers given at the meeting still concentrated on the traditional themes of French science, or on the utilitarian application of science. The most striking example of this latter was the paper (published in full) by two Lyons teachers, one from the military school, the other from the Faculty, on the thermal conductivity, (in both dry and damp conditions) of the materials used in army uniforms.[28] Physics was seen to be accomplishing both its patriotic and its utilitarian mission in this research at least.

But there were also three papers on wireless telegraphy, from Blondel and Broca, three on X rays, including one from the Rumanian working in Paris, Hurmuzescu, and three on cathode ryas, of which two came from P. Villard. Blondel also explained the principles of his '*oscillographe*', a type of galvanometer with a very small inertia, capable of tracing out the curve of an alternating electric current. It should, however, be emphasised that this work had all been reported before, and had appeared in other journals.

The meeting of 1899 held in Boulogne is of particular interest because of its cooperation with the meeting of the BAAS being held at the same time in Dover. There were trips across the Channel from both directions for joint fraternal functions, and wireless communication between the two meetings was established. Blondel was to give five papers on electrical subjects, while A. Turpain, a junior member of the Bordeaux faculty and student of Duhem, gave three. One of Turpains's papers was a very theoretical one entitled 'On the propagation of electric oscillations in dieletric media' in which he compared two theories of electromagnetic propagation, one by Maxwell and the other by Helmholtz and modified by Duhem.[29] Duhem always displayed a marked suspicion of the work of Maxwell and considered that in France his 'equations were accepted without discussion, as though they were a revealed dogma',[30] to the detriment of electromagnetic theory. Duhem supported the theories of Helmholtz against those of Maxwell arguing that;

> 'Helmholtz gave an electromagnetic theory which proceeds very logically from the best-establihed principles of electrical science,and their formulation in equations is exempt from the paradoxes arising too frequently in Maxwell's work'.[31]

Turpain develops this idea in the introduction to his paper, saying that the theory;

> '..built by Helmholtz and completed by Duhem, offers the advantage of linking the interpretation of Hertzian wave experiments to the classical doctrines of electricity'.[32]

The essential feature of the Helmholtz/Duhem theory is the existence not simply of a 'flux of transverse displacement'but also one of longitudinal displacement. Only the transverse flux is admitted in the theory of Maxwell. Turpain refers to discrepancies between experiments made by Blondlot and those of Aron and Rubens, and Cohn and Zeemann, discrepancies which as a result of his own experiments, he declares to be more apparent than real, if the Helmholtz/Duhem theory is accepted. It was certainly unusual for meetings of the AFas to hear reports of an experimentum crucis and it is probably not uncharitable to suppose that for most of the assembled members, the paper would have been rather difficult to follow. Turpain's second paper was on the other hand, much more accessible, for he analised the possibilities of telegraphy by Hertzian waves, the so-called wireless telegraphy and came to some cautious if not downright pessimistic conclusions.[33]

Excessively high aerials would be needed for transmissions over long distances, the rays would not be parallel and therefore would lose intensity with distance, the air would absorb them as it does light; these were the objections which Turpain put forward. Marconi was criticised for his extravagant hopes and claims, which;

> 'Far from serving the scope of this application of hertzian waves, he risks, in this way, compromising the useful effects, by vainly concentrating costly efforts in the research of a problem whose solution truly passes the limits of what can legitimately achieved by the employment of electrical oscillations'.[34]

With hindsight, this is a rather unfortunate prediction by Turpain, but it must be remembered that a large part of the scientific coommunity, not just in France, considered Marconi as something of a crass commercial adventurer who used apparatus developed by others; Righi's oscillator, Branly's coherer, Popoff's aerials for example, in his experimental work.

The last meeting of this period, the one in Paris in 1900, was a very disappointing one in spite of its being held in the capital. Turpain, despite his previously expressed caution regarding the possibilities of Hertzian waves, continued his research in this field, developing new types of coherers and resonators. But there were no reports on the ray research which was exercising and exciting the new generation of workers, and so it must be said that the Association's meetings seemed only to reflect in a fairly distant way, the ferment of ideas and experiment, which found an expression in the meetings of the Société de Physique. Funds continued to flow from the AFas into projects of publication and research, and Turpain was one of the recipients of these funds, but both the proportion and the actual sums coming to physics remained very small.

One interesting paradox about the *Association* is that, despite its utilitarian emphasis, it provided, through the intermediary of Turpain, the only national platform for the theoretical ideas of Duhem on electromagnetism. So an organisation which was avowedly republican and dedicated to the application of science, in a scientific community which had long favoured experimental physics at the expense of theory, found itself the unlikely mouthpiece of the advanced mathematical theories of the clerical, anti-republican Duhem.

d.*The Ecole Municipale de physique et de chimie industrielles.*

The *Ecole Municipale de physique et de chimie industrielles* which was founded in 1882 to provide an essentially practical type of education to young men who had studied at the *écoles primaires supérieures* , will be considered before the other more prestigious institutions in this section, because of its importance in the production of new knowledge at the end of the century. Established in the old buildings of the *College Rollin*, the objective of the school was to train engineers and chemists for employment in private industry, and the three year course of study comprised in the first year, physics and mechanics, theoretical and practical chemistry, and mathematics. In the second year the students specialised in either physics or chemistry, and in the third year they spent most of their time in workshops and laboratories, the physics students working on the fabrication of instruments and the chemists on the preparation of dyes etc.[35] Entrance to the school was by examination and the successful 30 students who were admitted received a grant of 50 francs a month. The social origins of the student intake, and their intellectual level would have been much lower than that of *lycée* students, and this, together with the emphasis on practical tuition and the municipal status of the school, made it an institution without much prestige in the French academic system. Shinn has stressed, however, that the school was unprecendented in the French educational system and significant for French industry because it alone;

> '.. stood, on the one hand, between the low level empirical knowledge of the skilled worker coming out of the *écoles d'arts et métiers*, and on the other, the abstruse mathematical content of the courses of the *Ecole Centrale* and *Ecole Polytechnique*'.[36]

It is clear that at such an establishment, little importance would be placed on the research of its teachers. Research would be an activity peripheral to the central objectives of the school, and in any case the heavy load of teaching and practical supervision which fell to its teachers, left them little time for research. It is considered here because it provided employment for Pierre Curie from 1882 onwards, and in the final decade of the century was the centre for his research and that of his wife Marie Sklodowska (1867--1934) in radioactivity.

Pierre Curie, during the 1880's had worked alone at the *Ecole Municipale* on the symmetry of crystals, continuing the research he had carried out with his brother, Jacques, in Friedel's laboratory in the Faculty. In the 1890's he turned to research on magnetism and successfully defended his thesis on this topic at the Paris Faculty in 1895. As this was work for the doctorate it will be considered in the section on the Paris Faculty and compared with doctoral work which was submittd by candidates from more prestigious establishments.

Pierre Curie, although he had gained an international reputation for his work on piezo-electricity and magnetism, by the end of the century, had none of the qualifications which really mattered in the French educational system. He had not even attended a *lycée*, being educated at home by his father, let alone one of the *grandes ecoles*. He is the first physical scientist of this survey, whose scientific formation comes only from the preparation for the *licence* in the physical sciences at the Paris Faculty.[37] He never took the competitive *agrégation*, which would have allowed him to teach in the *lycées*, and it was not until 1895, eighteen years after his *licence*, that he bothered to submit his research for the award of the doctorate. Although his relations with the original director of the Municipal school of industrial physics and chemistry, Schutzenburger, were extremely cordial, those with Schutzenburger's successor, the physicist and *Polytechnicien* C.M. Gariel (1841-1924) were often very difficult. Fortunately, Gariel only stayed at the school for about a year, but in that time he showed little interest, and was sometimes obstructive, towards the radioactive studies of Curie and his wife.[38] While carrying on these studies in the last few years of the nineteenth century, Pierre Curie, was receiving a salary from the school of only 6,000 francs a year, and struggling with a very heavy load of teaching and practical supervision. It was not, however, until October 1904, that he was finally made professor of physics at the Paris Faculty, and found better facilities, but not much more time, for his research.

The story of his wife, Marie Sklodowska, is well known and so only a few relevant remarks will be made here. She had studied at the Paris faculty and passed both the licence in physical science and the one in mathematics, in 1893 and 1894 respectively. In 1896 she was placed first in the womens' *agrégation* in physics,[39] and after Becquerel's discovery of the same year, that uranium salts spontaneously emitted rays with properties similar to X rays, began a series of experiments to determine whether there were other elements which emitted radiation. Before the discovery of polonium and radium by the Curies in the latter part of 1898, work for which Marie Curie received the Gegner prize of 4000 francs from the Academy,[40] it cannot be said that the French scientific community had much interest in the investigation of what had become known as 'Becquerel rays'. It was left to Marie Curie, a women and a foreigner, working with her husband in the primitive laboratory facilities of the Ecole Municipale, to see if there were other materials which emitted the rays. Madame Curie investigated dozens of different minerals (made available to her by Henri Becquerel from the extensive collection of the Museum), together with many metals and their salts and oxides, concluding that only compounds of uranium and thorium were 'radioactive'. Curie was the first to use the term 'radioactivity', rejecting the name 'hyper-phosphorescence' which was employed by S.P. Thompson among others, as 'giving a false idea of the nature' of the rays.[41]

Madame Curie pulverised the material and spread it as a thin layer on a metal plate; another plate situated above and close to the first, was maintained at a potential of 100 volts and the current flowing between the plates measured by a sensitive piezo-electric electrometer developed by her husband and his brother, Jacques, in their earlier joint work on piezo-electricity. In practice, the current through the electrometer was balanced by the output from the stressed piece of piezo-electric quartz. (See diagram below)[42]

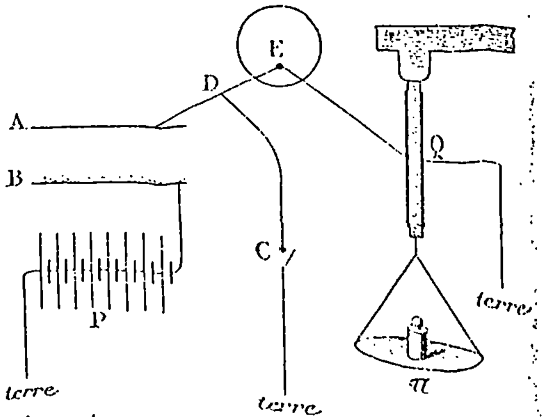


Fig. 1. — *Appareil pour la mesure de la conductibilité*
l'air sous l'influence des radiations actives. — AB, conde
sateur à plateaux, le plateau B portant la substance radi
active ; CD, fil à la terre avec contact mobile en
E, électromètre : P, pile ; Q, quartz piézoélectrique : π, poi

Let it not be thought that Marie Curie was simply a painstaking experimentalist, or that her research was a species of 'womens' work' in science. Meticulous it certainly was, but at the same time she was not content only to measure. She put forward a hypothesis to explain what she had observed;

> 'To interpret the spontaneous rays of uranium and thorium one could imagine that all space is traversed by rays analagous to Röntgen rays but more penetrating, which can only be absorbed by elements of high atomic weight,like uranium or thorium'. [43]

Thus, argued Madame Curie, these elements of high atomic weight absorbed the free radiation of space, and re-radiated it, in the same way as Georges Sagnac had shown that some metals absorbed X rays of one frequency and re-radiated other X rays of a lower frequency. She says nothing about the nature of the emitted rays, but her hypothesis suggests that she was still confused by Becquerel's original conclusion that they were a form of e.m. radiation.

As a result of these experiments, Madame Curie found that pitchblende, an ore containing uranium oxide, was more radioactive than metallic uranium. She first found that pitchblende formed, by sublimation in a vacuum, a product thirty times more radioactive than uranium. With the help of another member of the school, Bemont, she began a laborious process of isolating the active products of pitchblende. By July she had found a material somewhat similar to bismuth and 400 times more radioactive than uranium, whose discovery she announced in the statement;.

> 'If the existence of this metal is confirmed, we propose to call it Polonium, from the name of the homeland of one of us'. [44]

But the spectroscopist Eugene Demarçay, working in the Ecole Normale, could discover no new spectral lines in the sample given to him to examine. It was clear that more of the material would have to be prepared, and following the intervention of the Vienna professor Eduard Suess, the Austrian government made 100 Kg. of pitchblende available to the Curies. By the end of December 1898, a second new element, still more radioactive and with chemical properties similar to barium, was isolated and named radium.[45] With this material, Demarçay could find new spectral lines.

By the beginning of 1900, the Ecole Municipale was still the principal centre for radioactive studies. By this time the Curies were working there with materials 100,000 times more active than the original uranium salts, although now the chemist, Debierne, working in the Paris Faculty, isolated a third radioactive material which was named, actinium. In January, Marie Curie reported that the rays which were deviated by a magnetic field were more penetrating and travelled further in air than did those which were not deviated, while a study by her husband on the 'non-deviable' rays showed that the more material they passed through, the more absorbable they became.[46] It was noticed that their range in air stopped abruptly, and they were likened to a projectile which lost kinetic energy after passing through material. After this, the Curies collaborated to show that the deviable rays, like cathode rays, did convey a negative charge.[47]

So by the end of the century the Ecole Municipale, through the work of the Curies, had become a centre for experimental studies (both physical and chemical) in radioactivity. But this was work which was independent of the main function of the school, and unlikely to have been incorporated or utilised in Pierre Curie's lectures there.[48] For this reason there is no question of a research school being formed at the Ecole Municipale; the level of the courses was too low, the students, who could enter the school at fourteen, too young. It is true that a few years earlier Paul Langevin (1872-1946), had passed through the school and was later to make a distinguished career as both a researcher and teacher in physics, but his theoretical formation came from his studies at the Paris Faculty for the *licence*, and at the *Ecole Normale*.

e. The higher education institutions of the capital.

The excitement caused by the discovery of hertzian waves at the end of the previous decade, did not provoke any shift of direction in the research programmes of staff of the Paris institutions of higher education. The research into electro-magnetic radiation was largely left to Blondlot of the Nancy Faculty, and Branly of the Catholic Institute of Paris. Mascart at the *Collège de France* was no longer doing research, and there was little coming from the staff of the *Ecole Polytechnique*. Lippmann at the Paris Faculty had turned his attention to colour photography using interferometry (work which would gain him the Nobel prize in 1908), while his colleague, Bouty, was much more productive and won the Lacaze prize in 1895, for all his work on magnetism and electrical conduction though gases. At the *Ecole Normale*, Violle's research work had diminished by the early 1890's. As he had also been made professor of physics at the *Conservatoire des Arts et Métiers* in 1892, this may have been caused, at least partly by a heavy teaching load. The other *maître de conférence* in physics at the the *Ecole Normale*, Brillouin, was also concentrating on his teaching, which was to have a great influence on the next generation of French physicists, but his research publications were less prolific than in his youth. When there came the explosion of research spurred by Röntgen's discovery of X rays in 1895, it would not be these senior academics who took up the new work, but *préparateurs*, working under their supervision.

The theses presented for the doctorate at the Paris Faculty began by the end of the century to deal with the new research areas which were opening up. The first one to deal with the question of electro-magnetic waves after Hertz's experiment was the doctoral thesis of the *lycée* teacher H.A. Abraham (b.1869). The work was carried out in the laboratories of the *Ecole Normale Supérieure*, and published in the *Annales de chimie et de physique* in 1892.[49] Abraham's research sought to determine with greater precision than before, the relationship between the absolute units of electricity defined on the electrostatic scale with those defined on the electro-magnetic scale. This question was discussed in the *Congrès des Electriciens* of 1881, where after some debate, it was decided to adopt the electro-magnetic system of C.G.S. units. In the electrostatic system, k (the permittivity of air) is made equal to unity; in the case of the electro-magnetic system, the magnetic permeability of air is made equal to unity. The ratio v is the number of electrostatic units of charge contained in the electro-magnetic unit. Its importance from the point of view of Maxwell's theory, comes from the fact that it must also be the velocity of electro-magnetic waves and also (light being simply a particular range of e.m. waves) of light in a vacuum.

Abraham began his paper with an historical review of all the experiments which had been attempted to determine v since Weber and Kohlrausch in 1856 had found a value giving fairly good agreement with the velocity of light measured, separately, by Fizeau and Foucault. Seventeen research workers over a period of 35 years had found values of v varying from 2.71×10^{10} cm/sec, to 3.07×10^{10} cm/sec., while successively more and more precise determinations of the velocity of light were settling around the figure of 2.99×10^{10} cm/sec. It was the ambition of Abraham, in which he seems to have succeeded, to bring a new higher order of precision to the value of v, an accuracy of better than one part in a thousand. If this work seems to bear the hallmarks of French physics of the period, exquisite precision and absence of theoretical speculation, it must not be forgotten that it was a doctoral thesis. As such, it would have to be a topic approved by the Paris Faculty staff, and its originality would lie predominantly, if not exclusively, in its experimental ingenuity.

Abraham's lengthy discussion of past experiments, was not due to any abstract attachment to the history of the subject, but was done to weigh the strength and weakness of each method, and to select the best techniques from them. Thus, even the experimental methods cannot be said to be completely original, although Abraham used the best contemporary scientific techniques to improve their accuracy. For example, he determined the spacing of the capacitor plates using interferometric methods, while the speed of rotation of the commutator used to charge and discharge the caapacitor, was measured using a type of stroboscope devised by Lippmann. In five days of experiment in April and May 1892 Abraham made 14 determinations of ν , whose values ranged from a minimum of 299.04 x10^8 cm/sec. to 299.44x10^8 cm/sec. with a mean value of 299.2x10^8 cm/sec.[50] Certainly, this determination of ν, which Abraham claimed was exact to one part in a thousand, remained the definitive one for a number of years, and was considered of sufficient importance to be presented by Abraham, in abbreviated form, to the Physics Congress held in Paris in 1900.

If Abraham's thesis seems a little conservative in its subject matter, taking an old problem and simply using improved techniques to achieve a higher precision, the same cannot be said about Pierre Curie's thesis of 1895. Curie defended his doctoral thesis on 'Magnetic properties of materials at different temperatures'in March 1895, and it was published in the <u>Annales de chimie et de physique</u>,in July of the same year.[51] He investigated diamagnetic, paramagnetic, (Curie called these feebly magnetic substances) and ferromagnetic materials, to see if there could be transitions between these groups or whether they were absolutely separate in all conditions. This question had previously preoccupied Faraday, who had observed that although iron lost much of its magnetic properties when it was red hot, they were not completely obliterated and thus iron had become similar to a paramagnetic substance. Thus it appeared that a material might successively belong to different groups. To resolve the problem, Curie set out to study the magnetic properties of different materials over a wide a range as possible of temperature, pressure and magnetic field. His temperatures were to range from ambient to 1370°C, and his materials ranged from the diamagnetic ones like water, potasssium sulphate, sulphur and selenium; paramagnetic materials like oxygen and palladium, and ferromagnetic ones like iron, cast iron and nickel.

Curie's magnetic experiments showed the same attention to precision and detail which were later to be the hall-mark of his radioactivity research. They involved accurate chemical techniques to ensure that samples of material to be tested did not contain traces of impurities, for a paramagnetic substance containing a small percentage of a ferromagnetic one, would have its own intrinsic properties masked by the impurity. He also developed a precise torsion balance to measure the small forces that his paramagnetic and diamagnetic samples would be subjected to, in a magnetic field. A third problem which he had to overcome was that of convection currents in the furnace, currents which could easily disturb the measurements of force. To solve this problem, Curie was able to borrow a device from Blondlot (once a colleague of his at the Paris Faculty), who had developed it when he was doing his research on the conductivity of hot air.[52]

The conclusions of Curie's magnetic research were that paramagnetism was inversely proportional to absolute pressure,[53] that the coefficient of specific magnetisation of diamagnetic substances was independent of the strength of the magnetic field and generally independent of temperature,[54] and that a ferromagnetic material is gradually transformed as it is heated, into a material displaying paramagnetic properties. While Curie never put forward any theory to explain the phenomenon of magnetism, he did conclude that;

> '..these results favour theories which attribute magnetism (here he includes ferromagnetism and paramagnetism) and diamagnetism to causes of a different nature'.[55]

Today it would be said that diamagnetism, which Curie found to be independent of the physical state or allotropic modification of the material, is a specific property of atoms, while paramagnetism and ferromagnetism are properties of combinations of atoms. Curie also made an interesting analogy between the way in which the intensity of magnetisation of a magnetic body varies with temperature and field, and the way in which the density of a fluid varies under the influence of temperature and pressure.[56] Curie compared the shape of his curves for the magnetic properties of iron, with those which Amagat had found for the density of carbon dioxide at different temperatures and pressures. Such an analogy was interesting, argued Curie, because it suggested new experiments. For example, with CO_2 below the critical temperature, one sees sudden changes and the phenomenon of liquefaction. Similarly the intensity of magnetisation increases more sharply as the temperature decreases when the applied field is more feeble, suggesting that if the field were sufficiently weak, the increase in magnetisation would become similarly abrupt, making it possible to think of a kind of critical point in relation to magnetic phenomena.[57]

It is interesting to compare the doctoral thesis of Abraham, the normalien, with that of Curie. Abraham's research went down a well trodden path, simply bringing to an old problem, a new order of precision, while Curie explored a neglected area (also with great precision) and if he did not propose any new theoretical insights to explain the phenomena, he did use the method of analogy as a heuristic device. As we shall see, it was Curie's willingness to work outside the main lines of traditional research which was to prove so fruitful at the end of the century. Thus, Curie shows a certain independece of spirit in his choice of research topic, probably because, not being a product of the grandes écoles, and therefore knowing that certain career paths were not open to him, he was not looking for the approval of the 'notables' of the physics community.

