WILLIAM GILBERT'S SCIENTFIC ACHIEVEMENT

AN ASSESSMENT OF HIS MAGNETIC, ELECTRICAL AND COSMOLOGICAL RESEARCHES

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Abstract

The thesis aims at a more detailed and comprehensive evaluation of Gilbert's magnetic, electrical and cosmological work than has been carried out so far and at correction of important errors in its earlier historical and methodological assessments. The latter concern the general approach to Gilbert, who has, for example, been seen as a 'natural magician', but also specific mistakes such as the widely held view that his conception of magnetic action amounted to an effluvial theory or that he held magnetic forces to be paramount in the universe. Some historians have claimed that Gilbert's most important observations are theory-laden to the detriment of his results. The thesis considers these matters and the theoretical-observational distinction at some length. Gilbert's exploration and wide experimentation concerning magnetic and electric effects, his tests of the work of his predecessors, notably Peregrinus, Norman and Porta, and his experimental investigations of some important phenomena are closely examined as examples of the process of very early science. These matters concern Gilbert's first work, the *De Magnete*.

His cosmological views, although touched on in that book, are set out in more detail in his *De Mundo*. The wide neglect of this work has led to some of the errors in the appreciation of his cosmology. The thesis examines Gilbert's support of Copernicanism and his views on gravitation and cosmological forces.

The overall assessment considers the originality of his experimentation and his theoretical results in comparison to those of his predecessors and problems like the influence of his animism and some claims as to the orgin of his method.

The conclusion is that Gilbert took the decisive step in the history of science from occasional experimental investigations of isolated problems by his predecessors to a comprehensive examination of a whole area of physics, magnetism, and the creation of a qualitative theory employing genuine theoretical concepts. His electrical researches offer, similarly, one of the earliest examples of properly scientific work. His discovery of the earth's magnetic field provided a suggestion for the existence of cosmologicl forces and connected terrestrial with extra-terrestrial physics.

Contents

Chapter Nine. Gilbert's cosmology. The *De Mundo*.

Chapter Ten. Gravity.

Chapter Eleven. The Origins of Gilbert's Work.

Chapter Twelve. General Features of Gilbert's Views and Methods.

Introduction

The need for an assessment of Gilbert's work

Although nearly four hundred years have passed since William Gilbert's pioneering scientific work in magnetism, electricity and cosmology, there still exists no detailed evaluation of it and no agreement on some of its most important aspects. This is regrettable. Gilbert's main work, the *de Magnete* (Gilbert 1958), records the work of the first scientist who experimentally investigated a whole field of physics in detail and systematically, and who produced a qualitative scientific theory. The opportunity it offers to follow sound and comprehensive researches in very early science is of great methodological interest. His immediate predecessors and contemporaries produced haphazard collections of observations mixed with erroneous reports and old-wives' tales as well as a very few good experimental tests of isolated hypotheses in magnetism. A comparison of this with Gilbert's creation of a magnetic theory which covered the whole field as far as the experimental possibilities of the time allowed, and which was not to be improved upon for many years, is also of historical value.

When reading in the history of magnetism and electricity, I found that many modern comments misrepresent important individual facts and come to erroneous overall evaluations of Gilbert's work. This convinced me of the necessity of its new appraisal. Historians such as E.A. Burtt, H. Butterfield, A.C. Crombie, A. Koyre and J. Agassi have given short summaries of aspects of, or comments on, Gilbert's results, which contain serious mistakes. Somewhat longer descriptions in book chapters or papers by P. Benjamin, M. Boas, J.L. Heilbron, E. Zilsel and M. Hesse also display important errors in the understanding of details, of wider aspects, or of the totality of Gilbert's work. These comments often contradict one another and appear at times to be also self-contradictory, which only rarely seems to be due to real difficulties in Gilbert's text; for the occasional problems with the translation or interpretation of passages of Gilbert's books are usually resolved by comparing other formulations. This is made possible by Gilbert's habit of repeating his views, often several times, but in differing terms. Obvious difficulties of textual interpretation are consequently not mentioned by commentators.

The mistakes I will be dealing with are at times simple but very important ones, such as the claim that Gilbert postulated a magnetic effluvium. Others originate from a more general misreading which has led, for example, to the widespread claim that magnetism was to him the prime force in the whole universe. Some of the errors are caused by the fact that most commentators have ignored Gilbert's other book, the *de Mundo* (Gilbert, 1651), altogether, thus robbing themselves of the opportunity of clarifying important aspects of Gilbert's views. More serious consequences arise, not surprisingly, from ill-conceived methodological approaches and the attempt to use Gilbert's work to prove their applicability to a historical instance. This has been done occasionally in such a way that justice is done neither to Gilbert's theoretical nor to his experimental work. If such treatment is allowed to go unchallenged, the impression arises that the plausibility of the methodology has been enhanced by its apparently successful application to Gilbert's case.

The longer works on Gilbert by D.H.D. Roller and C.E. Benham do not offer a sufficiently detailed or complete evaluation. These books give summaries of the contents of the *de Magnete* with some assessments and comments aimed at clarifying aspects of Gilbert's position in the history of magnetism, electricity and cosmology. Benham's book is quite short and is of the nature of an introduction to the subject. Although it contains some perceptive statements, it also badly misjudges some of its important aspects. Roller, though concerned in very useful detail with some areas of Gilbert's work, summarizes others only briefly and does not treat of many important matters at all. His book, like the lengthy paper by M. Hesse, suffers from the methodological shortcoming of taking a pervasive theory-ladenness of observations for granted. S. Kelly's book on the *de Mundo* is a very useful and accurate summary of this work only. It is, however, too short to cover in any detail the points I will be concentrating on. I have paid particular attention to the *de Mundo's* sections on cosmology. The chapters on this subject in the *de Magnete* are much shorter and their proximity to the main sections on magnetism has perhaps been partly responsible for the exaggerated role many commentators have seen Gilbert to be giving to magnetism in his cosmology.

Only a detailed consideration of at least the important parts of Gilbert's experimental work can provide a secure basis for an appreciation of his results. I am aiming to give an exposition of his researches which takes account of the work of his predecessors and of the experimental possibilities and the use he made of them.

The assumption made by several historians seems to be that Gilbert had somehow conceived of his finished theories which he then set out to verify by wider experimentation. I believe, on the contrary, that Gilbert experimented at great length exploratorily, by re-testing his predecessors' results, and by forming numerous working hypotheses for further tests. His work was therefore probably to the greatest part not guided by a comprehensive theory but by many individual hypotheses which he may have formed on taking account of earlier experimental outcomes. If he had a grander theory at an early stage, he certainly did not assume its validity and then confine himself to the search for confirmatory experimental results. By closely examining his seminal experiments (and the important work done by others before him), one can avoid rash judgments such as, for example, one made by several commentators, viz. that Gilbert experimented with spherical magnets (his *terrellae*) only in order to support his claim that the earth itself is a magnet. For there are more immediate reasons of precedence and experimental practicability which may have led him to use loadstones of this shape; and without the *earlier* work with the terrella he would most probably not have discovered the earth's magnetic character.

An example of how I think one should not proceed is offered by Mary Hesse's paper. She sets out to prove that theories cannot be inductively arrived at from bare observations because such do not exist since all observations are theory-laden. She claims that his can be demonstrated with the example of Gilbert who, she says, had for instance a blind spot about magnetic repulsion, was not entitled by his results to separate attraction from the directional rotation of the needle, and to conclude that the earth is a magnet.

I believe that the experimental facts and possibilities should be carefully considered independently of the final theory the historian sees Gilbert to have held, for he does not know what working hypotheses may have occurred to his subject in the course of his experimentation. Any early assumption that he was just looking for support for his finished theory and therefore saw experimental results in theory-laden ways is a bad methodological principle. I will give the question of the theory-ladenness of observation reports some detailed consideration.

I have mentioned Gilbert's exploratory investigations. These I understand as aimed simply at finding out what the consequences of various arrangements and manipulations of magnetics or electrics were. Such exploration will have played a part in early magnetic and electric research carried out by Gilbert and his predecessors. I do not believe, however, that from such exploratorily made observations important laws or hypotheses could in general have been found by simple induction. I will assume that the observation of certain forms of behaviour of magnetics and electrics during exploratory work and reports of observations by earlier writers instigated the invention of various working hypotheses from which, usually, further consequences would then have been deduced and in turn tested. I am therefore adopting a generally Popperean stance. But I do allow that the early investigator, particularly, may be taking note of numerous phenomena in his subject area when he has as yet no hypotheses which concern them. A falsificationist view which held that a phenomenon could be registered or formulated as an observation only in the light of a *specific* and directly relevant hypothesis, would not be plausible. I strongly believe that all sorts of phenomena and repetitions of phenomena may be noticed by an observer at any time. Whether these are then integrated into considerations concerning his subject of interest or any hypothesis he may hold would depend on the (often accidental) occurrence of conceptual connections to current hypotheses.

I hope to demonstrate that Gilbert's theory of magnetism was in important respects the first proper qualitative scientific theory of modern science, based on thorough experimentation and arriving at genuine theoretical concepts, such as for example those of magnetic energy and the field. The main features of his electrical work will be described and evaluated. His cosmological views, particularly as expressed in the *de Mundo*, will be considered, and their degree of support for Copernicanism and their role in the downfall of the Aristotelian, and the development of the new, physics, will be assessed. The general evaluation of his position as the first modern scientist of his period will take account of comments on his alleged animism and the supposedly occult or metaphysical nature of his views which historians such as M. Boas have described.

I will often use the relevant remarks of, for example, the above-mentioned writers as launching pads for consideration of aspects of Gilbert's work, and the thesis will combine its examination with an often detailed evaluation of more recent historical comments on it. Although I will at times be very critical of much of what has been written about Gilbert, I am deeply indebted to these authors for the many apposite, interesting and challenging things they have said. In the absence of some of these this thesis would not have been written, for disagreement with several of their ideas first persuaded me of the necessity for an attempt at a new assessment of Gilbert's work.

In this I received useful help and critical advice from my supervisors, Dr. E. Zahar and Professor J. Watkins, to whom I therefore gratefully acknowledge my indebtedness though not all of my conclusions may coincide with their views.

Chapter One

Magnetic Poles

A. Gilbert's method.

Gilbert's manner of work, if we could retrace it, would be of great interest, for it would offer an important example of very early investigation which may be expected to display some basic features of scientific method that are perhaps less clearly discernable in more advanced researches. He reputedly devoted some 18 years to magnetic researches (cf. the address by Edward Wright at the beginning of the *de Magnete*) and will have gone over the same ground repeatedly. His book, probably for that reason, gives but a very limited account of the sequence of his discoveries and his working hypotheses. It is likely that many of his initial results will have been due to accidental and exploratory observations. An example of this may be his discovery of arming the stone .

It was Gilbert's first concern to investigate the full range of magnetic phenomena, to establish what the magnetic effects were (not even the boundaries of the subject area were clear, electrical phenomena still widely being regarded as magnetical). Gilbert knew the history of his field very well and considered all his predecessors' ideas in detail, re-testing their results thoroughly. But he did want to take a completely fresh look at all accessible aspects of magnetism and electricity, his knowledge of earlier work notwithstanding. Experimentation was his first concern and throughout his book he criticises mere book students of the humanist tradition who relied on authority or speculation instead of experimenting. He follows one of his admonitions to experiment (p. L in the preface, *de Magnete*) by a general remark:

"Hence the more advanced one is in the science of the loadstone, the more trust he has in the hypotheses, and the greater progress he makes; nor will one reach anything like certitude in the magnetic philosophy, unless all or at all events most of its principles are known to him."

This juxtaposition of 'hypotheses' and 'principles' is interesting. The latter are to be understood in the sense of 'phenomena' or 'facts' (the Latin has *'omnia aut . . . pleraque'*, i.e. 'all or most matters' where Motteley uses 'principles'). He seems to have felt that the hypotheses formed in connection with experimentation would be supported by other observations or tests so that a consistent theory (his 'magnetic philosophy') may evolve.

It is perhaps a good approach to an examination of Gilbert's researches to consider one or two of his statements about basic magnetic facts which are at least partly constituents of earlier knowledge of the subject to which they can then be related.

". . . the loadstone has from nature its two poles, a northern and southern; fixed definite points in the stone which are the primary termini of the movements and effects, and the limits and regulators of the several actions and properties. It is understood however, that not from a mathematical point does the force of the stone emanate, but from the parts themselves; and that all these parts in the whole - while they belong to the whole - the nearer they are to the poles of the stone the stronger virtues do they acquire and pour out on other bodies". (Gilbert, 1958, book I ch.3, p. 23. All quotations from the *de Magnete* below will be identified by book, chapter and page numbers only.)

"The loadstone ever has and ever shows its poles" (ibid.; here Gilbert places an asterisk in the margin to show that he considers this an original or newly interpreted discovery or experiment; he adopts this procedure throughout the book, important facts receiving larger asterisks than others).

"Poles are also found . . . in a piece of iron touched with a loadstone . ." (p. 24)

In these sentences Gilbert makes universal claims; he states laws of nature which perhaps today appear to be so simple that it is hard to imagine that there would have been many difficulties connected with their discovery.

In summary form, these laws are:

i) every loadstone has two poles which are always identifiable,

ii) the two poles differ from each other,

iii) the whole stone contributes to making up the poles' power and the nearer we get to the poles the stronger is the magnetic virtue in the parts of the integral stone,

iv) a piece of iron touched with a loadstone becomes a magnet.

His statements contain subtle and important claims and definitions (such as that of the term 'pole' in the first one), and although the discoveries of these fundamental magnetic facts are of course not all due to Gilbert, they comprise much that is original with him. But it is not possible to identify the novel aspects properly until more of Gilbert's theory has been examined, because the pre-Gilbertian term 'pole', for example, must be seen as a theoretical concept which in Gilbert's use derives its meaning from his theory. There have therefore been shifts or at least extensions in the meaning of such concepts when compared to earlier hypotheses, a phenomenon constantly met with in the development of a science.

But did the ideas of any of his predecessors merit descriptions as useful scientific hypotheses at all? For this to be the case they must be more than vague generalisations based on motley collections of correct and of erroneous individual observations, which is what many if not most of them amounted to, according to Gilbert. It is difficult to decide what sort of statement would qualify as a scientific hypothesis at this very early stage of science. I do not have a general rule or definition to offer but it seems reasonable to expect that the hypothesis should have been subject to some attempt at testing or, if this was impossible, at least connect to some extent with existing knowledge and be testable in principle, to distinguish it from pure guesswork or metaphysical statements; or else it should be soundly related to systematic observations, as perhaps in astronomy. It will emerge on examination of some of the more comprehensive sets of propositions about magnetic questions proposed by Gilbert's most important predecessors, such as Peregrinus, Norman and Porta, that we are dealing there with at least some isolated genuine and occasionally ingeniously tested scientific hypotheses, but that one finds on the other hand numerous widely accepted untested and often fabulous views on even the simplest magnetic phenomena before Gilbert. It will become clear that there existed nothing deserving of the name of a theory of the subject, nor thorough and comprehensive experimentation.

Gilbert refers in many places to the opinions of his predecessors on magnetic matters. Amongst these he quotes Giambatista della Porta's views most frequently. Porta devoted book VII of the 2nd edition of his *Natural Magic* (1589, I have used the English edition of 1669) to magnetism. In this he displays some fairly wide ranging knowledge of magnetic phenomena and reports numerous observations, but his work is unfortunately flawed by several elementary mistakes which at times directly contradict true statements in other chapters of the book. In any case, Gilbert owes a considerable debt to Porta for suggesting many matters and a great number of experiments for tests, re-tests and further work (Gilbert does very little to acknowledge this debt which brought him the charge of plagiarism from Porta and others). I will consider Porta's observations at the appropriate occasions and also in the chapter devoted to the originality of Gilbert's work in the science of his time. I am thus not going to start with a general survey of pre-Gilbertian views on magnetism but rather follow a course largely determined by the logical development of the important areas of his work or its exposition in *de Magnete*. To this end I will examine Peregrinus' results first and the direct use Gilbert makes of them. They are of considerable interest in their own right and their brief exposition here is well worthwhile.

B. Peregrinus and Gilbert.

Peregrinus described his researches in a letter written in 1269 (Peter Peregrinus de Maricourt *Epistula de Magnete*, published repeatedly in Latin and several vernaculars.). This contains the reports of the best experimental magnetic work to have been done before the second half of the 16th century. Gilbert owes crucial facts and suggestions to it. Some of his fundamental experiments concerning basic magnetic matters are repeats of those of Peregrinus', and the tests of the latter's reports and hypotheses (as well as of those of Porta's and Norman's) are examples of aspects of Gilbert's proper scientific method. What the reader sadly misses, however, is any acknowledgements due to Peregrinus, whose work he only mentions in passing (as a "pretty erudite book considering the time";

I.3, p. 9), together with that of many other authors who report anything from mere fables to good experiments.

Gilbert will originally perhaps have had little reason to consider Peregrinus' ideas to be superior to any of the others on the subject of which he knew. However, on repeating his experiments he must have soon seen that they were of a high quality and not in the same class as reports such as that garlic and diamond render the magnetic power ineffective, as claimed by Pliny, or that there are two loadstones, one pointing north, the other south, as Albertus Magnus thought.

Gilbert's failure to give Peregrinus credit for the experiments he repeats has brought him an early charge of plagiarism from the 17th cent. writer Thevenot (quoted by P.D. Timoteo Bertelli Barnabita in *Sopra P. Peregrino . . .*, Roma, 1868; reported by P.F. Mottelay in his translation of the de Magnete, fn. 2, p. 9 and fn. 1, p.166). This charge appears justified if it refers to the design of the relevant experiments by Peregrinus, and Gilbert's failure to mention that he was repeating the ingenious work of his predecessor, some of whose statements his own echo almost verbatim. However, as Bertelli (same source) remarked, this did not affect the quality and basic originality of Gilbert's scientific work as a whole. In fact the repeats of Peregrinus' experiments turn out in the end to be only a small, if important, part of Gilbert's work. They are, however, relevant in assessing the roots of the claims i) to iv) above. We will find that they were very useful to Gilbert but are not sufficient on their own to substantiate all four.

Peregrinus had described a procedure for finding the poles, "the one north, the other south", of a loadstone: this was to be made into a sphere on which a short iron needle was then placed in various positions in turn. Each time the longitudinal direction of this needle was to be marked with chalk and this direction prolonged right around the sphere, thus forming meridian circles. These circles will cut each other in two points, i.e. the two poles (fig 1). The image of the magnet covered with its meridians and poles resembles, as Peregrinus says, the celestial sphere. In fact, he thinks that the poles of the magnet receive their virtue from the poles of the 'world', because the needle points to them. (He was, by the way, aware of the fact that his celestial magnetic north pole would not coincide with the pole star.) The other parts of the magnet obtain their virtue from other areas of the heavens (Peregrinus, op. cit. ch. X).

Peregrinus knew that the poles of a magnet are the points of strongest attraction and suggests that they can also be found by testing, again with a small iron needle, for the spots of greatest pull on the surface of the loadstone. The poles are furthermore the only places on which short bits of iron wire will stand vertically. At all other places they will take up an angled position (fig. 2 from II. 6, p.122). Park Benjamin (1975, p 172) comments with respect to this discovery:

"That was the first definite recognition of the directive action of the magnetic field of force: the first revelation of the direction in which the strains and stresses therein are exerted, shown by the turning of the little bits of iron in response thereto, as an anchored boat swings to the tide, or a weathercock to the wind."

It cannot, however, be said that Peregrinus discovered the directive strains and stresses in the magnetic field and Benjamin's comment must be seen purely as an historical comment on what Peregrinus' statements imply. Peregrinus was not conscious of the existence of a directive field.

To distinguish one pole from the other, Peregrinus places a loadstone in a small vessel which floats on water in a bowl, and he names the pole which turns to the "north pole of the heavens", 'north', the opposite pole 'south'. By then moving a magnet around the outside of the bowl he discovers the law of attraction of unlike poles but does not clearly state that like poles repel in a corresponding manner. He appears to think that when like poles are brought near one another the floating stone turns round in order to allow the unlike poles to attract one another: ". . . the northern part seeks the southern, wherefore it will seem to repel the northern" (ch. VI). We will see that some traces of this view survive in Gilbert's theory.

Further important discoveries Peregrinus made were the reversibility of the polarity of a magnet by a stronger one, and the results with respect to polarity of cutting a magnet in two transversally to the polar axis. He then describes the behaviour of the two pieces when they are brought near one another in various ways. For this he uses - according to the drawings in the margin of the book - a pointed oblong stone.

The importance of Peregrinus' discoveries and their novelty can hardly be overestimated given the state of ignorance and confusion with respect to magnetic matters which existed before him. It is clear that he could reach his results only by extensive experimentation (his practical bent shows itself also in his important invention of the pivoted compass with sightline, together with what he thought was a magnetic perpetual motion machine, also described in the letter).

Benjamin (op. cit., p. 165-187) gives Peregrinus full credit for the development of the ideas expressed in his letter, as does A.C.Crombie (1953) who places the work in the context of the Oxford School and describes it as "the best known example of the use of the experimental method in the 13th cent" (p. 208). Roller (1959, p. 40) on the other hand says that "it is quite certain that Peter did not invent these ideas, despite the fact that we know of no earlier work on magnetism containing them." Unfortunately, Roller does not give any evidence for his statement. In the absence of this and in view of the fact that everything speaks for Peregrinus' experimental work having resulted in his theories one must assume that Roller is mistaken. The brevity of the remark does not allow one to assess his train of thought properly. All he quotes in support of it is (fn.111, p. 40) a sentence from an article by H.D. Harradon (1943) pointing out

". . . the similarity of ideas on the magnet [to those of Peregrinus'] which are found among scientists who lived before or contemporaneously with de Maricourt, such as Vincent de Beauvais, Albertus Magnus, Roger Bacon, and Jean de S. Amand".

I have already mentioned the badly mistaken views of Albertus Magnus on one of the relevant aspect of magnetism above (Albertus' opinions on magnets as expressed in *De Mineralibus* - I consulted the translation with notes by D. Wyckoff, 1967 - are probably simply quotes from the pseudo-Aristotelean *Lapidary* as Wyckoff points out on p. 104).

Vincent of Beauvais refers to the known directive and polar properties of the magnet and to magnetic rocks without apparently giving any new facts (according to Benjamin, op. cit. p. 101 and A. Crichton Mitchell, 1932, p. 105).

Bacon is thought to have been a pupil of Peter's, as Roller himself mentions on p 39 (cf. also Crombie op. cit.), and was in any case very familiar with his work (Bacon praises Peregrinus' mastership of experimentation and the breadth of his learning). There is no evidence that Bacon carried out magnetic experiments himself to any extent.

Jean de St. Amand makes interesting remarks on magnetism and also reports several magnetic experiments in his *Antidotarium Nicolai* (cf. Lynn Thorndike, 1946.) However, Jean was was still a child when Peregrinus wrote his letter.

Various speculative and unclear ideas on magnetic properties were in the air in the second half of the 13th century, but since Alexander Neckham first described the use of the compass needle in Europe (by then well established) in the last quarter of the 12th century, no important progress in the understanding of magnetism had been made before Peregrinus. The organic unity and logic of the latter's detailed experiments was indispensable for the results obtained. Any specific similarity to the views of earlier writers would have to be shown before one could claim that Peter 'did not invent these ideas'. I do not see how this could now be done unless new sources came to light, for the field of early magnetism has been well researched from known texts. It would in any case be most surprising if Peregrinus' well-designed experiments had not produced new ideas.

It has been claimed by commentators who remark on the import of the use of spherical loadstones by Peregrinus and Gilbert that their loadstones were of this shape because the stone was to resemble the shape of the heavens in the case of Peregrinus, and the earth in that of Gilbert. With reference to Peregrinus, Benjamin writes:

"Thus it [the stone] is caused to conform in shape to the celestial sphere" (p. 171), and "Peregrinus considered the magnetised needle as influenced by the poles of the spherical heavens represented in the loadstone globe" which he regarded as a "miniature heavenly sphere" (p. 278).

His comment on Gilbert's case is:

"In order to prove the like nature of the earth and the loadstone, Gilbert carved a piece of the stone into spherical form; because, as he says, that shape is the most perfect, agrees best with the earth, which is a globe, and is better adapted for experimental purposes." (p. 277).

Note that Gilbert, as paraphrased by Benjamin, here gives three reasons for the use of a spherical stone. Yet Roller says after commenting that Gilbert knew that oblong loadstones are more powerful:

"Nevertheless, Gilbert's theory that the loadstone achieves its power and virtue as a sample of the earth itself is so overbearing that this clear factual evidence is not effective in turning his mind from the spherical form; the greater attraction per unit weight manifested by the elongated loadstone is not of determining consequence compared to the Earth-loadstone relationship that his conceptual scheme makes so clear to him." (op.cit.,p. 136).

Mary B. Hesse (1960) makes similar comments:

"That he should conduct most of his researches into magnetism with loadstones of this shape is therefore determined by his theory that the magnetic power and shape of the earth are closely connected . . ." (p. 5)

The remarks just reported, apart from the fact that they do not all take proper account of the use of non-spherical stones by both Peregrinus and Gilbert, amount to the same claim: the spherical form was used in order to support a wider theory which had been thought up before the experiments with the spherical stone were made. I have commented above on the undesirability of making premature assumptions as to the sequence of the formation of hypotheses and the choice of certain forms of experiments in cases where the necessary evidence is lacking. As I have also suggested, it seems a better general principle, in the absence of such evidence, to look first for more immediate reasons for the form of a particular experiment in the nature of the physical problems as they affected experimental arrangements possible at the time. Any investigation of work carried out in a period when no theories existed and all matters concerning the subject were uncertain, should start with a close examination of the actual experiments. The connection between even apparently minor details of the practical work and the hypotheses suggested or tested by it can be crucially close at this stage. (Later, when developed theories exist, the details of an experiment are often less important, diverse alternative experimental possibilities which the theory suggests being usually available). In the case of early magnetic work, detailed investigation of the effects of the important aspects of the physical set-up seems not to have been carried out so far.

What, then, may have been the reasons for Peregrinus' use of spherical magnets (and Gilbert's similar choice)? It was known before Peregrinus that the parts of a loadstone somehow differed in effect. The existence of polarity in some sense was suspected. We may perhaps speculate that Peregrinus, by doing exploratory experiments with pieces of magnetite of various shapes and from earlier reports, came to think that it had indeed to have special points opposed to one another in a magnetic and perhaps a geometrical sense. It is important to realise that only by chance would a piece of magnetic stone broken from a rock in a mine have its poles opposite one another in positions which corresponded to some clear geometrically apparent sense of 'opposite'. Such a stone would therefore have to be cut and shaped into some regular form if it were to be used for systematic investigation.

If Peter suspected the existence of at least two poles in some way regularly opposite to one another this should show itself most clearly with the symmetry of a magnetic sphere (which also had the distinction of being thought of as the perfect geometrical body). Commentators have overlooked the fact that Peregrinus could not just assume that a piece of magnetite would move its poles so as to make the polar axis coincide with any clearly geometrically significant direction by being shaped into, say, some sort of block. This could, as far as Peter knew, only be reliably achieved if the position of the polar axis was known already and one cut accordingly. In the absence of this knowledge one would end up with a magnet whose poles might be anywhere as far as Peregrinus would at first have understood the situation. So the sphere is without a doubt initially the most promising shape. And the results justified the assumption.

The experiment of placing pieces of iron wire or needles on the stone to display the pole-to-pole direction is also greatly assisted just by the globular shape. The curvature of the surface provides a pivot point around which the needles would turn to reach their north-south alignment, something that often, because of friction, does not happen on flat or irregularly shaped surfaces of weak magnets (though with strong and smooth modern magnets the friction is overcome). It would therefore be almost a prerequisite to use a spherical stone so as to benefit from the pivoted support for the needles.

In the experiment of fig 2, it would also be quite impossible to make sense, on an irregularly shaped magnet, of the idea of systematic differences between the various slanting and vertical positions of the short bits of iron relative to that surface which were to show their sloping directions. Even magnets of rectangular shapes make for difficulties, and the problems of showing the directions of the wires' alignments to meet in a point on the stone are obvious when one considers their behaviour near the edges. It is hard for us to resist the temptation of assuming that a round or square section bar shape would have naturally offered itself to Peregrinus as the best shape to use. This would only be the case when one has some knowledge concerning the position of the poles, but not at the very beginning of research. Even to Gilbert, contrary to Roller's suggestion concerning the desirability of experimenting with the stronger elongated stones, it was not advantageous to use strong magnets of such shapes in experiments which explored basic magnetic facts. Though the sphere's field is usually not very strong, there would be no problems connected with very sudden changes of direction of the surface (edges or regions of very tight curvature). The experimental use of the spherical loadstone was therefore an ingenious way of exploring general features of magnets and of the field.