But later in the second half of this decade, the new ray physics, the examination of cathode rays, X rays and what were called Becquerel rays, was to provide the material for doctoral theses by several young research workers, who were to make their name in this area. Jean Perrin (1870–1942), while working for his doctorate as an agrégé-préparateur in the Ecole Normale showed in 1895, by mounting a Faraday cage inside a Crookes tube, that cathode rays convey a negative charge. As a consequence of this research he concluded that;

> '..this is difficult to reconcile with the theory of undulations but it agrees well with that of emission.'[58]

In his doctoral thesis on cathode rays and X rays, which was published in the Annales de chimie et de physique in 1897, Perrin, following a line of reasoning already advanced by J.J.Thomson, suggested an experimental method whereby the ratio of the charge to the mass e/m could be determined.[59] Perrin, like his contemporary, Langevin, was a pupil of Marcel Brillouin, the champion in France of the ideas of Boltzmann on statistical mechanics and an opponent of the 'energetics' of Ostwald, at the Ecole Normale, and both were influenced by Brillouin's ideas.

Consideration of Perrin's doctoral thesis has taken us to 1897, and we must go back two years to consider the reaction to the news of Rontgen's discovery of X rays in France. Rontgen, working in Wurzburg, experimenting with a Crookes' tube, noticed that certain materials outside and at a distance from the tube, were made to fluoresce. The mysterious rays which caused this fluorescence were found to be very penetrating, and with their aid, Rontgen was able to make a photograph of the bone structure of his own hand. Rontgen rays, or X rays as they soon came to be called, captured the popular imagination as no previous scientific discovery had done, and information about them , often sketchy and imprecise, reached foreign scientists almost immediately through the daily press. There was a veritable explosion of X ray research throughout the European scientific community; the apparatus to produce them was comparitively cheap and already in place in many laboratories which had been working on cathode rays.

Such a laboratory was the one at the Ecole Normale, where Jean Perrin had been investigating the properties of cathode rays, and now , working on the basis of 'quite vague scraps of information, drawn from the daily press, and which I still do not know are really his (Rontgen's) experiments',[60] Perrin repeated Röntgen's experiments. Perrin's work used photographic plates wrapped in black paper to demonstrate the transparency to the X rays of various materials, like paper, wood, paraffin and different metals. He then arranged his materials in a series of increasing opacity, headed by lead. He also employed all the usual techniques to make evident the wave properties of the X rays, but without success. He tried to reflect the rays, first using a metallic mirror and then a flint plate. He also sought to refract them using a prism of paraffin, and to obtain diffraction fringes, but all with negative results. Perrin did not conclude, from these negative results, that X rays were not waves, simply that if they were, they had a period very much less than that of visible light. Finally, collaborating with physiologists of the Ecole Normale and the Muséum d'Histoire Naturelle, Perrin made some X ray pictures of the bone structure of a frog. Perrin's paper, presented to the Academy in February 1896, and bearing the marks of being rather hurried, was the first on the subject, by a physical scientist, in France.

Rontgen's discovery provoked a sharp peak in the number of papers on X rays and cathode rays submitted to the Academy. There were 160 in this category of the index of the Comptes rendus in 1896, falling sharply to fifty four in the following year. In this category were also included the papers of Becquerel (one in 1896 and two in 1897) and those of Gustave le Bon (b.1841) on his 'black light' (one in 1897).

As the research into cathode rays and X rays was principally in the hands of agrégés-préparateurs and as there were no such posts at the Ecole Polytechnique, this explains, in part, the absence of the Polytechnique from the excited burst of activity which produced so much new knowledge in the mid 1890's. It is not true to say that polytechniciens played no part in this process of research and discovery , because of course it was the mathematician Henri Poincare, both a product and a professor of the Polytechnique, who had the insight that perhaps fluorescent materials might also emit X rays at the same time as they emitted light. it was this idea, which was to redirect the research of Henri Becquerel, physics professor at the Polytechnique and at the Muséum. Becquerel, working in the Muséum, and continuing what had been his personal line of research for many years, set out to see if a fluorescent salt emitted X rays when irradiated with light. Becquerel's experiment used a fluorescent salt, which he exposed to strong sunlight then left in close proximity to a photographic plate wrapped in black paper. His happy chance was to use uranium potassium sulphate, and then to develop his plates even though he had been able

able to expose his sample to very weak diffuse sunlight. Expecting weak images, he found images of great intensity. The circumstances of this discovery, in which chance played a role, has already been dealt with by Badash,[61] and so it will not be considered in detail here.

Becquerel, having found that uranium salts emit invisible rays, proceeded later in the year to compare their properties with cathode and X rays.[62] Apart from the photographic techniques which he used previously, he now began to employ a special type of electroscope developed in Paris by the Romanian, Hurmuzescu. This was esentially a type of gold leaf electroscope in which the divergence of the leaves could be read from a vertically mounted protractor. The rate at which the divergence of the leaves decreased, measured in seconds of arc per second of time, gave a measure of the intensity of the invisible radiation causing the electroscope to discharge. For example, a phial of double sulphate of uranium placed below the leaves, dissipated their charge at the rate of 22.5 seconds of arc each second. When a 5 mm. thick sheet of quartz was placed between the salt and the leaves, the rate of discharge fell to 5.43 seconds/ second. The ratio of the rates of discharge was 4.15:1. He then measured the effect which rays, emitted from a Crookes tube through an aluminium foil 0.15 mm thick, had on the electroscope. They discharged it at the rate of 1 degree in 1.4 seconds, or 2571.4 seconds/second. When the sheet of quartz was interposed, the rate fell to 163.63 seconds/sec.; 15.7 times smaller.[63] Becquerel commented that the weakening effect of the quartz was about four times greater for the rays from the Crookes tube than for the invisible rays emitted by the uranium,which he observed could be due to the difference in wavelength of the two radiations. It is important to mention here, that Becquerel does not use the term 'cathode rays',and in his description of the experiment,it is not clear whether he thought he was employing cathode rays, or X rays emanating from the phosphorescent glass of the tube.

He also used this apparatus to compare the intensity of the uranium salt,in two very different conditions. Firstly he used a phial of uranium salt which had been kept for eleven days in darkness, and then tested the same sample immediately it had been illuminated by the intense light of burning magnesium. The rate of fall of the leaves of the electroscope was 20.69 in the first case, and 23.08 in the second case.[64] Becquerel made no comment on this result; he did the experiment because he thought the radiation mechanism was similar to phosphorescence; i.e. that the invisible radiation was a different form of the energy which the salt had previously absorbed as light and then slowly emitted. But his experiment did not confirm this hypothesis although the radiation did seem slightly more intense after the magnesium light. In the same paper Becquerel reported on his experiments with different uranium salts, some of which were neither phosphorescent or fluorescent, but all emitted invisible radiation with similar intensities. He also made a qualitative study of the absorption of the rays by different materials; finding rather curiously, that blue glass was more opaque to the rays than either aluminium or tin.[65]

An experiment to show whether or not the rays were refracted through a crown glass prism, appeared to give a positive result, but the image on the photographic plate was so diffuse that Becquerel was unable to make any measurements of refractive index. Attempts to show the phenomena of polarisation and double refraction using crystals of Iceland spar and quartz, also produced images on the photographic plate, too weak to give any reliable information. On the other hand, however, Becquerel could say definitely, that other fluorescent materials, those not containing uranium, did not produce the invisible rays. This reversed the conclusion of an earlier paper in which Becquerel had reported that calcium sulphate gave results of the same order as that given by uranium salts.[66]

Whatever the later significance of Becquerel's discovery, it would be wrong to imagine that it triggered off an avalanche of research in the field. On the contrary it was seen as a secondary, subsidiary phenomenon associated with X rays, and as such, evinced little interest in France and less outside it. Even Becquerel himself gave up the work in 1897 to return to research on opticsl questions like anomalous dispersion and the Zeeman effect, and only returned to it after the discovery by the Curies of the new radioactive elements, radium and polonium in 1898. In 1896 for example, Becquerel had nine papers published in the *Comptes rendus* relating to the invisible rays from uranium,while in 1897 he produced only two papers, and one of these was a refutation of the work of LeBon on 'black light'. Much more popular topics in 1896 and 1897 were cathode ray and X ray research, although these too fell sharply from a peak of around 150 in 1896 to only nine in 1899. Work in these two fields was mainly in the hands of George Sagnac (1869-1928) working in the laboratory of Bouty in the Paris Faculty and Paul Villard (1860-1934) whose work was done in the chemistry laboratory of the *Ecole Normale*. Sagnac concentrated on X ray research, demonstrating that a metal surface irradiated with X rays, emitted what he called secondary X rays, X rays with a lower penetrating power than those which produced them. Villard's work was with cathode rays, and on the properties of fluorescent screens.

A brief mention has been made above, to the work of LeBon on "black light". The scientific and philosophical roots of LeBon's work have been very well dealt with in the paper of Mary Joe Nye [67] and so we will not repeat this analysis here. It is worth saying however, that the anti-rationalist, anti-materialist ideas of Bergson, which gained ground in the last decade of the nineteenth century in France, and which influenced LeBon, played little role in shaping the thinking of the academic physics community. LeBon clearly was influenced, but he was a medical doctor not a trained physical scientist, and although he dined regularly with Poincaré and exchanged ideas with him and several other scientists, there was no working collaboration between LeBon and anyone working in this field. In 1899 he did work with the Catholic physicist, Branly, in research on the absorbing effect of blocks of cement and stone on wireless waves,[68] but this is the only example, and in any case Branly was himself something of a outsider. LeBon's work on black light in 1896, had negligible impact on the physics community and was comprehensively demolished by Becquerel in 1897.[69]

After a period of nearly two years Becquerel returned to the field of radioactive studies, with a paper given to the Academy in March 1899, which corrected some of his earlier conclusions. He admitted that the phenomena were much more complicated than he had previously thought, but insisted on three fundemental aspects which he had discovered and which later researchers had confirmed;

> '..the spontaneity of the radiation, its permanence, and its property of rendering gases capable of conducting electricity'.[70]

Apart from making a re-analysis of some hundreds of photographs which he had made over the past three years, Becquerel began a series of experiments with samples of radium and polonium which the Curies had made available to him. Still using the same photographic methods,he investigated the reflexion, refraction and absorption of the rays from the two materials. It seemed to him that the rays from radium were much more penetrating than those from polonium, and this was the;

> '..only indication which would permit one to characterise the two rays as being of a different nature'.[71]

Becquerel still tended to think that the rays were similar to Rontgen rays, and that emission was a process analagous to Sagnac's secondary emission. That the emitting substance must possess a store of energy, which did not seem to diminish with time, was axiomatic for Becquerel, and he still tried to find an explanation for this using ideas from his studies on phosphorescence.

Later in the same year Becquerel reported to the Academy in a short note, some more profound conclusions about the properties of the radiation from materials supplied to him by the Curies.[72]
Using a powerful electro-magnet he investigated the effect of a magnetic field on the path of the rays, and found that the rays from radium were deviated while those from polonium were not. The rays from radium seemed to be similar in some respects to cathode rays but without all their properties.

If the rays had exactly the same properties as cathode rays they would convey a negative charge, and therefore would be deviated by an electric field. But neither the Curies nor Becquerel himself had been able to demonstrate any deviation of the rays by an electric field. On the other hand, Becquerel could calculate an approximate value for the velocity of the rays and this appeared to be of the same order as that of cathode rays. For this calculation he used the hypothesis which he had formed in his work on magnetic rotatory polarisation; that the magnetic field could be compared to an etherial medium endowed with giratory movement, like a whirlpool.

During the first half of 1900, the work on radioactivity using radium predominated over all other radiation studies. In the Comptes rendus for the first half of the year (vol.130) there were thirteen entries under radium, one under uranium, nine under X rays and four under cathode rays. Becquerel was responsible for six of the radium studies and the single piece of research using uranium. The Curies produced three of the radium papers, and Pierre Curie, with Georges Sagnac of the Paris Faculty produced one on X rays. Cathode ray work was mainly centred in the Ecole Normale with Paul Villard. The only provincial faculties to be engaged in this new work were Dijon, where Bernard Brunhes was doing work on the velocity of X rays employing the Kerr effect, and Lyons where Gouy was attempting to measure the wavelength of X rays.

In this final year of the century, Becquerel, although continuing to use photographic plates as detectors, pushed the knowledge of the radiation further forward by showing that an electric field could deflect the deviable rays. Now the value of the ratio e/m for the rays was calculated, together with the electrical energy radiated per second, the number of particles emitted per second and hence the mass loss per second.[73] Becquerel's figure of 1mg. in a million years, was of course grossly in error, and it was only after Einstein's special relativity theory that the true mass loss could be calculated.

Also in this year, Curie and Sagnac together showed that X rays convey no charge, and Villard concluded in his work on the radiation from radium, at the Ecole Normale, that the non-deviable rays consisted of two components, one of which was very penetrating. Later the same year he called the rays a type of X ray, and showed that they were more penetrating than the deviable rays.[74] Although the modern nomenclature of alpha, beta and gamma rays had not yet appeared in the literature, Villard is today credited with the discovery of gamma rays, the very penetrating radiation which he identified with X rays. In fact, the electro-magnetic nature of these rays was not established beyond doubt until 1914.

These final years of the century eere exciting ones in which a completely new field of study came into existence, and they were years in which the French could truly regard themselves as world leaders in the field. What may have seemed disturbing to the proponents of decentralisation, however, was that despite years of efforts to raise the status of the provincial faculties, this work was still almost the exclusive preserve of Parisians. The humble Ecole Municipale was now competing with the Parisian centres of scientific excellence in the new radiation studies, while the Ecole Polytechnique, once a centre of innovation in teaching and research, was playing no institutional role in radioactivity. It is true that Becquerel was a physics professor there, but it can be said that the school played no role because its facilities, laboratories, and junior staff members were not involved in the research as they were at the Ecole Normale and the Paris Faculty.

f. The provincial faculties

The research of some provincial faculty staff has already been mentioned when the work reported to the Société de Physique and the AFas was considered. Some faculties had, by this last decade of the century, become specialised in some restricted field of physics and had gained a national reputation for their work. Although the Faculty of Nancy was not quite in this position by the opening of the decade, the discovery by Hertz of electro-magnetic waves in the last years of the previous decade had opened up a new research area, and this was exploited by the Nancy Faculty physics professor, Rene Blondlot. One must say that he had few competitors in France when he began his work, in fact only Branly of the Catholic Institute in Paris concernd himself with this field, although later in the decade A. Turpain of Bordeaux was a regular contributor to the meetings of the AFas on the propagation of electro-magnetic waves.

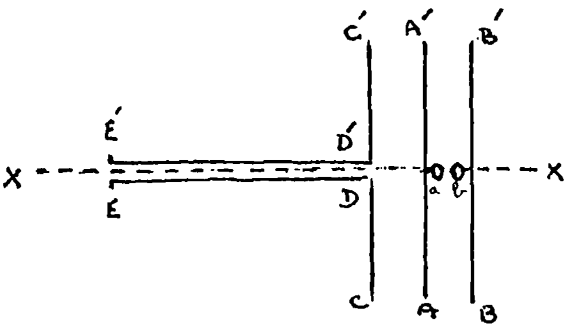
Apart from the work in this field in Germany, and by Lodge in England, the Swiss, Sarasin and de la Rive were investigating the properties of electromagnetic waves and their research was very accessible to the French, being as it was, published in French in the Archives des sciences physiques et naturelles of Geneva. Blondlot appears to have followed very closely the research in electromagnetic waves which was being undertaken in Germany and Switzerland, and in the early 1890's there was a steady stream of papers on this subject from him, often of great experimental ingenuity, and aimed at testing some of the, as yet untried, predictions of Maxwell's theory. Blondlot very quickly earned a high reputation and won powerful friends in Paris, in particular his fellow Nanceian, Henri Poincare, who was to be one of his allies in the later controversy over the existence of 'N' rays. Unlike most French savants, Blondlot, had no ambition to succeed to a Paris chair; his roots were in Lorraine and the Faculty of Nancy. And it should be remembered that Nancy, so close to Germany, was especially favoured in the question of funds, benefitting from both local industrialists and a particular generosity of the Ministry of Education. Blondlot's first piece of research in this field took one of the predictions of Maxwell and subjected it to experimental test. Electromagnetic theory predicted that the refractive index of a dielectric material was equal to the square root of its specific inductive capacity, or what would now be called its relative permittivity. J.J. Thomson had already reported to the Royal Society in 1889, that when glass is used in a capacitor and charged and discharged $25\text{x}10^6$ times in a second, it had a relative permittivity which was the square of its refractive index, but that this relationship did not hold good for lower frequencies. Another research worker, Lecher, reached exactly the opposite conclusions to Thomson about the way relative permittivity changed with frequency, concluding that it increased rapidly.

To decide between the two competing conclusions, Blondlot employed a method which used e.m waves but did not use the usual formula for an oscillatory circuit;

$$T=2\pi\sqrt{LC}$$

Blondlot had a preference for experimental methods which were independent of any formulae, and the method which he used, in this case, comparing the relative permittivity of glass and sulphur, was the first to use e.m. waves.[75]

The diagram of his apparatus together with an abbreviated description is given below.



It consisted of large rectangular plate AA' and a smaller plate BB' which together formed a capacitor. This could be charged from an induction coil and discharged through the balls a and b. The period of the oscillatory discharge is in the order of 25 x 10^{-6} secs.

When the plates are discharged, an e.m. wave is transmitted from AA' and received by CD and C'D'. In Blondlot's own words;

> 'When the coil is functioning, no spark is observed between E and E'; this results from the symmetry of the apparatus. If we interpose between AA' and CD, a sheet of glass, then the spark jumps between E and E'; this is because the induction received by CD has been stronger than that received at C'D'.[76]

Blondlot could restore the symmetry of the system by putting into the space between AA and C'D', a sheet of sulphur whose thickness was adjusted until the spark between E and E' disappeared. Using a sheet of glass of 3cm thickness, symmetry was restored when the thickness of sulphur was 3.15 cm. The dielectric constant of sulphur was found by a method given by J. Curie to be 2.94, and by a simple ratio of the thicknesses, the dielectric constant for glass was calculated to be 2.8. The square root of this, 1.67, gives a very rough agreement with the refractive index of glass at 1.5. As we shall see, Blondlot was to return to this line of research, but employing a different experimental arrangment, later in the decade.

Blondlot's next piece of electro-magnetic research was to verify as Maxwell had predicted, that e.m. waves travelled at the same speed as light.[77] Blondlot's method was to set up standing waves in two long wires, and to move a resonating device equipped with a spark gap along the wires, measuring the position of the nodes and antinodes in the standing waves. Being able to calculate the frequency of the exciting wave from the geometry and construction of the resonator, and measure its wavelength from the distance between successive nodes or antinodes, Blondlot could calculate the velocity of the e.m. wave from the product of frequency and wavelength. He found quite close agreement between the values for light and e.m. waves, a result which was favourable to the hypothesis of the identity of the two. The frequency of the received wave could also be altered by changing the dimensions of the resonator. Blondlot presented a table of data for the wavelengths which corresponded to 13 frequencies and obtained a mean value for the velocity of these waves,of 297,620 Km/s. He was also able to conclude that;

> '..electric undulations have a unique value of propagation,independent of wavelength'.[78]

The spread of his 13 results were within 5%, which corresponded to the precision with which he could measure the position of the nodes. At a node in the standing wave, the spark would be at its feeblest, and the spark gap would have to be closed up to allow it to jump across. At the antinodes, the spark would be strongest and would leap across the greatest width of gap. This method of locating nodes and antinodes by means of the length of spark gap, was the one introduced by Hertz, and required a fine micrometer adjustment to the gap. Later it would seem, Blondlot abandoned the measurement of spark gap length as an indication of the strength of the received signal, and simply used the intensity of the spark, its brightness. It was this rather subjective experimental method which was to lead to his later completely false observations in relation to N rays.

Blondlot does not say where his experiments were conducted, but one can only assume, considering the wavelengths he was using, and the need to measure between a number of successive nodes, that his wires were in the order of 100 m. long and that the experiment was conducted in the open. This must also have increased the difficulty of making precise measurements.

In the following year, Blondlot came back to continue his investigation of the relationship between refractive index and dielectric constant, but this time using the method he had employed to find the velocity of e.m. waves.[79] By considering the dimensions of the equation;

$$T = 2\pi\sqrt{LC}$$

Blondlot concluded that 'in a given oscillator, the wavelength of the waves which it can emit must remain the same whatever be the insulating medium in which the experiment is made'.[80] This permitted him to make a simple theoretical verification of the relationship which Maxwell had given between refractive index and dielectric constant;

$$K = n^2$$

Using an electric oscillator whose capacitor was filled with castor oil, and measuring the wavelength of the e.m. waves propagated through the oil, Blondlot found a value of 2.18 for K, which gave a value for n of 1.476, against the value obtained by optical means of 1.4674.

Profiting from the very cold winter of 1892-3, Blondlot with a colleague, Dufour, set out to determine the dielectric constant of ice using electro-magnetic waves.[81] This was done by measuring the wavelength of e.m. waves in ice, using Blondlot's usual apparatus of a resonator moving along the length of two parallel wires which were acting as transmitters of hertzian waves, and locating the position of nodes and antinodes. This gave a value for refractive index of 1.41, from which K for ice would be two. Unfortunately for Blondlot, two Parisian researchers, Bouty and Perot, working in the laboratory of the Paris faculty had found K to be 78.[82] Perhaps one is influenced by Blondlot's later research on N rays, but it is impossible to escape the feeling that, knowing the result he wanted to obtain, Blondlot sometimes deceived himself that he had found it.

Perhaps the shortcomings in Maxwell's relationship $K=n^2$ came to be discussed later in the decade, but in these early years at least, Blondlot seems to have believed that he had verified it. But in fact the law holds good only in the case of gases, because the electrical polarisation of the medium, which is supposed in Maxwell's theory to follow the changes in field strength instantaneously, actually lags more and more behind the applied field as the frequency increases. It is only for comparatively slow vibrations that the polarisation follows the applied field at all faithfully, so that it is not surprising that K , found for dielectrics using high frequency e.m. waves, does not agree with the value found by static methods, or those which involve slow charges and discharges. But whether or not it can be said that Blondlot had already began to deceive himself about some of his results, it cannot be denied that he had accumulated a considerable body of research by this time and its importance was first recognised by the Academy in 1893 by awarding him the Plante prize, and six years later with the more prestigious LaCaze prize.[83]

Some other work by Blondlot in this period measured the velocity of an electrical pulse of high frequency through a long length of cable, finding very poor agreement with the value found by Fizeau and the telegraph engineer Gounelle in the 1850's. Blondlot argued that the discrepancy came from the fact that in his experiment the disturbance travelled in the surface of the conductor,(a phenomenon which came to be known as the 'skin effect'), whereas in Fizeau's experiment the lower frequency pulse travelled in the whole body of the conductor.[84] More electro-magnetic work was carried out independently by Dufour in Nancy, but it used the apparatus of Blondlot and simply confirmed some of the earlier work of Sarasin and de la Rive in Geneva.

There was little or no research on cathode or X rays in the provinces, partly because of the lack of the Crookes tubes which Gariel had complained about (see the section on the AFas) , and partly because there were no agrégé-préparateurs in the provincial faculties. For it was these junior faculty members who were most enthusiastically tackling the new ray research in Paris. This is not to say that ray research was entirely absent from the provinces; there was the work of Gouy in Lyons and Brunhes in Dijon as was mentioned in the section on the provincial faculties. There was also no research into Becquerel rays in the provinces, and the study of radioactivity remained exclusively a Parisian activity until the outbreak of the Great War.

g.The intellectual climate of the last decade.

After the establishment of the Catholic Institutes in 1875, the next important educational reform was the measure , which in 1885, allowed the faculties to raise what funds they could locally, from the municipality and local industry. Between 1885 and 1900, industry, private donors and departmental and municipal authorities contributed more than 30 million francs (three quarters of the total employed) for the construction and refitting of laboratories in the local science faculties. Some faculties inevitably did better than others from this new funding; Nancy, Grenoble and Lille fared best, while faculties in smaller cities without much local industry, like Caen and Poitiers did less well. The Paris Faculty also benefitted from these new arrangements, which nonetheless tended to weaken the previous over-centralisation of the system and improved the links between theoretical science and technologically based industry. On the debit side, the growth of lower level instruction in applied science (which the technological institutes attached to the faculties offered) took up more of the time of the physics teachers.[85] But nevertheless, already in the 1880's, physics teachers on the geographical or institutional periphery, Blondlot of Nancy, Gouy of Lyons, Branly and Curie in Paris, were making contributions, which their contemporaries considered significant, in those areas of physics which the Paris Faculty and *Grandes Ecoles* wer neglecting.

From a wider political and social point of view, the final decade of the century promised, after the alarms of the Boulanger crisis and the Panama scandal of the 1880's, a period of stability and tranquillity. The intense State/Church antagonism of the early years of the Republic seemed to be weakening in 1892, when Pope Leo XIII declared the policy of *ralliement*, in which the Church abandoned its support for legitimism and called on French Catholics to support the Republic. But the Third Republic continued to espouse a philosophy, which was militantly anti-clerical, if not always anti-Catholic. This philosophy, which came to be called *scientisme*, was underpinned by the notion that science constitutes the ideal form of knowleldge, and that value-free scientific concepts can be applied to all aspects of humanity and society. A quotation of the Republic's favourite and most successful scientist, Marcellin Berthelot, concerning the social utility of science, and extolling its role as a foundation for all aspects of human activity has already been referred to in chapter three.[86]

Science was the republic's weapon against the Church and the legimists; ultimately science would explain everything and organise everything. Berthelot served as a government minister, and in general, scientists enjoyed a high prestige in the Republic, serving it as a kind of lay priesthood of *scientisme*.

But in the middle of the decade, the smouldering antagonism between *scientisme* and religious and idealist ideas, flared up again. The opening shot came with an attack on the underlying assumptions of *scientisme* in a novel, *The disciple*, in which a student influenced by Darwinist ideas of the 'survival of the fittest' commits murder. As a response to this, the partisans of science republished Ernest Renan's optimistic work *The future of science*, which had first seen the light of day forty five years before.[87] Then, in 1894, the literary historian and editor of the journal *Revue des deux mondes*, Ferdinand Brunetiere, returning from a visit to the Pope, published his article on 'The bankruptcy of science'. Science, avowed Brunetiere, had failed to fulfill the axtravagant claims people like Renan and Berthelot had made of it, and he denied that it was possible to 'draw from the laws of physics or the results of physiology, any way of knowing anything.[88]

Republican politicians saw the attack on science, not as an esoteric philosophical dispute, but an attack on the policies of the regime, and if some scientists saw the whole discussion as irrelevant and misconceived, Berthelot responded by continuing to express the most extreme formulations of *scientisme*; reiterating his confidence in scientific method as the most effective way of arriving at the truth, and expressing his confidence in a future in which science would banish war, poverty and ignorance.

But not only were there political attacks on *scientisme* but more fundamental philosophical ones on its foundation, positivism. A new philosophical outlook which was anti-rationalist, anti-materialist (in that it attacked the notion of the existence of an unchanging mechanical basis to the physical world) an outlook which stressed intuition above reason, came to the fore associated with the name of Bergson. Although it had been argued that Lebon was influenced indirectly by Bergson, it is not possible to see an influence on any professional French physicists. Probably Bergson is more important as being simply one part of the general reaction against science at the end of the nineteenth century, a reaction which in turn prompted scientists to reassess the aims and the scope of their physical theories. As Heilbron has argued for physicists in general, although it applies with much greater force for French physicists;

> 'The physicists of the fin-de-siècle sought to redefine their professional objectives so as simultaneously to achieve internal consensus and secure their place in the wider society'.[89]

In front of the criticism and even ridicule of a wider intellectual society, and faced with a bewildering array of new experimental facts, some French physicists withdrew any claim they might have previously made, to answer the big questions. What was important and lasting, were phenomena and the relations between them, hypotheses, models and theories were no more than convenient and abritrary aids, to help us grasp these relations. Certainly this was the position most systematically enunciated by Poincaré, and shared by many French physicists.

But what must also be examined is the discussion within the scientific community itself in the decade, on the mechanical view of nature. Had the French physics community been as thoroughly positivist as some have suggested, such a discusion would have been seen by its members as no more than a dispute between metaphysicians. It is true that notorious anti-atomists and positivists like Berthelot, Lippmann, and Lechatelier can be identified, but more characteristic of French physicists of the period, was a certain consistent, if often unstated, attachment to mechanical assumptions.

To explore this more fully we ought to go back a little and look at the development of the argument for and against atomism as it took place in France in the 1890's. We have already seen that, in 1888, the Lyons faculty professor, Leon Gouy, had alerted the French physics community to the importance of Brownian motion as both a visible experimental demonstration of the kinetic theory of matter, and a contradiction of Carnot's principle. In 1894 in a lecture in the Lyons Faculty, later reprinted in the Revue générale des sciences,[90] he took up again the question of Brownian motion, and did not shrink from;

> 'leaving the solid terrain of observation and experiment, to enter into the uncertain domain of hypotheses on the constitution of matter'.[91] He stressed that '..theoretical speculation has been the origin of the greatest progress and yielded the greatest harvest of discoveries'.[92]

Gouy went on to explain how the observation and measurement of the velocity of the solid particles agitated by molecular motion, would yield information about the velocity and the dimensions of the molecules themselves. He ended by calling the attention of physicists to this phenomenon, which they had previously neglected as being unimportant and outside their sphere of study. With such a study, ended Gouy;

> '.I have the firm confidence, that thanks to their efforts, we will penetrate more and more into the knowledge of the intimate properties of matter, already so fruitful and rich in promise for the scientific and industrial development of humanity'.[93]

Gouy's appeal was not immediately taken up, but Jean Perrin in the early years of the twentieth century was to use Brownian motion to show the reality of molecules and to deduce a value for Avogadro's constant. But this, and other successes for the atomic and kinetic view of nature, were to come later, and the middle years of the 1890's were probably more notable for the attack on this view by the 'energeticists' led by Ostwald. The sharpness of the debate between the two schools of thought was exacerbated in France by a clumsy translation of the title of Ostwald's 'Die Uberwindung des Wissenschaftlichen Materialismus'in the Revue générale des sciences of 15 November 1895. It appeared in French as 'La déroute de l'atomisme contemporain', a title which Ostwald himself, who had not read the proofs of his article, felt was too abrasive, preferring the more anodyne 'La reforme de la physique generale'.[94] But even given that the title expressed the ideas of Ostwald in a rather extreme and distorted form, the content too was offensive to the scientific materialists of France. Cornu found himself 'perhaps more than anyone else,wounded ' by the article,and particularly by the phrase ;

> 'It is a vain enterprise which has failed pitifully
> before all serious experience,this desire to explain all
> the known physical phenomena by relation to mechanics'.[95]

Moreover, Ostwald asserted that the undulatory theory of light, had been buried by the electromagnetic theory,and;

> 'making an autopsy on its corpse we find that the cause of
> death becomes evident; it has resulted from the failure of
> its mechanical parts'.[96]

By this, Ostwald meant the elastic solid ether, whose properties, Fresnel, Cauchy, and Boussinesq in France, and Green and McCullach in Britain, had struggled to define. Cornu defended the undulatory theory in what he considered its essence; propagation by wave motion of luminous or electric disturbances, and the transverse nature of the wave. To a convinced materialist like Cornu, the gratifying thing was to see that rational mechanics,'with such simple and restricted elements -material points and reciprocal action- could succeed in rendering account of so many diverse and complicated phenomena'.[97]

And Cornu was not the only one to raise an indignant voice of protest against the attack on principles which had shown themselves so fruitful over three centuries, for the same issue of the Revue also carried a short note by Marcel Brillouin. Brillouin with Gouy, can be considered as a central figure in the defence of atomism in France, and his pupils were to play a central role in the development of atomic physics in the twentieth century. In his article 'Pour la matière',[98] Brillouin makes a plea for a greater liberalism in relation to theories, seeing Ostwald's article as an attempt to stigmatise as backward, and to excommunicate those scientists who wanted to employ mechanical images. He argued that science was like a double entry account book; representations on one side and physical facts on the other. Some, because daily experience had familiarised them with mechanical phenomena, and Brillouin included himself in this category, liked to put mechanical images on one side of the account, others, he admitted, would prefer to use numerical representations, differential equations, but both would be representations of the facts.

One statement of Brillouin's is particularly interesting. He made the point that although now in 1895 he feels the need to defend matter, to argue for its very existence, some 15 years previously as a teacher, he felt the need to warn his students against the excessive use of material representations, above all in electricity, where it was most often resorted to.[99] Thus it is clear that the mechanical, material view of nature was well entrenched in the faculties in the 1880's.

Another important point which comes out of the discussion on the 'bankruptcy of science', and in the energetics/mechanics debate, is the heterogeneity of the philosophical positions of French physicists. It cannot be said that the centralisation of the educational system (weakening a little, to be sure, as a result of government reforms), led to a uniformity in the views of her physicists, they were as diverse as those of their peers in Germany or Britain. Thus, philosophical pluralism, the absence (natural in a time of revolutionary change) of authorities in the new physics, combined with comparatively inexpensive apparatus, meant that junior members of the Parisian teaching institutions could play an unusually important role in research into most aspects of 'ray physics'. However, the absence in the provincial faculties of able *agrégés préparateurs* enthusiastic to make their name in the new physics, the attachment of senior faculty members to their 'old and dear' lines of investigation, and the scarcity of Crookes tubes in the provinces, ensured that the new ray physics was almost entirely concentrated in Paris.

So it can be said that the physicists of France were engaged in an intense debate over a number of scientific and philosophical questions at the end of the nineteenth century. One could also add that they were not indifferent to the no less intense political discussion. When, for example, the underlying tension between the clerical/legitimist right and the republican regime flared into public debate, accusation and counter-accusation and street demonstrations, for and against the verdict of the court martial against Dreyfus, most scientists demanded the revision of the court's verdict of guilty. Perrin and Langevin were particularly active in this agitation, but many others showed their opposition to the verdict more cautiously.

It is, of course, to be expected that many scientists would feel obliged to defend the republican regime which had given them their education and their social status as academics in the state institutions. For the younger ones, political positions which the Ministry of Education might find dubious or disloyal could damage their career prospects and hence their ultimate social standing. This is not to say that Perrin, or Langevin, or the others who agitated for a revision of the verdict on Dreyfus, did so for cynical careerist motives, they were convinced of the justice of his case just as they were convinced by the optimistic message of *scientisme*, and this made them staunch supporters of the Republican regime.

It might be argued that an older member of the physics community, someone already embedded in the social and political fabric of the Republic, would be particularly careful, in the middle of all this conflict, not to make claims which could (in the fluid state in which physics existed) further damage science and the Republic which was closely associated with it. The theoretical caution of someone like Henri Becquerel who held positions of chief engineer in the *Corps des Ponts et Chaussees*, professor at both the *Museum* and the *Conservatoire*, and was a lecturer at the *Ecole Polytechnique*, can perhaps best be seen in this light. If Rutherford advanced much more audaciously in radiaactivity research than Becquerel, if there seems a contrast between a rough vigorous, colonial and an effete European, the contrast stems from the difference between someone who is restrained by a feeling of responsability towards both a scientific community and a political structure, and someone who is not. Becquerel found it safer to accumulate facts about radioactivity, not so much because of a positivist reluctance to speculate about the reality behind the phenomena, but because further experimental advance tomorrow might refute that speculation, and give further comfort to the enemies of science.

Pierre Curie, from whom, both because of his early formation and as a product of the faculty system rather than the *grandes écoles*, one would expect a strong allegiance to the Third Republic and its scientistic goals, is a rather different case to Becquerel. He did speculate about mechanisms, and make explanations for radio-activity, but never made the testing of these hypotheses the central axis of his research work. For him they were hypotheses, replaceable by others if need be, and secondary to the truths uncovered by experiment. On the other hand his personal temperament was introverted and cautious anyway, and it seems that he felt keenly that French physicists sadly needed the theoretical audacity of someone like Maxwell.[100]

But if the political, intellectual and scientific controversies of the last decade forced some physicists to adopt a defensive position, there were some among the younger generation who were both combative politicallly, (very early on giving their active support to Dreyfus) and audacious theoretically. Among such men were Perrin and Langevin whose early contributions to modern physics have already been mentioned and who would accomplish much more in the twentieth century.

Notes for chapter 5.

1. L. Poincaré,'Revue annuelle de physique',R.g.s.p.a., 1897, 8, 413-24, (413).
2. L. Poincaré,'Revue annuelle de physique',R.g.s.p.a., 1903, 14, 28-44, and 88-101,(28).
3. D. Landes,op.cit. (chapter 2 , note 1) p.236.
4. Speech of the retiring President, Cailletet, 17 Jan. 1896, S.s.F.p., 1896, pp.7-10, (p.9).
5. P. Forman, J. Heilbron, and S. Weart,' Physics circa 1900. Personnel, funding and productivity of the academic establishments', H.s.p.s., 1975,5, 52.
6. Ibid. p.88. The authors make the point that a Crookes tube cost 10-40 German marks, compared to 800-1000 marks for a Rowland diffraction grating, or 1,500 marks for an optical spectrometer.

7. Speech of J. Violle outgoing president in 1893. He gave the following figures; 17 papers in magnetism and electricity, 14 optics, 9 heat, 7 general phyics S.s.F.p., 1893, pp. 15-17, (p.16).

8. Speech of E. Bouty, outgoing president, 1897, S.s.F.p., 1897, p.7.

9. Ducretet, S.s.F.p., 1898, pp.51-61.

10. An example of an English word entering into French technical usage is that of 'self', in the term 'self-induction'.

11. A. Blondel, ibid, pp.77-78.

12. E. Branly, S.s.F.p., 1900, pp.9-15.

13. C. Guillaume, S.s.F.p., 1894, p.214.

14. P. Curie, S.s.F.p., 1894, p. 214.

15. Raveau, S.s.F.p., 1896, p.42.

16. J. Perrin, 'Rayons cathodiques, rayons X et radiations analogues', S.s.F.p., 1896, pp.121-29.

17. A. Cornu, 'Sur les observations ..des phénomènes découverts par le Dr. Zeeman', S.s.F.p., 1897, p.60*

18. C. Guillaume, 'Recherche de MM. Rubens et Nichol sur les radiations de grande longueur des ondes', S.s.F.p., 1897, p.40*.

19. H. Becquerel, S.s.F.p., 1896, p.87.

20. H. Becquerel, 'Recherches sur les phénomènes de rayonnement du radium', S.s.F.p., 1899, pp.179-185.
'Influence d'un champs magnétique sur la rayonnement des corps radio-actifs', ibid, pp. 186-93.
P. Curie, ibid, p.72*

21. P. and M. Curie, S.s.F.p., 1900, p.10*.

22. P. Villard, ibid., p.45.

23. P. Langevin, 'Sur l'ionisation des gaz', ibid., p.39*.

24. L.Poincaré,'Sur la différence de potential au contact d'une électrode at d'un électrolyte',C.r.A.F.a,s.,1891, 20, 169.