Having found the poles by drawing meridianal lines, Peregrinus seems to have been struck by the similarity of this system of great circles to the astronomical grid used for the celestial sphere. The details of the sequence in the genesis of Peregrinus' experiments and working hypotheses may of course have been different from the one here suggested, yet the drawing of the meridianal lines meeting in the poles on the magnet is almost certain to have *preceded* his appreciation that he had here an arrangement somehwat similar to that of meridians on a representation of the heavenly sphere. He does not draw any particular conclusions from the similarity of the two pictures of meridians other than to point it out, though with some emphasis on the remarkability of the fact with which he is very taken. This looks, therefore, much more like an unexpected discovery than the design of a purposeful imitation of the celestial sphere.

Peregrinus did not believe that magnetic mines near the poles on earth could cause the alignment of the compass needle as had been suggested by others; for magnetic mines exist elsewhere and the needle would therefore have to point into all sorts of directions at the same time. The thought that the earth itself might be a magnet, however, does not occur to him (we will see that much more knowledge of magnetism was necessary for this idea to suggest itself.)

Gilbert describes the experiment of placing needles on the spherical stone in various positions and remarks: "the ends of the wire move around their middle point, and suddenly come to a standstill". He then recommends to ". . . move the middle or centre of the wire to another spot . . ." (I.3, p 24). He thus clearly sees that the mid-point of support of the wire acts as a pivot for the turning motion of the needles on the sphere as they align in the pole to pole direction. He, like Peregrinus, was aware of all the advantages of using a spherical magnet. The resemblance in shape to that of the earth was certainly one of them but of course one which became important only after he had conceived of the hypothesis of the planet's magnetic nature. This hypothesis was most important for Gilbert and the continued use of the sphere for any experiments might have provided vital tests for it.

To summarize the considerations of the use of the sphere by Gilbert: he recognised some of its real advantages from its use by Peter whose results he wanted to test in any case. He further discovered the considerable benefit of the terrella's symmetry when he investigated the properties of the field, as we will see. Experiments with the sphere were also very advantageous because they provided tests for his theory of geomagnetism. But, as he says himself, elongated magnets are stronger, and so he uses them when he needs greater magnetic strength. He also tests magnetic properties with stones of non-spherical shape in order to check on the generality of his findings.

To gain even more freedom of movement for his probes, Gilbert repeats Peter's experiments with an iron or steel needle supported on a proper pivot-stand, his "versorium", which is placed on the sphere or very close to it. This form of experimenting clearly represents a considerable advance on Peregrinus' and is indispensable for an investigation of the effects of the magnetic force near the stone rather than on its actual surface. It was Peregrinus who invented a new form of pivoted compass needle, yet he used it only as a compass, whilst now it is employed as a scientific instrument. If Peter did employ the compass in his investigation of the properties of the spherical loadstone systematically, he does not report it; though he will surely have brought it near the loadstone on many occasions to observe its deflection. Unlike the versorium, which has the needle resting on the pivot point, Peter's compass was fixed to a vertical axle whose ends were pivoted in the casing; so it was not a suitable instrument for observation of both directional components of the force on or near the spherical stone, although very useful for navigation.

The important experimental advance the versorium represents lies in the fact that it does not touch the loadstone as Peregrinus' needles had done. It can be placed anywhere in the space near the stone and will show the direction of attraction. Lengths of iron wire or needles could not do this for they would be subject to translation to the loadstone.

With the introduction of the versorium the investigation of the magnetic conditions in the space around the magnet, and the forces there, could make progress. As long as the iron wire pieces showed their directionality only when touching the loadstone this could not happen. On contact, the iron may be seen as forming a magnetic whole with the loadstone itself and could therefore perhaps not indicate the condition in open space. Further effects would have to be observed before one would like to speak of a recognition of the characteristics of the field which Benjamin mentions. Discovery of the forces in the field would have to involve a knowledge of the directional alignments of the needle near the magnet. Peregrinus' observation of the alignment of the wires directly in contact with the magnet, and perhaps some unsystematic observation of deflections of a magnetic needle near it, cannot yield this.

Gilbert, on the other hand, would perhaps have been able to make a rough map of field lines in the regions around the loadstone, by making drawings of his versorium's directions at various positions there. But the idea did not occur to him, and he did not possess the concept of field lines. However, he placed his versorium in different and systematically-chosen positions around magnets and noted the directions it adopted. There are numerous drawings in various chapters of the book showing the alignments which were to become particularly important in comparisons of the positions on concentric circles in the space around the stone. The various explorations of the needle's directions around the sphere find their important employment in the investigations of the field which I will consider below.

C. Terrellae and other magnets.

Here I will only mention a case of the versorium's use near a non-spherical magnet which helps to generalise the findings at the terrella. Near a round bar magnet, the alignments of a versorium which is moved gradually along from the equatorial to the end positions, show the directions he draws in 23 places (fig. 3 from IV,7, p. 247). His investigation of the alignments along the bar is of interest in displaying directions similar to those at the terrella in important respects. It shows that the middle of the bar is magnetically equivalent to the terrella's equator. The versorium's direction changes from a position parallel to the bar's surface here through increasing angles to a vertical position at the end, in obvious similarity to the phenomena at the sphere. (He leaves out, however, the more complicated alignments with their rapid changes of direction near the ends of the long sides of the bar.) The phenomena in both cases are largely similar although the bar's sides remain parallel to the polar axis when the sphere's surface does not. There exists therefore a partial equivalence of the change of the "verticity" (Gilbert's general term referring to "directive force" or "turning power") near the surface of sphere and bar. Even where there are sudden small changes of the curvature locally, as when he furnishes the terrella with little hemispheric humps, Gilbert found the angling of the iron wire pieces to its main body to be hardly affected - fig 4, after that in III. 7, p. 200. (This unusual experiment concerning small local irregularities must have been the test of some working hypothesis whose exact character we can only guess at.)

All this indicates the existence of an aspect of the magnetic property which he will have seen as being independent of the geometry of the body. In modern terms, Gilbert has discovered that field lines of systematically differing angles originate on the parts of the surface of the bar magnet - as on that of the terrella - not only in the wider circumpolar region but all along the magnet's body with the exception of the equatorial line. Gilbert himself does not draw attention to this parallel in the directional changes of the versorium near terrella and bar. He will have thought that the effect was only to be expected. But it had to be shown experimentally and could by no means just have been assumed. He draws bits of wire or versoriums angled against magnets of various shapes (oblong, p. 154, egg-shaped p. 197, hemispheres, see below). The observations are clearly very important in forming his understanding of magnetism.

He also uses the sphere to investigate the contribution of the parts of the stone to its total magnetic strength. This is connected with proposition (iii) above ('the whole stone contributes to making up the poles' power and the nearer we get to the poles, the stronger is the magnetic virtue in the parts of the stone'). He finds that the parts near the poles are not in themselves magnetically stronger but are only made so by virtue of their position *while* they occupy this. So if segments of the terrella, between any two parallels near the pole, or even the polar cap are cut off and then tested, they are found to have no more magnetic strength than any other part of similar size of the same terrella. The magnetic power of a piece of the stone will vary with its mass, and also its shape, but it will not depend on its original position in the stone. "For no part has any supereminent value in the whole" (II.5, p. 119). Similarly, the stone will lose magnetic strength in proportion to the size of the removed part.

Clearly, for some of these experiments non-globular stones could be used. And various experiments he does with loadstones of other shapes show the same result. The importance of his statements lies in the fact that the polar area is now characterised as a focus of forces originating throughout the whole loadstone. It is a priviledged place but does not itself consist of more potent material when compared to that of the rest of the stone. Gilbert expresses this by saying that "the regions nearest the poles are the stronger, those remotest are the weaker; yet in all the energy is in some sense equal" (II.5, p. 115). Porta had said that even the smallest magnets have poles. He understood that the powers of parts of a magnet, when brought together, will unite to restore the full degree of magnetic virtue (cf. op. cit., book VII, ch. 10). Porta had also found that by cutting a magnet into three sections across the magnetic axis he could show that the middle piece, which had only weak magnetic powers in the integral stone (because it formed the equatorial region), becomes a proper magnet when cut out. From these useful experiments he drew, however, no theoretical conclusions.

Gilbert, on the other hand, speculates on how the polar region may be related to the other integral parts of the stone by developing detailed ideas in II.5, entitled "In what manner the energy inheres in the loadstone". From any point H, A, G, O of the terrella's equatorial plane the energy reaches only those of the points B, C, F, N of the sphere which lie nearer the polar half axis E-M, than itself (fig. 5, from the drawing in II,5, p. 120). The direction in which the energy adds up is shown in the figure by the lines connecting the respective points on the equatorial plane to those on the terrella's circumference. This demonstrates that the areas near the pole receive more energy than those further away from it, and the pole itself benefits from the energy of all the points of the equator (and by implication from that of all points in the hemisphere). All this provides a much deeper and more detailed description of the character of the pole's origin than Porta's statements. Gilbert is aware of the novelty of his exposition and sets an asterisk against the sentence

"by the confluence of the forces from the equinoctial plane toward the pole the energy increases poleward and absolute verticity is seen at the pole as long as the loadstone remains whole". (II.,5, p. 116)

This hypothesis is the result of his proof of the intrinsic equivalence of the degree of magnetic strength of different parts of the stone. (The relevant experiments may well have been the tests of an earlier working hypothesis to the effect that the polar materials would remain stronger even after removal from the stone.) There seemed to be no other explanation for the strength of the pole than this 'confluence of the forces'. But before he could be quite sure of the situation, he also needed to understand what happened to the positions of the poles in the parts of a cut terrella.

If the terrella is cut along the polar axis, each half will have a new pole not at the ends of the line of separation but further along the hemisphere's circumference (so that the distance from pole to pole in the hemisphere is now shorter than that in the original sphere). This leaves a large spherical cap on one side of the new polar axis and a segment on the other so that there is again magnetic material on both its sides (fig.6, after Gilbert's on p.117). The parts of this material contribute their magnetic strength in the formation of the new pole. Porta had in fact observed that if a loadstone was split along the polar axis this would move sideways into the middle of the new stones; this splitting of the pieces could be continued with similar results. On re-uniting the stones he observed the axis to be again in the original position. His comment on these very useful experiments, however, is not some attempt at a theoretical explanation as Gilbert offered but a touching "who will believe it unless he tries it?" (Porta VII, 5.). Again, Porta's various experiments of dividing stones to check on the polarity of the pieces almost certainly first suggested similar ones to his successor. Gilbert saw that the movement of the poles sideways from their original position in the complete sphere cannot be *explained* by Porta's other relevant discovery, viz. that smaller magnets combine to form a larger one. But if Gilbert's 'confluence from all sides' is necessary to form the poles then they could not remain at the edge of the hemisphere. (He also investigates the movement of the poles when the terrella is quartered; fig. 6 shows the new poles, F, G, H and I.) .

Gilbert relates the poles as foci of magnetic strength in a systematic way to the distribution of the magnetic material which gives rise to them. This relationship of the poles to the rest of the magnetic body is another example of the importance of the *formal, ordered,* property inherent in all magnetic materials, which Gilbert stresses throughout his book. Although the matter of the various parts of the loadstone is in itself all of the same strength, by its arrangement in one body, new effects of magnetic power and polarity emerge. Mass and shape determine the magnetic properties of any loadstone in its internal force-distribution and its effects on magnetics externally.

When Gilbert uses the small pieces of iron wire standing on the surface in the detailed investigation of the magnetic properties of various parts of the terrella, he perceives the steepness of their inclination to the sphere's surface as proportional to the attractive strength at the respective place. But although on the sphere there thus exists a proportionality between attractive strength and the size of the acute angle between needle and surface in each position, he stresses that a non-polar area may be strong enough to lift two ounces of iron, yet will not be able to make a small bit of iron stand erect (III.7, p. 201.). Attractive strength is different from, yet also proportional to, verticity; for test bodies stand up in a more nearly vertical position in the parts of stronger attraction. (The 'turning power' of verticity is, in more modern terms, indicated by the angles of the field lines with the surface.)

Gilbert also relates the length of the chord drawn in a great circle (of a sectional drawing) of the terrella in the direction of the test piece's slant to the magnetic strength at its point of contact (fig. 7, from the drawing on p.130, II.14). It is seen that the chord is coincident with a diameter only at the pole and is therefore there at maximum length. The chords then get ever shorter the nearer we get to the equator where the tangential direction of the needle does not allow us to draw a chord at all. The attraction at the equator is therefore null, and of the magnetic effects only the directional force remains, which aligns the needle north to south. (The equatorial region cannot be used to magnetise a piece of iron either, as Gilbert points out in III.11, p. 210.) The relationship between length of chord and magnetic strength is one of proportionality of some sort:

"For as a very small chord of a circle differs from the diameter, by so much do differ the attractional powers of the different parts of the terrella." (II.14, p. 130)

Gilbert would have had problems with an exact measurement of the amount of attractive power to establish the form of proportionality. He had no means of quantifying attraction accurately, although he often measured it roughly.

He does not mention that the bar magnet would show no consistent relationship between chord length and attractive strength but repeats the details of his views on the confluence of forces at the pole in the terrella and compares the overall strength of bar magnets:

". . . not that the pole holds this eminence in its own right, but because it is the depository of forces contributed by all the other parts; it is like soldiers bringing reinforcement to their commander. Hence a rather oblong loadstone attracts better than a spherical one, if its length stretch from pole to pole, and yet the two may be from the same mine, and be of equal size and volume. The way is longer from one pole to the other in the oblong stone, and the forces supplied by the other parts are not so scattered as in the spherical loadstone and the terrella; they are better massed and united, and thus united they are stronger and greater. But a flat or oblong loadstone is much less effective when the length is in the direction of the parallels, and the pole ends neither in a point nor in a circle or sphere, but lies flat on a plane surface so as to be held for something abject and of no account, for its unfit and unadaptable form." (II.,14, p.131)

He uses the terrella for many other experiments. Those described in III. 9 are particularly interesting as some of them concern assumptions about his magnet's interior. Throughout his work he proceeds quite naturally from observations of effects outside the magnet to conclusions about the properties inside it. The postulation of a confluence of the forces of the parts of the stone toward the poles followed the observation of the magnetic strength on the surface of magnets. Now he describes how one hemisphere of a terrella cut into two halves at the equator shows two poles: the original pole and another one in the centre of the equatorial plane which causes five versoriums to arrange themselves in a fanlike manner underneath it (fig. 8, from III. 9, p. 207). But if, after cutting, the two hemispheres of the terrella are separated by a gap a little wider than the length of a versorium, the needles placed at regular intervals in the gap will all remain parallel and at right angles to the two discs of the equatorial cut. The two hemispheres between them therefore still have only two poles, ". . . the needles are parallel, the poles or the verticity at both ends controlling them" (p. 207, see fig. 9). He had already said (p. 206/7):

"The points [of the needles] are directed in the centre of the earth and between the two halves of the terrella, divided in the plane of the equator as shown in the diagram. The case would be the same if the division were made through the plane of a tropic and the division and separation of the two parts were as above, with the division and separation of the loadstone through the plane of the equinoctial"(ibid.).

One notes particularly the extrapolation to the interior of the earth: the magnetic field in the gap between the two hemispheres which can be investigated is equated in its character with that of the field inside the solid body of the earth which he cannot observe.

Chapter II

Magnets, Iron and the character of Magnetism.

A. Magnetisation of iron by the earth.

I started the description of Gilbert's work with an examination of the properties of the terrella because his first systematic experiments may have been tests of Peregrinus' hypotheses concerning magnets of this shape. The characterisation of the behaviour of the terrella led on to other features of magnetism which are not confined to spherical stones only. However, Gilbert also investigated the more general properties of magnetics in which many of his ideas concerning the terrella have their presuppositions.

He thus examined in considerable detail the various types of loadstone found, and also described iron and steel manufacture and the processes of smelting at length. Most aspects of these matters need not concern us and it is sufficient to state that he knew of the different degrees of magnetic strength possessed by different types of magnetite and the varying degrees of magnetisability of different kinds of iron and steel. However, the effects of heating or melting of loadstone and iron which he deals with are important, and throw light on the character of magnetism in general. (The details he gives show Gilbert's familiarity with iron smelting and working). Rich iron ore, when floated on a raft in a bowl on water, is usually, if weakly, attracted by a like piece of ore brought near it. Stony brown ores do not attract one another and even the loadstone attracts them only after they have been heated and cooled again. Rich iron ore even directs itself north-south in a vessel on water by its native magnetism (I.9, p. 46-47). Wrought iron also is magnetic without having been near a loadstone. This can be shown with a piece of iron wire, floating in a cork, which will be attracted by another piece of similar wire, or with an iron bar hanging on a thread which will be attracted by another piece of iron (p. 47-8). A long piece of iron suspended on silk thread ("twisted differently and not all in one direction") will align itself in a north-south direction (I.12, p. 50). In fact all iron implements behave like very weak magnets, particularly those of some length (p. 51-2). The five short chapters containing these statements on pages 46 to 52 are each marked with an asterisk to show their originality. I have mentioned them here because they are of importance in connection with Gilbert's discovery of the earth's magnetic field. He knew that the process of smelting or melting destroys the magnetism of iron ore or iron. Wrought iron must therefore have acquired its magnetic properties after its manufacture. He knew that this occurs if it is kept in a constant orientation for some time. This fact had already been remarked on by Giulion Cesare in 1586 who noticed that a bent iron bar which had remained in a north-south direction for ten years on the church of San Agostino at Rimini had become magnetic. Gilbert refers to this case and takes it as confirmation of his own discoveries (III.12, p. 214).

The magnetisation of iron without the help of a loadstone can be speeded up in the way described in III.12. Newly produced wrought iron is used:

"Let the smith stand facing the north with back to the south, so that as he hammers the red-hot iron it may have a motion of extension northward, . . . have him keep the same point of the iron looking north, and lay the bar aside in the same direction." (p. 211, asterisk)

This iron will be found to be magnetised.

"Nevertheless, when the iron is directed and stretched rather to a point east or west, it takes almost no verticity, or a very faint verticity. This verticity is acquired chiefly through the lengthening." (p.212-13, asterisk)

If the process is repeated with the iron now facing the other way, polarity will be reversed. The iron will acquire verticity

"not only when it cools lying in the plane of the horizon, but also at any inclination thereto, even almost up to the perpendicular to the centre of the earth" (III 12,p.214).

This is an important discovery showing the connection between the dip of the needle and magnetisation of the rod by the earth, matters I will take up below. I have mentioned them here because of the contribution they make to the general characterisation of iron as a magnetic substance, one which is easily magnetisable by the earth to at least a slight degree. Gilbert saw that completely unmagnetised iron is in fact hard to find but that if it is found, magnetism is induced before long in any case by the earth.

The identification of its polarity proceeds via the effects of the earth on the magnet. Whilst Peregrinus had named the end 'north' which pointed northwards, Gilbert sees that this would be misleading. As it is the pull of the earth's north pole on one end of the magnet (and that of the south pole on the other), we must name this the south pole. The naming of the north-pointing end of the needle as the north pole repeatedly caused confusion in the history of magnetism. Gilbert is clear and consistent in his designation and use: it is the south pole of the magnet that points north. He is of course aware of the importance of his correct and consistent naming of the poles:

". . . all who have written hitherto about the poles of the loadstone, all instrument-makers, and navigatores, are egregiously mistaken in taking for the north pole of the loadstone the part of the stone that inclines to the north, and for the south pole the part that looks to the south." (asterisk; I.4, p. 27)

To name the earth's magnetic pole at geographic north as the south pole would have seemed nonsensical to Gilbert who thought geographic and magnetic poles coincided exactly.

The full appreciation of induction in iron by the earth provides the needed parallel to magnetic induction in iron by loadstones, displaying the equivalence of the effect of a loadstone's field with that of the earth. The importance of these investigations in connection with the discovery of terrestrial magnetism is obvious. There now even existed a way of manufacturing a magnet, if a very weak one, with a polarity which could be predetermined without the use of other loadstones simply by the decision to extrude and cool a piece of iron in the geographical north-south direction, a possibility of revolutionary theoretical if not practical implications, though Gilbert himself does not mention it in these terms.

B. Magnetisation of iron, magnetic keepers.

Gilbert was the first researcher to investigate the magnetisation of iron and steel in detail. Among his many advances is the clarification of the difference between permanent and impermanent magnets and of details of the process of magnetic induction. He also discovered magnetic conduction and disproved Porta's claim that only that part of an iron object would become magnetised which was in the magnet's *orbis virtutis* (this term for the region of the discernable field is due to Porta). Porta had said that a loadstone with an *orbis virtutis* of two feet could not magnetise a three feet long iron bar; only the two feet which extended into the stone's orbis would become magnetic (but if the bar was rubbed in its middle, both end would receive the virtue; Porta, op. cit. VII, chs. 43-4).

Gilbert discovered the effects of arming the magnet with steel caps with the resulting increase of its lifting power. I will consider the details of this below. The idea that the magnet would be strengthened by the proximity of iron-filings had been suggested by Paracelsus (1949, vol. VIII, p. 281: 3: *"Vom Konservieren der natuerlichen Dinge: Fuer Magneten gibt es nichts Besseres als Eisenfeilspaene die die Kraft erhoehen."* - "On conserving natural bodies: for magnets there exists nothing better than iron filings which increase the strength"). Gilbert does not credit Paracelsus with this useful observation. But the latter had claimed elsewhere that by heating and immersion in oil of vitriol

"the loadstone's force and energy may be increased and transformed to tenfold what it is naturally" (Gilbert's paraphrase of Paracelsus' remarks, II.12, p. 146),

and that

"in this way you can give to a loadstone such strength that it will pull a nail out of a wall, and perform many other like marvels impossible for a common loadstone".

Gilbert chides him for charlatanry and states that

"Magnetic bodies can restore soundness (when not totally lost) to magnetic bodies, and can give to some of them powers greater than they had originally; but to those that are by their nature in the highest degree perfect, it is not possible to give further strength" (ibid.).

Even the permanent magnet, the loadstone,

"loses some part of its attractive power, and as it were, enters into the decline of old age, if it be too long exposed in open air and not kept in a case, with a covering of iron filings or iron scales: hence it must be packed in such material" (I.6, p. 32). "But a loadstone is kept in iron filings not as though it fed on iron, or as though it were a living thing needing victual, as Cardan philosophises". (I.16, p. 61)

(Porta had already disputed Cardan's claim and buried loadstones in iron filings to weigh both later so as to check if any iron had been consumed; he had found no such effect.) For Gilbert the preserving faculty of the iron is due to the fact that the loadstone awakens the magnetic form of the iron which in turn assists the formal power of the stone. (The iron acts as keepers reducing the self-demagnetising effect, something Gilbert knew nothing about.)

C. The telluric element; iron ores.

Gilbert investigated in detail the various forms of iron ore and magnetite and suggested that they derived from what he calls the "common magnetic telluric element". This was the one element with which Gilbert, obviously after his discovery of the earth's magnetism, replaced the Aristotelean four. It was the original magnetic substance the earth had entirely consisted of, and from which all other terrestrial matter evolved, near the planet's surface, by corruption by various natural effects, such as the heat and light of the sun. The inner regions of the earth still consisted of the original element. (His exposition of all this can be found in book I, *de Mundo.*) Although he had no chemical knowledge about oxides, he knew that magnetite ". . . is nothing but a noble iron ore" (I.7, p. 37). The proper realisation of this fact was vital for the understanding of the connection between the stone and iron. The relevant relationship between the two could be at least partially clarified only by, on the one hand, an investigation of the magnetic properties of iron ore, as carried out by Gilbert systematically and in detail, and the knowledge that iron could be smelted from the loadstone on the other. His appreciation of these problems and of that of attraction in general may be compared with Porta's, who thought that in the loadstone iron and stone are mixed and that the iron desires the "force and company of iron" so "that it may not be subdued by the stone" and believed that "the loadstone draws not stones because it wants them not" (Porta, op. cit., VII, ch. 2, p. 191-2).

The very wide distribution of iron ores, the finds of pure natural iron, the fact that magnetite was a form of ore, and the possibility of so to speak partially reversing the process of natural decay of the original telluric element (by magnetising the products of smelting of ore) provided Gilbert with additional evidence for the reality of this element. (The idea that "the loadstone is true earth" in Cardan's opinion is mentioned by Gilbert on p. 70. But as Cardan did not think of the earth itself as a magnet it is not clear what he may have meant.)

As we have seen, Gilbert discovered that much iron ore not describable as loadstone was nevertheless magnetised to some degree. On the other hand iron ore is found in a form that is almost pure iron (this was due to the "coalescing" of "homogenic portions of the earth's substance to form a metallic vein", I.17, p. 69). And all iron is magnetised by the earth. Loadstone and iron were therefore clearly cognate materials, and what had theretofore appeared strange seemed to Gilbert quite natural, viz. that a stone could attract iron. ("The loadstone is by nature ferruginous, and iron magnetic, and the two are one in species", I.16, p. 63).

Iron was the impermanent magnetic substance but had the flexibility of being magnetisable again and again, even after heating or melting, whilst loadstone on heating would often burn and afterwards no longer be magnetic or magnetisable. But even iron hangs on quite strongly to its magnetised state which can only be completely destroyed by considerable heat (III.3., p. 189 ff). The magnetic condition of loadstone is not absolutely permanent either as we have already seen, yet vastly more so than that of iron. The magnetisation of iron has two main limits. Firstly a very long bar of iron will not become magnetised to its far end, or only very weakly so, if the magnet touches the near end. Gilbert likens this to the process of heating one end of a long bar which leaves the other still cool. The second limit concerns the fact that a piece of iron in a loadstone's field will be instantaneously magnetised as strongly as the stone's power allows while it is near it, but on removal from its influence the iron's magnetism will be much reduced, especially in the end which was not near the stone (III.3, p. 191). He says elsewhere that the degree of the initial, and the strength of the permanent, magnetisation depend much on the purity of the iron or steel.

D. The character of magnetic action.

If we ignore the postulation of the telluric element, Gilbert's analysis of the important relationships of ordinary iron ores, loadstone, and iron, is found to be substantially correct, particularly as he understood the fact that the difference between the magnetic and the unmagnetised state of any of these substances was a 'formal' one. Hitherto it had been quite uncertain whether perhaps a chemical change of any sort was taking place on magnetisation, or what else might be happening.

Gilbert saw that no effluvium was given off by loadstones or iron (as Lucretius had claimed). Some of the reasons which probably influenced the idea that he had to be dealing with a process of formal arrangement and not, for example, an exchange of material, may have been the speed of magnetisation of iron by loadstones, the difficult conception of a material magnetic effluvium which was directional in its action with instant reversal in appropriate circumstances, and the behaviour of magnets after being cut in various ways. There was also the fact that magnetic attraction is effective through a flame interposed between the magnetics. The flame would surely have carried away or burned Lucretius' atoms if such were given off by the magnet, as Gilbert says. Although these facts must have helped to form his views on the matter, he does not mention them - except for the last one - specifically as arguments for the formal character of the state of magnetisation. But he does say that the amount of any material magnetic effluvium would be gradually dissipated if a loadstone was used to magnetise many pieces of iron. The main reason why a material effluvium could not be involved was, however, the observation that the magnetic force travels through even the most solid interposed bodies. This phenomenon had already been remarked upon by St. Augustine and Cardan. Porta was also aware of it yet apparently still thought that magnetism was connected with a vapour (which escaped from the stone when this was heated; cf. Porta VII, ch. 51) and hairlike structures (he says in ch. 36 that to magnetise iron the stone's pole should be hit with a hammer when hairs would appear which then got passed onto the rubbed iron). Norman had already proved that no ponderable material was involved in magnetisation, or at least not one which could be discovered (see below for the details of Norman's work on this). Given all these facts, it seemed impossible to Gilbert that a material effluvium was given off my magnets. (His argument that an effluvium would soon be dissipated seems perhaps less cogent, for he himself mentions that smells can last for centuries with no perceptible reduction in the quantity of the olfactory materials and in any case, as he knew, the power of the stone does not necessarily last for ever.)

So he was initially faced with a major difficulty in characterising the magnetic property. One of the traditional ways of dealing with it would have been the assumption of a sympathy acting:

"Others have thought that the cause is a sympathy. But even were fellow-feelings there, even so, fellow-feeling is not a cause; for no passion can rightly be said to be an efficient cause". (II.3, p. 103)

He also dismisses likeness of substance on the same page. In what is one of his most important theoretical innovations he makes the assumption that magnetism is due to a *formal energy* which extends its influence in the *orbis virtutis* around the stone:

"Magnetic bodies [attract] by formal efficiencies or rather by primary native strength" (II.4.,p.105); "This form is unique and peculiar: it is not what the Peripatetics call *causa formalis* . . ." (ibid.)