25. P.Duhem'Sur l'interpretation théoretique des expériences Hertziennes', C.r.A.F.a.s., 1895, 24,219.

26. Dr. Darin,'De la mort par l'électricité', ibid, 225.

27. C.M. Gariel,'La vision et la photographie par les rayons Röntgen', C.r.A.F.a.s.,1897,26,75-87.

28. Dr. H.Bordier et P Kolb.'De la conductibilité calorifiques des étoffes employées pour les uniformes de l'armée', C.r.A.F.a.s., 1898, 27, 120.

29. A.Turpain,'Sur la propagation des oscillations électriques dans les milieux diélectriques', C.r.A.F.a.s.,1899,28,vol.II,274-83.

30. P.Duhem, op.cit.,(ch.4,note 1),p.91.

31. P.Duhem, ibid, p.90.

32. A.Turpain, op cit.,(note 29), p.274.

33. A.Turpain,'Sur la télégraphie par ondes Hertziennes; La télégraphie dite sans fils', C.r.A.F.a.s., 1899, 28,vol.II, 298-301.

34. Ibid., p.301.

35. 'Chronique', R.s.,1882,4, p.414.

36. T Shinn, 'From corps to profession; the emergence and definition of industrial engineering in modern France', in Fox and Weisz ,op.cit., (ch. 1 ,note 29) pp.183-210, (p.199).

37. The only other Licencié to be encountered in this study so far, has been the physical astronomer P.J. C. Janssen.

38. The information on the relation between Curie and Gariel is taken from the admittedly hagiographic, *Madame Curie*, by Eve Curie, Paris, 1930, (trans. V. Sheean, London 1939)

39. A.R. Weill, 'Curie, Marie', article in the *Dictionary of Scientific Biography*, 15 vols. (ed. G. C. Gillispie), N.Y. 1972, Vol. vi , pp.497-503, (p.498).

40. Madame Curie received this prize, awarded to scientists lacking financial means, twice more ; in 1900 and 1902.

41. M. Slodowska Curie, 'Les rayons de Becquerel et le Polonium', *R.g.s.p.a.*, 1899, *10*, 41-50, (42).

42. Ibid.

43. M. Curie, 'Rayons émis par les composes de l'uranium et du thorium', *C.r.*, 1898, *126*, 1101-2.

44. M and P. Curie, 'Sur une nouvelle substance radio-active contenue dans la pechblende', *C.r.*, 1898, *127*, 175-78. (178).

45. M. Curie, 'Sur une nouvelle substance fortement radio-active contenue en pechblende', ibid, 1215-17.

46. M. Curie, 'Action du champ magnétique sur les rayons de Becquerel. Rayons deviés et non-deviés. *C.r.*, 1900, *130*, 73-76.

P. Curie, 'Sur la penetration des rayons de Becquerel non deviable par le champ magnétiques', ibid, 76-79.

47. P. and M. Curie, 'Sur la charge électrique des rayons...' *C.r.*, 1900, *130*, 647-50.

48. Op.cit., (note 35).

49. H.A. Abraham, 'Sur une nouvelle détermination du rapport entre les unités C.G.S. électromagnétiques et électrostatiques' *A.c.p.*, 1892, 6th series, *27*, 433-525.

50. Ibid. 524.

51. P. Curie, 'Propriétés magnétiques des corps à diverses températures' A.c.p., 1895, 7th ser. 5, 289-405.

52. Ibid. 399.

53. Ibid. 397.

54. Ibid. 399.

55. Ibid. 401.

56. Ibid. 404.

57. Ibid.

58. J. Perrin,'Nouvelles propriétés des rayons cathodiques', C.r., 1896,121, 1130-33.

59. J. Perrin,'Rayons cathodiques et rayons de Röntgen. Etude expérimentale',A.c.p., 1897, 7th ser,11, 496-554.

60. J. Perrin,'Quelques propriétés des rayons de Röntgen', C.r.,1896,122, 186-88.

61. L. Badash,'Becquerel's 'unexposed' photographic plates', Isis, 1966,57, 267-69.

62. H. Becquerel,'Sur les radiations invisibles émises par les sels d'uranium',C.r.,1896,122,689-94.

63. Ibid, 690.

64. Ibid.

65. Ibid, 692.

66. Ibid.,693.

67. M.J. Nye,'Gustave LeBon's Black Light: A study in physics and philosophy in France at the turn of the century.' H.s.p.s.,1974, 4, 163-95.

68. G. LeBon and E. Branly,'Sur l'absorption des ondes Hertzian par les corps non-métalliques',C.r., 1899, 128, 879-82.

69. H Becquerel,'Explication de quelques expériences de M. G. LeBon', C.r.,1897,124,984-88.

70. H. Becquerel,'Note sur quelques propriétés du rayonnement de l'uranium et des corps radio-actifs, C.r., 1899, 128, 771-77.

71. H. Becquerel, ibid., 776.

72. H. Becquerel,'Sur le rayonnement des corps radioactifs', C.r.,1899, 129, 1205-07.

73. H.Becquerel,'Deviation du rayonnement du Radium dans un champ électrique', ibid ,809-15.

74. P.Villard,'Sur la rayonnement de Radium'ibid, 1178-79.

75. R. Blondlot,'Sur la détermination de la constant diélectrique du verre à l'aide d'oscillations électriques très rapides', C.r.,1891, 112, 1058-61.

76. Ibid.,1059.

77. R. Blondlot,'Détermination expérimentale de la vitesse de propagation des ondes électro-magnetiques',C.r.,1891, 113, 628-30.

78. Ibid,630.

79. R.Blondlot,'Sur la vitesse de propagation des ondulations électro-magnétique dans milieux isolants,et sur la relation de Maxwell', C.r., 1892, 115, 225-27.

80. Ibid., 226.

81. R. Blondlot ,'Sur la propriété des ondes e.m. dans la glace et sur le pouvoir diélectrique de cette substance', C.r., 1894, 119, 595-97.

82. Ibid, 597.

83. M.J. Nye,'N rays: An episode in the history and psychology of science', *H.s.p.s.*, 1980, *11*, 125-56, (129).

84. R. Blondlot,'Sur la propagation des ondes électriques', *C.r.*, 1893, *117*,

85. R. Fox. op.cit., (note 12, ch.2), p.117.

86. M. Berthelot, *Science and popular education*, Paris 1897, p.13. in S. Jaki, op.cit.,(note 55, ch. 2), p.86.

87. H. Paul, 'The debate over the bankruptcy of science in 1895', *French historical studies*, 1968, *5*, 299-327, (301).

88. F. Brunetière. This is taken from J. Heilbron,'Fin-de-siècle physics', in *Science in the time of Nobel*, 1987, pp.51-73, p.65.

89. J. Heilbron, ibid., p.51.

90. L. Gouy,'Le mouvement Brownienne et les mouvements moléculaires.', *R.g.s.p.a.*, 1895, *6*, 1-7.

91. Ibid, p.5.

92. Ibid.

93. Ibid., p.7.

94. Ostwald's preference for a title for his article was published in a footnote by the editorial board following criticism of the article by Cornu and Brillouin. *R.g.s.p.a.*, 1896, *6*, 1030.

95. A. Cornu,'Quelques mots de reponse à "La déroute de atomisme contemporain"'. ibid., 1031.

96. Ibid.

97. Ibid.

98. M. Brillouin,' Pour la matière', *R.g.s.p.a.*, 1895, *6*, 1032-33.

99. Ibid.

100. S.L. Jaki, op.cit., (note 36, ch. 4), p.289.

6. FROM THE CONGRES INTERNATIONAL DE PHYSIQUE TO THE OUTBREAK OF THE GREAT WAR.

a. Introduction.

In continuing this survey into the early twentieth century, it again becomes necessary to narrow the focus on the field of view. The scientific community continued to expand and the number of research papers which were published grew exponentially, making it impossible for the historian to do full justice to the whole range of research. For this reason the chapter will concentrate on the work done in ray and atomic physics up to 1914, partly because it was the field in which the French could justifiably claim to be in the forefront at the turn of the century, and partly because it was a new area, breaking new ground and provoking considerable excitement both inside and outside the scientific community. For a young research worker, atomic and ray physics seemed to be the areas where discoveries and reputations could be made.

It might be said that the event which closed the old century and began the new one for physics was the *Congrès International de Physique*, organised in the summer of 1900 by the *Société de Physique*. The material presented at the congress was published a few months later by the society in a work which its editors, Charles Guillaume and Lucien Poincaré announced would serve;

> 'To mark the stage attained today by the human spirit in its eternal voyage in search of the truth.'[1]

The congress began its deliberations with the opening session adressed by Cornu, and finished them a week later with a reception and exhibition of new apparatus and experiments in the mansion of the society's most socially distingished member, Prince Roland Bonaparte. Some thousand people attended the congress, including delegates from foreign governments and scientific societies. Although Europe was still the centre of the world of physics, and most by far of the participants came from within her boundaries, there were several prominent Americans, including the meteorologist Cleveland Abbe and the electrical inventor Graham Bell. Asia too, was represented ; the Japanese physicst, Nagaoka (later to gain some eminence through his 'Saturnian' model of the atom) gave a paper on magneto-striction, and J. Chandra Bose from British India spoke on the effect of electricity on both inorganic and living material.[2]

Cornu's opening speech was brief and bold. One can find no positivist caution here, no rejection of speculation about the reality behind appearances. For Cornu, physics was the search for the properties of the basic material of the universe, a search which would ultimately unify all physical laws.

> 'The deeper we penetrate into the knowledge of natural phenomena, the more does the bold Cartesian conception of the mechanism of the universe develop and become more exact, namely that in the world there is nothing but matter and motion. The problem of the unity of physical forces ...has again come to the fore after the great discoveries of this century. The constant concern of our modern leaders Faraday, Maxwell, Hertz....was to define nature more accurately and to unravel the properties of this subtle matter (matiere subtile, italicised by Cornu) the receptacle of world energy'.[3]

After the opening session, the meeting split into seven different sections, each one under the presidency of a prominent French physicist. The sections were; general questions, units and measurements, under the presidency of Benoit; molecular and mechanical physics presided over by Violle; optics and thermodynamics (Lippmann);

electricity and magnetism (Potier); magneto-optics and rays (Becquerel), cosmic physics (Mascart),and biological physics (D'Arsonval). Nearly eighty papers were presented, ranging over the whole spectrum of physics from the discussion of the basic units of measurement to the latest experiments on the emission of 'uranic' rays. Numerically at least, France was well represented among the speakers, for her savants contributed thirty six out of a total of seventy nine papers, but this is to be expected as she was the host. The question of their quality and originality is of course rather more difficult to assess. There can be little doubt, however, that the paper given by Poincare, entitled 'The relations between experimental physics and mathematical physics' remains today, a classic in the philosophy of science. This paper came at a time when theories which had been the cornerstones of nineteenth century science, like Fresnel's optics for example, were being called into question and where radioactivity seemed to cast doubt on the principle of conservation of energy. People outside the scientific community, seeing the impermanence of the laws and theories of science,, began to question its very rationality. Science had failed, it was 'bankrupt' as Brunetière had argued, now it was time for religion to have its say.[4] Poincaré addressed himself to this question of the 'bankruptcy ' of science, deriding the superficial scepticism of those who did not understand that 'even the ruins ..(of a theory) are good for something'[5] and redefining the aims and re-evaluating the validity of physical theories. This was a view of science which without doubt represented a retreat; theories were convenient aids to gain a better understanding of the relations between phenomena, they were not attempts to describe, however imperfectly, the reality behind appearances, and as such they could be taken up and discarded according to their usefulness in any situation.

But it would be dangerous to assume that all French physicists (as perhaps Heilbron tends to do in his chapter 'Fin-de Siècle physics'[6]) shared Poincaré's philosophical sophistication. As we have seen from Cornu's opening speech and the note by Lucien Poincare and Charles Guillaume, for them, physics was still a search for truth, the search for theories which could mirror reality, and they did not believe that these theories could be taken up and discarded at will. Possibly closer to the views of many French physicists, (although none of them acknowledged any debt to Duhem and few had read him), were those ideas enunciated by Duhem, when he said that the more complete a theory becomes, 'the more we apprehend that the logical order in which theory orders experimental laws, is a reflection of an ontological order.'[7] Such a view would tend to put stress on the importance of continuation from one theory to the next, would look for elements of continuity rather than breaks. Thus for Duhem, the advantage of the Helmholtz theory of electro-magnetism, over Maxwells's theory, was that Helmholtz's could be logically connected to classical ideas of electro-magnetism, while Maxwell's could not.[8]

Apart from the paper of Poincare, the French were well represented in those fields in which they traditionally excelled; metrology, precise measurement and interferometry. Guillaume and Benoit, both from the Bureau of Weights and Measures at Sevres, gave papers on units of measurement and on precision in the measurement of length respectively, while Mace de Lepinay of the Marseille Faculty reported on the measurement of length using interferometric methods, methods which were later to be developed by his student, Fabry.

The other principal French interest, optics, was represented by the paper by Cornu on his determination of the velocity of light, and by the paper of the Polytechnique teacher, Carvallo, on the theories and formulae of optical dispersion, but this type of 'normal science' would probably have had little interest for the new generation of physicists, both in France and abroad, who were looking for new fields where discoveries could be made and reputations established. An area which had been heavily researched by the French in the earlier years of the century, radiant heat or infra-red rays, now appeared to be wholly in the hands of the Germans. This had been an area of work of Ampere earlier in the century, it was continued by the Italian refugee, Melloni encouraged by Arago, and then in the middle years of the century it became the exclusive preserve of Provostaye and Desains. Desains with his assistant Pierre Curie were working in this field in the 1880's and it was carried on by Violle. But the three papers on infra-red radiation at the Congress came from the Germans, Rubens, Wien, and Lummer, and though they gave an historical account of the work, with credit going to the earlier French researchers, it was clear that all the latest theories on the energy distribution in the spectrum of a radiating black body, and the development of experimental work associated with this, were now coming from the other side of the Rhine. It also seems that the theoretical explanations of black-body radiation, developed by Stefan, Kirchhoff and Wien were not widely known in France until the early years of the twentieth century. Moreover, Violle, who had been working in this field in the 1890's, passed on to the less intractable problems of accurately finding the velocity of sound, (he presented a paper on this to the Congress), leaving the research on heat radiation to the Germans.

In the field of hertzian waves, the contribution of the French was stronger. It is true that the major theoretical contribution on the subject came from the Italian, Righi, but Branly reported on his work on 'coherers' and other methods for the detection of the waves. The other major contribution from France, on the determination of the velocity of hertzian waves, was given by the Nancy professor, Blondlot, and his assistant, Gutton. This was not simply a report of Blondlot's own work carried out eight years previously, but on all the experiments using different techniques and methods. The results of all the experiments, though differing slightly, one from the other, appeared to confirm the theory of Maxwell.

The French contribution at the Congress to the ' revolutionary' areas such as, cathode rays, radioactivity, ionisation etc, was however, very strong indeed, with contributions from Henri Becquerel, the Curies, and Paul Villard. In the same section there was also the paper from Lorentz on the ' Theory of the recently discovered magneto -optical phenomena', in which he gave an explanation of the Zeeman effect employing his particulate theory of electricity, and one from the Cavendish professor J.J. Thomson on the constitution of matter, establishing the material character of cathode rays by measuring their charge to mass ratio.[9]

Becquerel's paper 'On the radiation from uranium and on the different physical properties of the radiation from radioactive bodies',[10] gave a summary of his and other people's work in these fields since the discovery of radioactivity five years before. Although he referred to some work by the Germans, Giesel and Dorn, most of the research described was his own and that of Rutherford in Montreal. He reported his general conclusion that radiation was not due to any storing of light, ultra violet, or infra red energy, but was a property of the uranium itself. He could find no weakening in the activity of a sample over a period of four years, and no variation when the temperature of a sample was varied between -20 C and 100 C. He concluded that some of the emitted rays were similar to cathode rays.

Becquerel also described Rutherford's work using an ionisation chamber, in which it was found that the rate of production of ions is proportional to the intensity of the incident radiation, providing the pressure is kept constant. It was Rutherford, Becquerel asserted, who first called the more easily absorbed rays, alpha rays and the more feebly ionising but more penetrating rays, beta. The rays had been deviated in magnetic and electrical fields by Giesel, who had found from this, that their charge to mass ratio was of the same order as that for cathode rays, while the Curies had established that they conveyed a negative charge. Thus, while the nature of rays had been fairly well established by this time, the nature of the non-deviable rays was still unknown.

The paper of the Curies 'The new radioactive substances and the rays which they emit',[11] repeated some of the conclusions of Becquerel, but also introduced new apparatus and lines of research. They speculated whether radioactivity was a general property of materials and concluded that it was unlikely to be shown only by polonium, radium, and actinium, but that it was an atomic property, which could not be altered by physical state nor chemical combination. They also reported on the work on induced radioactivity, the effect which radium seemed to have on other materials brought close to it, causing them to radiate for a short time. They found approximately the same results using zinc, brass, bismuth, nickel, aluminium, and lead. Others, namely Giesel, Rutherford, Debierne, and Villard had found similar effects. One could probably say, that at this stage France was leading the world in the production of new radioactive elements like polonium, radium and actinium, but in the examination of the properties of radioactive emissions, and in the formulation of a theory of radiactivity, Rutherford and his team of research workers were establishing their pre-eminence.

Paul Villard's paper entitled 'Cathode rays'[12] had much more previous work to review, and there was litle new in it. It is rather strange that the representation of the Germans in this section was so weak. There were no papers on X rays, which were a German discovery, and nothing from Germany on photo-electric emission, which at that time was mainly in the hands of Lenard. The major paper on photo-electric emission entitled 'On the actino-electric phenomena produced by violet rays'[13] was the joint work of two provincial faculty profesors; Bichat of Nancy and Swyngedauw of Lille. As in most of the papers presented to the Congress, the authors gave a full historical account of research in the subject, as well as a review of modern work. Swyngedauw, whose doctoral thesis of 1897, and most of his subsequent research had been in photo-electric emission, was the foremost researcher in this field in France, and the joint paper carried many references to his work. Lenard's discovery that some metal surfaces irradiated with blue light gave off, what were essentially cathode rays, (Lenard measured their e/m ratio and found it substantially the same as J.J. Thomson's value for cathode rays), came just in time to be included in the paper. In the next few years Lenard, who had not attended the Congress, became the unquestioned leader in the experimental study of photo-electric phenomena, whose explanation had to wait for Einstein's photon theory in 1905.

In general one can say that the German intervention in the revolutionary 'ray physics' area was very weak, probably because they had started late in radioactivity research because they had initially shown some scepticism about Becquerel's discovery, putting it in the same category as Lebon's black light which enjoyed some notoriety in 1896. As mentioned before, the most significant German intervention, came in the field of blackbody radiation, from Lummer, Pringsheim, Rubens and Wien. It was the discussion, essentially among these German research workers, seeking to harmonise the experimental results with the theoretical law of Wien, which was to lead Planck to formulate the quantum theory. Planck was at the Congress but did not give a paper, and neither was his quantum explanation of black body radiation heard although it was formulated late in 1900.

It is to the credit of France and her *Societe de Physique* that the first international congress in the discipline should have been held in Paris. It came at a fortunate time for French physicists, because after a period in which many new developments in the subject had tended to leave them on one side, leaders in the new 'ray physics', Becquerel, the Curies, Villard, to a lesser degree Swyngedauw, Branly and Blondlot, were to be found in its ranks. But, characteristically, these were experimental triumphs for French physics; within a few years, theoretical formulations in radioactivity,in quantum theory and relativity, would place the leadership in physics in the hands of German and English speakers. Apart from the philosophical intervention of Poincare, none of the French papers made a significant theoretical contribution to the Congress; they were reports of experimental work. Cornu on the velocity of light, Violle on the velocity of sound, Blondlot on the velocity of radio waves, Abraham on the velocity v , Crova on the solar constant, Guilllaume, Benoit and Mace de Lepinay, on precise measurement, as well as the work on radioactivity, give some examples of the experimental character of the French contribution. It is true of course that, as in Bacon's Solomon's House, a large number of collectors of experimental facts will keep employed a relatively small number of theoreticians, and the vast majority of the papers presented to the Congress from all nations was predominently experimental, but, nevertheless, the absence of a French theoretical contribution to match that of Lorentz, Wien, Drude, J.J. Thomson, or the aged Lord Kelvin, is significant. It is true that Gabriel Lippmann's intervention on the 'Kinetic theory and Carnot's principle'[14] was as much in the realms of thought as experiment, for basically it sketched out a proposal for an experimentally realisable demonstration of the possible conflict between kinetic theory and Carnot's principle, which Maxwell had pointed to in his famous 'demon' paradox. It was a brilliant little contribution (some four printed pages in the collection of papers) showing Lippmann's positivist scepticism towards kinetic theory, but perhaps rather slight considering it came from an Academician and one of France's most distinguished physicists.

Of France's six members of the physics section of the Academy, four have already been mentioned as contributing to the Congress; Cornu, Violle, Becquerel, and Lippmann. Of the other two, one, Mascart made no contribution except in his function (appropriately since he was director of the French meteorological office) as president of the cosmic physics section, while the polytechnicien Potier spoke on a technological subject, polyphase AC currents.[15] Two supporters of kinetic theory in France, Marcel Brillouin and Jean Perrin, gave papers on gas diffusion and osmosis respectively.[16] For Perrin, this marked the start of his search for the experimental demonstration of the real existence of molecules using techniques based on Brownian motion, a search which would be crowned with success in 1908.

The new century which had started so gloriously for French physics, with its experimental triumphs in radioactivity and its organisation of the Congrès had, within a very few years, turned a little sour, despite the award of the Nobel prize for physics to Henri Becquerel and the Curies in 1903. The 'discovery' of the non-existent N rays by France's most illustrious provincial physicist, Rene Blondlot in 1903, and their subsequent rapid demolition did the international prestige of French science no good at all. But it must not be forgotten that it was the French physics community, particularly its younger members, who effected the demolition, even if the intervention of the American, Wood, was also an important factor. This episode has been fully chronicled by Nye and others,[17] and here we will only consider how it was received in the Société de Physique and the AFas, and see how the physics community responded to it, using the evidence of the survey which the Revue Scientifique made in 1904.[18]

The Revue Scientifique survey revealed that the split for and against Blondlot was more on hierarchical rather than on geographical lines, although all the Nancy scientists supported Blondlot. Of the twenty six Parisian scientists questioned, seven, confident in the reputation of Blondlot, thought that N rays existed. These were senior members of the community and four of them, Berthelot, Becquerel, Poincare and D'Arsonval were members of the Academy. Another member of the Academy, Mascart, while still in doubt at this time, was to be converted later, after a visit to Nancy. Violle remained in a state of 'Cartesian doubt'.[19] Two scientists from Paris, Gariel, physics professor at the Faculty of Medicine, and Moissan, chemistry professor at the Sorbonne, both objected to the survey because it seemed as if its objective was to determine scientific truth by plebiscite, but Gariel was of the opinion that there was some evidence for the existence of N rays. Six physical scientists; Abraham, Langevin, Janet, Sagnac, Perrin and Debierne, had carried out experiments, with negative results, and rejected the idea of the existence of the rays and criticised the methods employed in Nancy. The cautious Pierre Curie also obtained negative results, but did not entirely reject the existence of the rays because his own experiments were not systematic; he did comment that it would be easy to think up a suitable control experiment.[20]

In the provinces (with the exception of Nancy) out of twenty senior scientists questioned, three were in favour of the existence of the rays. Ten researchers had carried out experiments with negative results, but only four would say that they did not believe in N rays, others tended to express doubt about the accuracy of their own experiments. As their results conflicted with the received knowledge which came to them with the endorsement of the most eminent physicists of the country, they wavered. There was one blast of intemperate rejection from Monoyer, a physics teacher of the medical faculty of Lyons, who declared that ;'N rays don't exist, N' rays exist even less, they should be relegated to the rubbish-bin of history'.[21] But he was an exception, most were circumspect because careers might be jeopardised by a too definite rejection, which could later prove to be incorrect. It goes without saying that the five Nanceans surveyed, expressed absolute confidence in the objectivity of Blondlot and the existence of the rays.

So the split in the scientific community over the question, was not simply a geographical one of Nancy against the rest of France, it was also one of hierarchical position. Those senior members of the community, who had awarded Academy prizes to Blondlot and admitted him to the Academy as a corresponding member, stood by him now. In doing so they found themselves in a minority position against the most creative and able young sector of researchers both in Paris and the provinces, and when N rays were finally found to be imaginary, their authority was damaged.

This period from 1904 to the outbreak of war was, in any case, to be one in which the leadership of French physics, as represented by the members of the physics section of the Academy was to undergo a change as death carried off many of her most illustrious sons. Cornu had died in 1902, while Mascart, Curie, Potier, and Henri Becquerel were all dead by 1910, as was Henri Poincare before the outbreak of war. But their deaths did not signify that the new generation of research workers employed in ray physics was to take their place immediately. First would come the fairly elderly men, sometimes of second rank, whose creative work was often long behind them, and who seemed uncomfortable in the new climate of physics. Amagat was 61 years old when he defeated Pierre Curie in the election for Cornu's place in 1902, Gernez (once a protégé of Leverrier) 72 when he succeeded to Curie's place in 1906, Bouty 62 when he succeeded to Becquerel's place in 1908, and Branly 67 when he defeated Madame Curie in 1911. Only Paul Villard, 48 when he was elected to Mascart's place in 1908, was engaged in research in ray physics. Thus in the years immediately before the outbreak of war, the physics section of the Academy was predominently made up of men remote from the interests and preoccupations of the new physics, and wedded to their studies of the physics of the nineteenth century.

b. The Société de Physique

The society grew more rapidly in the first decade of the new century than it had for a long time. In 1902, women were admitted to membership, when Marie Curie and five female science teachers joined, but by 1910 the number of women members had risen to only thirty two. Also at this time, the student population was rising sharply and this sector, previously under-represented, began to enroll. But the most rapid growth rate, was of foreign members, (both teachers and students of physics) who by 1910 had come to make up nearly a third of the total membership. Perhaps this was, as the officers of the Society liked to think, an expression of the new prestige of French physics, particularly won in the new areas like ray physics and radioactivity.[22] But if it was a question of prestige of French physics it did not translate itself into large numbers of foreign students wishing to study at French universities. Most foreign students still preferred, (probably because they found the administrative problems of taking a doctorate in France too intractable),to complete their scientific studies in Germany, a situation which caused some anxiety in the administration of French higher education in this period.[23] But if more foreigners were joining the society, so were more provincials, and by 1910, for the first time the provincial membership equalled the Parisian. This growth of the society should be seen in the light of a growth of science students from 1,278 in 1890 to 7,330 in 1914, with a similar expansion in the awarding of science degrees and hence a much larger scientifically trained and interested population.[24]

The number of papers and reports on ray physics given before meetings of the Society declined during the period up to 1914. It is true that the founding of the journal Le Radium in 1904 gave scientists a new vehicle for the rapid publication of research in the field of radioactivity and ray physics which they lacked before, and this may have diminished in their eyes, the importance of presenting research to the meetings of the Society. But the decline must also have been related to the fact that the first simple stage of the work, with many physicists repeating the work of others in the field of cathode and X rays, was coming to an end. Research into radioactivity continued to be limited by the availability of radioactive materials, which were usually supplied by the Curies.

In 1901 sme 15% of papers presented to the Society were devoted to ray physics, all of them on X rays. This work examined the transparency of various materials to the rays, the secondary emission of X rays (lower frequency rays) and the emission of cathode rays, when X rays fell on a surface. Research in the first of these categories was reported by the Parisian lycée teacher, Benoist,[25] while Pierre Curie collaborated with Georges Sagnac , agrégé at the Paris Faculty to investigate the cathode ray emission.[26]

In the following year, Pierre Curie gave a short paper which stated that radioactive decay gave the means for the establishment of an absolute unit of time.[27] But as most of the original research had already been published in the Comptes rendus the most interesting aspects of the meetings of the Society were the discussions and reviews of the current state of the new physics. Paul Langevin's account of the work which was going on abroad on the question of the movement of ions in gases,[28] falls in this category. He referred to the work of C.T.R. Wilson and J.J. Thomson in England, and expounded the hypothesis, accepted in that country, that cathode rays are corpuscles carrying a negative charge and possessing a mass very many times less than that of the atom itself. The positive ions of the gas being much more massive, travel much slower than cathode rays. As on previous occasions, the review gave the impression that theory had advanced further abroad, than in France.

In 1905, the year of Curie's election to the Academy, a summary of a report by him on the latest work of Rutherford, Ramsey and Soddy appeared. The experiment, performed in England, which demonstrated that alpha particles were essentially helium nuclei, naturally figured prominently in this report. The report stated that;

> 'The most true theory today is the one of disintegration of atoms and transmutation of elements.. (a theory) accepted by most chemists and physicists'.[29]

The report went on to say that 'M. and Mme. Curie had announced this hypothesis, among others, as convenient in explaining the phenomena, from the start of their researches', but that, confronted with many uncertain hypotheses, they had preferred to pursue a study of phenomena without stating precisely any hypothesis concerning the origin of the energy.[30] Thus Curie demonstrates a 'conventionalist' approach to theory; he may well have believed that atoms transmuted and that the energy of radioactive decay stemmed from this atomic transmutation, but he never considered it important enough to organise a research programme around, and simply went on with his study of the facts of radioaactivity. There were no more reports on experimental work on radioactivity given to the society before the end of the decade, a deficiency which can partly be explained by the death of Pierre Curie in 1906. There was in 1907 a brief summary of the work of A. Moulin of the *Ecole de Physique et Chimie Industrielles*, on the study of secondary cathode rays produced when a target is bombarded with alpha rays, and in the same year, Raveau, of the *Conservatoire des Arts et Métiers*, gave a long paper on 'recent researches on the transformation of radioactive bodies.'[31] Once again, this was a general history of the recent progress in the subject, quoting from the work of Bragg, Kleeman and Rutherford as well as Mme. Curie, and in it, the idea of half-life (called Curie's time constant) was first used. Also by this time, Henri Becquerel had not published on radioactivity for six years, and although his administrative work in the Academy continued until his death in 1908, he made no further contribution to research. France's research effort in the field of radioactivity now came to be, in the main, concentrated in the Paris Faculty, in the hands of a group of young scientists supervised by Marie Curie, as we shall see later.

In the Easter meeting of the society in 1908, the Italian physicist, Righi, delivered a paper reviewing the current thinking on the structure and constitution of the atom. Again, as ten years previously with the discussion over cathode rays, there appeared to be only two hypotheses, one emanating from Germany and the other from Britain. According to Righi, the Germans considered the atom as a system of electrons of two types, while the British preferred to view;

> '..the positive part of the atom as a sphere with the negative electrons moving around its centre.'[32]

Thus it appeared that, once again, the French were observing a German-British theoretical debate from the sidelines, having formulated no position on the question, and it is true that some of her scientists were still reluctant to accept the real existence of atoms. But despite their caution, they were, as Deslandres, the retiring president of the society in 1909 was to say; 'Like a blind man with a stick',[33] being forced by experiment to recognise the existence of;

> 'Particles which escape our imperfect senses and even our most delicate apparatus, but whose intervention seems necessary in order to explain the more and more complex phenomena, which observations reveal.'[34]

Later in 1909, the society heard a lively discussion between two of its members over the structure of the atom. Jean Becquerel, with all the confidence which came from being the fourth generation of a hundred -year old scientific dynasty and professor at the Muséum d'Histoire Naturelle by inherited right, published the results of an experiment which claimed to show the existence of,

> 'Free positive electrons analagous to the known negative electrons or at least possessing a charge to mass ratio of the same order of magnitude'.[35]

Becquerel's results were soon contested in a meeting of the society by a teacher from the lycee Louis-le-Grand, A. Dufour. Dufour, who was soon to move to the Ecole Normale also took issue with Becquerel over his interpretation of some aspects of the Zeeman effect (which had led Becquerel to hypothesise about the real existence of positive electrons in atoms) carried out a series of carefully planned and executed experiments in a specially designed cathode ray tube. This experiment, which was carried out in one of the meetings of the society, showed that the positive electrons of Becquerel were the result of drawing erroneous conclusions from the results of slipshod experiments.[36] Becquerel, who was also present at the meeting, was at a loss to explain the difference between his results and those of Dufour, and proposed that the two should carry out some joint research on the matter, a suggestion which was rather curtly turned down. Dufour simply challenged Becquerel to bring his apparatus to the meeting and demonstrate the effect he had claimed to have found. Thus the society provided the stage for the rapid correction of an error, which otherwise, given Becquerel's position in the scientific community, might have damaged French scientific prestige in a similar way to that which N rays had done.

In fact,in this section on the *Société de Physique*, we have not yet said anything about the excitement which the 'discovery' of N rays provoked in 1903. The reason for this is that the society was almost completely immune to this excitement. In the brief period from 1903 to 1906, some 300 papers, by about 100 scientists, were written on N rays, and a large number of these were submitted to the *Comptes rendus*..[37] The *Société de Physique* heard no more than five.

The first of these appeared in the summer of 1903 and was an examination by Sagnac of a paper by Blondlot (the discoverer of the waves) in which he claimed to have refracted a beam of N rays from an incandescent lamp using a quartz lens, and to have observed a focus at a position which gave a refractive index of 2.93. Three subsidiary foci corresponding to other refractive indices were also found. Sagnac regarded the three subsidiary maxima not as normal focal images but as intensity maxima due to diffraction effects.[38] Using the normal theory of optical diffraction, Sagnac calculated the wavelength of the waves as being aound 0.2mm, which placed them between the very long infra-red waves discovered by Rubens and the very short Hertzian waves which the Austrian, Lampa, had been working with. However, electro-magnetic theory and previous experiments seemed to deny the possibility that such long radiation could penetrate quartz. Sagnac was also worried by the fact that theory predicted other intensity maxima at shorter distances from the lens, corresponding to refractive indices greater than that of the 'true' value 2.93.[39] So this paper,which was also published in the Comptes rendus a few days later showed considerable scepticism towards the existence of the rays.

A more sympathetic view was given by Broca, a supporter of Blondlot, who suggested that a metallic grid with a wire spacing of about 2mm to 5mm could be employed as a diffraction grating suited to the wavelength of the waves.[40]

In the Easter meeting of the following year, Dr Charpentier of the Faculty of Medicine of Nancy, gave a paper on the emission of N rays from the human body. Starting by insisting on the objective nature of the rays, he confidently asserted that the human body was a powerful N ray source, and that the nervous system was the most active seat of their propagation. He ended his rather startling report with the information that the body also emitted N' rays. N' rays, also discovered by Blondlot, had similar properties to N rays (they could be refracted, diffracted, polarised,etc.) but instead of increasing the brightness of a spark detector, they were supposed to diminish it.[41] There does not seem to have been any dissenting voices at the meeting and indeed many famous names, Mascart, Henri and Jean Becquerel, D'Arsonval, Henri Poincare among them, were more or less convinced of their existence, while others like Curie, Langevin and Perrin remained sceptical.

The final paper to appear before the Society on this question was the one given on 16 March 1906 by Albert Turpain of the Faculty of Sciences of Potiers, as a culmination of several years of work, and it found against their existence.[42] Using the fluorescent sulphide screen whose brightness was supposed to increase under the effect of the rays, Turpain used various sources which Blondlot had used, and found that when he knew N rays were falling on the screen, its luminescence increased. When he used a 'control' experiment and did not know whether N rays were arriving or not, he found very poor agreement between observed and predicted results. He concluded that there was a strong possibility of auto-suggestion, although he did not put too much stress on it, because he did not wish to contradict such powerful figures as Mascart, who had recently worked with Blondlot and his assistant Virtz in Nancy, and expressed himself satisfied with the results. Turpain also suggested that in Blondlot's experiments examining the spectrum of N rays refracted through an aluminim prism, experiments which superficially seemed to give evidence for the existence of the rays, Blondlot had unconsciously remembered the setting of the apparatus, and so was able to return to the same point, to pick out maxima when moving a detector through the spectrum.

Making the point that Rubens had failed to verify the effect, and recalling the successful collaboration between the American Pender and the Frenchman Cremieux to verify the Rowland effect (the existence of which had been denied in France) Turpain proposed that Rubens and Blondlot should work together to settle, once and for all, the existence of N rays. In fact of course, Blondlot always refused any invitation to collaborate. A number of other members at the meeting, Villard, Raveau, Cotton, and Guebhard, also expressed their continuing scepticism andd their failure to conform their existence. By the early part of 1906 the failure to verify the existence of N rays by any objective experimental method, led to a rapid collapse of interest in the subject, and the society could congratulate itself that, in the main, it had not allowed the authority of Blondlot to blind it to the obvious faults in his experimental methods.

We have only surveyed here a small fraction of the work presented at meetings of the society during the period. There were still a fair number of technological papers, on electro-chemistry, electro-technology, telephony, and wireless telegraphy and there began to appear some on aeroplanes and the problems of powered flight. The French tradition of precise measurement was well represented by several papers from

Fabry and Perot, including one which proposed to define the standard metre in terms of a fixed number of wavelengths of cadmium red light. The widespread acceptance of Stefan's fourth power law, linking intensity of radiation to temperature, rekindled the interest in determining the sun surface temperature, and Charles de Fery of the Ecole de Physique et de Chimie industrielles was particularly active in this area.

One last point to make about the meetings of the Society, was that throughout this whole last period from the mid-1890's at least, the contribution of engineers and instrument makers, so important to the life of the society, in the 1870's, now was negligible. Physics research was now exclusively the preserve of academics, and ray physics and radioactivity the more specific preserve of academics of the institutions of Paris. On the other hand, membership of the society was very wide, for as Deslandres stressed in the 1909 meeting;

> 'Evidently the brilliance of recent discoveries has attracted to physics, people of many vocations, from the most famous to the youngest student'. [43]

c.The AFas.

The Association continued to be more interested in 'applied' rather than 'pure' science and several applied physicists from the capital, such men as Guillaume, Fery and more especially Blondel, continued to contribute in the early part of the period, but those researchers who were working on the moving frontier of physics, seldom bothered to present their work to the annual meeting. Thus the new areas of physics which were yielding more and more information about the structure of matter; radioactivity, X rays and positive rays, were subjects which only rarely appeared on the agenda of meetings of the AFas.

There were two papers on X rays in this period, both from Parisian savants. The first was by the lycée teacher Benoist in 1902 and the second by Broca in 1906, and these two, plus one from abroad, make up the complete total.[44] The reason for this absence of papers in areas which evoked the most general interest, is not clear. One would not really expect the results of the most recent research to find their first presentation at the annual meeting of the AFas because of the slowness in publication and the level of the audience, but one might have expected more reviews of this type of research. A partial explanation may be that fewer Parisians were presenting papers anyway, and more contributions were coming from provincial faculties, and here the academic staff showed less interest in the subject possibly because their laboratories were not well endowed with either X ray apparatus or radioactive material. But it must also have been partly due to the fact that these areas were 'pure' physics research which had, at this time, little practical application, and therefore did not respond to the principal interests of the Asociation. It is true that medical applications had already been found for both X rays and the radiations from radium, but these were more likely to be presented to the 13th section in the Natural Sciences group, 'Electricity in medicine' which always attracted a large number of papers.

Wireless telegraphy was one new applied area of research which always featured prominently in the meetings of the Association. Although others contributed to this study, it was mainly in the hands of three researchers; Blondel of the Ecole des Ponts et Chaussées, Turpain of the Faculty of Poitiers, and Tissot of the naval school at Brest. Tissot, not surprisingly was interested in wireless telegraphy as a means of communication between ships at sea and as a navigational aid. This second application had the support of the Bureau des Longitudes, and gave the means of fixing the longitude at sea by comparing local noon, as observed with a sextant on board, with a noon time signal sent by wireless from Paris or Greenwich. Turpain's interest was turning towards the use of wireless in the short range forecasting of storms, and he presented papers on this question to both the physics and the meteorology sections. This was a question of using the electrical disturbance associated with lighting, to actuate a recording device, and thus to warn of the approach of heavy storms and what the farmer particularly feared, hail. Turpain stressed the importance of this method for agriculture.[45] Blondel worked on the improvement and development of the apparatus of wireless telegraphy. Electrolytic and valve detectors were by this time much more sensitive than Branly's original coherer, and improved aerial and tuned circuit design had very much increased the range and the quality of wireless telegraphy by the end of the period. The practical application of wireless telegraphy was underlined in the 1910 meeting by a Le Havre sea captain, Faveau, who stressed its importance in warning ships of the presence of ice-bergs in the North Atlantic off Newfoundland.[46] This was of course,two years before the British 'Titanic' equipped with wireless telegraph was to founder on an iceberg with the loss of hundreds of lives.