He contrasts his 'formal efficiency' as the cause of magnetic phenomena with Aristotle's four causes, none of which fitted the bill. Magnetism acted by the direct efficiency of the form which possesses, or represents, energy. He characterises the action of magnetism further and speaks of ". . . the force that is diffused through the air . . ." (I.7, p. 123). "The rays of magnetic force are dispersed in a circle in all directions" (II.27, p. 150). Thus here, as elsewhere, he speaks of a kind of forcefield that surrounds the magnet and asks in II.4, p. 106, "is any magnetic effluvium emitted, corporal or incorporal?" He answers himself on the next page: "Therefore the magnetic forces have no such conception, no such origin as this."

In spite of Gilbert's clear statements throughout the book, many modern commentators thoroughly misunderstand his conception of the mechanism of magnetism. A. C. Crombie (1952) writes:

"His explanation [of magnetism] was really an adaptation of Averroes' theory of 'magnetic species' in a setting of Neoplatonic animism. Beginning with the principle that a body could not act where it was not, he asserted that if there were action at a distance there must be an 'effluvium' carrying it. He assumed such an effluvium to surround the loadstone and to be released from electrified bodies by the warmth of friction." (p. 321)

This is doubly falsifying Gilbert. Firstly by claiming that the loadstone was surrounded by an effluvium which Gilbert specifically denies, and secondly by equating this magnetic effluvium with the electric effluvium from which apparently it differed only by the manner of its release. Nor can I find anywhere in Gilbert's books any reference, explicit or by implication, to the principle that a body cannot act where it is not or that for action at a distance an effluvium would be needed. In his statements on magnetism and cosmology Gilbert propounds the opposite view.

H. Butterfield (1980, p. 141-2) is similarly mistaken in his interpretation:

". . . this [the magnetic] attraction was not regarded as representing a force which could operate at a distance or across a vacuum - it was produced by a subtle exhalation or effluvium, said Gilbert".

(Butterfield relates this to Gilbert's view on gravity and to the mutual influence between earth and moon, apparently thinking that Gilbert held the view that the effluvium must fill the space between these two bodies.) E. A. Burtt (1980, p. 165) also speaks of Gilbert's "magnetic effluvium emitted by the loadstone" which "he supposes to reach around the attracted body as a clasping arm and draw it to itself", saying that Gilbert meant it to be "extremely thin like a rare atmosphere". Even Benjamin, who has made a more thorough study of Gilbert's work, says with respect to the magnetic orb

"These orbs or spheres which Gilbert speaks of as 'effused', and as produced directly from the earth's exhalations, are magnetic because so generated . . ." (op. cit. p. 272).

But, as we will see, the earth's exhalations form the air, and have nothing to do with magnetism. These authors' deeply mistaken views on the facts of what Gilbert says about magnetism falsify important aspects of the historical development of concepts such as force and field and the understanding of aspects of the history of cosmology.

To return to what we actually find in Gilbert's text, he describes the workings of the magnetic action in a necessarily circumlocutory manner on p. 109, II.4.:

"one loadstone gives portion to another loadstone by its primary form. And a loadstone recalls the cognate substance, iron, to formate energy and gives it position . . . the forces of both harmoniously working to bring them together".

He is quite taken with the idea that the magnetic power should be likened to a soul:

"Thales ascribed to the loadstone a soul, for it is incited, directed, and moved in a circle by a force that is entire in the whole and entire in each part, as later will appear, and because it seems most nearly to resemble a soul. For the power of self-movement seems to betoken a soul, and the supernal bodies, which we call celestial, as it were divine, are by some regarded as animated because that they move with wondrous regularity". (II.4, p. 109/10)

Interestingly, not too many years later Descartes was to argue that self-movement was impossible because for this to occur matter would have to be animated. This was a standard Cartesian argument for some time against Newtonian action at a distance, employed by Descartes himself already against Roberval's theory of universal attraction between bodies in a letter in 1646 (cf. P. Duhem, 1977, p. 15, giving the source, R. Descartes, 1893, vol. IV, Letter CLXXX, p. 396). This of course still relied on an animist premiss (the assumption that 'self-movement betokens a soul' was after all accepted); but this time it was used with reverse force. An advance as compared to Gilbert's position must be seen in the fact that matter was separated from *anima* but this separation, because it rested on a similar premiss, was of little specific value in this case. I will suggest below that Gilbert's souls and Kepler's anima were scientifically harmless and that the postulation of particular animated forces simply to account for certain forms of motion did not hinder developments at that stage of physics. The idea of forces, such as Gilbert's magnetic force, able to act across empty space, was rather vital to the progress of science.

To return to the more mundane details of the characteristics of the magnetic form in the stone and in iron, which show themselves, for example, in the fact that a very hot iron bar is not attracted by a magnet: Gilbert says that "iron made white-hot by fire has a confused, disordered form, and therefore is not attracted by a loadstone, and even loses its power of attracting, however acquired". (II.4., p. 108)

He mentions that Cardan was not injudicious when he had remarked that "red-hot iron is not iron, but something lying outside its own nature, until it returns to itself". Here (p. 108-9) Gilbert disposes of Porta's opinions that the vapour given off by roasted magnetic ore is the cause of the attraction of the iron; and of Fracastorio's that iron could not be altered by the loadstone. With respect to Fracastorio he writes

"'for', says he, 'if it were altered by the loadstone's form, the form of the iron would be spoiled', yet this alteration is not generation, but restitution and re-formation of a confused form".(p. 109)

Although the loadstone is the permanent magnet, it is ore in respect of iron and steel which can be gained from it by the removal of its impurities:

". . . because of the purging of the ore and its change into a purer body, the loadstone gives to iron greater power of attracting than exists in itself".(II.4, p. 112)

This is an important discovery and the next sentence is marked with an asterisk:

"For if you put some iron-filings or a nail on a large magnet, a piece of iron joined to the magnet steals the filings and the nail, and holds them as long as it remains alongside the magnet; so, too, iron attracts iron more powerfully than does a loadstone, if the iron be afformed, and remain within the sphere of the form given out to it. Again, a piece of iron nicely adjusted to the pole of a loadstone holds a greater weight than the loadstone does." (ibid.)

(Porta had already prepared the ground for these findings when he observed that iron is more strongly attracted by the stone than is another stone and that a stone will let another stone fall and lay hold of a piece of iron brought near - Porta, VII, 21.)

The fact that iron will remove other bits of iron from the stone is also described in more detail in II.32, p. 159:

"If a small iron bar be set erect on the pole of a loadstone, another bar-iron pin in touch with its upper end becomes firmly attached thereto, and if it be moved away pulls the standing bar from the terrella."

Because

". . . iron is liker to iron than is loadstone, and in two pieces of iron within the field of a loadstone, the neighness of the latter enhances the power of both: then, their forces being equal, likeness of substance becomes decisive, and iron gives itself up to iron, and the two pieces are united by their most like (identical) and homogeneous forces. This is effected not only by coition [Gilbert's term for translational magnetic attraction], but by a firmer union . . ." (II.26, p. 149)

The two pieces of iron were subject to a strong tie because of the greater degree of magnetisation of iron. As he had said that iron receives greater power of attraction than the loadstone itself possesses, it may be this to which the effects of 'likeness of substances' here refers.

Even a smaller terrella or loadstone, B, takes a standing iron bar, C, of a larger terrella, A, A and B originating from the same mine (fig. 10, after that in II.32, p. 159). He explains:

"here the iron bar C coalesces with the terrella A and thus its force is enhanced and awakened magnetically both in the end in conjunction and also in the distal end by reason of its contact with the terella A; the distal end furthermore receives energy from the loadstone B and the pole D of this magnet also gains force by reason of its favourable position and the nearness of the pole E of the terrella. Hence many causes cooperate to make the bar C, attached to the loadstone B, cling more strongly to that than to the terrella A. The energy called forth in the loadstone B, and

B's native energy, all concur; therefore D is magnetically bound more strongly to C than E to C." (p. 159-60).

(He had already found that any magnetic pole receives extra strength from the proximity of a strong opposite pole belonging to another magnet and that, on the other hand, proximity of a like pole weakened the attractive power of a magnet's pole (II.25, p. 147)).

His treatment of these facts displays as good an understanding of the difference between the magnetisation of loadstones and of iron and of the role of field density as could be expected at the time. As so often in his work the thoroughness of experimentation and observation enable him to put more complicated matters in a form that necessarily contains somewhat vague conceptions, yet appears incomplete rather than mistaken in at least the vital respects.

E. The armed stone; magnetic conduction.

The phenomena Gilbert saw as possibly arising from the likeness of substance of iron test pieces are further investigated by experiments concerning his important discovery, the armed loadstone. For these he uses oblong stones fitted with helmets of thin concave hemispheres of iron on the poles (fig. 11, from that in II.22, p. 141); on his illustrations three of presumably four wire fasteners - doubtless made of iron - are visible, which connect the two polar caps along the body of the stone. He gives figures for the great increase in weightlifting power of the armed stone (this is trebled from 4 to 12 ounces) and makes various experiments, some of which are a little surprising but also ingenious:

"On a plane surface lay a cylinder too heavy for the unarmed loadstone to lift; then, with paper between, apply at the middle of the cylinder the pole of an armed loadstone: if the cylinder is pulled by the loadstone, it follows after it with a rolling motion; but when there is no paper between, the cylinder, joined to the loadstone, is pulled by it, and does not roll at all. But if the same loadstone be unarmed, it pulls the rolling cylinder with the same velocity as does an armed loadstone with paper between or wrapped in paper." (asterisk, II.22, p. 140/1)

So it is shown that the interposition of a piece of paper which only just prevents the vital direct contact nullifies the special effect of the iron cap on other iron objects.

The implications of direct contact and likeness of materials are further elaborated upon:

". . . iron situate near a loadstone takes away from it pieces of iron of suitable weight, provided only it be in contact with them; else however near they may be, it does not match them. For masses of magnetic iron do not, within the field of a loadstone or near a loadstone, attract more strongly than the loadstone attracts any iron; but once they are in contact with each other, they unite more strongly, and become as it were clamped together, though with the same forces at work the substance remains the same." (II.18, p. 138)

The remarks qualify those from p. 112, quoted above, to the effect that loadstone gives to the purer 'iron greater power of attracting than exists in itself'. But although an iron rod near a loadstone cannot remove a little iron bar from it except on contact, Gilbert had found that the small bar standing on the terrella will follow and lean toward the rod brought near and moved along close to the stone's surface (cf. II.26, p. 148), thus showing the effects of the iron on the field.

Under the chapter-heading "That Unition is stronger with an armed loadstone; heavier weights are thus lifted; the coition is not stronger but commonly weaker" (II.19, p. 139) he investigates the properties of the armed stone further when he points out that "iron is drawn from the same distance, or rather from a greater distance, to the loadstone when the stone is without the iron helmet. This is to be tried with two pieces of iron of the same weight and form at equal distance, or with one and the same needle, tested first with the armed then with the unarmed stone, at equal distances." (asterisk)

The instruction to use two different pieces of iron must be designed to take account of the magnetisation of the pieces after the inevitable contact with the stone which would induce an undesirable degree of remanence if one piece was used twice. In the case of the needle, it seems from the prescription to use the armed stone first that we are here dealing with the demonstration of the same hypothesis: as the unarmed stone in fact attracts more at a given distance than one armed (this is the hypothesis to be tested), the weaker stone is to be used first lest the earlier use of the stronger increase the remanent magnetisation of the needle and thereby reduce the difference in the effects. The original working hypothesis will no doubt have been that the armed stone also attracts from a greater distance. Its falsification surely caused Gilbert some surprise, and he proceeded particularly carefully with the relevant demonstrations.

Gilbert explains the effects of the arming of the stone by two steel caps or helmets on a piece of iron as follows:

"When the armature has imbibed the magnetic energy by reason of the presence of the loadstone, and another piece of iron adjoining at the same time derives force from the presence of a loadstone, the two unite energetically." (II.17, p.137)

The armature is to fit closely and should have an even surface. It is designed to 'imbibe' as much of the stone's verticity as possible and this is assured by the tightness of the fit. As Gilbert had said that verticity passes through iron better than through air, a close contact would avoid gaps between stone and cap. He understood the increase in weightlifting power as a gathering of the magnet's energy with its direct conduction from loadstone into iron cap. This energy could then flow into the iron weight with which the cap was in contact. This would itself become strongly magnetic and the whole form a larger strongly cohering magnetic body by the 'unition' of cap and weight.

We can only speculate on his opinion as to the cause of the weakening of the power of attraction or rotational alignment due to the arming of the stone. He nowhere explains why the armed magnet should attract less strongly than one unarmed, a fact he should have found most puzzling. To understand it, he would have had to have taken full account of the effect of his iron fasteners which connected the caps. These gather some of the fieldlines which would otherwise pass through the air and be efficacious in attracting at a distance. This therefore involves a reduction of the energy available in the air space around the magnet. If he had taken more seriously the idea that iron conducts the magnetic force better than air, he might have appreciated the shortcircuiting where iron directly connects one pole with the other as it does in the case of the cap fasteners of the armed stone. It is clear that he did not see the situation in these terms, and hence had no explanation for the reduction in the field strength near the armed stone.

His failure to test the effect of shortcircuiting the poles of a magnet by means of simply laying a piece of iron on it which would connect the poles is perhaps surprising. It is particularly strange because he often remarks that adding a piece of iron to one (or to each) of the poles strengthens the power. It seems incredible that he should have omitted tests of the effect of laying a piece of iron on the *sides* of a bar magnet. It is perhaps understandable that he did not think initially of the effect the seemingly insignificant iron fasteners would have. But the observation of a reduction in attraction with the fastened caps should have made him investigate matters more deeply, for it must have been very puzzling. We have to conclude that his experimentation and reasoning in respect of the problems here touched on was perhaps not up to the usual standard of his work.

Further experiments are made to show the effects of joining iron to a stone by attaching a piece of iron to just one of its poles, when the opposite pole can carry a greater weight than before. This proves that "magnetic bodies in conjunction form one magnetic body; hence the mass increasing, the magnetic energy increases also" (asterisk, II.22, p. 141).

But Gilbert did not think the effect of the armature to be due to the resulting increase in total magnetic mass which would in any case have augmented the weightlifting power of the magnet. The arming had a particularly great effect on the increase in strength which could not be accounted for by the very small additional mass. The armour had to be of best steel and suitable closely fitting shape, rather than bulky, to show the great increase in power.

One of the important outcomes of these experiments is the above mentioned discovery that although the loadcarrying capacity of Gilbert's armed stone is greater than that of the unarmed, the magnetisation of the magnet is not increased by arming, so that it cannot magnetise another piece of iron to a greater degree:

"Take two pieces of iron, one magnetised with an armed and the other with an unarmed loadstone, and apply to one of them a weight of iron proportioned to its powers: the other will lift the same weight, and no more. Two needles also turn with the same velocity and constancy toward the poles of the earth, though one needle may have been touched by an armed magnet and the other by an unarmed." (II.18, p. 138)

Gilbert also discovered the difference between total magnetisation of magnetic masses and magnetic susceptibility, to use modern terms: when an iron rod is joined to a stone it is not regarded as just increasing the total magnetic mass but "the energy of the loadstone awakens verticity in the iron and passes in and through iron to a far greater distance than it extends through air" (II.33,p. 162). The rod is here obviously seen as a better conductor of magnetism than the air. He had explained the experiment:

"A loadstone that in the outermost verge of its field of force, at a distance of one foot, can hardly stir a rotating needle, will, when connected with a long iron rod, strongly attract and repel (accordingly as its different poles are presented) the needle at the distance of three feet, and this whether the loadstone is armed or unarmed. The iron rod should be of fitting quality, and of the thickness of the little finger." (p. 162, asterisk)

It is the verticity of the stone which causes the effect by getting to the needle through the iron more effectively than through the air. The description of the iron rod to be used points to its use as a conductor here to be employed instead of the air. For if it was just a matter of increasing the power of the stone to make it effective at the original edge of the field, he could have used any piece of iron large enough to increase the total magnetic mass, and Gilbert reports on many similar effects in the terms of increase of the magnetic body's bulk. Here, however, he homes in on the passing of the verticity through the rod.

He finds that "the force passes through a number of pieces of iron conjoined at their extremities, yet not so surely as through one continuous rod." (II.33, p. 162, asterisk). With respect to this phenomenon J.A. Ewing (1900, p. 285) wrote:

"The fact that a joint offers magnetic resistance seems to have been first noticed by J.J. Thomson and H.F. Newell, who found that when an iron bar was cut in two, and the pieces were put in contact, the susceptibility of the bar was considerably reduced."

Gilbert's discovery seems therefore to have been overlooked.

Chapter Three

Coition and Repulsion.

A. M. Hesse on Gilbert.

To Gilbert the fundamental magnetic phenomenon is that of attraction: of magnets to one another; and of iron and magnets. Gilbert uses the term 'attraction' in various places but corrects himself from time to time to use 'coition' instead, which he considers to be the proper description for the magnetic coming together of iron and magnet or of magnets. By contrast, 'real attraction' is an electric phenomenon. He criticises some of his predecessors for confusing the two:

"But all these, besides sharing the general misapprehension, are ignorant that the causes of the loadstone's movements are very different from those which give to amber its properties." (II.2., p. 75)

The misapprehension is a lumping together of attraction (postulated to be effective in various instances, for example in medicine - "at the bidding of Galen" - "through likeness of substance and kinship of juices") and the action of loadstones whose "coition commonly called attraction" is "an impulsion to magnetic union" (II.1., p. 73). Electric attraction is not a case of union. But "Epicurus holds that iron is drawn by the loadstone as straws by amber" (II.3, p. 98). Some of the writers Gilbert names, who have confused the two effects or given the wrong explanation of magnetic coition, are Thales, Plato, Plutarch, Cardan and Cornelius Gemma. Gilbert's mechanism of electric attraction will be described below.

Here I will consider his conception of magnetic attraction or coition as related to his views on repulsion; for one commentator, Mary Hesse, has homed in on the connection between the two phenomena in Gilbert's work to support her methodological views on the theory-ladenness of scientific observation reports. The questions surrounding Hesse's comments on Gilbert's views are worth investigating, for they offer important and interesting examples of the problems of an analysis of early qualitative work and the difficulties of its appreciation.

Hesse (1960) is critical of Gilbert because he did not see magnetic "attraction and repulsion as symmetrical" (p.8). Gilbert says (II.4., p. 110) "there is, properly speaking, no magnetic antipathy". Hesse, after quoting this, concludes on p. 9 of her paper that ". . he lacks the notion of repulsion". On p. 7 she had mentioned that "even in the case of magnets he reports that magnetic substances are more sluggishly repelled than they are attracted . . ".

Hesse wishes to show that Gilbert, although he was a careful experimenter, could not have given the bare descriptions of observations which are independent of any particular theory, and which would be required on an inductivist view of science. After referring to some of Gilbert's views about magnetism, she says on p.5:

"But, inductivists will argue, here is a clear case where the metaphysical froth can be blown away, leaving some undisputed facts. It was no doubt unfortunate that Gilbert chose to experiment with a magnet [i.e. a spherical one] whose shape lessened its potential power, but still he discovered how magnetic bodies behave, and when he contented himself with bare descriptions of what he saw, his results stand. In replying to this thesis I shall try to show that if it is true, then it is never possible for the experimenter himself to know which of his reports are in this sense 'bare descriptions' and which are theory-loaded, and that what counts for the historian as a 'bare description' is dependent on subsequent theories and therefore relative to the time at which the history is written. Thus the thesis contradicts itself, because one of its premisses is that 'bare descriptions' are invariant to change of theory . . . We have to show then, that what counts, for the inductivist, as a 'bare description', is always dependent upon later theories, and this will be done by showing that of various experimental reports which Gilbert would undoubtedly have regarded as careful descriptions, some turn out to be acceptable in the inductivist's sense, and some turn out in this sense to be distorted by discarded theories, and that the way in which these two classes of reports are now distinguished is dependent upon later theories, none of which could have been forseen by Gilbert. We shall consider Gilbert's distinction between coition and attraction, his statements about repulsion, and his distinction between rotation and coition."

To deal with her claims it will be necessary to examine whether Gilbert's relevant reports really are 'bare descriptions' or 'bare observations'. She does not offer a definition of these but Gilbert's statement concerning the relative strength of attraction and repulsion appears to be a 'phenomenal statement' as defined by her in an earlier paper (Hesse, 1958). She there describes a phenomenal statement as one "whose truth or falsity is known directly by observation, although it is not necessarily known indubitably", (p. 14) and says

"The phenomenal statements of science are almost always general in this way: they do not describe what happened on a particular occasion to a particular observer, but what always happens and will happen on sufficiently similar occasions to all normal observers. Such statements cannot be claimed to be incorrigible, but they are characterised by a high degree of *empirical* certainty, for the possibilty of mistakes is minimised by experimental techniques, and of invariance with respect to repeated tests and different observers."(p. 15, her italics)

I do not think Gilbert's comparison of the strengths of attraction and repulsion is necessarily the sort of bare observation statement Hesse's inductivist would have in mind as an example at all; for, as we will see, Gilbert's result represents a summary of the qualitative impressions of several different types of experiment. Although it is very important, we may put this question aside for the moment.

Presumably Hesse's claim of the theory-ladenness of Gilbert's statement on attraction and repulsion arises from the fact that she has no other explanation for its origin, perhaps because it seems so obviously erroneous. I will therefore examine below whether there are not perhaps in fact at least some good experimental reasons for Gilbert's view on the matter. For if there were, one would certainly not even need to consider that it might have been due to the influence of his theory that there was 'properly speaking no magnetic antipathy', i.e. that it be theory-laden in her sense. But we need to become clear about the implications of her claim first.

Hesse maintains that Gilbert's observation reports are theory-loaded with respect to the theory he propounded. She must therefore presuppose that he either completely changed the reports of his own observations when he formed his final theory to *make* them support it or that he already held this theory when he noted his observations down. I am not willing to entertain the former alternative because scientific work would be quite impossible if observations had no stability at all even to the experimenter himself. The latter alternative could apply only if the experiments providing these observations were tests of just the completed theory or of one which was equivalent to it in the relevant respects. Hesse ignores the lengthy process of the scientific work - reputedly eighteen years in his case - and deals only with Gilbert's completed theory. She overlooks the fact that he must have formed and tested various working hypotheses and that the observations he reports were tests of these as well. Now it may be claimed that in that case the observations could not have been bare either, but would have been made in each case in the light of just the respective working hypothesis. This, though, would not help her, but be purely speculative, because we do not know what the latter was; it may after all have been the negation of part of the final theory or have amounted to a completely different approach. (In any case an application of her claim to the case of the earlier hypothesis would be just another example of the point she wishes to prove.)

It is most likely that Gilbert developed the finished theory in the light of his knowledge of the outcome of the tests of several or many working hypotheses. Therefore, test results originally obtained from the latter must at least generally be seen as stable and must stand, such as they are, independently of the final theory although the latter must also pass them; otherwise Gilbert would have been completely at sea and could never have arrived at any useful theory at all. This stabiliy in the germane respects would have to obtain (taking due account of changes in auxiliary assumptions, *ceteris paribus* conditions, and so on which would take place during the work).

In our example the description of the observations in the final version of the theory is a re-formulation because it is a summary of several types of experiments; but we cannot assume that either Gilbert's original observation reports or this formulation are 'laden' with respect to the theory in Hesse's sense. If, then, an observation-based statement is incorrect we need to consider the experimental details and difficulties instead of assuming the error to be due to theory-ladenness.

B. Gilbert on repulsion and attraction.

Before turning to Gilbert's own claim concerning the greater sluggishness of repulsion let us see what Porta says in connection with the question of the relative strength of attraction and repulsion. In chapter 13 of book

VII (Porta, op. cit.), entitled "The attractive part is more violent than the part that drives off", he says:

"The part that attracts, draws more vehemently; and that which drives away doth it more faintly; namely, the part opposite to it . . . If any man desires to try, let him hang them [magnets] up with threads, or balance them on a pin, or put them in Boats, and he shall finde their readiness to draw, and their feebleness and sluggishness to drive off from them."

Porta's statements on the lesser strength of repulsion are so similar to some of Gilbert's that we can be sure that Gilbert's attention was first drawn to the putative phenomenon by his predecessor's work. It would be a serious mistake, however, to think that Gilbert took it over without extensive critical consideration and re-testing. He misses no opportunity to criticise Porta and would have been very keen to show his greater thoroughness and ability by proving Porta wrong on this point as he did on many others. Unless there was a definite impression of a greater sluggishness of repulsion, it is very unlikely that Porta and Gilbert would have both come to the same conclusion. Hesse, who unfortunately does not refer to Porta at all, would have to show that he too saw the effect only because he held a theory which would be supported by it and was consequently incapable of making bare observations. But Porta in any case has no theory.

To quote Gilbert on the question (II.32, p. 155, small asterisk in spite of Porta's similar remarks):

"With magnetic bodies that are equal, coition is more vigorous and quicker than repulsion and separation. That magnetic bodies are more sluggish in repelling than in attracting, is seen in every magnetic experiment, as when loadstones are borne on suitable floats on water, or when magnetised iron wire or little bars are driven through cork and set afloat in water, as also in experiments with a needle. The reason is that, since the power of coition is one thing, the power of conformation and ordering in place is another, therefore repulsion and aversation are the act of the force ordering in place; but the coming together is the result of mutual attraction to contact as well as of the force that orders in place; i.e., it is due to a twofold force."

When an experienced and careful experimenter such as Gilbert sees an effect 'in every magnetic experiment' we should investigate the possible reasons carefully.

Are there, then, any phenomena which would support the general claim of a greater sluggishness of repulsion when compared to attraction? The answer is 'yes'. For when, for example, two similar magnets lying on some surface and facing one another with equal poles at some little distance are released, their moving apart goes through only a short stage of acceleration. This is soon followed by a deceleration due to friction as the repulsive force decreases with the greater separation of the poles. The magnets very soon come to rest. But the release of the magnets with opposite poles facing at the same initial separation results in a motion of increasing acceleration to the moment of forceful contact. The pronounced difference in these motions is clearly an observational matter. Gilbert was of course aware of the reduction in magnetic power with increasing distance, although he knew nothing of the inverse square law of force. His failure to draw the appropriate conclusions from this does, however, not invalidate the accuracy of the relevant phenomenal observations just described but is due to a lack of general understanding of the action of forces on moving bodies.

The one repulsion experiment of apparently great energy acting is that in which two equal sign poles of permanent magnets are held together and then released. The separation is then so vigorous that one no longer has the impression that repulsion is weaker than attraction. The problem is that this latter case may possibly have seemed exceptional to Gilbert in as much as he would not have known with which attraction-case to compare it. It therefore lacks the symmetry of the tests in the former example where the attractive and the repulsive experiments start from positions of equal separation. However, Gilbert remarked repeatedly on the forcefulness of the separation in the second type of test by ascribing "hostility" to equal poles.

The described experiments with wires through corks on water and with the needle can also give an impression of a greater vigour of attractive motion as Gilbert claims. If the pole of a stationary needle is approached with the equal sign pole of a magnet, the turning away of the needle starts from rest but the momentum of the turn is soon increased by the attraction of the approaching opposite pole. The attracted pole approaches the magnet then more quickly than the repulsed pole had first started to recede.

That Gilbert did not appreciate the inertial effects of the motion of the needle in general, is clear from the following remarks in II.38, p. 158:

"The point of a long needle repels the point of a short one more strongly than the point of the short needle repels that of a long one, if one of them be poised free on a sharp point and the other held in the hand; for though both have been equally magnetised by the same loadstone, still the longer one, by reason of its greater mass, has greater force at its point."

(The magnetic forces acting between the needles are of course exactly the same in the two cases and the difference in the turning motion is due to the greater inertial mass of the longer needle.)

During the opposition of equal poles of loadstones and magnetised iron objects a certain lowering of the repulsive force may also occur because of some remagnetisation. In fact partial remagnetisation can be a factor in some repulsion-experiments involving a permanent magnet and a weaker less permanent one, for example magnetised iron. Induction may obviously also affect the attractive case so that attraction can become relatively stronger, thus increasing the difference in comparison to repulsion. The degree of these effects depends on the details of the experimentally relevant circumstances. Gilbert did not consider these details although he was of course aware of the effects of complete re-magnetisation and says on p. 157 (II.32):

"South parts of a stone retreat from south parts, and north parts from north. Nevertheless, if you bring the south end of a piece of iron near to the south part of a stone, the iron is seized and the two held in friendly embrace; as the verticity fixed in the iron is reversed and changed by the presence of the more powerful loadstone, which is more constant in its forces than the iron. For they come together in accordance with nature, if either by reversal or change there be produced true conformity and orderly coition as well as regular direction."

Hesse unfortunately ignores all the factors described above although they obviously affected Gilbert's observations. This, then, is the reason for her assumption that Gilbert's error was due to theory-ladenness. What contributed to this is the fact that she mistakenly thinks that Gilbert had no notion of repulsion, something she seems to have concluded from his remark that there 'exists properly speaking no magnetic antipathy'. Yet he refers to the phenomenon of repulsion as freely as to that of attraction throughout his book. He thus observed repulsion (in a strong sense - he often describes equal poles as "mutually hostile", e.g. on p. 157) in practice perfectly well *in spite* of his theory that there was properly speaking no magnetic antipathy, whilst Hesse claims that he had no notion of the phenomenon *because* of it.

Although the problem of Gilbert's erroneous appreciation of the relative strength of repulsion and attraction has now been considered, the question of the status of bare observation reports requires further examination if that of the theory-ladenness of observations is to be answered. Gilbert's work as an example of very early researches seems to be a particularly good case for investigation. I will therefore discuss the observational-theoretical distinction in some more detail in the next chapter.

Chapter Four

Observation Terms.

A. Observational and theoretical statements.

In her paper on Gilbert, Hesse did not define what a *bare* observation report may be, but she had done so in her 1958 paper where she says that there is a phenomenal language, i.e. one independent of theories, "containing only the common-sense descriptions of ordinary objects and processes, and also, if necessary, description of what come near to sense-data. . ." (p.15). She has also given a deep and instructive analysis of the status of observational and theoretical languages elsewhere at some length (cf. Hesse, 1974). She there explains her 'network model', which has considerable bearing on some of the questions I am trying to examine here. However, it seems that her later views would not have revised the relevant aspects of her appreciation of Gilbert's work. But I will quote some of her more recent statements below.

The question of how to characterise observational vis a vis theoretical terms has received attention in the philosophy of science during the past 50 years or so. On what Frederic Suppe (1977) calls the 'received view', a strict natural separation between the two types of terms had been assumed, such that their ordering was thought to be obvious. From one type to the other there was a connection via the 'correspondence rules' (op. cit. ch. I & II pp. 45 to 50 et passim). The history of the criticism of this view is too well-known to need outlining here. Many philosophers today see the distinction as relative and flexible.

Watkins (1984, p. 191) characterises a modern 'sliding scale view' (from definitely observational to purely theoretical predicates) under the following three points.

"1. The more observational a term is, the easier it is to decide with confidence whether or not it applies.
2. The more observational a term is, the less will be the reliance on instruments in determining its application.
3. The more observational a term is, the easier it is to grasp its meaning without having to grasp a scientific theory."

(Watkins here quotes with approval W.H. Newton-Smith, 1981. The distinction between the terms is, according to the latter, one on a 'rough spectrum'.)

Watkins says that one may be liberal as to where the borderline between observational and theoretical predicates is drawn, but he also requires that one sticks to the bifurcation once it has been made in a certain way. Thus a predicate which is more observational than another must not be termed theoretical if the first is described as observational. However, he stipulates that predicates which are "assuredly observational, or assuredly theoretical, are classified as such". I will examine the question of the classification largely with reference to statements and reports, not just single predicates.

B. The distinction in historical research.

Watkins says that

"we are free to pursue a liberal policy in line with scientific practice and to classify predicates in which there is a considerable admixture of theoreticity as 'observational'". (op.cit., p. 192)

In historical considerations it may, though, depend on the development of the subject whether we would want to do so. Whilst 'pole' should have been classified as a theoretical predicate at the start of magnetic researches, it may be permissible to describe it as more 'observational' at a later stage. Peregrinus postulated the entity 'pole' following certain observations. But only after Gilbert's researches had well progressed did the existence and character of the poles become properly understood (it should be remembered that Porta and others, in the period between Peregrinus and Gilbert, were not clear about the properties of the poles). Once matters had become better understood, 'pole' became almost an observational term for Gilbert who had sorted out the characteristics of magnetism to a sufficient extent to make it well entrenched. (I take 'entrenched predicate' in Goodman's sense of a predicate already successfully used in projections to new instances - cf. N.Goodman, 1979, ch.IV. Such a predicate has passed tests in various senses of the word. The looser sense of 'entrenchment', indicating common usage, is not necessarily useful to us in general because it could, for example, apply to metaphysical concepts employed in 16th century philosopher's language. In any case it would not characterise magnetic terms at that time.)

The concept of the 'ordered form' of the magnet's interior, on the other hand, remained at the theoretical end of the spectrum. Gilbert sees the ordered form of the material of the magnet as giving rise to the force of the field. Whilst the latter can be observed by its immediate familiar effects, Gilbert ingeniously invented the former as the cause of the force, but it is itself far removed from observabililty. Its postulation appears very speculative (but remarkably prescient of aspects of modern theory).

The liberality of the sliding scale view, which seems appropriate in considerations of a theory as a completed entity, may represent dangers in the analysis of the early historical development of a subject. Classification as 'observational' of a term which would be described as 'theoretical' if tighter standards were used, could, for example, lead to an overestimation of the scientist's understanding of his subject. But a thorough examination of the best classification of a predicate is clearly vital when the problem of theory-ladenness is considered in the light of historical examples chosen to support a philosophical point of view as in Hesse's paper.

Hesse, as evidenced by her later publication, takes a sophisticated view of the distinction, based on her network model which, for reasons of space, I cannot outline here, except by quoting briefly from her conclusion. She there says:

"At any given stage of science there are *relatively* entrenched observation statements, but any of these may later be rejected to maintain the economy and coherence of the total system. This view has some similarity with other non-deductivist accounts in which observations are held to be 'theory-laden'. . ."

Her model, she says further on, allows that

"at any given time *some* observation statements result from correctly applying observation terms to empirical situations according to learned precedents and independently of theories. . .".

She also states that there are no theoretical *predicates* which are never applied in observational situations to any object; but there are "theoretical *entities* to which predicates observable in other situations are applied. It follows that there is no distinction in kind between a theoretical and an observation language." (1974, p. 43/4, her italics)

I find this very instructive. Pace her principal intention, one notes that she allows for the correct use of observational statements 'independently of theories'. Her 'learned precedents' may inter alia refer just to the employment of observational terms in daily language. However, it does not appear that she would have revised her claims about Gilbert in such a way as to change what I believe to be her mistaken assessments of the theory-ladenness of his observation statements. I will in any case maintain the above sliding scale type bifurcation and attempt to offer a solution at least to some of the problems and arguments by introducing the concept of a '*relatively* bare observation statement'. This simply allows that a particular observation predicate or report may be 'laden' with respect to some theories yet be 'bare' in the important sense at issue with regard to the germane aspects of the particular theory or hypothesis under investigation or exposition. This will be considered in a little more detail below.

It is not implausible to deny that predicates or descriptions used in ordinary language, including those which refer to apparently directly observed characteristics of everyday objects, can be strictly theory-independent. The presuppositions of conceptions and terms of everyday discourse on physical objects or events may justifiably be claimed to be theory-laden in many respects; to originate in 'common-sense' theories, for example, which may contradict accepted theories of physics. Granted then, that all language may be regarded as theory-laden in a strict sense, the question is whether this entails the abandonment of the observational-theoretical distinction. I do not believe that it does and that the distinction should therefore be retained because of its occasional usefulness to methodology and history.

It is perhaps difficult to decide in general when modern language, with its multifarious levels and rapid changes, should be seen as 'ordinary'. With respect to that of the 16th century this problem and that of a gap between everyday language and a science of physics seem, however, less daunting (apart from religious and metaphysical concepts of which account has to be taken). The lack of detailed physical theories insured a generally more direct link at least between appearances and their common (and 'common sense') linguistic formulations. Existing theories, such as Aristotelean astronomy, were in important respects themselves guided by common-sense interpretations of appearances. (This is of course not to claim that Aristotelean physics in general expressed no more than common sense would have suggested, or that it employed no theoretical concepts.)

What, then, is the possible meaning of 'bare or assuredly observational term' and what are the special considerations germane to the predicates 'observational' and 'theoretical' in historical research? The answer to the first problem, suggested by the sliding scale conception, is that in our examination of Gilbert's work the predicate 'bare observational' should be reserved for familiar terms from the everyday language of the 16th century used in descriptions which concern macro-features of objects and events. (The particular observations may also involve some use of simple instruments such as rulers, clocks or scales.) The objects and phenomena concerned will, for example, include material bodies, dimensions, shapes, colours and simple forms of motion whose characterisations would be generally understood. This may also be in agreement with Hesse's view, at least as expressed in 1958.

Detailed examination of the employment and meaning of the language of the time is at least occasionally desirable in the analysis of formulations of early scientific statements. (One may for example want to have regard to initial anthropomorphic implications of the term 'attraction'.) The expected advantage of proceeding in this manner with respect to Gilbert's work would be the identification in his vocabulary of terms from metaphysics, religion or those belonging to traditional untestable assumptions in his fields of research if he employed such. It is striking how Gilbert himself clears away some of the undergrowth of such unsuitable concepts (as by his strictures on the use of 'sympathy' quoted above). Roller (op. cit.) points to our general lack of knowledge of the implications of many important words in use in the 16th century. If I mention anywhere examples of the extension or adaptation of the meaning of everyday terms to Gilbert's scientific use (the 'language shift' to which Stephen Toulmin points - 1953, pp. 13 & 152), this must remain, then, somewhat speculative. Gilbert's own explanation of his use of 'attraction' is quite detailed, and he also enlightens us on its employment by other writers. He defines his new terms, such as 'coition', 'verticity', 'versorium' and 'electrics', on the whole quite well. The more interesting examples of certain changes of use or extensions of meaning as compared to the writings of his predecessors concern important concepts such as 'pole', 'energy', 'force' or 'field' (the latter would be Porta's *orbis virtutis*, or terms translated as 'field' in the appropriate context). A problem in our case may be the relationship of Gilbert's language to the everyday language of his time possibly arising from the fact that he wrote in Latin. Gilbert used terms both from the learned Latin literature and from the language of craftsmen, artisans and sailors as well as the more formally educated. Occasionally he may have translated vernacular terms into Latin in his own particular way. I cannot consider the implications of the language-use of Gilbert's time and my purpose has only been to point out the possible problems detailed examinations of the question would face.

Judgements as to which predicates should be described as 'observational' require considerable care with respect to the specific aspects involved in the employment of the terms at issue at a particular occasion. To take the example of 'attraction' again: Roller states (op. cit. p. 96, his italics) that ". . one does not observe attraction - nor does one observe magnetic coition. The *observation* in these cases is . . . the change in position." This seems too narrow a view to take. The force and phenomenon of attraction, when this concept already has some pre-scientific meaning, manifest themselves observationally when two magnets are held in the hands in appropriate ways. The felt pull of the one magnet on the other and the spontaneous movement of one or both upon release may justify a decription of this form of attraction as 'observational'.

When Gilbert loosely uses 'attraction' for both the electric and for the magnetic case, we may understand this as an observational term describing this kind of phenomenon of a spontaneous approach of bodies not instigated by any familiar mechanism of an external agency. However, in his more precise use of the term he makes a point of distinguishing the cases in a deeply theoretical way by reserving 'attraction' for the electric phenomenon. Here a postulated medium, the electric effluvium, carried the attracted object to the electrified body (he did not believe that the former also exerted any sort of influence on the latter). In the magnetic case he could not use 'attraction' once this term was defined in this idiosyncratic way; for here no medium was involved. As he also found the forces to be mutual he described this motion as 'coition'. Clearly, then, when Gilbert uses 'attraction' in the magnetic case he is using the term as an observational one to describe the actual or potential phenomenon of a movement or perhaps a force (as when a magnetic pull can be felt).

Gilbert's statement to the effect that magnetic repulsion is more sluggish than attraction seems at first sight to be readily classifiable as observational because it refers to such kinetic and dynamic aspects. But this is not so, for it is a very general statement with quantitative implications of a kind which Gilbert could not have supported by the necessary understanding of the concept of force and inertia and, importantly, by measurements. It does not refer to specific and detailed observations but represents an apparent conclusion resulting from various experiments. I would therefore not classify it as a bare observation statement but perhaps as a qualitative law. The examples given show that we may have to postpone a classification of a report at times until a thorough analysis of the stage of development of the particular theory has been concluded. Would Gilbert himself have been aware that he had not given a bare observations statement? It seems indubitable that he would, for the statement is couched in terms which invite further comment and the citation of instances (which Gilbert indeed gives).

C. The interpretation of experiments in observation reports.

Feyerabend says about the relationship of theories to the observation language

"The interpretation of an observation language is determined by the theories which we use to explain what we observe, and it changes as soon as those theories change". (P. Feyerabend, 1958, p. 160.)

Feyerabend's example illustrating this claim is convincing: he envisages an observation language of colour predicates each of which the users may ascribe to particular self-luminescent objects, whether these are observed *or not*. A new scientific theory shows, however, that the colours in which objects appear depends on the relative velocity between observer and object so that the conception of the colour words changes to a relational one.

Hesse states with respect to Gilbert

"It is therefore unprofitable to contrast, as many historians have attempted to do, Gilbert the experimenter with Gilbert the speculative theorist, *for Gilbert's theories determine throughout the interpretations he makes of his experiments*." (1960, II, p. 130, my italics)

Hesse is critical of Feyerabend's general later position (cf. the introduction to Hesse, 1974, p. 3), yet - not inconsistently with her criticism - the two quotations aim in similar directions. I will not here consider Feyerabend's wider position, but it is clear that his example and claim as quoted show that there may not exist a completely bare observation language independent of any theory whatever, as I have already envisaged above.

Hesse's italicised remark might also be acceptable if it implied only that in the theory's final exposition the observations would find interpretative formulations which relate them in an appropriate way to its theoretical entities and postulates. But as is apparent from the foregoing, she claims that the observation reports are theory-laden in the sense that they so to speak cannot help supporting the theory because they are not, and could not be, independent of it. Her paper (1960) makes clear that she is implying that Gilbert did not give, or *could* not have given, a description of his experiments which is not 'laden' with respect to the relevant theory. In fact she fails to notice that he (like most other scientists) usually gives this as well as the interpretative formulation, or that the reader can at least easily recover it from Gilbert's various statements. As mentioned, observation description can be *bare relative to a theory* in the sense that it contains no intrinsic theoretical admixture originating from this theory. It need not be, and is not likely to be, absolutely bare of any theoretical admixture from any theory whatever, but would contain such admixture only from theories perceived as unproblematical .

Hesse, in her later paper, envisages withdrawal from a description such as "particle-pair annihilation" to "two white streaks meeting at an angle", says that a process of redescription formulated in better entrenched predicates could go on but that all these descriptions have "law-like implications of their own", and that the number of the latter is in fact likely to be greater the better entrenched the predicate (Hesse, 1974, p. 24). This seems true and is of importance in her network model of science. But the "law-like implications of their own" of the well entrenched predicates of ordinary language are in general not directly conceptually connected to the *particular* theory at issue in such a way that their use must result in the theory-laden observation of the test Hesse has in mind. They need not affect the bareness of a description relative to the theory. Thus a report of a series of simple experimental procedures designed to investigate a problem in electrostatics may rely on the theory of mechanics yet be bare relative to *all* electrostatic hypotheses. The description may be couched in terms which refer to mechanical phenomena only, describing for example the motions of a small object and certain experimental arrangements and events: "A glass rod was rubbed with wool and brought near a small pith ball and the latter moved quickly to it. This experiment was repeated with balls of different weights and from different distances; the numerical results were as follows. . ." In such cases only terms from mechanics would be used and no causal explanation of the object's behaviour need be cited by the report. Other possible descriptions of the observations which contain the terms 'attraction', 'force' or 'electric' need still not make the observations of the motions of the pith-ball theory-laden in Hesse's deeply prejudicious sense. They may just amount to more theoretical and *consciously* interpretative forms of descriptions of the observations, alternatives couched in more phenomenal terms being easily formulated, as we have just seen. The import of such phenomenal reports to the theory is of course itself a theoretical matter as Hesse also claims (see below).

In general it is not the whole related theory which is on test when a hypothesis is experimentally assessed, and the relative bareness of the observation report with respect to the hypothesis assures that it can play its part in such an assessment. But when a complete theory is modified, its "purely representative part enters nearly whole in the new theory" as Duhem says (op. cit. p. 32; the 'representative part' of the theory would contain observation reports even if they be contained in phenomenal laws). This can only occur if the experimental results are describable in ways which are bare relative to the theory. Formulations of observations are always conceivable which are bare in respect of a single hypothesis which introduces perhaps a new concept into a theory, or of the latter's main tenets.

It is, then, not only the historian who is in general able to assess the status of the observation report in the light of later theories as Hesse maintains. The scientist who invented the first theory may after all also be the one who develops a new one. A decision between two rival hypotheses often requires the evidence of observations which are formulated in such a way that their independence from relevant presuppositions of either is seen to be assured. Hesse (1958) had envisaged such a situation herself, when she said that a phenomenal commonly understood language for descriptions of experiments is possible and could be used

". . . for instance when radical revisions of a theory are in progress, most descriptions could in principle be reduced to phenomenal statements as here defined".

But she goes on to say that

". . . if phenomenal statements are to be tests of a theory, they cannot be independent of the theory in the sense of being derivable from the latter . . ." (p. 16)

She adds that that is so because the phenomenal statements could then not have scientific significance, and

". . . if they are to have such significance there must be connections of meaning between them at a higher than common-sense level, and therefore the condition of complete theoretical independence between them must be dropped". (ibid.)

The scientific significance of the statements is indeed itself a theoretical matter as Hesse claims. Yet this does not mean that an observation report must be theory-loaded. Quite apart from its significance, it is open to Hesse's inductivist to state, for example, that the description of the observations in the electrostatic experiment envisaged above is a relatively bare one. In other examples of observation reports, the situation may have to be analysed in each individual case, and a relatively bare formulation will either have been given or is recoverable, provided the question of its scientific import is separated.

In the example of the relative strength of magnetic attraction and repulsion, Hesse implied that Gilbert saw repulsion as being weaker than attraction, because he had already formulated a theory which lacked the notion of repulsion. Because his observations were theory-laden, so we must assume Hesse is reasoning, he was not able to see that the experiments falsified his theory. This conclusion does not follow, not only because it presupposes prematurely that the theory preceded the observations. Gilbert's reports of the experimental outcomes can give the impression of the lesser vigour of repulsion because of the absence of measurements, the lack of understanding of inertial effects, and of the action of the forces. His statement is, however, in any case not a bare description but a low-level law.

Chapter Five

Direction

A. Coition, direction and repulsion.

I still have to conclude the examination of Gilbert's ideas on coition and repulsion, and this will involve the directive force. As mentioned, he saw coition or attraction as the fundamental property of magnetism. In insisting on the correctness of the former term, he stressed the mutual action of magnets or magnets and iron and the importance of induction in the latter. Attraction could not occur unless both bodies were magnetic. Near the loadstone iron would immediately become as active a magnet as the stone which just instigates the process of coition by 'informating' the iron, i.e. by induction. When free to move (given similar mass), both bodies approach each other. This, or the attraction of even a very small magnetic to a magnet, could occur only inside the latter's 'orb of coition'.

Magnets had another fundamental property: the force which aligns and directs magnetics by turning them (this is his basic force of 'verticity', cf. III.2., p.183). It appears to be identical with the 'force that orders in place' which Gilbert holds also expressly responsible for repulsion. As repulsion is involved in the directional behaviour of the compass needle as well (cf. Section C. below) it seems justified to *identify* his directive and ordering forces. I will consider 'direction' in more detail in the next section.

Only the directional force is effective outside the orb of coition, as long as we remain inside the *'orbis virtutis'*, i.e. the magnet's field (in its practically uniform regions). It would show itself, for example, in the alignment - Gilbert often refers to it as 'rotation' - of needles which, when suspended on a thread, would turn into definite directions that depended on the magnet's shape and position.

But inside the orb of coition this directional and ordering force also instigated the translational repulsion of two magnets when like poles were brought together. Its effects could then be very strong. Gilbert understood the movement of repulsion to be part of the alignment of magnetics caused by the directive force (". . . repulsion and aversation are the act of the force ordering in place. . ."; II.32, p.156). He also saw in cases of appropriate geometrical arrangements of magnetics that "the ordering force is often only the forerunner of coition . . ."(ibid.). In the case of the unstable arrangement of two suitably suspended magnets, for example, the repulsion caused by bringing like poles together will be followed by a turning motion and attraction to contact.

Gilbert's force of ordering in place to which two magnets are subject, then, appears to do two jobs on a phenomenal level: it causes the rotating or aligning movement and, in other cases, translational repulsion. If, due to repulsion, the magnets moved translationally apart and remained that way (i.e. the usual case of repulsion), a magnetic 'ordering in place' had still occurred, or so we may understand him. But none of this means that repulsion is only a weak secondary effect. It is very strong when like poles face one another, and Gilbert in practice had the concept of a repulsive force in a full sense:

"If you bring the part or point A [of one magnet] up to C [the like pole] of the other, they repel one another and turn away: for by such a position of the parts nature is crossed and the form of the stone is perverted . . . nature makes war and employs *force* to make bodies acquiesce fairly and justly." (I.5, p.30, my italics).

And

"Pieces of iron that have been magnetised at one and the same pole of a loadstone repel one another at the magnetised ends; and their other extremities are also mutually hostile" (II.32, p.157).

There are of course numerous other references to repulsion throughout the book.

His explanation for the claimed phenomenon that "coition is more vigorous, and quicker than repulsion and separation" (II.32, p.155) is that repulsion is due to the

"force ordering in place; but the coming together is the result of mutual attraction to contact as well as of the force that orders in place; i.e., it is due to a twofold force." (p.156)

So much for repulsion. The quotation also allows the conclusion that there are two basic forces involved in Gilbert's one force of direction or ordering in place, viz. a form of the attractive force which causes one end of the needle to turn towards the magnet (this would be the part of the directive force which also assists the coming together of coition in appropriate cases), and repulsion.

Having considered the character of repulsion as an aspect of the action of the turning force we can summarise the effects which proved to Gilbert that attraction was the predominant magnetic effect when compared to repulsion. They are: induction in, and attraction of, iron; the apparently greater momentum observed in many attractive movements when compared to repulsion; and the sequence of repulsion-rotation-attraction in unstable arrangements when attraction is the final result even if repulsion preceded it. A sequence attraction-repulsion does not exist. Pole reversal with premagnetised iron, when it occurred, always resulted in attraction, never repulsion. Thus attraction seemed (usually) stronger, occurred much more widely, and was at times the end result even of cases of actual or potential repulsion, i.e. after rotational movements or on pole reversal.

It was no doubt his views on the predominance of attraction which prevented him from postulating a specific and separate repulsive force of completely equal standing with his three main magnetic properties, the attractive force, the directive or rotational force, and induction. A repulsive constituent was, however, part of the directive force, and this could account in his view sufficiently well for the phenomenon. Considerations of economy may have played a part in this, possibly reinforced by the important fact that he had finally done away with the mediaeval theamedes, the special magnetic stone which had the property of repulsing iron. He may have felt that he would be opening a door for it again if he postulated a special repulsive force. Nevertheless, as we have seen, Hesse is mistaken when she says that he "lacks the *notion* of repulsion" (1960, p. 9, my italics). Repulsion, in Gilbert's scheme, acts as a force throughout although it is an aspect of the force which orders in place.

The concept of the aligning force is intimately connected to that of 'coming into harmony' which is the governing magnetic characteristic of which coition and direction are seen as aspects. Repulsion as a separate force would perhaps seem to be less natural to such a conception. This is of course by no means a compelling interpretation of the vague metaphysical principle of 'coming into harmony', for he could have seen even a separate repulsive force as necessary for the achievement of a harmonious state. This seems to indicate that the supervenience of the principle of 'coming into harmony' rather represents the conclusion Gilbert came to at the end of his magnetic researches, having formed the view that repulsion was a lesser force at an earlier stage. In any case I doubt whether Hesse is right when she says (1960 p.8):

"It may be that Gilbert dislikes the notion of 'repulsion' just as he dislikes 'attraction' because it savours of the occult sympathies and antipathies postulated unnecessarily of all kinds of processes which he has himself investigated."

For although it is true that he dislikes the idea of sympathies and antipathies, the postulated principle of 'coming into harmony' seems not really less occult than repulsion (or attraction) in the germane sense, and he does use the term 'repulsion' freely.

B. Direction: Norman and Gilbert.

Although the previous section has dealt with Gilbert's directional force to some extent, this merits further detailed attention because of its important place in the history of the conception of the field and the couple.

As we have seen, Gilbert observed the following phenomena of the force of directionality in magnetic experiments: small iron pins would be attracted to touch the magnet's surface at angles which depended on their position relative to the nearer pole; where translational attraction did not take place, a test body would align itself in definite directions in the space around the magnet (including the earth); and when like magnetic poles faced one another closely enough, translational repulsion would occur. These several cases have later been pictured as indicating the effects of the direction and density of the field affecting the ends of the test body.

Gilbert uses the term 'direction' for the pole to pole component of the alignment of the needle in the horizontal plane and usually separates the phenomenon of dip from it, particularly when he considers the situation on earth:

"The earth causes magnetic bodies to rotate and directs them poleward strongly at the equator; at the poles there is no direction, but only fast coition of terminals that agree. Hence direction is weaker at the poles, because the versorium, by reason of its tendency to turn to the pole, dips greatly, and is but feebly directed . . ."(IV.10, p. 254; let us ignore the 'fast coition' which he presumes to occur at the earth's poles.)

Gilbert refers to the components of the alignment here but it will often be unnecessary to follow his separation of direction and dip. I therefore conflate the two under the term 'direction' or 'alignment' unless we specifically want to consider components in particular cases.

Gilbert's usual form of separation of directionality from the attraction effective in coition, possibly facilitated progress in magnetic research. His advance on Robert Norman shows itself strikingly when we consider what Norman had concluded from the fact that the compass needle does not move translationally whilst still taking definite directions when floating on or below the surface of a vessel of water. He seems to have thought that, as the 'point respective' in the earth to which the dipping compass needle points, does not attract it translationally, the needle's turning was entirely due to its own virtue, not to the power of the earth or of the point in it:

"this stone hath wholy and fully in himselfe Power, Action, Propertie and Vertue of his own Appetite to shewe and to cause the Needle to shewe the point Respective, without any Attractive qualitie or external cause of the

Rockes of the Magnes stone, or by Attraction in the Heavens or elsewhere whatsoever." (Norman, op. cit., ch.VIII, p. 29; 'this stone' refers to the loadstone which may align itself in a certain direction or with which the needle is magnetised)

Norman had no explanation at all for the fact that the needle dipped. It just had the property of doing so:

"Now paradventure you will aske mee howe this stone hath his Power, and howe it is engendered. I am no more able to satisfie you heerein, then if you should aske me howe and by what means the celestiall Spheres are moved" (ibid.).

Sydney Chapman (1944, p. 134) stated

". . . he [Norman] concluded . . . that the earth, not the sky, controls the direction of the magnet, and he showed that the earth did not attract the magnet but only turned or directed it."

But Norman affirms the opposite on p.30, where he says that the

"stone has power in itselfe, to shewe one certaine point, by his owne nature and appetite, and not subject to any other accident in Heaven, nor in Earth . . ."

Chapman is therefore mistaken, and his statement also obscures the important fact that in spite of Norman's discovery that one of the needle's poles points toward the earth's interior it was obviously still very difficult to draw the conclusion that the earth had any magnetic powers. It is not clear what Norman thought would happen to this power to show the 'point respective' when the needle was near a magnet. Here he knew that forces acted, but as he most probably used no terrellae (which might have suggested an analogy to the case of the earth), he did not see the needle's alignment as a 'dip' with his non-spherical magnets.

C. The notion of a couple.

With respect to direction in the uniform field A. Crichton Mitchell (1939, p. 79) remarked in a reference to Norman that in his time

"the conception of a couple, in mechancis, had not been reached, and for lack of this, his explanation of the magnetic inclination was defective".

Norman specifically denies the existence of external forces or causes when searching for an explanation of the dip of the compass needle. (He had also found that its movement could not be caused by a ponderable substance in it.) For him, therefore, various aspects of magnetic phenomena remained mysteriously unconnected to one another and to mechanical events.