The design and theory of the aeroplane also found its way into the discussions of the physics section before the First world war, particularly through the contribution of the Montpellier professor, Amans. The theory of aircraft design was very much a French development, and they were the first to make it an academic discipline when a chair of aeronautics was founded at the Paris Faculty in 1909, with the help of a large grant from the arms dealer Basil Zaharoff.

While the 'discovery' of N rays in 1904 by Blondlot caused great excitement in the French scientific community and produced many papers to the Academy (and a few to the *Société de physique*) none were ever heard by the *Association*. This was probably because N ray investigation would best be described as 'pure' research with no practical applications. However, Blondlot's collaborator at Nancy, Gutton, came to the 1905 meeting with a report on the action of magnetic fields on the intensity of a weak light source. Using a solenoid, Gutton observed the effect of the magnetic field on a number of calcium sulphide dots painted onto a screen and illuminated by a weak light source.[47] He claimed that the intensity of the phosphorescent dots was increased by the field if it was either changing with time or non-uniform in space. He also claimed that the magnetic field increased visual acuity, but proposed no control experiments, and seemed to be be content with two different explanations for the phenomenon; the subjective and physiological one that the field increased the sensitivity of the eye, and the objective one that the dots did actually become brighter. Thus, although dealing with a different subject, Gutton's research bore all the hallmarks of Nanceian physics research of this period, which was characterised by auto-suggestive experimental techniques and the absence of adequate control experiments.

But it would be wrong to think that the contributions to the <u>AFas</u> in this period were always either theoretically dubious on one hand, or simply pedestrian examples of utilitarian applied science on the other. Duhem, for example used the 1902 and 1904 meetings to give himself a national platform to explain his original ideas on the action of high frequency currents on dielectrics (1902) and electric fields in dielectrics (1904).[48] Both these papers were highly mathematical and probably quite inacessible to many among the audience.The 1904 meeting also heard the Paris Faculty professor, Bouty, on the dielectric properties of air, and the Austrian, Charles Zenger, (the most prolific foreign contributor) on 'The electro-dynamic theory of the world and radium'.[49] So pure physics, including speculative syntheses, could sometimes find a place in the <u>Association's</u> meetings.

The general sessions at the beginning of each Conference, were, from time to time, used by prominent figures of the scientific establishment to express opinions or voice disquiet about national policy in relation to the sciences. Such an address was made by the Academician Gabriel Lippmann, to the 1906 meeting in Lyons, in his speech entitled 'Industry and the University'.[50] This was both a criticism of the French system of higher education, and an appeal to industry to employ more research scientists in their enterprises. Lippmann argued that it was necesaary, for any sort of technical progress, that there should be;

> 'scientific personnel provided with research laboroatories installed in the factories'.[51]

He compared the situation wih Germany, where factories employed scientists and mathematicians with doctoral qualifications and provided them with laboratories and libraries but still paid their shareholders 20% to 33% dividends.[52]

He went on to castigate the whole system of French education, which he claimed was modelled on clerical forms from the old regime and declared that it was urgent to 'deliver teaching from bureaucratic pedantry and to liberate the universities from the yoke of the executive power'.[53] He finished by expressing his support for the idea of the autonomy of the University of Lyons.

This consideration of the work of the AFas in the twentieth century would not be complete without a reference to the address given by Henri Poincare on the occasion of his receiving the gold medal of the Association at its 1909 meeting in Lille. This address entitled 'On the new mechanics',[54] must have made the head of many a worthy provincial pharmacist,teacher or doctor, spin. The polytechnician engineers who considered that they had mastered a body of complex but immutable knowledge laid down by the immortal Newton and developed by Laplace, must also have felt rather disturbed. Poincaré made a summary in a popular form, without mathematics, of all the ideas he had been exploring since 1895, and which he had named 'relativity' in 1902. Here were explained the ideas which later became associated with the name of Einstein; increase of mass of a body as its velocity approaches that of light, decrease of length in the direction of movement under those same conditions, as well as the most 'counter-intuitive' idea of the relativity of time. Starting from certain ideas of Planck and Lorentz among others, Poincare arrived at the concepts of special relativity, but his ideas were different from those later elaborated by Einstein, in that they admitted the existence of the ether.

His death in 1912 at the age of only fifty eight robbed France of her most original thinker in theoretical physics. But in the French tradition, Poincaré spent most of his intellectual energy in pure mathematics, and he is unusual because he did sometimes apply his immense gifts to physical problems. The tremendous social prestige given to mathematics in France was underlined later in the century by Leon Brillouin;

> 'There was a glory in pure mathematics, which was so much above applied mathematics or theoretical physics, that if someone was able to do pure mathematics he would not condescend to do anything else'[55]

It might be said that in this short statement, is contained the key to the character of French physics during the period of this thesis, its predominently experimental nature.

d. Ray physics research carried out in the Paris institutions.

In this period up to the Great War a certain specialisation of research took place in the different institutions, although several of the fields of research overlapped. The Curies and their students in the Paris Faculty continued to concentrate on research into radioactive materials, their decay and the energy of their emitted particles, while the work of the *Ecole Normale* ranged more widely over conduction through gases, positive or canal rays, spectroscopy, ionisation effects with ultra-violet light, and photo-electricity. Work on ionisation in gases and solids went on in the laboratories of the Collège de France, and up to 1910 there was still some radioactivity research going on in the laboratories of the lowly Ecole Municipale de Physique et Chimie Industrielles. On the other hand the Ecole Polytechnique, once a centre of innovation in both teaching and research, played no part in physics research in this period. The work of the Muséum, which after the death of Henri Becquerel in 1908 was that carried out by his son Jean, related to magneto-optics, the Zeeman effect, and culminated in his hypothesis on the existence of positive electrons. We have seen that the existence of these electrons was contested by other researchers, particularly Dufour, and the argument concerning their reality took place both in the meetings of the Société de Physique,[56] and in the columns of a new journal which appeared in July 1904, Le Radium.

We will take the work published in this new journal as an accurate indicator of the work in ray physics being undertaken at this time. Most of it would also appear in the Comptes rendus, although Marie Curie after her failure to win election to the Academy in 1911, ceased for a time to submit her research to its journal.[57] The full title of Le Radium could be translated as Radium, Radioactivity and Radiation. The sciences which are allied to them and their applications,[58] and it dealt with the discoveries and developments which had been made in the fields of radioactivity, cathode rays, ionisation effects, the study of the whole of the electromagnetic spectrum from Hertzian waves to Rontgen and X rays, as well as effects in the visible spectrum like spectroscopy, luminescence and magneto-optical rotation. Its first year of publication coincided with the initial period of excitement over the supposed discovery of a new type of ray, the so-called N ray by the Nancy professor René Blondlot, and there was a series of articles summarising all the research which had appeared on the subject,[59] but after 1904 nothing more was published on N rays in the journal. The medical uses of visible light and UV (phototherapy) and of Rontgen and radium rays (radiotherapy) to cure skin growths and cancers, and the diagnostic use of Rontgen rays figured prominently in the first two volumes. Published in a large format on high quality paper with numerous photographs of technical apparatus, instruments, and distressing medical conditions, carrying translations of important foreign contributions in the field, together with research papers by French scientists and reviews of new books and apparatus,it was clearly an ambitious and prestigious project.

It could also claim to be international as well as inter-disciplinary. Its editorial board included Rutherford, the German, Rubens, known internationally for his work on long wavelength Infra-red, and the Danish physician Finsen, but their contribution to the monthly production of the journal must have been minimal and their presence essentially decorative.[60] In the first two years of its existence, the medical applications of 'ray physics' were dealt with in many articles including several on the work of Finsen, who had won the Nobel prize for medicine in 1903 for his work on the effect of light on biological processes. Finsen died in 1904 but the medical representation on the editorial board continued to be considerable, with such men as Charles Bouchard, one of the most eminent medical academics of the Third Republic, and the Parisian doctors, Beclere and Oudin. Oudin had been the first to exhibit medical X ray photographs to the Academy some eight years before, and was one of the principal workers in the field in France. The other member of the editorial board concerned with the medical uses of the new rays, was D'Arsonval of the physiology laboratory of the <u>Collège de France</u>. The only professor from the provinces on the board, was Blondlot of Nancy. Another member of the editorial board was Charles-Edmond Guillaume of the International

Bureau of Weights and Measures at Sevres, who, in a series of articles in 1904-05 gave a historical review of the development of the notion of radiation, placing the new research within the French optical wave programme starting with Fresnel.[61]

By the third volume (1906) however,the medical, curative and diagnostic aspects of the new waves assumed much less importance in the journal. There were now several papers from Rutherford as well as from his students and collaborators in Montreal; Eve, Levin, Bronson and Bragg. The principal workers in radioactivity in France, Henri Becquerel and the Curies contributed, giving accounts of their earlier research or reviewing contemporary work, while Villard and Langevin reported on the investigations they were currently undertaking. Langevin's paper was on the mechanism of spark discharge , which he explained as being due to ionisation by the collision of 'electrified centres in movement'.[62] In the fourth volume, the change in direction was formalised by a change in title to _Radium, radioactivity, and ionisation: Journal of Physics_,[63] and in the following year the medical names, Bouchard, Bouclere and Oudin, disappeared from its title page. Madame Curie took the place of her dead husband on the editorial board, which now came to include also Langevin, Sagnac, and Villard. There now appeared a section in which the results of new research was published, and in the next few years this became very much the vehicle for the publication of the research of students and colloborators working under Madame Curie in her laboratory in the Paris Faculty of Sciences. In 1908 for example, there were seven papers reporting on original research from Curie's laboratory out of a total of nine emanating from the Paris Faculty. Other Paris institutions were also quite productive in this year; three papers came from the _Ecole Normale_, three from the _Ecole municipale_ and five from other centres in the capital, including three from Jean Becquerel at the _Muséum_. There were also two papers from provincial faculties; Rennes and Nancy.

An important feature of the journal was the rapidity with which foreign papers were translated and published. The work which Rutherford, Geiger, and Marsden were doing in Manchester in 1908 and 1909, was published in Le Radium only a month or two after it was published in England. But it is significant to notice that, whereas papers from Britain, Germany, and the United States came from a wide geographical range of centres, France was almost completely dominated by Paris. It tends to show that twenty years of attempted scientific decentralisation in France had achieved little, at least in relation to this new field of physics.

The 1910 issue of Le Radium published 54 original memoirs, of which twenty came from abroad, eighteen from Madame Curie's laboratory four from the Ecole Normale and three from the Collège de France. In fact, this year marked the high point of the output from Curie's laboratory, at least from a quantitative point of view, with Curie herself producing two papers ,the radio-chemist Debierne one, and several students like Kolowrat, Blanquies, Herschfinkel, and the American, Duane responsible for most of the rest.

Justice would not be done to the research at the Paris Faculty in this period, if the contribution of Jean Perrin to the study of molecular physics, were to be omitted, although it has already been thoroughly chronicled in Nye's, Molecular reality.[64] Perrin, already noted for his experiments on cathode rays in the 1890's, was appointed lecturer,(chargé de cours), of the new course of physical chemistry at the Paris Faculty at the turn of the century, and so he worked in a different laboratory, and taught a different course, from the Curies, in this decade. The discipline of physical chemistry was developing, by the end of the nineteenth century, as a bridge between the two sciences of physics and chemistry, whose separation, in Perrin's view, came from two opposing viewpoints; the notion of discontinuity, fundamental in chemistry, against the notion of continuity, equally fundamental, in physics. For Perrin, one viewpoint was not necesarily more appropriate than the other, they could both be aspects of the same thing.[65] Perrin, in his Les Principes of 1903, set out the methods and principles of physical chemistry, dealing with temperatures, pressures, and volumes, the identification of pure bodies and their laws of combination, the phase rule, and discusses the two laws of thermodynamics.[66] His course had no examination at the end of it, and most of his students (who were few at first) were already in possession of their first degree.

While he was a student at the Ecole Normale, Perrin had absorbed some of his teacher Brillouin's sympathy for a discontinuous, material, kinetic view of nature, and in 1906 began his series of experiments on the Brownian motion of colloidal particles to provide experimental evidence for the reality of molecules. He published his first four papers on Brownian motion in the Comptes rendus in 1908, and in the following year reported his findings to the Société de Physique, in a long paper entitled,'Brownian motion and molecular size' at the Easter meeting.[67] In this paper he paid particular attention to the 'beautiful theoretical work',[68] of Einstein. The formula derived by Einstein allowed the calculation of the displacement of a particle with time when it was undergoing Brownian motion. This displacement being experimentally verifable, could be used to calculate Avogadro's number. The equation, however, employed the assumption that Stokes' law could be applied for a body moving in a homogeneous viscous medium, and there was some doubt among research workers in the field that such an assumption could be applied in the case of movement on such a microscopic scale. Doubts on its validity had been cast by the experimental work of Victor Henri, a préparateur in physiology at the Paris Faculty, who studied the movement of very fine particles suspended in a liquid (a studied the movement of very fine particles suspended in a liquid (a colloid) with a cine-camera which took pictures every 20 seconds. Henri presented his work, to a meeting of the society during 1908, and gave a mean velocity of the particles about four times that which Einstein had calculated.[69] Perrin however, suggested that there were some factors in Henri's experiment which disturbed the colloid being investigated, particularly the large amount of heat produced by the lighting which would have affected its temperature.

Later, in 1909, with the aid of a student working for his doctorate, Chaudesaigues, Perrin experimentally verified Einstein's formula on the displacement of a randomly moving particle.[70] For this work he was awarded the Gaston Plante prize in 1909 and in the following year was appointed to the chair in physical chemistry, especially created for him at the Paris Faculty.

But from a quantitative point of view, the laboratory of Mme. Curie at the Paris Faculty would continue to be by far the most productive in papers on questions of radioactive decay, α, β and γ radiations, research into the 'emanation'of radium (the gas radon produced by the decay of radium) and work with the new materials like polonium and actinium. Some similar work was going on in other Paris institutions but either with only very weak emitters, or with materials generously supplied by Mme.Curie. For example, a certain Henriot at the <u>Ecole Normale</u> was carrying out research on the very feeble radioactivity exhibited by some salts of the potassium family. This had first been discovered in Cambridge and work was being done on it at this time in Dublin. Henriot started with photographic techniques for detecting the radiation but soon rejected them as being too insensitive, and went on to use an electrometer. Because of the weakness of the emitters, he had to use very large surface areas of salts (1000-1200 cm^2) in order to produce a detectable ionisation current.[71]

Henriot concluded that potassium salts did emit particles, although its activity was only a fiftieth of that of uranium, but he could find no activity at all in salts of sodium and lithium.

Other work at the *Ecole Normale* in 1910 was that carried out by Eugene Bloch (1878-1944) on the photo-electric effect.[72] Bloch irradiated many surfaces with different wavelengths of light and measured the resultant photo-electric current; he also heated the surfaces to see if this affected the emissions of electrons (for it was widely accepted by this time that electrons were being emitted from the surface) and he arrived at an estimation of the maximum energy of the emitted electrons. He also showed that the polish and cleanliness of the surface was very important; for contamination of the surface very quickly led to a diminution of the rate of emitted electrons, an effect known as 'photo-electric fatigue'. But it cannot be said that Bloch carried the research any further than Lenard had taken it , and he seemed to be completely unaware of Einstein's quantum explanation of photo-electricity published five year before in the *Annalen der Physik* of 1905. This might be an indication of a certain resistance to quantum ideas in France, or may simply show the difficulty which Einstein (something of an outsider in physics at this time) encountered, generally, in finding acceptance for his explanation which broke with the classical view of the wave nature of light. It might be expected that French physicists would show a particularly strong allegiance to the wave theory, the theory of Fresnel, Arago and Cauchy, and a theory which had provided a research programme for many a successful scientific career.

In the laboratories of the *Collège de France*, T. Bialobjeski, with radioactive material supplied by Mme. Curie, carried out a series of experiments on the effect of radiation on a number of dielectric solids, namely sulphur, paraffin, amber and wax.[73] When the substance under test, being used as an insulator between two metal plates, was irradiated, the current between the plates gradually increased as if 'the ions were accumulating slowly in the material under the uninterrupted action of the rays.'[74] When the radioactive source was removed, the surrent slowly diminished. Developing a theory of ionisation in solids, Bialobjeski, concluded that, unlike gases, solids each have a particular characteristic; the 'phenomena of the pasage of electricity in solid dielectrics depends upon their individual structures.'[75] This work was carried out under the supervision of Langevin who in 1909 had succeeded to Mascart's physics chair at the Collège. Langevin's interest in the phenomena of ionisation had began in the days when he had worked with Thomson in Cambridge in the 1890's, and ionisation continued to be the principal area of research at the Collège until the war.

For the very rich it was possible, as it had been in the past for Foucault and Fizeau, to carry out research work in private laboratories. In fact, it is probable that some experiments of the new ray physics were considerably less expensive and difficult to set up than much of the classical work. From 1910 onwards, work on ionisation in gases began to be carried out in the private laboratory of Maurice de Broglie (1875-1960). De Broglie, a member of an old aristocratic family had been a brilliant student at the Toulon naval academy, had engaged in wireless wave research for the navy and then resigned and studied for his doctorate under the supervision of Langevin, presenting his thesis on ionic mobility in 1908.[76] In 1910 and 1911 he published the results of joint work with his old teacher from the Toulon naval school, Brizard, in Le Radium.[77]

If we divide the work published in Le Radium into two general categories; work on the nucleus, and work on the orbital electrons (under which heading would come spectroscopy, the Zeeman effect, cathode rays and ionisation) it can be seen that, from 1911 the first category of work began to decline and the second to increase. The first category of work was undertaken almost exclusively in Curie's laboratory at the Faculty, while the second went on in many different institutions.

1912 saw a general decline in the number of original memoirs, and a further increase in the proportion coming from abroad, but Curie's laboratory maintained its share of around 20% of the published papers. The following year saw this trend continuing; half of the original memoirs came from abroad, and only six papers reported on research done by Curie or her collaborators at the Paris Faculty. One of these was by Curie herself on the experiment she had carried out in the cryogenics laboratory in Leyden, Holland, with Kammerlingh-Onnes, to show that the rate of decay of radium is unaffected by temperatures as low as that of liquid hydrogen.[78] In this year there was only one paper from the Ecole Normale , on spectroscopy, but the Collège de France was represented with three papers from Langevin and Rey on ionisation.[79]

During this period Le Radium was also publishing work from Rutherford and Geiger in Manchester, Millikan in Chicago, and from Germany, Rubens on infra-red research and Von Laue on X rays, without making an exhaustive list. De Broglie's work on X ray diffraction by crystals, produced after he had read of the research of Von Laue and Bragg, appeared in Le Radium in 1913.

An indication of the role which French savants had played in the short history of ray physics up to 1910 can perhaps be assessed through their participation in the International Conference on Radiologie et Electricité held in Brussels in 1910. The titles of the papers, and short summaries of them, were published in Le Radium. The journal divided the papers into four categories; 'Radiometry and standards', 'radioactivity', 'èlectrons,ions, atoms and molecules', and 'cosmic phenomena and technical matters'. The table below shows the national origins of the papers in the first three categories (the fourth, which was very small, is less relevant to the discussion here and is not considered). It can be seen that numerically at least the French contribution matched that of Britain and Germany. The surprisingly large contribution from Romania came from one individual, Hurmuzescu, who had studied at the Paris Faculty and been an active member of the Société de Physique. If his contribution is added to the French figures one sees that the contribution of French (or French trained) scientists was very weighty at the conference.

80.

	France	Britain	Germany	Romania	USA	Others	Total
Radiometry and standards.	2	2	3	2	0	0	9
Radioactivity.	4	7	1	2	0	4	18
Electrons, ions atoms, molecules.	7	3	6	1	2	4	23
Total	13	12	10	5	2	8	50
%	26	24	20	10	4	16	100

e. Ray physics in the provincial faculties.

Apart from the 'N' ray research at Nancy, research which was repeated in a few provincial faculties in the years 1904 and 1905, ray physics work tended to be be concentrated in the capital. This is absolutely true as far as radioactivity is concerned, but cathode rays, X rays, ionisation studies and photoelectricity found some practitioners in the provinces, as we have already seen.

The photoelectric work of Swyndgauw at Lille at the turn of the century does not appear to have continued there and the centre of this research moved to the Ecole Normale with Bloch. The staff of the

Faculty of Rennes published a number of papers in Le Radium around 1910 on ionisation. The physics chairholder at Rennes, Morreau, carried out experiments to find the mass of the negative ions produced in a flame. He found that they were less massive than hydrogen ions but drew no further conclusions.[81] One of France's most distinguished provincial physicists, Louis Gouy, elected corresponding member of the physics section in 1901, who had been one of the first to work on determining the wavelength of X rays, now began to examine the motion of cathode rays in magnetic fields.[82] As was mentioned earlier in the section on the AFas, Duhem's student, Turpain, now the physics professor at the Faculty of Potiers, was the principal provincial researcher into wireless waves and their propagation.