One should, however, not underestimate contemporary knowledge of the general action of couples. As we are concerned with the same problem in Gilbert's work it is perhaps timely to point out that although its treatment in terms of, say, forces as anything like mathematical vectors was of course not possible, there existed a considerable degree of understanding of what a couple amounts to. The lever and balance were the best-understood simple contrivances in early science, and their theoretical appreciation goes, of course, back to Archimedes. In everyday life and technology, couples had been employed for millenia in all sorts of applications where forces acted on the arms of balances and it was obvious what effects they have (albeit that these forces had causes much more directly perceptible than magnetism). As it was understood how two suitable forces applied to the ends of a lever with a fulcrum would turn or balance it, it was possible to see that if the ends of the needle were attracted and repulsed magnetically, rotation would be observed. Norman thought he had proved for the compass needle that there was no attraction or any other external cause acting, and was unable to see that the dipping of his needle falsified this hypothesis. The reason was not that he could not have understood how a couple would have resulted when forces acted. But he was unable to extrapolate from the dip of the compass-needle to the existence of the relevant forces. He says that near magnetic mines, like those on Elba, the compass needle is not drawn or changed (cf. op. cit. ch.II), and must have concluded that if even there no effects of an external field was percetible, then the needle's dipping could not be caused by an external magnet at all. His knowledge that the needle aligns in the field of ordinary magnets did not suggest the analogy to him.

The contrast to Gilbert's treatment of matters is striking, and I will also show that he understood the action of a couple in magnetism. The difficulty concerning the difference between coition and directional alignment was not at all that Gilbert did not understand the turning of the needle in terms of attractive and repulsive forces. No comments on Gilbert I have seen, have pointed out that the fundamental question which could not be answered, but affected all aspects of the understanding of magnetism, was that of the *mechanism of translational attraction*. The effects and characteristics of the *uniform field were not appreciated in comparison to those of non-uniform regions* that would affect for example a short needle suspended successively in two such areas. From the vague knowledge Gilbert had that the magnetic virtue diminishes somehow with distance, it would not seem clear why the magnet at some range, or the earth apparently anywhere, should be strong enough to turn a relatively weighty length of magnetic, but would not translationally attract even a very light one. It is one thing to see that coition does not occur but a couple results when balanced forces act, but quite another to see why the forces are as they are in cases of coition. Because of his necessarily qualitative approach Gilbert did not know that translational attraction of a magnetic is the result of a sufficient difference of the field strength at its two ends. This of course had to affect his overall understanding of magnetism detrimentally, although it has surprisingly little negative effect on his work in practice.

Hesse says about the problem of attraction and rotation due to a couple with reference to Norman and Gilbert (1960, p. 9):

"In the first experiment Norman magnetises an iron wire and floats it on the surface of water. It shows, he says, no tendency to move bodily, but only to rotate to point towards the north. Gilbert comments: 'this assertion of the Englishman, Robert Norman, is plausible and appears to do away with attraction.' In the other experiment, Norman arranges that a needle stuck through a cork floats wholly immersed in a glass bowl of water both before and after magnetisation [fig.15 after that in V.IX, p. 302]. Again it is not found that the needle moves downwards towards the earth's pole, but only that it rotates about the cork into a dipped position. 'But', says Gilbert, 'it must be understood that as it is a curious and difficult experiment, so it does not remain long in the middle of the water but sinks at length to the bottom, when the cork has imbibed too much moisture.' It must be concluded that Gilbert had tried both these experiments and failed to obtain clear evidence of the effect reported by Norman, yet he is prepared on the basis of them to distinguish coition from the rotating property."

This is a surprising misreading of the situation. The very simple experiment of the needle on a little raft floating on the surface of the water, which goes back to Peregrinus, shows clearly a rotational alignment but no translation northward. Gilbert says in IV.6, p. 244: "Clearly the wire with its cork does not move toward the rim of the vessel, as it would do if attraction came to the iron from there . . ." There are further confirmatory remarks which I do not need to quote. Gilbert's comment about the second experiment does by no means lead to Hesse's conclusion either. He expresses no doubts about it at all. Though he says that it is a difficult experiment, it is possible and will have been repeated by Gilbert. He should after all have had no more difficulty in executing it successfully than did Norman, who obviously managed it.

To these two we can add experiments with suspended needles, where a tendency to translational movement would have shown up by an angling of the thread from the vertical, quite apart from analogous experiments around the terrella which Hesse mentions, saying that their results were definite but adding without further explanation that "the conclusion to be drawn from them is not quite unambiguous". She continues (ibid.):

"The distinction [of coition from rotation] cannot be said to be, for Gilbert, an experimentally based descriptive statement such as an inductivist would approve. And yet, as it happens, Gilbert was right in the light of the later theory according to which the earth exerts a couple but no force on a small magnet in its field. Gilbert himself however cannot approach an explanation in terms of resultant force and couple, since he lacks the notion of repulsion, and his own interpretation of the distinction between coition and rotation does not accord with the later theory."

Again, Gilbert can and does supply bare observation statements of which Hesse's inductivist would approve, and which capture the distinctive phenomena of coition and rotation. He does not lack the notion of repulsion and although - for the reasons I have given above - his interpretation of the distinction between coition and rotation could indeed not entirely accord with later theory, he also has the concept of a couple. He says on p. 183 (III,2, my italics), in a chapter devoted to the direction of the compass needle:

"And the earth's energy, with the force inhering in it as a whole, *by pulling toward its poles and by repelling*, arranges in order all magnetic bodies that are unattached and lying loose."

This very important sentence describes the essentials of the mechanism of a couple very well. Other interpretations of his statement would have to assume quite arbitrarily that Gilbert was talking about successive processes of pulling and repelling, or that he meant that only one of the earth's poles either pulled *or* repelled only one of the needle's poles, whilst the other did nothing to the other end of the same needle. (It is perhaps timely to point out that he nowhere suggests that there might be magnetic monopoles, nor would this idea have fitted into his theory.) Clearly all such interpretations would be far off the mark.

Gilbert's appreciation of the couple seems to have been generally overlooked. Rom Harre (1981, p. 53), in a description of Norman's experiment of the needle's dipping in the cork under water, says

"With hindsight we know that there was another hypothesis that neither Norman nor Gilbert thought of, that there were both attractive and repulsive forces which depended for their strength on the distance of the sources. Thus the needle would turn to the north pole of the earth because of a balance between forces of attraction and repulsion, between both poles of the magnet and both poles of the earth. Happily this more complex force theory, the central focus of argument between Ampere and Faraday some 250 yrs. later, occurred to neither of the great Renaissance students of magnetism. Norman and Gilbert after him dealt with the problem of explaining terrestrial magnetism by the invention of the idea of field of force, the foundation idea of the modern physics of electricity, magnetism, and gravity."

The first and second sentences, then, are in fact not applicable to Gilbert, though we must grant Harre that Gilbert did not sufficiently appreciate all the implications of the distance of the sources of the forces. If the last sentence implied that Norman had the concept of the field in any sense approaching that of Gilbert's, it would be mistaken. The contrast Harre sees between the idea of the field and that of attractive and repulsive forces does not apply to Gilbert's appreciation of magnetism; for his experiments and explanations homed in on both aspects, although he did not understand the crucial importance of field density.

Zilsel (1941, p.20, fn.28) makes a remark somewhat similar to Harre's statement. Referring to Gilbert's explanation of the experiment of the needle in a cork floating on top of the water without being attracted to the rim of the vessel he says: "He (and Norman) forget that the needle has two opposite poles which are drawn to opposite direction." Maybe Norman had forgotten, but Gilbert certainly had not.

Gilbert's achievement in describing attraction, direction and repulsion and the couple is considerable. Without definition of force, lacking the ideas of the inverse square law and field-density he yet characterises matters very perceptively and in any case in such a way that phenomena can be sensibly, if mainly qualitatively, forecast when the necessary parameters are to hand.

Chapter Six

The Magnetic Field.

A. Directionality and the field.

Some of the properties of the magnet's field identified by Gilbert have already been described, but I have still to address others, as well as its overall conception.

The idea that the loadstone be surrounded by a definite region in which the magnetic power manifested itself is found in Porta's and, independently, Norman's book:

> "If this [magnetic] virtue could by any means be made visible to the eye of man, it would be found in a spherical form extending round about the stone in great compass, and the dead body of the stone in the middle thereof, whose centre is the centre of his aforesaid virtue." (Norman, op. cit., ch.VIII)

The term *orbis virtutis* had been introduced by Porta who says:

> ". . . the pole sends its force to the circumference. And as the light of a candle is spread every way, and enlightens the chamber; and the farther it is off from it, the weaker it shines, and at too great a distance is lost; and the neerer it is, the more cleerly it illuminates: so the force flies forth at that point; and the neerer it is, the more forcibly it attracts; and the further off, the more faintly; and if it is set too far off, it vanishes, and does nothing. Wherefore for that we shall say of it, and mark it for, we shall call the length of its force the compass of its virtues." (1658, book 7)

The aptness of the comparison of the propagation of the magnetic force with that of light is remarkable; but the inverse square law was not known to apply to either case, though Porta obviously had an intuitive understanding of the similarities in the two cases. He only mentions attraction as the force here and would have described the deflection of the needle everywhere as due simply to attraction; he seems to have known very little of the behaviour of magnetics in the uniform field.

Gilbert's investigation of the field involves many specific dip experiments. In V.5, p.290 he states that the dip is not caused by attraction. His statement shows up his difficulties in appreciating some aspects of the action of attraction (in this case of its directionality):

"For did the versorium dip under the action of an attractive force, then a terrella fashioned out of a very powerful loadstone would pull to itself more than would one made of an indifferent loadstone, and iron stroked by a strong loadstone would have greater dip; but that is never so."

For Gilbert the importance of the dip in the investigation of the field lay in the fact that its degree varies with the distance from the pole (unlike direction which is always in the north-south line).

Two hundred years later matters were still couched much in Gilbert's terms, and Charles Hutton (1795) summarises a later view which echos Gilbert's exactly:

"there are two qualities in all magnets, an attractive and a directive one; neither of them depend on, or are any argument of the strength of the other." (vol. 2, p. 71)

But an unhelpful assumption had been added at the end of the 18th century, for now the two forces were due to special particles:

"It is probable that iron consists almost wholly of the attractive particles; and the magnet, of the attractive and directive together . . ." (ibid.)

In a lengthy article on magnetism this work largely ignores Gilbert, ascribes some of his discoveries to others, and makes several statements which Gilbert would have recognised as false. It defends in particular an effluvial theory of magnetism. All this shows how slow and uncertain the progress of magnetic knowledge was in some respects, even though the inverse square law had by then been experimentally confirmed by Coulomb.

B. Magnetic forces and surrounding media.

Gilbert characterises the field as follows:

"The magnetic force is given out in all directions around the body; around the terrella it is given out spherically; around loadstones of other shapes unevenly and less regularly [but so as to follow the shape of the body approximately, as he makes clear elsewhere]. But the sphere of influence does not persist, nor is the force that is diffused through the air permanent or essential; the loadstone simply excites magnetic bodies situate at convenient distance [the Latin reads "*nec tamen in rerum natura subsistit orbis, aut virtus per aerem fusa permanens, aut essentialis; sed magnes tantum excitat magnetica convenienti intervallo distantia*"]. And as light - so opticians tell us - arrives instantly, in the same way, with far greater instantaneousness, the magnetic energy is present within the limits of its forces; and because its act is far more subtle than light, and it does not accord with non-magnetic bodies, it has no relations with air, water or other non-magnetic body; neither does it act on magnetic bodies by means of forces that rush upon them with any motion whatever, but being present solicits bodies that are in amicable relation to itself. And as light impinges on whatever confronts it, so does the loadstone impinge upon a magnetic body and excites it." (II.7, p. 123)

Commentators differ on whether Gilbert has the concept of a magnetic field. Roller says

"Gilbert's magnetic form is quite remote from Faraday's field. Gilbert's influence in this direction lay in his insistence upon the region around the magnet as being a region of interest" (Roller, op.cit. p. 148), and "It is important to emphasize that this [i.e. the magnetic] virtue is by no means the property of the space that it becomes in later science." (ibid.)

It is obviously much too restricted a reading of Gilbert's conception to say that the region around the magnet was no more than a region of interest, and Rom Harre is of a very different opinion. Gilbert in fact has a very good conception of the field. I will return to this question presently after considering Gilbert's ideas on one or two related questions.

His field needed no carrier-medium. This assumption is implied in his speculations that the earth's magnetic field and that of the moon extend across the void of space (cf. VI.3, p. 326: "the space above the earth's exhalations is a vacuum"; on the mutual magnetic influence of earth and moon more in the chapter on his cosmology). It is an assumption which, strictly speaking, he was not entitled to make. For from the fact that the magnetic virtue passed through all non-magnetics, it does not follow that it needed no medium for its propagation. He was of course not able to check that the field spread through a vacuum and so for Gilbert this hypothesis is not corroborable. He obviously saw justification for it in the magnetic virtue's 'instantaneous' crossing of any medium (as different as air, water, stone, or non-ferrous metals). The medium did not seem to affect the virtue in any way. If it had been its carrier, then some changes in the virtue's character should also have resulted from its passing through media of different types, Gilbert may have thought. It behaved as though the media were not there, so presumably they were not needed for its propagation. As they were not affected by its presence either, and the field does not 'persist' (when the magnet is removed), they appear to have nothing to do with it whatever. So

". . . neither has the energy of the loadstone entrance into their interior, nor are their forms excited magnetically; nor if the energy did enter in, it could effect aught, for the reason that there are no primary [i.e. magnetic] qualities in such bodies, mixed as they are with a variety of efflorescent humors and degenerate from the primal property of the globe." (II.13, p. 217).

I will consider aspects of these quotations in more detail below in connection with some historical comments on Gilbert's field conception.

C. The geometry of the field.

The shape of the field was a matter which he considered in some detail:

"The rays of magnetic force are dispersed in a circle in all directions; and the centre of this sphere is not in the pole (as Baptista Porta deems, Ch.XXII) but in the centre of the stone and of the terrella . . . though magnetic bodies are not borne direct toward the centre in the magnetic movement save when they are attracted by the pole. For as the formal power of loadstone and earth promotes simply unity and conformity between things separate, it follows that everywhere at equal distances from the centre or from the convex circumference, just as at one point it seems to attract in a right line, so at another it can control and rotate the needle." (II.27, p. 150)

He continues to say that if at a given distance over the pole the needle is attracted, then over the equator, at the same distance, the stone rotates and controls it.

In these considerations he does not point out that the orb of coition behaves quite differently from that of the directional force, though he was of course fully aware of this. There would in fact be two part-spherical spaces in which coition occurred, centred on the two poles, there being no attraction whatever at the equator (it is this to which Porta draws attention).

Benjamin criticises Gilbert for stating that "the rays of magnetic force emanate in all directions from the loadstone's centre" (Benjamin, op.cit., p. 351-2), saying that Porta was right and that Gilbert in this case

"allowed theory rather than experiment to guide him; for, when he carried his iron needle around the terrella, he saw plainly enough, as Peregrinus had seen centuries before, that it never pointed to the centre, except when it was exactly at the poles". (ibid)

It is regrettable that Benjamin (whose work on the whole is very perceptive) accuses him of having allowed his theoretical views to override experimental evidence, as Hesse was to do later with respect to other aspects of Gilbert's work. Gilbert was much too careful an observer to be guilty of such a simple mistake as this case would represent. He is talking about the total sphere of virtue, i.e. the range of all forms of the magnet's effects on magnetics, including directionality and induction. Within his experimental means this is indeed a sphere in the case of a spherical magnet.

It is hard to understand how Benjamin could have gone wrong on this point. Gilbert does not say that the rays of magnetic force "emanate from the centre of the sphere". They are

"dispersed in a circle in all directions; and the centre of this sphere is not in the pole but in the centre of the stone and of the terrella". (III.27, p. 150)

He is not plotting a path of the rays from the centre of the terrella but rather showing us that the rays of force are found in a spherical space centred on the terrella's own centre. His remarks in II.6, p. 121, are quite clear:

"The terrella sends its force abroad in all directions . . . but whenever iron or other magnetic body . . . happens within its sphere of influence it is attracted . . . Such bodies tend to the loadstone not as toward a centre nor towards its centre: that they do only at its poles i.e. when that which is attracted and the pole of the loadstone as well as its centre, are in a right line. But in the intervals between they tend to it in an oblique line, as seen in the figure below, wherein is shown how the force goes out to the magnetic associate bodies within the sphere." (the figure shows short iron pins standing on the terrella at their various angles)

The quotation answers Benjamin directly. (In view of the fact that in the spherical magnet the poles actually lie fairly near the centre, Gilbert, who appears to have believed them to be on the surface, would actually not have been too far out had he said what Benjamin claims, viz. that the terrella's centre was the centre of the attractive power.)

Benjamin further asserts (ibid., after referring to Peregrinus' locating of the poles by plotting the needle's direction):

"At the equator, the needle stood at right angles to this position [the sphere's equatorial diameters], and between the equator and the poles it assumed various inclinations to the latter. Of course, a needle put successively in different places along a meridian of the terrella would map out, so to speak, the direction of the lines of force from pole to pole. But Gilbert did not perceive this any more than he saw the inconsistency between his theory and his experiments; for clearly, if the magnetic virtue emanated radially from the centre of the terrella, his needle should always point to the centre and so take the same position at the equator as at the poles."

In fact, Gilbert clearly saw that the needle placed along the meridian plotted lines of force from pole to pole (he describes the magnetic meridians in detail on p. 126-7). And he does not claim that the virtue emanated radially from the centre of the terrella.

The matter would have been hardly worth a detailed discussion if Benjamin had not claimed that Gilbert had allowed his theory to blind him to the evidence. As this thesis is particularly concerned with the role of theory and experimental evidence in this very early work, it seemed necessary to deal with the question.

D. The field's character.

Gilbert investigates the field in some detail and is aware of the novelty of his concept (if we ignore Norman's and Porta's vague ideas on the matter):

". . . we have by good fortune discovered a new and admirable science of the spheres themselves - a science surpassing the marvels of all the virtues magnetical. For such is the property of magnetic spheres that their force is poured forth and diffused beyond their superficies spherically, the form being exalted above the bounds of corporal nature . . . The potencies of a terrella, too, are of the same kind throughout the whole sphere of its influence . . . "(p. 304 in V. 11, entitled "Of the formal magnetic act spherically effused"; large aserisk))

This refers to imaginary spheres arbitrarily chosen in the space around the terrella and concentric with it. He draws the circumference of a terrella and the alignments of needles in eight positions directly on it (two at the poles, two at opposite sides from one another at the equator, and four intermediate between poles and equator (cf. fig. 11, after that on p. 306). The figure shows another three concentric circles, to represent some of the imaginable spheres in the field, and the alignments of the needles on these circles at the respective ends of the prolonged eight radius vectors on which the terrella's needles were shown (only the radii of the polar axis and equatorial plane are actually drawn). At the polar and equatorial positions the needles align in the same directions as those on the terrella itself. In the intermediate positions they are also parallel to the corresponding directions of the needle on the terrella and in each case point along the chord to the equatorial position opposite on the respective circle, *not to that on the terrella*. In other words, the needles behave on each of these three spheres as though they were placed on the surface of a terrella of the size of the circle. Their directional chords miss the actual terrella altogether. As he says,

"the versorium, as it there moves into various positions in the same circle, will always have regard to the dimensions of that sphere and not those of the terrella, as is seen in the diagram of the effused magnetic forms" (p.307); [it] "regards its own sphere in which it is placed and its diameter, poles and equator, not those of the terrella". (p. 305)

This is a most important fact. At the terrella itself the needle points toward the stone (except at the equator). The present experiment shows that the (outer) field has a so to speak independent character,

"as though the spheres of influence were solid materiate loadstones . . . and so the spheres are magnetical, and yet are not real spheres existing by themselves". (p.305)

The spheres "may be imagined as infinite" (ibid.) by which he means that they could be pictured as infinite in number inside the *orbis virtutis*.

His experiment reveals a

"diagram of the forces magnetical effused by the form . . . and this, though it is subject to none of our senses and is therefore less perceptible to the intellect, now appears manifest and visible before our very eyes through this formal act [of aligning the needle] . . ." (p.307)

All this, if taken together with his other remarks about the *orbis virtutis*, displays his conception of the magnetic field. "Here in Gilbert's own words is the moment of birth of the true field conception" as Harré (1981, p.54) says with reference to some of the statements just quoted, though Harré does not deal in his short chapter with the details and difficulties which some of Gilbert's formulations represent, and which caused Roller and Hesse to come to different conclusions. These problems will have to be examined. For Gilbert's formulations are at times less than clear.

Roller homes in on the sentence quoted above (first quotation, section B.) from II.7.: *"nec tamen in rerum natura subsistit orbis aut virtus per aerem fusa permanens, aut essentialis"* and translates "Nevertheless there exists in nature no orb or permanent or essential virtue spread out through the air" (Roller, op. cit., p.148), thus misleadingly using 'exists' instead of 'persists', the term Motteley uses. Motteley's translation implies correctly that the virtue be present as long as the magnet is, although it would of course not remain there ('be permanent') once the latter was removed. It is perhaps unnecessary to speculate on what 'essential' may mean, perhaps it just indicates effectiveness.

Roller states: "Gilbert . . . is not mapping a magnetic field: he is exploring the sphere of influence" (p.150) and "is at best able to say that a magnet simply possesses the property of acting at a distance upon a magnetic" (p.153). This appears, however, to be contradicted by Gilbert's "the magnetic energy is present within the limits of its forces [i.e. the orbis virtutis]. . and . . being . . present solicits bodies which are in amicable relation to itself" (II. 7, p.123). There is little doubt, then, that his magnetic force filled the space around the magnet. It seems to me that one should not speak of an 'action at a distance' view, when Gilbert says that the energy is present in the orbis virtutis.

Hesse (1961, p.91) comes to a conclusion similar to Roller's and sees Gilbert as holding an action at a distance view, adding "Hence it would be misleading to ascribe to Gilbert any kind of continuum or field theory". In support she quotes a passage from p. 305, *de Magnete* (she uses the Thomson translation where the page is 205):

"Still we do not mean that the magnetic forms and spheres exist in the air, or water, or any other medium not magnetical, [as though the air or water took them on or were by them informated - regrettably she omits this important clause] for the forms are only effused and really subsist when magnetic bodies are present".

In the light of the passage which Hesse left out, the first two clauses together mean that air and water are not in any way shaped or altered or affected magnetically. The last clause refers to the forming (i.e. aligning or inductive) influence of the magnet. For as Gilbert had said that the energy was present and the force poured forth, we cannot here equate 'magnetic *forms*' with 'magnetic *energy*' without unnecessarily involving him in a self-contradiction. He claims that the magnetic energy is present in the space around the magnet but that it does not shape the medium into spheres or any other forms. However, if we place the needle in various positions on an imaginary sphere, we will see its alignments in ways which suggest such a shape. Although his formulations are not quite clear, the choice of the different terms 'energy' and 'force' on the one hand, and 'magnetic forms' on the other, is surely no accident. The medium is not 'formed'. The space is filled with the energy which has a formal, ordering, power so that a magnetic there is ordered (in two senses of the word, externally by taking direction but also internally by induction, the ordering of its matter). But although the energy is present, its ordering aspect can only be said to become effective ('really subsist' as 'magnetic forms') when a magnetic is there. We may therefore paraphrase Gilbert's sentence: 'Still we do not mean that in any non-magnetic an ordering effect occurs, such as would for example show itself as spheres, for these ordered features are only manifest with magnetic bodies'. The quotation from p.217

"neither has the energy of the loadstone entrance into their interior [i.e. that of non-magnetics], nor are their forms excited magnetically; nor, if it did enter in, could it effect aught . . ."

is ambiguous, but the final clause and his claim of the energy's presence seem to allow for the energy's travelling through the medium.

All this is again stated in a way which does not clarify matters much more:

"hence the magnetic body within the forces and limits of the spheres is taken hold of, and in the several spheres magnetic bodies control other bodies magnetical and excite them even as though the spheres of influence were solid, materiate loadstones; *for the magnetic force does not proceed through the whole of the medium, nor exists really as in a continuous body*; and so the spheres are magnetical, and yet are not real spheres existing by themselves." (p.305, my italics for purposes of identification)

The interpretation of the italicised clause is obviously important. This may mean that Gilbert wanted to stress the fact that the energy is differentiated in an infinite number of levels (he had just said that the number of spheres may be imagined as infinite), so that it cannot have the character of a continuous body. Of course the force cannot spread through the 'whole of the medium' as though this was a conductor. On the other hand the formulation allows specifically that it at least be present in part of it, i.e. in the orbis virtutis. His remarks on the field are difficult to understand and represent an exception to the general clarity of his expositions.

What makes it justifiable to ascribe to him the concept of a field in spite of this is - among his other ideas - the postulation of a specific, directed, level of energy for any point of the space which can be made palpable by placing a test body in this position. Gilbert was familiar with Porta's experiments of iron filings and their ordering around a magnet. He describes relevant experiments in II.33, without referring to Porta. The filings filled the space closely and showed the presence of the ordering energy with its changing directions everywhere in it. Gilbert claims that the "force [of the magnetic spheres] is poured forth and diffused beyond . . ." and says that the energy is present everywhere in the orbis virtutis. A "true action at a distance view" which Hesse ascribes to Gilbert would not choose such formulations because it can confine itself to dealing with the effects of one body on another.

Gilbert chooses light as a comparison to the magnetic effect:

"And as light impinges on whatever confronts it, so does the loadstone impinge upon a magnetic body and excites it . . . In the absence of light bodies and reflecting bodies, the forms of objects are neither apprehended nor reflected; so too, in the absence of magnetic objects neither is the magnetic force imbibed nor is it again given back to the magnetic body." (II.7, p. 123/4)

The comparison to light has led Heilbron to reject an action at a distance view of Gilbert's conception. Heilbron says:

The scheme differs from multiplication of species chiefly in the medium of propagation, which in the old representation was material, and in Gilbert's appears to be the space or 'incorporeal aether' surrounding the earth" (1979, p.172).

I have tried to show that we can best understand Gilbert as holding an early sort of field theory. He does so in the sense appropriate to a first attempt, believing that the magnetic energy is present in the space surrounding the magnet, where it will show itself when it has an opportunity to affect a magnetic. That his field does not have the same character as Faraday's over two centuries later is not surprising. Hesse refers to Faraday's statement "to the effects that lines of magnetic force exist as 'a condition of space free from . . . material particles'" (her ref. to Faraday, Experimental researches, Vol.III, p. 414), saying that Gilbert would certainly have rejected such a statement (Hesse, 1960, II, p. 138). But Gilbert would have gone along with this to at least some extent as his reference to the energy's presence in space (incl. the vacuum) shows. As we saw, he had also spoken of the 'rays' of the force, a conception reminiscent of lines or force, although he had no proper conception of fieldlines. Too strict a distinction between 'condition of', and 'presence in', space should not be drawn where one is dealing with a first attempt at the postulation of a kind of field. (In any case, Faraday's particular conception represents only one of the possible ideas on the field.) All told, one may also say that Gilbert's understanding of the field had less of an operational character than one might have expected.

E. Gilbert's field as a theoretical entity.

I can now summarize the field's properties as Gilbert conceived of them:

The magnetic field is a field of 'virtue', a forcefield, whose effects show themselves in two ways: firstly it acts through attraction, repulsion, and direction, where the latter effect is once described in terms of both the former ones ('the earth's poles by attracting and repelling arrange . .'), whilst elsewhere he sees repulsion as a form of the directional force; secondly the field has the capability of magnetising other magnetics, which instantaneously creates new fields around these. This induction is a precondition of the first mentioned forces and effects in the case of bodies of iron and steel.

The field needs no material carrier and is only affected by other magnetics. Its shape roughly follows the shape of the magnet. It does not persist in a space or medium once the magnet is removed.

The field is divided into two equal halves. That on the side of the north pole of the magnet induces a south pole, that on the southern side a north pole in the near end of a magnetic.

From these general properties important magnetic phenomena can be deduced provided the initial conditions are known. His magnetic field is a genuine theoretical entity (as Watkins also mentions; op.cit., pp. 189-190). It shows itself in the space surrounding the magnet through properties whose effects are predictable. It is differentiated as to levels and directions with respect to these properties, and by postulating specific experimental arrangements, one can make predictions as to what phenomena will be observed in certain chosen respects. One would have to know the approximate strength, pole positions, and the shape and size of the magnet whose field it is. If one asks what will happen to a magnetic placed in a certain position in this field, the theory will make a qualitative prediction as to its behaviour. The phenomena will depend in a manner specifiable in advance on the make-up, size, shape and proximity of the object and its position relative to the poles. If it is pivoted, it will align itself in a predictable direction (at least if the magnet is of a shape already investigated) and will become magnetised to a degree of strength and permanence depending on the nature of the material, its location, and the time it is left in the field. The signs of the two poles induced in it will be predictable as a function of its position. If it is free to move translationally it will do so if it is inside the orb of coition. Thus every region of the field has a certain character which can be at least approximately specified by its relevant qualitative and even semiquantitative effects.

Although it involved the two forces of coitional attraction and direction, which in Gilbert's form do not accord with our conception, together with the inductive power the field's character could account for the macrophenomena. For the first time in the history of science a theoretical entity had been specified to this extent, a truly revolutionary advance. This was achieved by careful and comprehensive experimentation which no doubt falsified many a working hypothesis Gilbert must have formed, leaving a set of qualitative assumptions to specify no more than appeared necessary.

Chapter Seven

The Earth as a Magnet.

A. Earlier ideas on terrestrial magnetism.

As is widely agreed, the most important individual result of Gilbert's researches was the discovery that the earth acts as a huge magnet. It is of interest to examine the facts which gave rise to this idea and the observations employed to test it, though we cannot now separately identify these reliably in all instances. The invention of the hypothesis has its roots in three sources: the behaviour of the compass needle, that of iron objects subject to the earth's field, and the experiments concerning the terrella.

Knowledge of the movements of the compass had not suggested the idea of terrestrial magnetism at the end of the 16th century. An assessment of the hypothesis, had it been thought up, would have been almost impossible without Gilbert's work with the terrella. The compass needle had been known for centuries and its behaviour variously ascribed to the influence of magnetic mountains, the heavens (Peregrinus), or more specifically the pole star.

Magnetic mountains had been held responsible by Fracastoro who read about them and had seen them drawn on maps. The evidence was based on reports from travellers (cf. Fracastoro, op. cit. 76 A & B). To the argument that the compass needle did not point to magnetic islands when ships passed by, he had answered that the hyperborean magnetic mountains were incomparably bigger and therefore directed the needle in spite of their great distance. Even Norman's experiments and his discovery of the dip, which would have thrown considerable doubt on any theory concerning magnetic mountains and did not seem to support that of celestial poles, did not lead to the correct idea until Gilbert considered the problem.

There are two earlier possible hints at some kind of magnetic terrestrial properties. The 13th cent. writer John of St. Amand, in a medical commentary, had said that in the magnet

"there is a trace of the *world (orbis)*, wherefore there is in it one part having in itself the property of the west, another of the east, another of the south, another of the north. And I say that in the direction north and south it attracts most strongly, little in the direction east and west." (cf. Lynn Thorndike, 1946)

John's views are not mentioned by Gilbert who probably did not know his work, though it was printed in 1508. It is not clear whether 'orbis' refers to 'world' or 'earth'. In any case, John's suggestion is extremely vague. Mercator had written in a letter that the earth may have a magnetic pole, but this was not published until the 19th century.

B. The role of the terrella.

The claim by several writers that Gilbert experimented with a spherical magnet in order to support his thesis that the earth was a magnet, presupposes that Gilbert first formed the hypothesis and then made his terrella to 'prove' or test it. Writers who suggest this, would be under an obligation to show a plausible reason why Gilbert should have formed the hypothesis without work with the terrella, when it is obviously much more likely that the terrella suggested the idea. Gilbert probably repeated Peregrinus' investigations with a spherical stone, noticed some of the analogies of the compass-needle's behaviour and then tested his hypotheses, as I have already suggested. The analogy of terrella and earth was vital because ready experimentation or new observations with respect to the earth did not seem possible. It is interesting that Gilbert discovered only two observational phenomena pertaining to the magnetism of the earth: the degree of induction in iron by the earth and the effect of the earth's field on the strength of the pole of a terrella (see quotation from p.163 below). But his work with the terrella showed many effects which mirrored those known from the behaviour of the compass needle.

The relevant analogies, apart from the obvious one of shape, consisted in three main phenomena:

the north-south direction of the needle;

the dip;

the magnetisation of pieces of iron.

Gilbert's early work will have enabled him to see some of the similarities of effects and patterns of the behaviour of his test needles and iron bars near earth and terrella. On their appreciation he may have developed his idea of the earth's magnetism and then formed hypotheses concerning further analogous behaviour for which he thought up appropriate tests. There were, however, also important disanalogies to be resolved.

The fortunate choice of a spherical magnet was almost a precondition for his discovery of terrestrial magnetism. The pole to pole alignment of the needle on the terrella was of striking similarity to that in the approximate direction of the lines of longitude on earth. The dip was a very remarkable effect which showed itself readily near the terrella, and had been found by Norman for the earth. (In fact Norman was not the first to discover it. It was described by the German astronomer Georg Hartman in a letter to Duke Albert of Prussia in 1544;

but this will not have been known to Norman or Gilbert.) Gilbert sees the phenomenon of dip as a decisive matter (VI.1, p. 314):

"The dip of the magnetic needle (that wonderful turning of magnetic bodies to the body of the terrella by formal progression) is seen also in the earth most clearly. And that one experiment reveals plainly the grand magnetic nature of the earth, innate in all the parts thereof and diffused throughout. The magnetic energy, therefore, exists in the earth just as in the terrella, which is part of the earth and homogenic in nature with it, but by art made spherical so it might correspond to the spherical body of the earth and be in agreement with the earth's globe for the capital experiments."

Crucial for an appreciation of the analogy of dip on terrella and earth was his knowledge that the dip on earth varied with latitude, as it did around the terrella. Of this phenomenon he must have learned from sailors' observations made in the years after Norman's discovery. Magnetic mountains, if situated at the earth's poles, could not explain the dip and in any case Gilbert knew that such mountains would have to be neutral at any distance.

Because the needle's horizontal alignments around the terrella mirrored those on earth, the further similarity of its inclination from the horizontal in the two cases appeared most striking:

"As in other magnetic movements there is strict agreement and a clearly visible, sensible accordance between the earth and the loadstone in our demonstration, so in this inclination is the accordance of the globe of the earth and the loadstone positive and manifest." (V.1., p. 278/9).

However,

". . . this movement is produced not by any motion away from the horizon toward the earth's centre, but by the turning of the whole magnetic body to the whole of the earth." (p. 276)

Norman was therefore mistaken when he

"originated the idea of the 'respective point' looking, as it were, toward hidden principles, and held that toward this the magnetized needle ever turns . . . albeit he exploded the ancient false opinion about attraction." (IV.6, p. 244)

Gilbert measured the dip at various positions around the terrella and found that its amount does not correspond to the degree of latitude. Yet he discovered a relationship between the needle's directions and the changing latitudes when it is moved from the equatorial position toward a pole:

"On the equator the magnetic iron stands in horizontal equilibrium, but toward the pole on either side of the equator, at every latitude from the beginning of the first degree even to the 90th, it dips; yet not in ratio to the number of degrees or the arc of the latitude does the magnetic needle dip so many degrees or over a like arc; but over a very different one, for this movement is in truth not a dipping movement, but really a revolution movement, and it describes an arc of revolution proportioned to the arc of latitude."(V.6., p. 292/3)

So that a versorium

"while it passes round the earth [should one admire the boldness of this claim?], . . or a terrella, from the equator toward the pole, rotates on its centre and midway in its progress from the equator to the pole points to the equator [opposite its starting point] as the mean of the two poles; therefore ought the versorium to rotate much more quickly than the centre travels in order to regard . . . [the opposite equator point] in a right line by rotating. For this reason the movement of this rotation is quick in the first degrees from the equator . . . but slower in subsequent degrees" (V.6, p. 293)

This was so because - as he had in effect said already on p. 276 (see quotation above) - the versorium did not regard a force from the terrella's centre (for in that case it would have always pointed to the centre),

"but it obeys the whole and its mass and outer limits, the powers of both cooperating, to wit, those of the magnetized versorium and the earth." (p. 294)

He then gives diagrams to describe the relationship between dip and latitude, and from which he believed latitude - the dip being known - could be read off for any place on earth. This he considered a most important discovery:

"We can now see how far from idle is the magnetic philosophy; on the contrary, how delightful, how beneficial, how divine! Seamen tossed by the waves and vexed with incessant storms, while they cannot learn even from the heavenly luminaries aught as to where on earth they are, may with the greatest ease gain comfort from an insignificant instrument, and ascertain the latitude of the place where they happen to be." (V.8, p. 298)

His investigations of the dip on the terrella and in its wider sphere of influence had been very thorough, or so it appears, for he devised a special instrument for measuring and relating dip to latitude on the terrella (V.3, p. 285ff, asterisk). For use on earth he describes Norman's inclinometer without even mentioning the inventor of this important instrument.

His unfortunate suggestion that latitude could be found from observing the dip, applauded in Edward Wright's address at the beginning of de Magnete, may have done his reputation some harm in subsequent decades. For when it was found that latitude could not be so discovered, a doubt may have been cast on Gilbert's abilities as a researcher with a consequent neglect of his work (his claim that variation did not change at a given place may also have contributed to this after the opposite fact had been discovered).

The variations in dip at different places on the same latitude were - like the declination which I will deal with at greater length below - due to irregularities in the earth surface such as high elevations or "magnetically powerful earthmass" or when "elevations have less force than is called for by the general constitution of the globe, or when the energy is overconcentrated in one part, and in another is diffused, as we may see in the Atlantic Ocean" (V.10, p. 303/4); "and this discrepancy of constitution, this variance of effect, we easily recognize in certain parts of every spherical loadstone." (p. 304)

So this indicates to him a further analogy between earth and terrella.

The changes of the dip with different positions on the same latitude of which Gilbert knew were in fact mainly the systematic differences which exist because the earth's magnetic and geographic axes do not coincide. It is not clear what reports he may have had from seamen about the amount of dip in different parts of the world. He probably did not obtain many values. His claim that latitude can be determined with the help of his inclinometer is the result of his assumption that the analogy with the terrella must hold as far as inclination is concerned. And an analogy in principle does of course exist. The isoclinals, or lines of equal dip of the compass needle, are much more regular than the isogonals, the lines of equal variation, and approximate to circles on a sphere. But these circles do not coincide with those of the latitude which they cut.

If he had had sufficiently many numerical values, he would have seen that his method was flawed for some systemtic reason. He was somewhat careless in recommending an untested method for the vital problem of finding latitude in bad conditions of visibility. It would have been possible to obtain values for the dip at least for various places in England and Europe as well as the nearer seas. He therefore neglected his own principle of always checking observationally what reasoning suggested in the light of the obvious analogy (to the terrella) in this case.

C. Experimental evidence for terrestrial magnetism.

An iron bar or needle is magnetised when left in a terrella's field, as are iron bars lying in the north-south direction on earth. This could not be an effect of magnetic mountains near the north pole, again because of their neutrality. A celestial field could perhaps induce magnetism in the iron bars just as a terrestrial one, but Gilbert does not even remark on this as a serious possibility.

An interesting observation suggesting support of the theory of the earth's magnetic character is described in II.33, p. 163:

"In northern latitudes raise the true north pole [of a terrella] above the horizon straight toward the zenith [so that the terrella's south pole is facing the northern hemisphere of the earth]. Plainly it holds erect on its north pole a larger bar of iron than could the south pole of the same terrella if turned in like manner toward the centre of the sky. The same demonstration is made with a small terrella set atop of a large one."

(In the preceding sentence he suggested the same experiment with an oblong stone, to show a like effect.) This experiment, showing that the strength of the appropriate pole of a magnet is increased by the earth's field, is important. The behaviour observed in an experiment with a large and a small terrella, which he also describes here, is exactly alike, the larger's stone's field assisting the coitional power of the smaller in an arrangement similar to that of the earth-terrella case. Gilbert gives the description of these experiments two large asterisks.

In an important experiment he reports marking the ends of a 20lbs. loadstone found in a mine, noting their direction in situ and then later checking on the alignment when the stone floated in a boat after it had been brought to the surface. He found that the end which pointed north in the mine then still pointed north. Yet a piece cut out of a terrella and suspended near to it will take on an opposite direction to the one it had in the integral sphere. The reason for the difference was that it was not the whole earth which was a magnet but only the sphere of the telluric element in its interior (this also explained why the earth's surface was outside a terrestrial orb of coition). In the earth's crust this element was corrupt and had decayed so that stones being magnetised by the inner magnetic sphere really behaved just like an iron bar being magnetised on the earth's surface. But a piece of material cut out of a loadstone was itself part of the stone whose polarity it therefore has.

These experiments and their interpretation were an important part of the tests of the idea of the earth's magnetic character. For pieces had been chopped off loadstones before and the observation that their alignment relative to the stone became reversed (as Porta found) would have cast serious doubt on an hypothesis of terrestrial magnetism if it had occurred to anybody. It was not possible to equate the earth to the terrella in some important respects. The former did not attract like a loadstone and the latter's polarity in the mine was opposite of that of the earth.

Norman had found the important effect of inclination which showed Gilbert an analogy to the dip near terrella. Inclination had to differ at different latitudes, and this was found to obtain in the years between his construction of an inclinometer and the completion of Gilbert's work. (Norman had thought the point respective would be found by prolongation of the directions of the dipping needle in different places.) Norman's discovery of the absence of attraction, however, might have seemed to make the thesis that the earth was a magnet even less likely. It took Gilbert's detailed experiments in the orb of virtue *outside* that of coition to show how a small needle aligns around the terrella, to make sense of both inclination and the absence of attraction on earth. If the compass needle was outside the orb of coition on earth then the magnetic body of the earth had to be well below the surface. And in that case the magnetic alignment of a loadstone not too far underground would have to be contrary to that of the earth as a whole. For the stone could not have kept its polarity (if it had originated from the magnetic core) in the direction of the earth's field for long periods, and it had to have an opposite magnetisation if it had first received its virtue in its position in the mine.

D. The problem of variation.

The important facts concerning the analogies between terrella and earth have now been considered. Variation, to which Gilbert devotes the whole of book IV, represents one of the disanalogies in the behaviour of the compass needle near the ordinary terrella and the earth. Because of its importance it will have to be examined in some detail.

The phenomenon of variation or declination had been known for a long time and had presented a problem for navigation. The difficulty for Gilbert was one of principle: if the earth was a great magnet on which magnetic and geographic poles coincided - as he believed - how could the needle fail to point to the correct position? His answer is that terrestrial surface features account for the effect. The continents, containing large quantities of terrestrial matter raised high above the level of ocean floors, would affect the needle's direction. In the northern hemisphere the north-pointing end of the compass would be attracted a little towards the large continental masses, in the southern it would be the needle's other end. Gilbert says in IV.1, p. 234:

"From the coast of Guinea to . . . Norway, the land on the right and to the east is all continent, vast regions forming one mass; and on the left, immense seas and the mighty ocean extend far and wide: now we should expect that (as has in fact been observed by diligent investigators) magnetic bodies would deflect a little eastward from the true pole toward those more powerful and extraordinary elevations of the terrestrial globe. Very different is the case on the east coasts of North America, for from the region of Florida . . . to the north, the needle turns to the west. But in the mid spaces, so to speak, for example in the western Azores, it regards the true pole."

(The null variation at the Azores was supposedly discovered by Columbus and had presented him with problems, for his sailors worried about this discovery and feared navigational difficulties; A. Crichton Mitchell, op. cit., part II, gives the sources but points out that there are considerable doubts as to the interpretation of the evidence regarding the author or date of the discovery. The agonic line, the part of the meridian with null variation, was shown on a map published by Cabot in 1544 (same source)).

It was a consequence of Gilbert's arguments that on the great continents the amount of variation could also change from place to place but would be about zero in their central regions (IV, 19). Gilbert does not mention factual evidence for this. The degree of variation also differed with the latitude; nearer the poles it was greater:

"Other things being equal, variation is less along the equator, greater in high latitude, save quite nigh the very pole. Hence it is greater off the coast of Norway and Holland than off Marocco . . . [there follow some numerical examples of the variation at different places]. For just as, when the direction is true, magnetic bodies tend toward the pole (the greater force and the entire earth co-operating), so do they tend a little toward the more powerful elevated parts under the action of the whole and in virtue of the concurrent action of their iron." (IV.2, p. 235)

The greater variation in circumpolar regions was due to the fact that "direction is weaker at the poles, because the versorium, by reason of its tendency to turn to the pole, dips greatly, and is but feebly directed; but the force of the lands and eminences is strong, with an energy proceeding from the entire earth, and besides, the causes of variation are nearer: therefore the versorium deflects more to those eminences . . . Direction becomes weaker and at the pole itself is null. For this reason a weak direction is easily overcome by powerful causes of variation, and near the pole the needle deflects more from the meridian." (IV.10, p. 254/5)

He does not tell us what values he had for variation near the pole.

On the facts known to Gilbert, the general hypothesis concerning variation seemed to account reasonably well for the phenomena of declination, and a lesser experimenter would have left it at that. He, however, considered that if his hypothesis was correct, then it should somehow be reproducible on the terrella. So he made one with slightly raised irregularly shaped imitation-continents and found that the needle on its surface was in fact deflected toward them in analogy to the deflection he perceived to show itself toward the continents on earth. He found similar effects on terrellae with hollowed out or imperfect or 'decayed' regions (imitation seas). A needle placed in such a place would only point to the pole if it was in the centre - as at the Azores - otherwise it would be deflected to the nearer edge of the sound material of the stone. Gilbert investigates all these phenomena in some detail and finds that the explanation of variation on earth accords well with those observed on the terrella. And indeed, his reasoning and experimenting are ingenious and appear at first sight seductively sound. It was not, perhaps, too unlikely that the great continental masses might somehow affect the needle's alignment and the reproduction of the effects on the terrella seemed to clinch the matter. At bottom, variation would be due to the fact that the earth was not a perfect sphere, as were sound terrellae.

Yet exactly how the continents achieved the effect, and how this compared to the situation on the imitation earth terrella, had to remain vague. Gilbert knew that islands, even those like Elba which contained many loadstones, did not deflect the compass, "albeit they are more magnetic than the sea" (IV.5, p. 243). He makes several remarks pertaining to the question:

"direction being a movement produced by the energy of the entire earth, and not due to attractive force of any prominence but to the controlling power and verticity of the whole mass, therefore variation (which is a perturbation of the directive force), is a wandering from the true verticity and arises out of the great inequalities of the earth . . " (p. 243); ". . by reason of the position of countries and the differing nature of the uppermost parts of the earth's globe (certain more magnetic projections of the terrestrial sphere prevailing), variation is ever fixed in a given place, but it differs and is unequal between one place and another, for the true and polar direction, having its birth in the entire globe of the earth, is slightly diverted toward particular eminences of great magnetic force on the broken surface." (IV.3, p. 242).

It was therefore not simple attraction of the continents but rather their disturbance, by reason of their vast masses and inhomogeneous material make-up, of the uniformity of the earth's field which produced the effect. This explanation leaves matters vague enough to allow for almost any uncertainty and unpredictability of the amount and at times the direction of the variation in different places. It postulates only the roughest qualitative connection of the distribution of sea and land masses with the variational values he knew. However, more could perhaps not have been asked from Gilbert.

Where he makes a palpable mistake, though, is in the evaluation of the supposed evidence of the effects of the eminences on the terrella. These are very much higher relative to the terrella's size than the height of the continents to the diameter of the earth. And the prominences here consisted of solid loadstone. So the analogy was in fact far from perfect. There is another objection he overlooked: on the terrella the needle's variations due to eminences or areas of decay would have been within the orb of coition even with a weak stone. Yet on earth they are all outside it. Would he have seen variation near a terrella in the weaker parts of the field beyond the orb of coition even in the presence of raised areas or of regions of decayed loadstone? Gilbert does not investigate, and had he done so, he could not have discovered an effect. We must conclude that his reasoning in all this was too hasty.

The question of when, how, and why magnetic phenomena on terrella and earth would be analogous in detail is one which could not have been answered until much more was known about terrestrial magnetism. One aspect of the problem I have just mentioned was, however, clear: the positive analogy between earth and terrella could only concern the outer parts of the latter's field. Gilbert could have seen that his comparison of the effects on the field of the terrestrial continents with that of the protruberances on the terrella was defective as far as a causal explanation was concerned. (Interestingly, though, he was not wrong in assuming that the masses of the continental material influence the magnetic field, albeit in ways and to a degree which he could not possibly have discovered.)

Gilbert's comparisons of known amounts of variation was however not without some practical value in disproving some false assumptions. Fracastoro, to whom Gilbert does not refer in connection with the problem of variation, had explained the effect by assuming that large magnetic mountains on the geographical north pole attracted the needle too feebly to make it point to true north both east and west of the agonic meridian. This was due to the distance to the pole. He implies that the Azores are situated in a straight line to the pole (it is quite obscure why this should be), and only here could the attraction be strong enough to line the needle up to due north. The idea is that it would take a certain minimum amount of force to make the needle turn sufficiently to point to the polar magnetic mountains. In other places - so he seems to imply - the force of attraction would be only partially successful in turning it. (Referring to the attraction, he says *"Propter distantiam autem quum debilis sit, non moveret quidem magnetem, nisi esset in perpendiculo . ."*, op.cit. ch.7, 63.A. There is a drawing in the 2nd edition of *Opera Omnia* showing the earth, a line from the pole which presumably passes through the Azores, and one geographical position east and one west of this from each of which one line points to the pole and one to the point of variation for that position.) Porta had said that longitude could be found by observing systematic changes of variation because the further east we are, the more the needle varied eastward. He says in ch. 38, book VII, op. cit:

"if . . . sailing under the equator we do observe the chief motions of the Needle, and the declinations of it, and shall accomodate the same to the proportion of our Voyages, we shall easily know the Longitude of the world, beginning from the Fortunate Islands [the Azores] whence both Longitute and Latitude in dark nights, and the greatest Tempest may be certainly discovered".

(This idea may have been based on Fracastoro's explanation which appears to allow for a systematic relationship between position and variation.) It was of course a reckless claim, and Gilbert takes him to task for his "vain hope and baseless theory" (IV.9, p. 251). For

"variation is in divers ways ever uncertain, both because of latitude and longitude and because of approach to great masses of land, also because of the altitude of dominant terrestrial elevation; but it does not follow the rule of any meridian, as we have already shown." (p. 252)

He then criticises Stevinus who in 1599 had given values for the variation at several places and proposed navigational methods based on them. The values Stevinus gave, says Gilbert, are wrong, not the entire meridian of the Azores was an agonic line and Stevinus made many errors in his book. But Gilbert concedes that

"the method of finding the port on long voyages to distant parts by means of accurate knowledge of the variation (a method invented be Stevinus and recorded by Grotius) is of great importance, if only fit instruments be at hand wherewith the deviation may be positively ascertained at sea." (p. 254)

Gilbert describes ways of accurately determining variation with the help of two or three instruments and a table of star positions. These should not be thought of as leading to a general rule for determining position from variation, yet could perhaps be of help in specific cases. For if variation at a certain place was known accurately, it could occasionally serve as a check on that position determined in other ways, a recommendation already made by Norman. This would be possible because, as he, like Norman, insisted, the amount of variation does not change over time at a given place (a claim that before too long was found to be mistaken, for Gellibrand discovered that the variation at London had diminished from eleven degrees in 1580 to four degrees in 1633). Gilbert's theory of the causes of variation would have seemed in agreement with his view that it cannot change, and he also says that on an uneven terrella a very small needle shows no change in the variation at the same place over several tests (IV.3, p. 241).

In book IV we find an example of his putting his theory of the causes and his knowledge of the amount of variation to practical use: a northern passage to the East was possible, for the variation in the far north was so greatly westward that there could be no large land mass extending to the east here (IV.16, p. 269).

Gilbert reached the correct conclusion about the planet's magnetism because he had clearly seen how far the situation on earth is analogous with that on the terrella and exactly where the analogy breaks down or needs supplementing with further assumptions (even though he had not been able to do so with the more subtle problem of variation and had made some premature assumption concerning inclination). Gilbert's hypothesis that the earth has a magnetic core is the result of perspicacious reasoning based on thorough knowledge of numerous magnetic facts, principally derived from the work with the terrella.

The practical and theoretical importance of Gilbert's discovery of terrestrial magnetism need not be stressed. I will, however, return to its significance for the relationship between terrestrial physics and cosmology in later chapters.

Chapter Eight

Gilbert's Electrical Researches.

A. Earlier electric work.

The characteristics of magnetic phenomena could only become completely clear to Gilbert if they were separated from those of electric effects in *all* respects. There were putative points of contact and overlap between the two areas which arose from ignorance of basic phenomena in both fields. It was, for example, not certain whether magnetism really concerned iron and loadstone only. What interested Gilbert most, however, was of course the difference in the manner of magnetic and electric attraction. To solve this problem, a detailed investigation of electric phenomena would be necessary.

A specific relationship between a non magnetic and the loadstone had been claimed for the diamant by Porta who stated that if the iron needle is rubbed with this, it would turn north. Gilbert disposed of this claim by experimenting "with seventy-five diamants in presence of many witnesses" (II.14, p. 218, asterisk). Fracastoro (1574) had seen the magnet attract silver (cf. op.cit.62.D). Cardan had already doubted this (Cardan, op. cit. lib.VII, p. 277):

"Refert & Hieronymus Fracastorius vidisse, quod argentum traheret: generaliter autem argentum haud quaquam trahit . . . argentum vero vel ferri aliquid continuit, vel genus erat alius Magnetis, de quo, ut re mihi incognita, verba facere non decrevi. Attamen hoc nostris satisfacit principiis, argentum & reliqua metalla a valido lapide si ferri quicquam contineant, trahi posse."

(H. Fracastoro reports to have seen that it [loadstone] pulls silver, but in general it does not do so by any means . . . either the silver contained a little iron or it was a different sort of magnet about which, as I know nothing of it, I feel I should not speak. But yet it satisfies our principles that silver and the other metals can be drawn by a proper stone if they contain some iron.)

Gilbert agrees (II.38, p.170). But Cardan speaks elsewhere of the attraction of silver by a special kind of magnet - and even mentions a flesh-attracting magnet (his "*magnes creagus*"). Gilbert disposes of all these ideas (II.37) and shows magnetic effects to concern only loadstone and iron and thus to be different from electric ones, a most important separation in the very early history of the subject.

Gilbert was interested in all forms of attraction also as a medical man, for the efficacy of many medicines had been ascribed to it, and medicinal properties had been claimed for the loadstone. There are numerous other problems which will have interested him in connection with the status of electric effects. Benjamin gives a well-considered list of the sort of questions he will have asked himself (Benjamin, op. cit. p. 295/6). I believe, however, that a more general problem was also very important to Gilbert: the basic or original terrene element was loadstone, but electrics and their effects were more widespread than magnetic ones. Were electrics after all fundamental in the earth's make-up, and was electric attraction a universal force?

The most important pre-Gilbertian advances in electric knowledge and in that of the differences between electricity and magnetism were reported by Cardan (1580, lib.V, p. 207):

"*Succinum . . . attrahit omnia levia, paleas, festucas, ramentatemia metallorum . . . Magnes ferrum solum . . . Succinum interposito corpore non movet paleam, Magnes ferrum. Succinum non trahitur vicissim a palea, Magnes trahitur a ferro etiam. Palea a succino in nullam partem dirigitur, ferrum modo ad Boream, modo ad Austrum contactu Magnetis tendit.*"

(amber draws everything light, chaff, straw, bits of metal, . . . the magnet only iron. . . amber does not draw chaff when a body is interposed, but the magnet draws iron; amber is not in turn attracted by chaff, the magnet is also attracted by iron; chaff is not directed to any particular part of the amber, the loadstone draws only towards its North and South poles)

These are most important results. Gilbert considers them without any acknowledgments and retests extensively, for as usual he does not accept any of the many contradictory reports concerning magnetism and electricity. Cardan had offered an explanation for the amber effect (p.207):

"causa est huius, humidu habeat pingue & glutinosum, quo emisso res sicca combibere cupiens, versus fonte, id est succinum ipsum movetur. Omne enim siccum postquam humidum combibere coeperit, ad ipsum etiam fertur, ut etiam ignis ad pabulum unde si fricetur vehementius, etiam trahit ob calorem."

(i.e., briefly: the attraction is due to the emittance of a fatty and glutinous humour from amber, for dry objects, after they have started to absorb moisture are drawn to its source like fire to its food. Therefore, when the amber has been violently rubbed, it draws because of the heat.)

But Cardan also likened the effect to that of the cupping glass (he does not explain how electrics could act like this) which is not consistent with this explanation, and Gilbert criticises him for it and for the assumption that the higher the amber's temperature, the stronger it draws.