Compared to the work which was going on in Paris, the above list, taken from the index of Le Radium and from examination of the meetings of the Société de Physique and the AFas, is a very modest one. It tends to confirm that in spite of years of attempted decentralisation by successive republican educational ministries, the crushing superiority of the Paris institutions over the provincial ones, was still a factor of French academic life until the outbreak of the war.

f.Conclusions on the French contribution to the new 'atomic' and 'ray' physics.

At the begining of the twentieth century, France was the undisputed leader, through the work of the Curie's and Becquerel, in the study of radioactivity and radioactive materials. In spite of the loss of her two most distinguished workers in the field, France continued, up to the outbreak of the great war,to produce (mainly from the Paris Faculty) a considerable quantity of radioactivity research. But today much of this work appears to be no more than a kind of rather sterile empiricism, unilluminated by flashes of insight into the processes and mechanisms behind the phenomena. But lest we open ourselves to the charge of 'whiggism' by such a judgement, we are on firmer ground to say, at least, that the French contribution to this work was principaly in the realms of experiment, while the theoretical development, the hypotheses obout the basic structure of matter were in the main left to others.

This was certainly the impression when considering the discussions in the *Société de Physique*, where French savants appeared to be on the sidelines when ideas, mainly German or British, concerning the nature of X rays, or atomic structure were discussed. But it is not entirely true to say this; the Curies speculated about the sources of the energy emitted in radioactive transformations, Becquerel put forward the model of the gyratory ether to explain the Zeeman effect, and as early as 1901 Perrin had put forward the idea of the 'solar system' model of the atom.[83] In this lecture, given to friends and students of the Paris Faculty, Perrin envisaged the atom as a sort of highly charged positive sun, surrounded by much less massive negative corpuscles, circulating like planets and held in orbit by electrostatic attraction. The sum of the negative charges being equal to that of the positive 'sun', the atom was electrically neutral, but sufficient electrical force could detach a small planet which would be ejected as a cathode ray. The further a corpuscle was from the sun, the easier it was to detach. Although it cannot be said that this hypothesis guided Perrin's later research it underlines his conviction about the reality of atoms which was central to his scientific work. But even with the work of Perrin to determine Avogadro's number we are talking about meticulous experimental research, precise measurement, exactly those things which were the principal contributions of French scientists to the new physics.

Notes for chapter 6.

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2. H.Nagaoka,'La magnétostriction',ibid, ii, pp.256-83.

   J.C. Bose,'De la généralité de phénomènes moleculaires produits par l'électricité sur la matière inorganique et sur la matière vivante',ibid, iii, pp.561-85.
3. M.A.Cornu,'Seance Génerale D'Ouverture', Congrès International de Physique, Paris, 1900, i, pp.5-8.
4. J. Heilbron, op.cit., (ch.5, note 88), p.65.
5. Henri Poincaré, 'Relations entre la physique expérimentale et la physique mathématique',op.cit.,(note 1),i, pp.1-29.
6. J.Heilbron, op.cit.,(ch.5, note 88).p.51.
7. P.Duhem, op.cit., (ch.4, note 1), p.26.
8. P.Duhem, op.cit., (ch.4, note 1), p.90.
9. H.A. Lorentz,'Théorie des phenomènes magnéto-optiques récemment découvertes', op.cit.,(note 3), iii, pp.1-33.

   J.J. Thomson,'Indications relatives à la constitution de matière fournies par les recherches récentes sur le passage de l'électricité à travers les gaz,ibid,iii, pp.138-52.

   The French expressed the ratio in the form m/e, the British, e/m .
10. H. Becquerel,'Sur la rayonnement de l'uranium et sur diverses propriétés physiques du rayonnement des corps radio-actifs',ibid,iii,pp.47-78.
11. P. and M. Curie,'Les nouvelles substances radioactives et les rayons qu'elles émettent',ibid, iii, pp.79-114.
12. P. Villard,'Les rayons cathodiques', ibid, iii, pp.115-151.

13. E. Bichat and R. Swyngedauw,'Sur les phénomènes actino-électriques' produit par les rayons violets', ibid, iii, pp.164-82.

14. G. Lippman,'La théorie cinetique des gaz et le principe de Carnot ibid,i, pp.546-50.

15. A.. Potier,'Sur les courants polyphases',ibid,iii, pp.197-263.

16. M.Brillouin, 'La diffusion des gaz sans paroi poreuse dépend-elle de la concentration ? ',ibid, i, pp.512-30.

J.Perrin,'Osmose. Parois semi-perméables',ibid, pp.531-45.

17. M.J. Nye,'N rays: an episode in the history and philosophy of science',*H.s.p.s.*,1980,*11*,125-56.

J. Rosmorduc,'Une erreur scientifique au debut du siècle; les rayons N',*Revue d'histoire des sciences*, 1972, 13-25.

18. 'Les rayons N, existent-ils ?',*R.s.*,1904, 5th ser.,*2*, 590-91,(590), 620-25, 752-54,

19. Ibid., 622.

20. Ibid., 658.

21. Ibid., 6

22. H. Deslandres,*S.s.F.p.*,1909,p.2*

23. This topic is fully dealt with by H. Paul in; *The sorcerer's apprentice; The French scientists view of German science,1840-1919*, Gainsville,1972, Ch. 1.

24. Figures taken from R. Fox and G. Weisz, op.cit., (ch.1, note 31), table 2, p.12.

25. . Benoist,'Lois de transparence de la matière pour les rayons X', ibid, pp.204-219.

26. P. Curie and G. Sagnac,'Electrisation négative des rayons secondaire issus de la transformation des rayons X', *S.s.F.p.*,1901,pp. 187-203.

27. P. Curie, *S.s.F.p.*,1902,p.60*.

28. P. Langevin,'Sur les gaz ionisées', _S.s.F.p._,1902, p.45*.

29. P. Curie, _S.s.F.p._, 1905, pp.36*-40*.

30. P. Curie,ibid.

31. A.Moulin, _S.s.F.p._, 1907, p.65*, and X.Raveau,ibid,p.21*.

32. A.Righi, _S.s.F.p._, 1908, pp.47-74.

33. H.Deslandres, _S.s.F.p._, 1909, p.2*.

34. Ibid.

35. J. Becquerel,_S.s.F.p._,1909, p.

36. A.Dufour,ibid,1909,pp.61-72.

37.The index to the _Comptes rendus_,gives seven papers in 1903,sixty two papers in 1904, one paper in 1905, two papers in 1906.

38. G.Sagnac. _S.s.F.p._,1903, pp.177-219.

39. G.Sagnac. ibid.

40. Broca,ibid,p.48*.

41. Dr. Charpentier, _S.s.F.p._, 1904, pp.32*-34*.

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43. H.Deslandres, op.cit., (note 22).

44. From the indexes of the _C.r.A.F.a.s._,1900-1914.

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47. . Gutton,'L'Action des champs magnétiques',_C.r.a.F.a.s._, 1905,pp.168-72.

48. P.Duhem,'Actions exercées par des courants alternatifs...', _C.r.a.F.a.s._,1903,p.178.

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49. Zenger, ibid.,p.

50. G. Lippmann,' L'Industrie et les universités.',*C.r.a.F.a.s.*, 1906, pp.35-42.

51. Ibid., p.38.

52. Ibid.

53. Ibid.

54. H. Poincaré,'La mécanique nouvelle',*C.r.a.F.a.s.*, 1909, pp.38-48.

55. D. Pestre,*Physique et physiciens en France 1918-1940*, Paris, 1984, p.

56. A. Dufour, op.cit., (note 36)

57. **R.Reid ,*Marie Curie*, London, 1974, p.189.**

58.In French,*Le Radium.La Radioactivité et les radiations.Les sciences qui s'y rattachent et leurs applications.*

59.Marcel Moulin,"Les Rayons N",*Le Radium*,1904,*1*,71-77,and 103-105, and 145-151

60.Rutherford was in Montreal at this time and therefore could only have played a minimal role in the journal.

61. C. Guillaume,*Radium*,1904, *1*,.96-102.

62.Paul Langevin,'Recherches récentes sur le mécanisme de la décharge disruptive', *Le Radium*,1906 ,*3* , 107-115, (109).

63.In French ,*Le Radium, Radioactivité et ionisation. Journal de Physique.*

64. Mary Joe Nye, *Molecular reality. A perspective on the scientific work of Jean Perrin*, London and New York, 1972.

65. Ibid. p.73.

66. Ibid. Nye refers here to Perrin's *Les Principes*, Paris, 1903, p.198.

67. J. Perrin, *S.s.F.p.*, 1909, pp.155-188.

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69. V. Henri, *S.s.F.p.*,1908, pp.45*-47*

70. Chaudesaigues,'Le mouvement Brownien et le formule d'Einstein', C.r., 1908, 147, p.1045.

71.E. Henriot,'Sur les rayons émis par les sels des métaux de la famille de potassium',Le Radium,1910,7, 40-48.

72.E.Bloch,'Recherches sur l'effet photo-électrique de Hertz',ibid,125-36.

73. T. Bialobjeski, 'Recherches sur l'ionisation dans les diélectriques solides et liquides', Le Radium, ibid., 48-56, and 76-80.

74. Ibid., p.51.

75. Ibid., p.56.

76. 'M. Broglie', article by A.R. Weil-Brunschvicg + J.L. Heilbron Dictionary of scientific biography, (ed. C.C. Gillispie), vol. 2 ,pp.487-89

77. L. Brizard et Broglie,'Ionisation de gaz en presence d'un réaction chimique', Le Radium, 1910,7,165-69, and 1911,8, 181-89 and 273-79.

78. M. Curie and Kammerlingh-Onnes,'Sur la radiation de radium à la température de hydrogène liquide', Le Radium, 1913, 10, 181-86.

79. P. Langevin,ibid., 113-22.
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80. Table from Le Radium, 1910, 7,

81. G. Moreau,'Sur la masse de l'ion négatif d'une flame', ibid, 70-74.

82. L. Gouy,' Les raies magnéto-cathodiques', le radium, 1911,8, 129-34.

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7. GENERAL CONCLUSIONS

Could it be said that the physics practiced, published and rewarded in France in the period 1850-1914 was in some way peculiar to France ? This thesis has attempted to bring out the essentially experimental character of physics in France; physics signified the prosecution of a 'normal science' type of activity based largely on theoretical foundations laid down by French savants before mid century. Reasons for this have been sought in the institutional structure of French science.

Leaving aside for the moment the polytechniciens, the majority of the individuals considerd here, whose area of study and research was physics, earned their living either as teachers of this discipline in higher education, or as teachers of the wider discipline, physical science, in secondary education; they were scientific civil servants in a centralised educational system. At the begining of the period considered, teaching was neither a highly paid nor highly regarded profession, except perhaps for the fortunate few who obtained positions in the prestigious institutions of the capital. Teaching science in the provincial faculties was not an attractive proposition for a young scientist who wanted to combine research with his pedagogic duties, because the faculties were poorly funded, saw few high quality students and the level of the licence was scarcely above that of the baccalaureat. This situation changed only slowly later in the century, with the attempts of the Third Republic to develop the provincial faculties, allowing them some autonomy and permitting them to raise finance from local government and industrial sources. But for most ambitious science teachers, (and this is particularly true throughout the nineteenth century), social status, attractive salaries, and adequate research facilities, could only be achieved through the winning of a chair in Paris.

Thus we have examined here, in the main, the career of people who were educated in the *Ecole Normale*, and worked for their doctorate (either while teaching in Parisian *lycées* or while employed as *agrégés préparateurs* in higher education institutions of the capital) to become elegible for a faculty position. Usually going first to a provincial faculty, they aspired to work their way to a position, and ultimately to a chair, in one of the Parisian institutions of teaching and research. And their progress through the system, even for the most favoured ones from the *Ecole Normale*, who had ranked high in the *Agrégation*, depended on the yearly reports on the quality of their teaching, the soundness of their political attitudes, and increasingly as the century wore on, on the quantity and the quality (judged by the leaders of the scientific community) of their research. French physicists shared, as any group or community engaged in research must share, a committment to established theories, tools, and methods. In any scientific community there is a certain natural, legitimate, conservatism which seeks to protect and preserve the hard won body of scientific knowledge from the attacks of cranks, charlatans and shoddy practitioners of science. The quality of research is naturally judged by the leaders of the scientific (in this case, physics) community in the light of existing theory.

By the time this study opens, France already possessed a substantial body of physical theory, which gave the basis for research programmes in optics, electricity, radiant heat, heat conduction, acoustics, behaviour of gases etc. Scientific reputations were to be gained by the meticulous, precise experimental work based on this body of theory. It cannot be said that this in itself is particularly different from the situation in other national scientific communities. One is put in mind of the comment by Rayleigh when he discoverd and published for the first time, nearly 50 years after it was written, Waterston's work on kinetic theory. Excusing the rejection of the paper by the Royal Society, Rayleigh remarked that a young scientist should establish his reputation by 'work whose scope is limited and whose value is easily judged'[1] (my emphasis). Although it might be argued that Rayleigh had a particularly aristocratic mentality, and that other more democratic British scientists might have looked more favourably upon precocious talent expressed through unorthodox research, Rayleigh expresses a universal dilemma in scientific communities; how to judge the quality of work other than by reference to accepted paradigms. In France the value of the research undertaken by a young scientist was judged by more experienced members of the community, and promotion and progress depended on that report. Rayleigh suggested that only when a young scientist had established his reputation should he then embark on 'higher flights'.[2]

In France a whole lifetime spent on work which is 'easily judged' could certainly lead to the highest ranks of the scientific establishment, but would it then be possible to turn to 'higher flights',to work of a greater scope ? Pasteur, writing in 1868, bemoaned the fact that unlike in Germany, where a successful scientist might be offered a research and teaching post and a well equipped laboratory in order to continue the line of research which had brought him recognition,success in France was rewarded - by another and perhaps yet another teaching post.[3] While teaching in itself can be a benefit to a scientist, as Duhem pointed out in relation to his work in the Faculty of Lille, where bright and interested students forced him to clarify his own thinking on difficult concepts,[4] the accumulation of teaching duties of the type which fell to Pierre Curie in the early twentieth century represented a drudgery which certainly did not improve his capacity for creative research. This accumulation of teaching positions, often together with administrative and political positions, was one factor which must have adversely affected the work of French scientists, particularly those working in the capital.

Moreover, when we enter the period of the Third Republic, the position of physicists, (as other scientists), becomes more closely bound up with the state. From its establisment in 1871, up to the Great War, the state espoused a 'scientific' ideology of varying degrees of anti-clericalism. After 1881 its primary schools sought to inculcate republican virtues through little lessons on morality based on rationalism as opposed to Catholicism,[5] and it sought to justify the social order by reference to science rather than to religion. The methods of the scientist, and the value-free knowledge he acquired by his unstinting and disinterested toil, were extolled as the model for other branches of human activity.

Perhaps it is a result of this that French physicists, even the most theoretically able, showed a great reluctance to abandon the old theories, preferring always to emphasise the elements of continuity between old and new, when, at the turn of the century, the demolition of these old theories occurred. Moreover, after the humiliation of the defeat by Prussia it became a patriotic duty to defend French theory against attack by foreigners, a patriotic duty to continue lines of research, like the velocity of light, which were traditionally French and had brought France so much glory in the past.

But by the end of the last decade of the century, when much of the classical theoretical edifice was in ruins and opponents of the Republic were attacking the very rationality of science, the response of some physicists (and their position was most coherently articulated by Henri Poincare) was to redefine the aims of physical theory. To them, theories were not attempts, limited and imperfect to be sure, to describe the ultimate reality of nature , they became simply convenient devices to predict phenomena. Thus a caution in respect to the formulation of new hypotheses about underlying processes and mechanisms which could explain phenomena, tended to be replaced by an attitude of relative indifference to the validity of these hypotheses. If one hypothesis could be found to explain the phenomena and their relations, another ten could equally be found. For example, writing later in the twentieth century, Marie Curie claimed that she and her husband had been the first to suggest that the energy from radium had come from the disintegration of its atoms.[6] This may be perfectly true, but it was a hypothesis to which they gave little importance, it did not suggest to them a line of investigation and they continued with their experimental investigation of radioactive phenomena. On the other hand, Rutherford did give importance to the hypothesis of atomic distintegration using it to embark on a series of experiments, which were to prove extraordinarily fruitful.

But it would be a big mistake to attribute a homogeneous 'conventionalist' position to all French physicists at the turn of the century, just as it would be to attribute to them a rigorous positivist one, in spite of a fairly universally expressed positivist rhetoric.[7] Positivists like Berthelot, or Gabriel Lippmann, or Le Chatelier can be identified, but they did not establish schools or influence great currents of scientific opinion. Perhaps it can be said that the _Ecole Normale_ physics teacher Bertin, who saw his job as being one of training modest science teachers, and who communicated an ironic scepticism towards mechanical theory to his students, thereby influenced later generations of physicists. This may be true, and many of the _normalien_ science teachers trained by Bertin, became and remained, excellent _lycée_ pedagogues, content to demonstrate effects and expound on the laws which related phenomena. But those who embarked on research wanted something more, for they found, as Bertin's most famous pupil, Duhem, expressed it:

'(that) ..the extreme demands of positivism are repugnant to the human mind'.[8]

And so Duhem, who expresses his positivism by asserting that it is not the task of physical theory to explain experimental laws, nevertheless claims that this theory;

> '..through its successive advances, tends to arrange experimental laws in an order, more and more analagous to a transcendent order..'[9]

Thus the way that physical theory orders experimental laws is a reflection of an ontological order. This is not the same as Poincare's conventionalism, and, more important, it means that new theories should be logically linked to the older theories they replace. Duhem insists on this when he argues for the Helmholtz electromagnetic theory (with some of his own modifications) in preference to that of Maxwell. The Helmholtz/Duhem theory can be logically connected to classical ideas of Weber and Neumann on electromagnetism whereas Maxwell's cannot.[10] This way of thinking, involving a succession of logically linked theories closer and closer approaching its limiting form 'namely that of a **natural classification**',[11] tends to favour a 'gradualist' conception of scientific advance, rather than a 'revolutionary' one where there is total discontinuity between theories. Although one cannot say that Duhem, exiled to Bordeaux and permanently out of favour with the pro-republican scientific leadership, had much influence on his contemporaries and his students were few, he does seem to be expressing a view held by other French physicists of the turn of the century when he insists on this continuity of theory. For example, Cornu writing on the electro-magnetic theory of Maxwell, goes to great lengths to stress what is common in that theory, with the earlier theory of Fresnel.[12] Although Cornu's over-riding interest in this question is probably to defend French priority, he is at one with the Catholic, Duhem, in emphasising continuity in theory. Moreover, the recent study of French physics between the wars by Pestre,[13] has commented that French textbooks of the 1930's were concerned to show continuity with classical theories, rather than revolution, when discussing quantum physics and relativity, indicating that the tendency displayed in the period of this thesis was continued later into the twentieth century.

If we accept Heilbron's analysis that Poincaré's conventionalism was a way of closing ranks against external attack and achieving the maximum possible internal consensus among physicists, we are still not much closer to understanding what philosophical beliefs French physicists actually possessed at the turn of the century. Certainly none of them admitted any debt to either Poincaré or still less Duhem, in the formation of their theoretical ideas. What we can say is that many French physicists of the late nineteenth century shared, even if they rarely mentioned them, mechanical assumptions about the world they were investigating. That is, they reduced or tried to reduce, everything to the basic mechanical concepts of motion and configuration of particles of matter. This reductionism was expressed by mid nineteenth century figures like Jamin and Verdet and later by Violle,[14] and is an expression of the powerful molecular-mechanical tradition inherited from Laplace, Poisson and Cauchy. Others were more forthright in their defense of a mechanical view; Cornu at the 1900 Congrès International de physique spoke of the modern discoveries in physics which demonstrated the triumph of the '...audacious Cartesian concept.. that there is nothing in the physical world except matter and motion',[15] Brillouin defended atomism and the ideas of Boltzmann against Ostwald, Gouy argued that Brownian motion gave direct evidence of kinetic theory, and Perrin and Langevin did much, experimentally and theoretically, in the early twentieth century to raise the status of mechanical explanation.