Gilbert starts book II, ch. 2, the chapter devoted to electricity, with some remarks about attraction in general and the misuse of the term in medicine, the number of speculations about recondite causes and mysterious effects ascribed to amber and loadstones, and complains about the lack of experimentation concerning the amber effect. He derides philosophers for thinking that only amber and jet attract light bodies and then gives a long list of other materials which attract, such as diamant, rock cristal, opal, glass, sulphur, sealing-wax, mica etc. (large asterisk, p. 77), coins the important term 'electrics', and says:

"These several bodies not only draw to themselves straws and chaff, but all metals, wood, leaves, stones, earth, even water and oil; in short, whatever things appeal to our senses or are solid: yet we are told that it attracts nothing but chaff and twigs. Hence Alexander Aphrodiseus incorrectly declares the question of amber to be unsolvable, because that amber does attract chaff, yet not the leaves of basil . . ". (p. 78)

This is a characteristic statement: Gilbert ignores the progress that has been made since antiquity, in this case in particular the results reported by Cardan, and, as frequently when reading him, one gains the impression that he does not really admit authors such as Porta, Cardan, or Fracastoro as having advanced magnetic and electric researches much, if at all, presumably because of the many remaining uncertainties and contradictions, but probably also to accentuate his own intellectual *bella figura*.

B. The versorium.

To help him in the identification of cases of electric attraction Gilbert used a 'versorium', a pivoted needle "of any sort of metal, three or four fingers long, pretty light, and poised on a sharp point after the manner of a magnetic pointer" (p. 79). This electroscope is shown in an illustration, but he does not place an asterisk in the margin near its description. Heilbron (op.cit. p. 175) says that he "lifted" it from Fracastoro. Benjamin believes that Gilbert invented it himself. There seems to be no clear evidence for Heilbron's claim. The passage in Fracastoro's text which could be relevant (op.cit., 62.D) says that he used a *"pendulum quale navigatoria pyxis"*, i.e. a kind of compass needle *and that*

". . . ac manifeste vidimus magnetem trahere magnetem, ferrum ferrum, tum magnetem trahere ferrum, ferrum magnetem: porro electrum parva electri frustula rapere, argentum attrahere argentum . . . item Electrum non solum surculos & paleas movere ad se, sed & argentum"

(we saw clearly that the magnet attracts the magnet, iron iron, then the magnet pulls iron, iron the magnet: further, amber snatches small particles of amber, silver attracts silver . . . so does amber not only attract to itself twigs and chaff but also silver.)

This does not appear to justify the assumption that the needle was in turn made of magnetite, iron, and silver for the various trials. The first listed experiments are all magnetic ones, and the use of the compass needle for these was not new. Any novel design - like the possible fashioning of the needle from silver - can only have worked because of the adulteration of the silver with iron. As to the electrical experiments, it is possible that the amber was applied to a needle made of silver. This would have to be the implication of the *"Electrum non solum surculos & paleas movere ad se, sed & argentum"*. But as it comes after the *"porro electrum parva electri frustula rapere"* it seems to be a reference to an observation of attraction of little bits of silver rather than a silver needle, i.e. an electroscope. It is therefore very doubtful that Fracastoro invented the electroscope. But his text proves that he carried out various relevant experiments systematically, though his silver must have been of poor quality.

One's confidence in Fracastoro's observations in general is, however, somewhat shaken when one reads the next two sentences, which refer to magnetic observations:

"Vidimus quoque idem frustum magnetis per unam faciem magnetem trahere non ferrum, per aliam ferrum non magnetem, per aliam utrunque: quod indicium est in una parte plus esse magnetis, in alia plus ferri, in alia utrunque aequaliter, unde fiat diversitas illa tractionis."

(Gilbert paraphrases and comments on p. 114: "As for what Fracastoro writes, of having seen a bit of loadstone that on one side attracted loadstone but not iron, on another side attracted iron but not loadstone, and on another attracted both, - proof, according to him, that in one spot there was more loadstone, in another more iron, in the third the two were present equally; hence the difference in the attraction, - all this is utterly erroneous, and the result of mal-observation on the part of Fracastoro, who did not know how to present one loadstone to another properly.")

It speaks for Heilbron's theory that Gilbert did not place an asterisk in the margin, suggesting perhaps that Gilbert, at least, understood Fracastoro to be describing some sort of electroscope. But he may have thought that the pivoted needle was too common an instrument to deserve an asterisk even when it is made of a non magnetic and used in electric experiments. For as Fracastoro had said that amber attracted silver, he may have thought it not to be really original to make a needle of silver, and perhaps of other metals. Whatever view we take of the question, Gilbert's specification and use of the instrument gave its first proper design and application. It seems therefore appropriate to say that Gilbert invented the electroscope and that he could very well have awarded himself an asterisk.

The use of this needle, the first electric instrument in history, was very important, as it can show weak charges much more readily than can attempted attraction of small bits of matter. Induction in metals is strong, and the end of the needle is not immediately close to larger bodies on which the attractable objects in other experiments often lie. These bodies are themselves subject to induction when the rubbed electric is brought near, resulting in complicated patterns of charges.

A further advantage is that the versorium's induced charges do not remain and that it can therefore be re-used immediately for a new experiment.

C. Electrics and their properties.

Gilbert's main discoveries concerning electricity can be summarised as follows:

He showed that many substances attract electrically and gave a list of those he found doing so. He also identified some of those which don't (including emerald, marbles, flint, ivory, and some woods). As hard gemstones, for example, are found in the categories of both electrics and anelectrics the difference can not be due to superficial physical similarities between the groups.

Amber does not attract by heat ("for when heated at a fire or by the sun and brought near to straws, whether it is merely warm . . or even burning hot . . it has no attraction"; p. 80, asterisk)

Great heat otherwise also prevents attraction so that amber will not attract near a flame (nor will it attract the flame itself). On the other hand some heating seems to be required but it must be that caused by rubbing.

A large polished piece of amber or jet attracts, if not strongly, occasionally without friction (he does not consider that it may have received some friction at an earlier time); but in all other cases electrics seem to need rubbing before they will attract (asterisk). On the other hand very strong rubbing may result in only weak attraction.

Moisture, whether present directly on the electric, or around it in a damp atmosphere or breath prevents the electric action. Yet amber will attract a drop of water. It will also attract when rubbed with warm oil.

Bodies interposed between the electric and the chaff will prevent attraction but a piece of thin silk between a rubbed electric and the small body will not entirely stop it. Yet this silk laid directly on the rubbed electric will prevent it. ("That is because it is one thing to suppress the effluvium at its rise, another to destroy it after it is emitted." p. 91)

Attracted bits of material stay on the electric often for some considerable time.

The versorium moves instantly when the electric is presented.

Electric attraction reduces with distance.

As in the case of magnetics, the motion of the attracted body is quickened as it "comes nearer" the electric because the forces pulling it are stronger there. Gilbert says that this is so with electric, magnetic and "all natural motion" (p. 90).

Further experimental discoveries will be referred to below.

D. The mechanism of electric attraction.

Pari passu, with the exposition of his various observations, Gilbert gives details of his explanations of the causes of the electric action. Here it is the difference to the mechanisms and causes of magnetic effects which interests him:

"In all bodies everywhere are presented two causes or principles whereby the bodies are produced, to wit, matter *(materia)* and form *(forma)*. Electrical movements come from the *materia*, but magnetic from the prime *forma*; and these two differ widely from each other and become unlike . ." (p. 85)

Electric attraction had to be due to a material effluvium released by the rubbing because the electric action was stopped by interposed bodies or flames, neither of which stopped the magnetic power. It could not be the result of likeness of electric and attracted body which causes the latter to move to the former to "be perfected" by it. "For *all* bodies are drawn to all electrics . ." (p. 82, my italics). Fracastoro had also denied that attracted and attracting bodies were alike. But, says Gilbert, Fracastoro was still wrong when he said that hairs and twigs may be attracted to amber and diamant because of a common principle (or possibly air) imprisoned in the attracted objects which "has reference and analogy to that which of itself attracts". This principle occasionally had a spiritual character (*"interdum autem spirituale illud, quod trahendi principium est . . "*, Fracastoro, op. cit., 62.C).

Unlike magnetic coition, electric attraction had to be a *contact phenomenon*, which did not alter the substance of the attracted body in any way. What was wanted was a characteristic common to all electrics which could account for its release. It was due to a very tenuous effluvium which would be emitted upon rubbing of these substances. Gilbert introduces his ideas by mentioning some physical characteristics which his electrics have in common and which provided a necessary but not a sufficient condition: the electrics were hard bodies which could be rubbed smooth. The real source of the electric property, however, was of a chemical origin: the earth's crust was made up "of a twofold matter, a matter, to wit, that is fluid and humid, and a matter that is firm and dry" (p. 83). Bodies

"that derive their growth mainly from humours, whether watery humour or one more dense; or that are fashioned from these humours by simple concretion, or that were concreted out of them long ages ago; if they possess sufficient firmness, and after being polished are rubbed, and shine after friction, - such substances attract all bodies presented to them in the air, unless the said bodies be too heavy. For amber and jet are concretions of water; so too are all shining gems . . . " (p. 84)

There follow some more details about the origins of such substances from watery humour under various conditions. He then says that

"rock crystal, mica, glass, and other electric bodies do not attract if they be burned or highly heated, for their primordial humour is destroyed by the heat, is altered, and discharged as vapour." (ibid.)

The preceding sentences were:

"so clear glass is reduced from sand and other substances that have their origin in humid juices. But these substances contain a quantity of impurities of metals, or metals themselves, stones, rocks, wood, earth, or are largely mixed with earth; therefore they do not attract."

Apparently, then, the heating and melting of the sand in glass manufacture preserved the moist humour but high heating of the glass afterwards would destroy it. This claim seems implausible, for one would expect a moist humour to escape in the high temperatures of the melting process of the sand. He continues:

"Hence all bodies that derive their origin principally from humours, and that are firmly concreted, and that retain the appearance and property of fluid in a firm, solid mass, attract all substances, whether humid or dry." (p. 84)

The explanation for the fact that some gems which have the necessary superficial characteristics are anelectrics, is that they may contain impurities or else their humour is not subtle enough. In general, there was no independent test for the electric humour other than the electric effect itself. The postulation of the humour is therefore in the common sense of the term an *ad hoc* hypothesis, but although no independent test for it was available at the time, this by itself need not have vitiated a provisional assumption, believed by Gilbert to be based on a seemingly more generally supported chemistry of an entity of which other manifestions might after all be found later. Gilbert anyway thought - by way of another *ad hoc* assumption - that anelectrics also released humours on being rubbed, but that these were too "thick and vaporous" to cause attraction (p. 90).

Why was a very attenuated watery humour to be the agent of electric attraction at all? Gilbert had considered the proposal of air being the medium responsible for the effect. Could there be an effluvium which caused attraction by

"rarifying the air so that bodies, impelled by the denser air [returning after the rarefaction], are made to move toward the source of the rarefaction"? (p. 88)

Versions of this idea go back to Plutarch whom he mentions in this connection. But

"if that were so, then hot bodies and flaming bodies [which draw the air toward them] would also attract other bodies; but no lightest straw, no rotating pointer is drawn toward a flame. If there is afflux and appulsion of air, how can a minute diamond of the size of a chick-pea pull to itself so much air as to sweep in a corpuscle of relatively considerable length, the air being pulled toward the diamond only from around a small part of one or the other end?" (p. 88)

There was also the fact that an attracted water droplet is elongated into a cone towards the amber, something not easily explicable on the moving air theory (p. 89). Also, the air returning, especially to a broad flat piece of amber, would be heaped up on its surface and rebound, thus causing the attracted body to slow down or stop before contact with the amber. If moving air was involved, this should, but does not, make a flame flicker near an excited electric. Furthermore, the motion of air would cause bodies at first to be repelled with the air, and they could not be held to the electric once attracted (p. 90). The reasons Gilbert gives against air being the agent in the process of attraction are generally very good ones, but were ignored by several later electricians who propounded air-theories of electric action.

He concludes that "it is probable that amber exhales something peculiar that attracts the bodies themselves, and not the air" (p. 89). It appears that he hit on the idea of a very tenuous watery humour on observation of some effects of the surface tension of water. Little sticks, he says, come together if the parts above the water's surface are at least partly wet. Similarly, drops of water merge when they just touch. When he placed a water drop at the end of a little rod and then touched the point of his versorium with the drop, he observed that the versorium was quickly drawn to the rod as the versorium was wetted. Thus water can have the force of an effluvium. But all this is merely an analogy. The moist humour given off by electrics, "being the subtilest matter of soluble moisture" (p. 92), i.e. much thinner than water, is so thin that it does not affect, or is affected by, the air. In fact in the electric process water itself is thick enough, even in the form of moisture in the air, to block the passage of the electric effluvium. The fact that the latter attracts water drops shows that water behaves much like a solid substance vis a vis the electric effluvium.

Hesse surprisingly thinks that Gilbert saw the cause of electric attraction in "a tendency towards unity among *like substances*, which, however, only manifests itself when effluvia from the attracting body actually touch that which is attracted" (1961, p. 89, my italics).

This, she says, explains to him

"the phenomena of electric attraction, cohesion, gravitation and surface tension, all of which seem to require some kind of attractive force between parts of matter". (ibid.)

Gilbert says with respect to electric and gravitational attraction:

"All bodies are united and, as it were, cemented together by moisture, and hence a wet body on touching another body attracts it if the other body be small; and wet bodies on the surface of water attract wet bodies. But the peculiar effluvia of electrics, being the subtilest matter of solute moisture, attract corpuscles. Air, too (the earth's universal effluvium) unites parts that are separated, and the earth, by means of the air, brings back bodies to itself; else bodies would not so eagerly seek the earth from heights. The electric effluvia differ much from air, and as air is the earth's effluvium, so electric bodies have their own distinctive effluvia; and each peculiar effluvium has its own individual power of leading to union, its own movement to its origin, to its fount, and to the body that emits the effluvium." (p. 92)

Likeness of substance, however, was useless in explaining electric attraction of substances so unlike each other as different metals, organic materials, or water droplets, by one of the many different electrics. The idea of the role of likeness of substance is in fact one of those old metaphysical principles (like that of sympathy) the rejection of which contributes to making Gilbert one of the first modern scientists. (He mentions the role of likeness of substance in the case of the strong magnetic cohesion of pieces of iron. But, as we have seen, he there gives specific reasons for the role of the likeness, i.e. the degree of magnetisability and conductivity of iron.) He says, in fact, expressly: "And likeness is not the cause of amber's attracting . . Besides, like does not attract like - a stone does not attract a stone, flesh flesh." (p. 81/2)

As the above quotation shows, Gilbert sees an analogy between electric and gravitational attraction with respect to some aspects of the effective mechanism. He also claims that "The matter of the earth's globe is brought together and held together by itself electrically" (p. 97), thus giving electric effects a very important and universal role, which, probably because Gilbert mentions it only once or twice, many commentators thought he had reserved for magnetism. An effluvium-mechanism is able to effect cohesion of the earth's matter, gravitational attraction and electrical attraction on earth, and at least gravitational attraction and probably also cohesion on the celestial bodies.

What may have prompted Hesse to think that Gilbert considered 'likeness of substances' to be important may be his statement that gravity is partly due to a tendency of "cognate" material to hold together because the heavenly bodies also pull back to them parts which originate from them. (I will consider this aspect in the chapter on cosmology.) But it appears that for Gilbert substances which were cognate need not share any other known property, because electrics, though cognate with all terrene matter, were usually unlike the attracted body which could be 'anything material'. 'Likeness of substance' was, therefore, not a factor.

E. Methodological comments on Gilbert's researches.

Gilbert's electrical work has been considered in some detail by Roller who takes the theory-ladeness of observations for granted. This has the odd consequence of leading him to argue specifically that if an observation statement of Gilbert's seems to contradict a hypothesis which Gilbert favours then the observation must *therefore* be based on an experiment. Starting from the premiss that Gilbert wanted to show that magnetism had different causes and mechanisms from electricity, he says that various observations Gilbert makes must be experimental because they tend to hinder rather than help his attempts to distinguish electricity from magnetism. With respect to the parallel of the reduction of the force of attraction with distance, both in the case of magnetism and electricity, Roller says:

"Since Gilbert's general theory of the dissimilarity of magnetic coition and electric attraction would have been supported by the lack of variation of force with distance in the electrical case, this discovery is empirical." (op. cit., p. 121)

One does of course not need such an indirect proof of the empirical origin of the discovery of a fact which Gilbert describes as an observation. Lest Roller's remark appear coincidental and perhaps methodologically harmless, I will quote one or two more of his statements on the subject. On p. 124 we read about Gilbert's discovery that rubbing the amber with a finger dipped in oil still results in electric attraction (which Gilbert had explained by saying that the "light and pure" oil does not suppress the effluvia): "His explanation is not very convincing, in the light of his theory, which is evidence of the experimental nature of these discoveries". He says on p. 120 with respect to Gilbert's finding that amber or jet warmed near a flame will not attract small objects because it becomes covered with a vapour:

"The envelopement of the electric by a vapour that is alien to its nature is not an explanation that is very well in accord with Gilbert's basic theory, but is somewhat ad hoc, so that we may presume that this discovery is experimental." Again, on p. 116: "The difficulty with which Gilbert accepts the failure of heat [heat not caused by rubbing] to produce attraction makes certain the experimental nature of this information."

Referring to a related matter: "The argument thus seems unconvincing, even to Gilbert, evidence of the experimental origin of the information".

Such claims that descriptions of phenomena or explanations must be empirical in origin because they seem at odds with his theory are quite unnecessary, because Gilbert describes the experiments himself and there is no need whatever to doubt that he performed them and observed the results. One must wonder how Roller is able to assess the status of reported observations which do *not* seem to contradict one of Gilbert's hypotheses when examining Gilbert's other results. For if the observations fit in with the theory, where would Roller get the evidence for their experimental nature from which he seems to require? (In fact he repeatedly states without question that various observations are of experimental origin without any such additional evidence.)

The conclusion that observations apparently recalcitrant to a fit in the theoretical framework are likely to be empirical in origin is, taken by itself, not implausible. But Roller has also claimed an occurrence of another outcome. He says in footnote 3, p. 12 (after remarking on the theory-ladenness of observations): "William Gilbert completely failed to observe electrostatic repulsion, a phenomenon not in accord with his conceptual scheme for electricity". Roller thinks that Gilbert was exposed to the stimuli necessary to discover repulsion yet failed to do so *because* repulsion did not fit in with his concepts. But this is not acceptable in the light of the earlier claim that Gilbert did make many observations which did not fit his theories. It is of course possible to make some observations of effects which appear not to be of help to, or even to contradict, a theory, but to overlook others. What one must object to is the attempt to use these obvious facts as evidence for an empirical origin of observations or, respectively, for the theory-ladenness of observations which allegedly led to oversights of actually palpable effects.

To what problems the pervasive claims of the theory-ladenness of experimental reports can lead is shown by the fact that Hesse says that

> "Gilbert did not report . . . repulsion, even though the possibility of repulsion would have accorded better with his theory of effluvia emanating from the electric" (after stating that "he describes experiments in which bodies move more quickly as they approach the electric and in which it is difficult to believe that he did not also observe them bounce off with considerable force, as is later reported by Cabaeus and by Browne"; 1960, I, p. 7).

While she is, therefore, saying that Gilbert did not observe repulsion although it would have fitted in better with his theory, Roller's view is that it was not observed because it was not in accord with his conceptual scheme for electricity. The explanation of this strange situation may be that Roller is referring to Gilbert's straightforward theory of electric attraction which he seems to think has no room for repulsion. This may be due to the fact that he takes Gilbert's simile of the attraction of sticks floating on water seriously. Hesse, on the other hand, appears to ignore this analogy when she assumes that effluvia emanating from the electric could account for repulsion because they should carry the attracted body away (this is a mistake anyway because repulsion in these cases would have to follow attraction to contact). Like Roller, Hesse thinks that Gilbert failed to observe repulsion because he wanted the electric phenomena to be as different as possible from the magnetic ones.

One may accept the differences of opinion about the consequences that observation of repulsion would have had for Gilbert's aims of describing electric effects correctly on the one hand, and separating electricity and magnetism on the other. (In Hesse's case Gilbert's aims would be at odds with one another, and that of showing magnetism and electricty to be phenomenally different would have the decisive influence). But Roller's and Hesse's treatment of the methodological question is higly unsatisfactory. Again one would find it hard to understand how either writer could explain Gilbert's achievements in general, on the assumption that his theories affected his observations in these ways. Successful research would then appear possible only in cases where there was a lucky match of the right sort between a theory and the theory-laden observation of a test result. Hesse, as I have said, is concerned to show that an inductivist construction of a theory from bare observations cannot take place because such do not exist. But a falsificationist view could not account for the successes of scientific activity either if the observations were not bare relative to the hypothesis under test. Although it may seem intuitively likely that out of the many events occurring during an experimental test, those which appear to confirm the hypothesis be 'favourably' observed, it is by no means something that can be assumed and then used in evidence by the historian. For, as we have just seen in the examples of Roller's and Hesse's views, it can easily lead to contradictory conclusions and can in general not account for the emprirical success of science.

Gilbert's electrical experiments are very ingenious and comprehensive. Anybody who has carried out primitive electrostatic experiments knows just how difficult it can be to come to any consistent conclusions. The conductivity of surfaces on which the electrics or the attractable small bodies lie, effects resulting from moisture in the environment or on the fingers, inductive effects of all sorts and the ever present possibility of accidental earthing are only some of the factors which can play havoc with the aim of obtaining clear results. Heilbron (op. cit., p.3.) has expressed this nicely by saying that "the malevolence of inanimate objects is nowhere better instanced than in the phenomena of frictional electricity".

As we have seen, Gilbert has been criticised at least implicitly for not reporting observations of repulsion. Electrostatic repulsion in the sort of experiments Gilbert performed is a secondary phenomenon following transfer of charges on contact with the electric after attraction. However, this transfer does by no means always take place very readily when bits of dry chaff are used. It is therefore often only observed as a phenomenon clearly distinguishable from that of the eventual falling off from the electric after some short time of contact when conditions are favourable: induction must be strong enough and the transfer of charges facilitated by the surface conditions and conductivity of the attracted matter (as with scraps of metal). However, if the repulsion follows the contact quickly, it may be mistaken for simple mechanical rebound. Roller thinks that Cabeo discovered repulsion although he was exposed only to the same sort of stimuli as Gilbert. According to Roller the Anticopernican Cabeo discovered it because he started with the assumption that the Copernican Gilbert must be mistaken. Hesse also mentions Cabeo's discovery of repulsion. In fact, like Gilbert, Cabeo, who also adopted an effluvial theory for electricity, denied the possibility of *electric* repulsion but saw the flying off of attracted particles he occasionally observed as an effect of moving air (cf. Heilbron, op. cit. p. 181-3); so Cabeo at least comments on the fact that rebounds of some kind occur whilst Gilbert does not. In Gilbert's case we can only speculate on the question. If he saw the phenomeon, he may have thought it perhaps not worthwhile commenting upon (if he believed it to be due to mechanical rebound). On Roller's and Hesse's views we are forced to assume that Gilbert formed his theory first and then tried to fit his observations in with it, a supposition for which there is no evidence. Even if Gilbert had observed repulsion, he could have found a way of keeping his effluvial theory going, for later electricians managed this to their satisfaction.

The formidable problem of the discovery of repulsion proper is described by Heilbron as follows:

"Several old electricians have been sponsored for the honour of discovering repulsion. None of them will do, however, if one requires the discoverer to have recognised repulsion as a distinct effect, coequal with attraction, and associated with conduction via [the sequence of] attraction-contact-repulsion . . . The recognition of repulsion was completed by Dufay in the 1730s." (op. cit. p. 5)

This then would be 130 years after Gilbert. Even if one credits one of the 'old electricians', for example Guericke, with the discovery of repulsion in some different or, on Heilbron's understanding, incomplete, sense, it must not be overlooked that Guericke had the advantage over Gilbert of having his large sulphur sphere at his disposal which he could electrify to a considerable charge (in any case, as Heilbron points out in op. cit. p. 217, Guericke was not at all of the opinion that the observed repulsion was an electric effect). It is therefore appropriate to seek the reason for Gilbert's oversight of the phenomenon not in his being blinded by his theory or lack of careful experimentation, but at least largely in the considerable objective difficulties.

The example of Benham shows to what sort of incorrect assessment of Gilbert's electric work can lead the commentator who ignores the experimental problems of the subject which are evidenced by its history before and after Gilbert, and of which the writer could easily have convinced himself by carrying out some experiments. He says on p. 44, op. cit., with respect to Gilbert's chapter on electricity:

"But for his coinage of the potent words 'electrics' and 'non-electrics', and for his description of a few interesting experiments with these substances, one could almost wish that for his own sake he had blotted out this unfortunate chapter, so full is it of wild dreams and fanciful conjectures."

F. The importance of Gilbert's electric work.

A more appropriate evaluation would consider at least the following facts: Gilbert was the first systematic investigator of electric attraction and tested dozens of substances under various conditions. The discovery that there were many electrics and that they attracted any material was most important. The attracted objects were of the greatest physical variety and the electrics themselves also had only one or two observable physical properties in common. Electric phenomena had become even harder to explain than Gilbert's predecessors had assumed. The limited range of electrics and substances capable of being attracted known to them seemed to leave room for some common principle which could serve in some mechanism of sympathy. Gilbert could not accept an explanation in terms of likeness or sympathy, an unscientific postulate. He had to reject the other explanation proffered, viz. that of an effluvium moving the air, for good physical reasons. The basic chemical theory that there were dry and moist principles in the make up of matter, though speculative, was perhaps not too implausible, and the large number of electrics he had discovered seemed just about to be explicable by its adoption. The effects of surface tension on water, finally, showed that objects may come together as if attracted to one another when separated by small distances. In spite of all its weaknesses, Gilbert's theory therefore had the advantages of being of a physical nature, having been derived from a perhaps not entirely phantastic chemistry, and explaining the wide occurrence of electric attraction.

Gilbert's moist effluvia were rejected by Cabeo, who said that many substances which seemed to contain moist humour did not attract, overlooking the possible objection that the moist humour they contained might not be capable of becoming effective because of the admixture of impurities or their lack of tenuity, as Gilbert had claimed. Gilbert himself had in any case pointed out that many polished gems do not attract. Cabeo substituted the action of the air after rarefaction by subtle effluvia as the effective mechanism in attraction. In the following periods all sorts of mechanisms were suggested to explain the phenomenon, such as sticky effluvia; effects of air with various additional factors due to, for example, heat; thin filaments; vortices and so on. One cannot really say that Gilbert's explanation was less plausible than many of the later ones.

Gilbert's adoption of a material medium for the electric effect, though an advance over that of a vague sort of sympathy, may be supposed to have been unfortunate for the subsequent development of the subject. This view would perhaps be mistaken, however, for his successors tried all sorts of other approaches to the problem, even though effluvial theories predominated. The shielding of the effect by interposed matter was the main obstacle to the assumption of an immaterial force, as this seemed so different from the case of the immaterial magnetic force.

One feels that if he had devoted as much time to electric researches as to magnetism, Gilbert might well have advanced the subject much further. This assumption is not purely speculative as witnessed by the high quality of his overall achievements. The demarcation of electric from magnetic effects was essential for the further advance of electric researches. He laid the foundations of the subject by marking the distinctions to magnetism and by investigating many fundamental aspects of electrostatic attraction. The electroscope was a very important innovation, not simply because of its usefulness in the actual electrical investigations, but as an example of a scientific instrument useful in researches in a subject which up to then had to rely on very primitve methods.

The main importance of his work in electricity lies, similarly, not perhaps in the individual discoveries he made, but in providing an example - in addition to that of his magnetic work - of scientific enterprise by systematic experimental exploration of the field, the purposive formulation of hypotheses, and their testing: which substances are electrics; which can be attracted? Under what conditions does attraction take place (the effects of moisture, liquids other than water, heat, degree of rubbing, shielding of the electric by interposed objects and flames, dependence of attraction on distance)? What may electrics have in common and what is the mechanism of attraction, distinguishing it from magnetism? Such a logically connected range of comprehensive questions concerning a subject of terrestrial physics had rarely, if ever, been asked before, nor had the systematic and ingenious experimentation been carried out to try to find the answers.

Chapter Nine

Gilbert's Cosmology.