This is not to deny entirely the influence of positivism. By the end of the century there was, as previously mentioned, an easily identifiable positivist tendency within French physical science, particularly concerned with the study of thermodynamics. Berthelot, Lippmann, Le Chatelier and Duhem eschewed, as Lippmann was to say, any reference to;

> '..supplementary hypotheses and theorems which came later and were grafted onto thermodynamics under the title of explanations: molecular hypotheses, or mechanical theories of gas..'.[16]

Duhem could write a textbook of thermodynamics (Thermodynamique et Chimie, 1902) and mention Maxwell only once in it and Boltzmann not at all.[17] A later writer, Bruhat, whose thermodynamics text book went through several editions between 1926 and 1942, devoted only 18 pages out of 400 to the kinetic theory, relegating it to the status of a 'scientific curiosity'. [18]

This thesis has also attempted to bring out the difference in style between the physics taught and practiced by polytechniciens and normaliens. The elite character of the student body of the Ecole Polytechnique, its acute sense of superiority tempered by a sense of social responsability and a St. Simonian conception of the utility of science for the development of society, has been very well analysed by Shinn.[19] What is particularly important to this thesis is that students acquired at the Polytechnique, knowledge of a complete, well organised, and finished, set of principles and laws. The school transformed its students, who were already a social elite, into a technological elite, with a reputation for objectivity and infallibility, and whose self-confidence was underpinned by being in possession of a body of abstruse scentific truths inaccessible to the general population.[20]

Throughout the century, the principal research interest of the Ecole Polytechnique was optics, (as it had been since the time when Arago had considered the school a nursery for the Paris Observatory), and most of its research was based on the wave theory of Fresnel and Cauchy. Fizeau, though never a student at the school, obtained his theoretical formation from studying its lecture notes and later became an examiner there. Cornu, Potier, Deslandres, Henri Becquerel and Carvallo were polytechniciens of the second half of the century who carried on the tradition of optical research. But the advent of Maxwell's electro-magnetic theory, introduced to France by the polytechniciens, Cornu, Potier, and Sarrau, (although in a form modified to make it acceptable to the French [21]) and the attacks by Ostwald on the mechanical aspects of Fresnel's wave theory in the 1890's left the polytechnicien in a vulnerable position; his infallibilty and hence his authority was threatened, his coherent, immutable body of theory now looked like an untidy package of outmoded scientific concepts. The response was to stress the continuing utility of these concepts in predicting phenomena, for it is a canon of St. Simonianism that the scientist is useful to society and superior to ordinary men because of his ability to predict. Moreover, once again, the common bond linking the new theory with the old was stressed, emphasising the elements of continuity. If we leave aside both Henri Becquerel and Henri Poincare, who were both products of the school from a slightly earlier period, we can say that its later students and teachers played little or no role in the new areas of physics research which opened up around the turn of the century.

In respect to the Ecole Normale it has already been mentioned that its physics teacher of the 1860's and 1870's, Bertin, considered that the highest attributes of normalien scientists (i.e. science teachers) were clarity of expression and dexterity in experimental manipulation. As normaliens represented the overwhelming majority of physics teachers, and it was the membership of this profession which carried out most of the research, normalien values permeated the community and gave a predominantly experimental rather than theoretical stamp to French physics in the period considered. It is true of course that there were scarcely any career opportunities for theoretical physicists in France; there was one post in the Paris Faculty, one at the Collège, and one other specially created to reconcile Duhem to his continuing exile in Bordeaux. Those who were good at mathematics preferred to do pure mathematics, or applied advanced mathematical techniques to older physical problems.[22] Although it is dangerous to make sweeping generalisations about Britain or Germany without making a full analysis of the scientific communities of these two countries, one cannot fail to be struck by the absence in France of theoretical physicists of the status of Kelvin and Maxwell in Britain, or Clausius, Helmholtz or Planck in Germany in the second half of the century. Perhaps in this question the vocational objectives of the Ecole Normale, no less than those of the Paris Faculty, militated against the formation of theoretical or mathematical physicists.[23] While normaliens prepared for and dominated the national competitive agrégation, they also attended the Paris Faculty for lectures to prepare them for the licence. As physics and chemistry were associated in the same lycée teaching post,(mathematics being separate), aspiring teachers with an interest in physics would normally prepare for the physical sciences licence, which contained more chemistry than physics and little mathematics. Thus the qualification itself tended to keep physics and chemistry together (emphasising the manipulative character of the two disciplines), while distancing them from mathematics.

But if clarity of explanation and experimental dexterity were the hall-marks of good normalien science throughout most of the nineteenth century, we must not neglect to give emphasis to the change which came over the style of physics teaching when Marcel Brillouin was appointed Maître de Conférence in physics in 1888. Brillouin himself obtained doctorates in both mathematics and physics in 1881, and from 1900 held the chair in mathematical physics at the *Collège de France.* Although certainly an able experimentalist, Brillouin was interested in theoretical questions, unlike his predecessor, Bertin, and became a brilliant defender of the ideas of Boltzmann. It was his preoccupation with theory, which was to be so important in the formation of a new generation of physics researchers, the most notable of whom were Perrin and Langevin.

The role of the faculty system in the training of science teachers and the contribution of the faculties to research have also been examined. Not until we encounter Pierre Curie in the 1870's can we find a *licencié* of the Paris Faculty who was able to make, albeit with enormous difficulty, a career as successful as that made by scientists who were products of the *Grandes Ecoles*. The only example of a successful science *licencié* previous to Curie, was Jules Janssen the physical astronomer, who attended the Paris Faculty in the Second Empire, was awarded his *licence* in 1857 and his doctorate three years later, and was elected to the astronomy section of the Academy in 1873. But in over half a century these are rare successes and only in the early twentieth century, with the rapid expansion of radioactive research, did the Paris Faculty become a major centre both for research and for the training of a new generation of physicists. This was particularly so after 1907 in the laboratory of Mme. Curie, although still the most able and sucessful of the new generation found their training in the *Ecole Normale*.

The specific contribution to French physics by the provincial faculties and the Catholic institutes has also been considered. The Catholic Institutes provided positions for such people as Branly and Amagat, and perhaps it can be said that their research did not follow the well worn paths beaten out by many who made their career in the State system. But even though the Catholic Institutes can be said to have provided a competitive stimulus to the faculties, their influence should not be overestimated; the number of students who studied there was so small compared to the state sector.

The provincial faculties were a different matter, and recently Nye has made a reassessment of the usual centre/periphery argument.[24] This thesis has, however, shown that the contribution of the provincial faculties to the production of new knowledge in physics was extremely limited, although it was increasing as all the strategies to decentralise the higher education system began to bear fruit around the turn of the century. Some provincial faculties, like Caen, Dijon or Montpellier for example, in centres which were badly placed to receive any funds from local industrial concerns, never made any contribution which was judged at the time to be significant, i.e. there was little or no research from them published in national journals or referred to by other scientists. One of the faculties best endowed by local industry was Nancy, and this began to achieve a certain national reputation through the research of Blondlot in the last years of the nineteenth century, research which gained him the Gaston Planté prize in 1893, and the prestigious Lacaze prize in the early years of the twentieth century. But even here, where the team of researchers, Blondlot, Bichat and Gutton, were linked by regional loyalties to influential 'Nanceians' of the capital like Poincaré, the Faculty owes its fame principally to the 'N' ray fiasco. Bordeaux appears now to have some national importance because of its association with Duhem, but his scientific status at the time should not be overestimated, and his work and that of his students, Marchis and Turpain, seemed somewhat out of step with the rest of France. Lyons, with the work of Gouy, acquired some national standing, as did Marseilles through the metrological work based on interferometry carried out by Macé de Lepinay, while Lille with the acoustics of Terquem, and the mathematical physics of

Boussinesq (which was to take him to Paris) bear some mention. But most of the work done in the provinces, which had any national resonance, was carried out by young 'birds of passage' on their way to Paris, although Blondlot and Gouy preferred to remain in the provinces and became corresponding members of the Academy.

In the period of the Second Empire the research of professors at the *Ecole Centrale*, the *Conservatoire des Arts et Métiers*, and the *Museum* has been considered because it was specific to the respective institution. However, in the Third Republic there was no physics research coming from staff of the *Centrale*, and work at the *Conservatoire* was in the hands (at different times) of Edmond and Henri Becquerel and Violle, all of whom had chairs in other institutions. As for the physics chair at the *Muséum*, it continued to be a Becquerel family fief. This raises particular difficulties in examining whether there is a variety of institutional research styles in the French experience. While it might be possible to associate certain types of physics research with particular academic centres in Britain or Germany, the practice of *cumul*, in which an individual holds a number of posts, makes this much more difficult in France. For example, Jamin held simultaneously the physics chairs in the Paris Faculty and the *Ecole Polytechnique* in the 1860's, Violle was *maître de conference* at the *Normale* and professor at the *Conservatoire*, while Pierre Curie taught and did his research at the *Ecole Municipale*, but was also a *répétiteur* at the *Polytechnique*, and taught a low level science course for medical students at the Paris Faculty. Thus we have seen the personal style and interests of the individual carried around with him from institution to institution blurring any definite institutional style. In the provinces of course, *cumul* could hardly operate, (although some faculty professors also taught in local *lycées* in the earlier part of the period), and so it was easier for a specific faculty style to develop, as we have seen for Nancy or Marseilles, for example.

The influence of the three national scientific societies has been considered although the first, Leverrier's Association scientifique de France has no importance after the demise of the Second Empire. The contribution of the AFas to physics has been examined, and although the Association distributed some small research funds to provincials like Crova and Turpain, and some work on the moving frontier of physics was reported to its meetings, it cannot be said to have had the importance of, for example the BAAS in Britain. As its meetings were for the edification of the local and provincial lover of science, they tended to be exercises in 'haute vulgarisation' and for this reason alone, physics never played such an important part in the annual meetings as, say, natural history, geology, rural economy or medical science. Duhem, however tried to use the meetings of the AFas, to overcome his isolation in Bordeaux and bring his theoretical ideas to a wider national audience.

The Société de Physique, founded like the AFas immediately following the disaster of the Franco-Prussian war, naturally has a much greater significance in the development of the science. The results of a few pieces of original research first saw the light of day in its meetings or in the columns of its journal, although in general, physicists still preferred to publish first in the Comptes rendus. One rare example of a 'first' for the Journal de Physique was Gouy's paper on Brownian motion in 1885, which was perhaps considered too speculative for the Academy. But whatever the objectives of the organisers of the Société in relation to decentralisation, its meetings were always held in Paris and half its membership came from there. The meetings tended to be dominated by Parisians, and at least in the 1870's and 1880's the large number of instrument makers and engineers, made the demonstration of instruments and apparatus the major preoccupation. Because there were no scientific qualifications demanded of its members, this again militated against the presentation of difficult theoretical topics. Nevertheless there was a slow process of advance in the theoretical content of the meetings (not always to the liking of the membership) and by the mid-1890's when there was an explosion of new research in cathode rays, X rays, photo-electricity, and radioactivity, the Society was able to play a very positive role in the dissemination of the new knowledge.

This thesis has not attempted to say anything new in relation to the question of the funding of science, except for some consideration of the prizes and grants distributed by the Academy and and scientific societies. Heilbron et al. have shown that around 1900 the level of funding in France was comparable to that in Germany, taking into account the difference in size of the two populations.[25] We know that French physicists from the time of the Second Empire always compared French research facilities unfavourably with those available to scientists in Germany, and there can be no doubt that most French provincial faculties were less well endowed than most German universities, although by 1900, the Paris Faculty was probably the equal, in terms of funding, to the best institutions in Germany. But, in any case, there is not necesarily a direct relationship between funding and creativity. At the turn of the century the example of Curie at the Ecole Municipale de Physique et de Chimie Industrielles demonstrates that some of the most original work in France was coming from the most poorly funded sector of the educational system. Moreover it can be said that Poincare's insight which directed Becquerel to look for X rays from fluorescing materials, or Gouy's realisation that Brownian motion gave evidence of kinetic theory, required no expensive apparatus. This is not to say that it cost nothing; there is the previous investment in the training of the scientist, the current funding which permits him time for reflection, even if he does not handle equipment. But nevertheless, one is tempted to think, that in the French context, a higher level of expenditure on research might have simply meant more apparatus to carry out yet more precise experiments in the traditional research areas. It is true that when the Ecole Polytechnique was presented with a high resolution Rowland concave diffraction grating in the 1880's, Deslandres was able to put it to good use in the study of molecular spectra, but one suspects that if the school had received a Crookes' tube, it would have gathered dust in a corner of the laboratory. Funding is important obviously, but what is more important is an intellectual climate which permits and encourages the scientist to go outside the well-trodden paths of traditional research programmes.

In the opening chapter an attempt was made to define the content of the discipline of physics at the beginning of the period. Some physics textbooks and syllabi of courses in the Third Republic have been used to see how the subject boundaries extended to accommodate new knowledge later in the century. New discoveries like those of hertzian waves or X rays were assimilated into the corpus of physics knowledge without any difficulty; the panoply of experiments to establish their wave nature was part of the stock-in-trade of physics teachers from before mid-century. Certain areas of work which lay between physics and chemistry, like electro-chemistry for example, which was and continued to be part of some physics courses, or the study of solutions or changes of phase, succeeded in establishing themselves as a separate discipline (that of physical-chemistry) with its own course of study at the Paris Faculty in 1893 and with a special academic chair by 1906. Radioactivity research tended to blur the distinction between physics and chemistry, although the preparation of new radioactive elements and their classification through their properties was clearly the province of the chemist, while the physicist concerned himself with the nature and properties of the radiation, and of atomic nuclei. Thermodynamics and the concept of energy and its conservation, which had, from mid-century played the role of a unifying concept for the apparently disparate elements of what had once been called physique particulière and later physique expérimentale, had become as much the province of the chemist as the physicist, by the end of the century.

Different institutions showed different degrees of enthusiasm in assimilating new areas of knowledge into the teaching and research programme of physics. This enthusiasm depended partly on the institutions' traditional interests and partly on the vocational objectives of their teaching programmes. For example, in the Third Empire it was in the Conservatoire, with its more practical courses, that research on the mechanical equivalent of heat was carried out, while some 30 years later it was the Ecole Polytechnique which brought the ideas of Maxwell to France. As the educator of state engineers who were responsible for telegraph, telephone, and (later on) wireless telegraph communication, the staff of the school needed to acquaint their students with Maxwellian theory, even if their own grasp of it was not magistral. The most prolific writer on the subject of telephone communication in the late 1880's in France was the Polytechnique lecturer, Vaschy, who by 1887 was still giving a modified electrostatic theory (i.e. without taking into account magnetic effects), of signal transmisssion in telephone lines, and who published a clear account of the benefits of self-induction in telephony, only after Oliver Heaviside in England had done so.[26] But it must be said that even in Britain, practical telephone engineers like Preece continued to argue in the 1880's that the quality of telephone signals could be maintained simply by reducing the resistance of the line, while Heaviside, well versed in Maxwellian electrodynamics, found considerable opposition to his ideas.[27]

While the application of science to technological processes and industries has always been a very marked feature of French engineering practice, (see the recent study by Kranarkis [28]) the influence of industrial technological development on French physics is much more difficult to see. It can of course be seen at the level of the utilisation of electrical, vacuum, or cryogenic equipment etc. in the laboratory, but it is not possible to see, for example, any parallels to the way in which Kelvin's grappling with the problems of signal transmission in the Atlantic telegraph cable, helped him understand better the role of resistance and capacitance in circuit theory. The intrusion of the workshop and its practices into British physics, which Duhem disliked, was not an important feature in the French experience of this period, although it could be argued that it was, in an earlier period, when the pure science of hydrodynamics was much influenced by the design and operation of water wheels and turbines. This weak interaction between technological practice and scientific theory is partly a reflection of the greater industrial strength of Britain compared to France throughout the nineteenth century. In spite of the development in Paris of the first efficient motor ond generator by Zenobe Gramme, France was slow to develop a heavy electrical industry which soon came to be dominated by German and American companies, and in the *Congrès International des Electriciens* held in Paris at the end of 1880, her scientists were able to make little contribution to the discussion on 'absolute' electrical units.[29]

Even into the twentieth century, where it is true France had developed new technological industries like those of car and aircraft production, and what might be called science based industries like liquid gas production and the manufacture of radioactive materials for research and medicine, much of her industry remained, as Cornu was to complain to the Association of Instrument Constructors, artisanal.[30]

Thus we can say that industry did not play a powerful role in shaping the characteristics of physics in France; what was much more powerful was the experimental tradition, the centralised educational system which tended to reward 'normal science' based on French theoretical work of the early part of the century, and an excessive allegiance to this body of theory prompted in part by the humiliation of 1870-71.

Notes for Chapter 7.

1. J.Rayleigh. Rayleigh made this statement in an introduction to Waterston's 1846 paper published in the *Philosophical Transactions* of 1892. Taken from S. Brush, *Kinetic Theory. Vol.i, The nature of gases and heat*, Oxford, 1965, p.18.
2. Ibid.
3. L. Pasteur,' Quelques reflexions sur la science en France', *Oeuvres,vol.vii,* Paris,1871, pp. 199-221.
4. P. Duhem, op.cit., (note 1. chapter 4), p.277.
5. T. Zeldin, *France 1848-1945, vol.iii, Intellect and Pride*,Oxford, 2nd ed., 1980, p.178.
6. M. Curie, *Marie Curie*, New York , (trans. C. and V. Kellogg), 1923, p.120.
7. This point is made for the first half of the century, (but it applies also to the second), in;

   C.W. Smith,'"Positivism" and "Reductionism" in early Victorian physics', *Rivista di Filosofia*, 1982, *22-23*, 9-35.
8. P. Duhem, op.cit., (note 1. chapter 4), p.296.
9. Ibid., p.26.
10. P. Duhem,'Sur l'interpretation théorique des expériences hertziens' *C.r.A.F.a.s.*, 1895, p.219.

11. P. Duhem, op.cit., (note 1. chapter 4), p.335.

12. A. Cornu, op.cit., (note 89. chapter 5), p.1031.

13. D. Pestre, op.cit., (note 56. chapter 6), p. 57.

14. L. Poincare, 'L'evolution de physique', R.s., 1906,5, 481-86,(483).

15. A. Cornu, op.cit., (note 3. chapter 6), p.7.

16. D. Pestre, op.cit., (note 56. chapter 6), p.47.

17. Ibid.

18. Ibid., p.49.

19. T. Shinn, op.cit., (note 16. chapter 2), pp.52-59.

20.. Ibid., p.90.

21. H. Paul, op. cit., (note 12. chapter 2), p.256.

22. D. Pestre, op.cit., (note 56. chapter 6), p. 109.

23. No distinction has been made in this thesis between 'mathematical' physics, and 'theoretical' physics. Kuhn makes the point that the mathematical physicist 'tends to take the physics problem as conceptually fixed,and to develop mathematical techniques for dealing with it; the second (the theoretical physicist) thinks more physically, adapting the conception of his problem to the often more limited mathematical tools at his disposal.' T.S. Kuhn, op.cit., (note 3. chapter 1), p.65.

24. M. Nye, Science in the provinces; scientific communities and provincial leadership in France, 1860-1930. Oklahoma, 1986.

25. P. Forman,J. Heilbron,and S. Weart op.cit., (note 5. chapter 5), p. 52.

26. Vaschy's contribution to the theory of telephone signal transmission is dealt with in;

D.W. Jordan,' The adoption of self-induction by telephony 1886-1889', Annals of science, 1982, 39, 433-461, (449).

27.B. Hunt,'"Practice vs. theory": The British electrical debate, 1888-1891', *Isis*, 1983, *74*, 341-55.

28. E. Kranakis,'Social determinants of engineering practice', *S.s.s.*, 1989,*19*, 5-70.

29. See the proceedings of this congress, dominated by the British, in 'Congres International des Electriciens', *Comptes rendus des travaux*, ed. G. Masson, Paris, 1882.

30. A. Cornu, quoted from an article by J. Payen,'Les constructeurs instruments en France au XIX siecle'. *Archives Internationales d'histoire des science*, 1986, *36*, 84-161, (156).

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L'Annee scientifique et industrielle (ed. Figuier)

Journal de physique pure et appliquée. Only very limited use was made of this source, but much of its material also appeared in the journal of the *Séances de la société Française de physique.*

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