A. Copernicanism in England. The *De Mundo.*

Gilbert made use of his magnetic and electric theories in his cosmology, which I will now consider. His cosmological views are relevant to an assessment of his scientific achievements as a whole in the context of the science of the time, and to that of his influence on his contemporaries and successors. The situation in astronomy in the 16th century was very confused, and if it is possible to prove that he applied an independent scientific judgement to it, this will have a bearing on our estimation of Gilbert's standing. He was keenly interested in astronomy, and Heilbron (op.cit., p.172) is wrong when he says ". . . neither Gilbert nor Aristotle cared particularly for the physics of things lying beyond the moon". Gilbert showed considerable perspicuity in judging the astronomical evidence, in many respects surpassing that of Tycho's who provided much of it by his revolutionarily accurate observations. Where Gilbert had to speculate - for example on the causes of the movement of the heavenly bodies - he does so in an intelligent way, and he made suggestions which seemed fruitful to others, such as Kepler.

Although he gives the most important features of his cosmology in the *de Magnete*, for many details we must turn to the astronomical chapters of his second work, the *de Mundo.* This did not appear until nearly fifty years after Gilbert's death; but the manuscript - collected from Gilbert's writings by his younger half brother - will have been available to some interested students (Kelly, op. cit. p.17, says that one or two copies of it were made), and was certainly closely read by Bacon. The work was only published once, in Amsterdam in 1651. (I have used the facsimile by Menno Hertzberger Ltd. and all references below, which are given as page numbers only, are to this work. As it was never translated from the Latin, I give all quotations with my own translations.) It is not known when Gilbert wrote it but it is likely to be the product of intermittent work over several years and to have been unfinished at the time of Gilbert's death in 1603. I will show that he wrote at least some of it after the *de Magnete*. Suzanne Kelly (op.cit.), the only writer who considers the *de Mundo* in any detail, has made a useful comparison of both works with respect to Gilbert's astronomical views. Although there is nothing radically new in the cosmology of the *de Mundo* as compared to that of the *de Magnete*, there are some changes of emphasis and some new suggestions which show the development of his ideas. Those Kelly points to are important, as are some others which she ignores.

Historians have given the *de Mundo* little consideration. This may partly be due to its having been published at a time when science had advanced considerably beyond the state of knowledge of Gilbert's own period. However, it is a very interesting work and a historical document of some importance. Lynn Thorndike (1941, p.380) says that "it is to be borne in mind that Gilbert himself never saw fit to publish it and that he left it in an incomplete and unfinished state". Though the latter part of this statement is most probably true, the implication of the former, viz. that Gilbert did not consider it to be important, is surely mistaken. He died suddenly and we should rather assume that his death occurred before he had an opportunity to finish the work properly. There is much in its five parts, the *"Physiologiae nova contra Aristotelem"* (first two books), and the *"Nova Meteorologia contra Aristotelem"* (last three books) which Gilbert perhaps might have left out had he published it himself. These deletions could not be made by the editor who collected the writings as he found them, so that they may include opinions Gilbert had held at an earlier time of his life and later abandoned. Several parts of the book give an impression of being unfinished and less than systematically arranged. I will ignore much of it and concentrate on the important cosmological sections.

Gilbert was one of the early proponents of important aspects of the Copernican system in England. He was not, however, as we shall see, "a Copernican of the Copernicans, a castigator hip and thigh of those who believed that the sun, moon and stars were attendant satellites around a central earth . . .", as Charles Benham (op. cit. p.55) claims. But he was by no means the first defender of even only some of Copernicus' ideas in this country. Benjamin was quite wrong in saying: "From Bruno it may be presumed that Gilbert imbibed the ideas which made him not only the first of English Copernicans . . ." (op.cit. p.268). Sydney Chapman makes a similar claim, saying he was ". . . the first Englishman to accept and propagate the revolutionary views of Copernicus and Bruno on the motions and the nature of the celestial universe" (op. cit. p.132). He had important predecessors in Robert Recorde, John Dee and principally Thomas Digges, to none of whom he refers in connection with the new astronomy. (Dee and Digges are mentioned only in remarks about observations of the new star in Cassiopeia of 1572). Recorde praised Copernicanism in his *Castle of Knowledge* of 1551 and John Dee had accepted its truth in 1566, though in 1558 he had spoken ". . . of the rapid motion of the celestial vault and of the sun" (cf. Rene Taton, 1964). Digges had defended the Copernican views in his book *"Alae sive Scalae Mathematicae"* on the nova of 1572, published in February, 1573, i.e long before Bruno's publications and visit to England (Gilbert, by the way does not refer to Bruno anywhere). Gilbert probably owes to Digges the idea that the stars are at varying and vast distances from the earth and that the universe may be infinte. The latter had put this important suggestion forward (seven years before Giordano Bruno defended it) in his appendix "A Perfit Description of the Celestiall Orbes" to his father Leonard's *Prognostication everlastinge* of 1576. His influence on the scientists of the 16th century in England was most important, as F. R. Johnson (1936; also 1937, e.g. p.169) points out. Digges insisted that science must proceed by observation and experiment.

B. The earth's diurnal motion.

Gilbert employs the standard Copernican arguments of the time for the diurnal rotation of the earth:

The planets and stars could not be fixed to spheres which revolved around the earth because this revolution would have to be impossibly fast, and in any case there could be no such spheres. The same applied to the "insane idea" of a primum mobile (*de Mag.*, VI,3.) It was nonsense to worry that the earth might be torn apart by its daily revolution, but that this would not happen to the celestial spheres.

The space above the earth and its exhalations was a vacuum offering no resistance to the earth's revolution for which the latter was eminently fitted by its spherical shape.

He adds the reasoning from simplicity characeristic of many Copernicans:

"From these arguments, therefore, we infer, not with mere probability, but with certainty, the diurnal revolution of the earth; for nature ever acts with fewer rather than with many means; and because it is more accordant to reason that the one small body, the earth, should make a daily revolution than that the whole universe should be whirled around it" (*de Mag.*, VI.3, p.327).

The claim that the space between the heavenly bodies was a void was very important in an attack on some of the prevalent forms of the Aristotelean system. Gilbert remarks that it was absurd not to admit the exsitence of a vacuum in nature (cf. p.63-4). Aristotle had rejected the possibility of a void, a "non-being", which seemed a logical contradiction. His later followers also declared a vacuum to be impossible, God himself could not make one, as some of them claimed. Its prohibition played an important part in Aristotelean kinetics and dynamics. It would, if it existed, for example have allowed falling bodies infinite velocities because of the absence of resistence in it (an argument against the vacuum because of the absurdity of an infinite velocity). Gilbert's acceptance of a void was important in supporting the abandonment of the material spheres and in showing that the earth would be able to move freely. He says in VI.3, p.326, *de Mag.*

". . . since it revolves in a space void of bodies, the incorporeal aether [Gilbert speaks about 'vacuum' or 'aether', allowing for the existence of either without characterising the latter in any way], all atmosphere, all emanations of land and water, all clouds and suspended meteors, rotate with the globe: the space above the earth's exhalations is a vacuum; in passing through a vacuum even the lightest bodies and those of least coherence are neither hindered nor broken up."

If the earth suffered friction with any material around it, its atmosphere would be set in motion (cf. p.51). He considers the question of the vacuum in general with references to antiquity (Hero of

Alexander) and concludes that on earth a vacuum cannot exist, for example between atoms of substances (p.64.). But he argues strongly for a void between the celestial bodies, for *"Si non esset vacuum, videremus nos singulis noctibus umbram pyramidalem telluris: quod nunquam contingit"* (If there was no vacuum, we would see every night a pyramidal shadow of the earth which never occurs, p.65). For the light of the sun would be scattered by the substance in space right up to the path of the moon (cf. p.65). The planets and comets also behaved as though they were flying through a void, otherwise their substance would be dissipated, and because of the vacuum the comets move freely.

Gilbert's treatment of the question of the vacuum is soundly removed from any form of *a priori* and metaphysical reasoning. The rejection of the vacuum of the atomists by Gilbert is based on the experimental evidence as he understood it. (Some of the relevant experiments show that liquids cannot flow out of vessels unless air is admitted; if there was a vacuum between the atoms anyway, there seemed to be no reason for this.) The acceptance of the vacuum for outer space relies on reasoning based on observations, or absence of effects which should be observed if a material substance filled it. His evidence is of varying degrees of persuasiveness. That dependent on arguments from the behaviour of light presupposes that the material present between the heavenly bodies scattered and absorbed it, an assumption to which he was entitled if he thought that any matter would do so over the enormous distances in space. The arguments concerning the wider effects of and movement through, any allegedly present material with its observational consequences for the earth and its atmosphere are cogent (at least if we disregard the possibility of a generally very tenuous distribution of matter as we now know it to exist).

The force of his various standard Copernican reasons for the existence of a void and the absence of spheres is cumulative. They are important, if to the most part not novel.

The striking appearance of the new star of 1572, the most important and consequential astronomical event of the time, was evidence against the immutability of the heavens and was of major importance in the course of the downfall of the Aristotelian system. It must have influenced Gilbert's views profoundly.

Since antiquity there had existed not only different astronomical systems, but each of them was interpreted by some proponents as a purely mathematical theory designed to 'save the phenomena', and as a realistic picture of the physical situation by others. (This applied of course also to the Copernican system: cf. Osiander's foreword to *De Revolutionibus* with Copernicus' own physical interpretation.) When a system was understood as only a mathematical construction it was vulnerable merely to attacks arising from inaccuracies of description or prediction (and perhaps rarely to those referring to a perceived lack of simplicity). It was not usually in danger to any great degree from religious opinions. The physically interpretated systems, on the other hand, had to face a whole gamut of mathematical, religious, observational and 'common sense' arguments as well as possibly those concerning the degree of simplicity. Gilbert is aware of this difference and says in VI.9, p.353, *de Mag*:

"We must pardon slips in mathematicians, for one may be permitted in the case of movements difficult to account for [he is referring to attempts to explain the supposed inequalities in the precession of the equinoxes] to offer any hypotheses whatever in order to establish a law and to bring in a rule that will make the facts agree. But the philosopher never can admit such enormous and monstrous celestial constructions."

The philosopher in his terminology was therefore the astronomer or physicist in our sense.

He saw that the Copernican conception was meant to be a realistic description of the universe. His reasoning is therefore whereever possible based on observations and he attacks the (realist) Aristotelean systems with their help. The assumption of the diurnal revolution of the earth was consistent with everyday common sense experience and the interpretion of the observations as those of the earth's proper motion was reasonable. Against counterarguments to this he employed the standard reasoning of the Copernicans: one cannot feel the forward motion on a ship, and the illusion can easily arise that the sea or land be moving. The earth's atmosphere and other objects would be turning with its body and no relative motion to it would be observable. This does not prove that the earth turns. But against a revolution of the heavenly spheres other arguments were telling.

The fact that comets were found beyond the moon and traversed space freely (which had also been claimed by some writers in antiquity but without astronomical proof) was clearly most important because it showed that there could be no material spheres. According to Aristotle comets were sublunari or even atmospheric phenomena. But the observations of comets beyond the moon (*"Cometa anni 1568 supra Lunam in Mercurii orbe constitit"*, p.236) were new and powerful arguments for Gilbert. He himself observed comets (for example on the 2nd of November, 1569 at Canterbury, cf. p.227).

Tycho Brahe had laid great stress on the importance of the comets' superlunary positions and their behaviour. He proved by his accurate measurements of their parallaxes that they were much further distant than the moon. There could therefore be no spheres which would have impeded the comets (cf. a letter from Tycho to Kepler of 1598; see Kepler, 1858, I, 44). Tycho kept, however, the sphere of the fixed stars, whose enormous speed of revolution he considered evidence for the power of God (cf. J.L.E.Dreyer, 1890, p.208). This is in interesting contrast with Gilbert's remark on the supposed existence and motion of the primum mobile: ". . , and what mad force lies beyond the primum mobile?" (*de Mag*. VI.3. p.322).

Gilbert accepted the great astronomer's calculations of the positions of comets but rejected his arguments against the diurnal revolution of the earth, one of which was that on a turning earth shots fired in a westerly direction should fly further than if fired toward east (*de Mag*. VI.5, p.341). It is perhaps ironical that this most accurate observer of the heavens was not better able to picture to himself what observational consequences a revolution of the earth would or would not have. As Dreyer, op. cit. p.356, says, Tycho could have made the experiment of dropping a pebble from the mast of his moving ship to see where it landed; this would have served as a form of check on his claim that on a revolving earth a bullet fired vertically upwards would not fall straight down. Johnson (1937, p.164) draws attention to the fact that Digges had recommended the experiment of dropping an object from a ship's mast and had probably carried it out. It is therefore strange that Tycho did not accept this, particularly as Digges was his friend and correspondent.

It is by the way not quite clear what direct knowledge Gilbert may have had of some of the details of Tycho's ideas (except with respect to those in Tycho's publication of 1573 about the new star which he will have read). Johnson (op.cit., p.220) points out that Tycho's *De Mundi Aetherei recentioribus Phaenomenis* was published in a small edition in

1588 and was only available to friends and correspondents, including Digges and Dee, from whom Gilbert could have learned about it. Gilbert probably read the *Astronomiae instauratea Mechanica* of 1598. The posthumous publication of Tycho's *Progymnasmata* in 1602 by Kepler came after the *de Magnete's* appearance and it is perhaps only just possible that Gilbert read it before his death. In the *de Magnete* Gilbert accepts Copernicus' irregularities in the precession of the equinoxes, cf.VI. 9. (The precession was to be explained by Copernicus' third motion of the earth, the movement of the earth's axis which kept it parallel to itself in the course of a year. It took place, however, in slightly less than a year, the difference accounting for the annual part of the precession. In this movement of the earth's axis Copernicus also included the librations to account for the alleged trepidations - inequalities in the precession of the equinoxes -, and the anomaly of the obliquity of the ecliptic). In the *de Magnete* Gilbert seems to accept Copernicus' explanations of the inequalities in the precession of the equinoxes and the obliquity although he says that

". . . all these points touching the unequal movement of precession and obliquity are undecided and undefined, and so we cannot assign with certainty any natural causes for the motion".

This, as he had just said, was due to lack of accurate data over a long enough time (VI.9, p.358. We can therefore assume that he had at least an open mind about Copernicus' third motion and its details. But in the *de Mundo* he says on pp.165-6:

"Tertius his motus a Copernico inductus, non est motus omnino, sed telluris est directio stabilis, dum in circulo magno fertur, dum unam partem coeli constanter respicit."

(This third motion introduced by Copernicus is no motion at all but the earth has a stable direction, as it moves in its great circle it points constantly to one part of the heavens.)

In the *de Mundo* there is no mention of a supposed inequality in the precession of the equinoxes nor of the change in the obliquity. It seems to me that the reason for this can only be that in the mean-time he must have become familiar with Tycho's exact measurements and dismissal of these irregularities (from the *Astronomiae Instauratea Mechanica* or by second-hand report about this work). And this can only have occurred after Gilbert wrote the *de Magnete*. I therefore believe this to be proof that Gilbert wrote these parts of the *de Mundo* after the *de Magnete*.

C. The problem of the annual motion.

Gilbert nowhere commits himself clearly to one of the central tenets of the Copernican system, the annual motion of the earth around the sun. A diagram on p. 202 (fig.13) shows a large sun in the centre of the planetary system with circles indicating the paths of Mercury, Venus, Mars, Jupiter and Saturn around it. In the wide gap between Venus and Mars there is only a dot representing the earth, and this is shown encircled with the moon's path. There is no path shown for the earth around the sun (nor one of the sun around the earth). The fixed stars surround the system at various distances. No attempt is made at a represention of even rough relationships of distances of the planets from the sun or the fixed stars from Saturn (the nearest star is shown closer to Saturn's circle than the latter is to Jupiter's). Gilbert discusses the fixed stars in various places, saying that they were too far from earth for any proper motions they may have to be perceptible.

They were held in their positions (or paths) by their own forces acting between them (e.g. p.112-113). As to the size and shape of the universe, whether it be spherical or of another figure, no reason was persuasive, no demonstration compulsive (*"De ambitu namque & forma universi, utrum circularis sit, an alicujus alterius figurae, nulla urget demonstratio, ratio nulla persuadet"*, p.113). He demonstrates with the help of a simple drawing on p. 114 that whatever the arrangement of the fixed stars be, it would appear to us that they are situated on the surface of a hemisphere.

There are some interesting differences between Gilbert's diagram of the solar system and that of Digges' (1576) of the Copernican system. Digges had changed Copernicus' scheme by showing the fixed stars not on the surface of an orb but at varying distances ("fixed infinitely up" as he inscribed on the drawing) around the solar system. This Gilbert took over. But whilst Digges' diagram shows a large void space between the path of Saturn and the nearest fixed star to indicate the distance needed to explain the absence of a parallax, Gilbert - as just mentioned - shows no appreciable gap at all. The most striking difference to Digges' drawing, however, is the absence of the path for the earth around the sun. The lack of the earth's circle is an indication of Gilbert's refusal to commit himself openly or completely to the whole Copernican system, which he does describe in some detail.

He says on p. 135:

"Terram circumvolvi diurno motu, verisimile videtur: an vero circulari aliquo motu cieatur, non huius est loci inquirere"

(that the earth revolves with a daily motion is likely: whether it indeed revolves with any other motion is not to be examined here), a disingenuous statement in a chapter entitled *"Terram circulariter moveri"* and in the overall context. Yet there are very good reasons for this: as Johnson (op.cit., p.216) says, Gilbert did not commit himself positively to the annual motion of the earth because "he was no doubt deterred from this step by the fact that he had no scientific evidence to offer concerning this feature of the Copernican system". If he wanted to remain objective on the question, he had to consider the fact that there was no observational difference between the phenomena on Tycho's and on the Copernican system. Johnson (op. cit. 220 ff.) points out that in being non-committal in this respect between the two systems he was followed by most English astronomers of the time (though - as Johnson also says - they were very sympathetic to Copernicus). Yet in spite of this, one feels that Gilbert should have discussed the problem of the annual motion in some detail and, setting out the arguments for and against, explained his views. As it is, the reader remains dissatisfied, feeling that the question has been largely avoided.

There is little doubt, though, that Gilbert's sympathies lay in fact with the Copernican view. For he had employed the arguments from simplicity and economy of means in nature with respect to the diurnal revolution: it was simpler and therefore more likely if the earth rather than all the heavens turned. This, one might expect, should also apply to the relative annual motion of the earth, sun, and planets. But if he felt this way, he did not say so clearly. He mentions that the earth was like the planets in size relatively to the sun, and in receiving sun light. Therefore it was likely, he may have thought, that it was in fact one of the planets. But again he does not maintain such a claim.

In at least one place he appears perhaps to accept the Copernican scheme without hesitation, as Roller has pointed out. With reference to

VI.6. p.344, where Gilbert considers the motion of the moon, Roller (op. cit. p.173) draws attention to Gilbert's

"appeal to the solar month as being the true period of the moon. In a geocentric system of the world there is no reason to relate the period of the moon in any such way to the Sun: some relationship may be found between the periods of the various heavenly bodies, but those periods are for their motions against the background of the stars. But in a heliocentric world the sun has the predominent role . . . "

Gilbert uses the solar (synodic) month of about 29½ days as the moon's period instead of the slightly shorter sidereal time here because, as he says,

"the sun is the cause of both the earth's and the moon's motions. Also because (as more recent astronomers suppose) the month, as measured between solar conjunctions, is really the full period of revolution, because of the earth's motion in her great orbit." (VI.6., 344)

In spite of this, Roller says, Gilbert was certainly not a Copernican if judged by the *de Magnete*. He became a Copernican only in connection with the "numerological experiment with the moon's motion" (ibid.). But, it seems, if the *de Mundo* is also taken account of, we see perhaps a more pronounced slant toward the heliocentric system. The interpretation depends on the weight one attaches to Gilbert's relevant remarks in both books and to the absence of a definite statement anywhere about the question. He says on p. 120:

"Locus telluris non in medio, quia planetae in motu circulari tellurem non observant, tanquam centrum motionum, sed solem magis"

(the position of the earth is not in the middle because the planets in their circular motion do not observe the earth as a centre of motion but rather the sun.)

It seems impossible to imagine that Tycho could have written such a sentence with respect to the solar system. For even though the planets, in their immediate circular motions around the sun, do not observe the earth on Tycho's view, one could hardly deny that on the same view the earth would be in the middle, and a centre for their motion. In as much as Gilbert's choice was between Tycho's and Copernicus' systems, he would, from the evidence of this statement, appear to be a Copernican.

However, Lynn Thorndike (op.cit., p.380) appears to be in error when he says that "Gilbert states that the earth is moved circularly which also seems to imply that it revolves about the sun", as though Gilbert had not repeatedly mentioned the question of an annual motion as one that was separate from that of the diurnal rotation. Thorndike states as Gilbert's the view that the earth moves "about its axis to (around?) the sun, the moon about the earth and so about the sun". In support of this Thorndike quotes in a footnote *"tellus circa axem ad solem"* from p.120. The full wording there, however, is

"At nonnulli globi & insitis viribus, & actu aliorum corporum, aguntur circa quaedam corpora, ut planetae circa Solem, aut circa tellurem, tellus circa axem suum, ad Solem, Luna circa tellurem & erga Solem"

(But many globes through implanted forces as well as through the action of other bodies, are carried around some bodies, as the planets around the sun, or around the earth, the earth around its axis toward the sun, the moon around the earth and around the Sun).

Gilbert mentions both possibilities again quite clearly in the next paragraph:

". . . si Sol in medio quiescit, ut Canis, ut Orion, ut Arcturus, tum planetae, tum etiam tellus, a Sole aguntur in orbem . . . si vero tellus in medio quiescat (de cuius motu non est huius loci disceptare) aguntur circa ipsam caetera moventia."

(if the sun rests in the centre, like Canis or Orion or Arcturus, then the planets, even the earth, are moved by the sun in orbit . . . but if the earth rests in the middle (whose motion this is not the place to dispute about) the others are moved about it).

The reading of Dreyer, Johnson and Kelly as already given, viz. that Gilbert hesitated between Tycho's and Copernicus' system, is perhaps correct if it does no more than to stress Gilbert's hesitation to make a proper *public* commitment.

Gilbert points to one of the main problems faced by the Copernican system as a whole on p. 193:

"Copernici vero ratio magis incredibilis, licet minus in motuum convenientiis absurda; quod terram triplici oportebat motu agitari, tum vel maxime, quod nimis vastam capacitatem inter orbem Saturni & octavam sphaeram esse oportet, quae prorsus sideribus vacua relinquitur"

(but Copernicus' system is harder to believe in; it seems less absurd because of the harmony of the motions, as the earth be moved by a triple motion, but mainly because it would allow the excessively vast space between the path of Saturn and the eighth sphere by which a vacuum up to the stars is left).

The great distance to the fixed stars was an assumption Copernicus' system was forced to make because no annual stellar parallax could be detected. Copernicus mentions the fixed stars' "immeasurable distance, compared to which even the size of the earth's orbit is negligible and the parallactic effect unnoticeable" (Copernicus, op. cit., book I, ch.10, p.20). This unfortunately also necessitated the assumption of a very great size for the stars which at the time were thought to have an observable apparent extension (Tycho had 2' diameter for first magnitude stars). At these distances such a star would of course have to be tremendously large. Gilbert was fully aware of these problems, and if he hesitated between the Copernican and the Tychonic view, it was due to them. The statement that Copernicus' system was 'harder to believe in', however, occurs in connection with a survey of astronomical systems and Gilbert in fact does no more than point to a problem it faced in convincing astronomers of its veracity, rather than saying that it could not be correct. We see once more that Gilbert remained independent in outlook and took account of evidence which seemed to contradict Copernicus' assumptions even though his sympathies lay clearly with them.

The 16th century scientist who readily accepted all Copernican ideas may seem to us to have been the most progressive; but this can only be so with important caveats, for there were up to date observational facts which could not easily be accomodated in parts of his system, and the advantages of its greater simplicity and perhaps intuitive naturalness (particularly to one who accepted the diurnal motion) had to be weighed against the apparent problems. As we have seen, Gilbert was aware that Copernicus was mistaken in assuming the third motion of the earth (details in ch.XXII of Liber II, *de Mun.*). This showed that Copernicus was not immune from error, although the mistake as far as the librations (the trepidations and the change in obliquity of the ecliptic) were concerned would have been due to lack of accurate observational data. His views were in any case to be examined critically and strictly in the light of observational evidence. The accepted size of 2' for first magnitude stars and their lack of parallax made them seem such vast bodies that Johnson (op.cit. p.110) says that it was as hard to believe in this as in the Ptolemaic velocity of rotation of the fixed star sphere.

D. Magnetic forces in cosmology.

The interesting novelty which Gilbert added to the cosmological speculations of the time concerns his proposals of the magnetic nature of some of the forces which influenced the earth and the moon. He thought that the directional stability of the earth's axis in space was due to its being a magnet. As a terrella turned to align to the earth's field, so the earth itself took a position in space through its magnetic force. If therefore the earth's axis was ever pushed from its proper direction it would no doubt re-align itself again

"the whole earth would act in the same way [as a terrella in the earth's field], were the north pole turned aside from its true direction; for that pole would go back, in the circular motion of the whole, toward Cynosura"(*de Magnete,* p.327).

He does not say that this re-alignment would happen because of the existence of a cosmic magnetic field, as he would have had to have done in order to be consistent with the experimental evidence from the terrella. I do not, by the way, believe that the postulated magnetic stability of the directional alignment of the earth's axis influenced Gilbert's views of he question of the earth's movement around the sun. For he would not have thought that a sideways movement of a magnet floating on water would be hindered by the directional pull of the earth's field. The fact, therefore, that the earth's axis pointed always to the same region of the heavens would not hinder a motion around the sun.

The general cosmological facts were that

"By the wonderful wisdom of the Creator, therefore, forces were implanted in the earth, forces primarily animate [i.e. magnetic], to the end the globe might, with steadfastness, take direction, and that the poles might be opposite, so that on them, as at the extremities of an axis, the movement of diurnal rotation might be performed" (VI.4, p.328).

and the

"circular movement of the loadstone to its true and natural position shows that the whole earth is fitted, and by its own forces adapted for a diurnal circular motion" (VI.4,p.332).

But Luisa Muraro (1978, p.144) is mistaken in saying

"Come annuncia nel titolo del suo libro Gilbert teorizza che la Terra e un magnete e da cio deduce che essa ruota intorno al proprio asse"

(as he announces in the titel of his book, Gilbert theorizes that the earth is a magnet and from that deduces that it rotates around its own axis).

As we have seen, Gilbert gave many Copernican arguments for the diurnal revolution of the earth, and his statements do not at all amount to a deduction of the reality of the motion from the earth's magnetic nature. Gilbert describes the diurnal motion of the earth as magnetic. But he does in fact make little of this in a detailed defence of the hypothesis of the daily rotation. Ch. 3 of book VI, *de Magnete*, for example, is entitled "Of the daily magnetic revolution of the Globes, a probable hypothesis" - yet the text contains no reference to magnetism whatever, but brings forward only the usual Copernican arguments for the earth's revolution. The magnetic nature of the earth provides the axis of rotation and one of the causes of the revolution but need not be cited in a proof of the rotation's reality. Like Muraro, Johnson is therefore mistaken when he says ". . . and when Gilbert discovered that his terrella rotated in a magnetic field, he immediately seized upon this fact as a physical proof of the earth's rotation" (1936, p.407).

A re-alignment of the magnetic axis of a magnet to the direction of an external field is of course something different from a rotation around this axis, as Gilbert well knew. That the latter was also partly magnetic in character was something he assumed but would not have used as proof of the revolution's reality. He had experimental evidence for the direction-seeking of the axis of the floating terrella in a field, but knew of no way to show how it could rotate around this axis. There are speculations of such a motion in Peregrinus' letter, and Gilbert expresses deep doubts about these in VI.4, p. 332:

"I omit what Petrus Peregrinus so stoutly affirms, that a terrella poised on its poles in the meridian moves circularly with a complete revolution in twenty-four hours. We have never chanced to see this: nay, we doubt if there is such movement, both because of the weight of the stone itself, and also because the whole earth, as it moves of itself, so is propelled by the other stars; but this does not occur proportionately in any part of the earth, a terrella for example ".

In *de Mundo* (p.138) he added to this that Peregrinus was mistaken and that such a "new machine of perpetual motion . . . can in no way be constructed". (This remark also seems to show, then, that this part of the *de Mundo* was written after the *de Magnete*.) Gilbert is seen to stick to the evidence, however convenient a demonstration of a diurnal rotation of the terrella might have been to his ideas.

It would have been a fairly elementary mistake for Gilbert to have made, had he cited the turning movement of alignment of the polar axis of a magnet to a field as evidence for a rotational movement around this axis. He believes that some of the forces causing the daily turning be of a magnetic nature. But he has to leave it completely open how this could come about because the experimental analogy from the terrella or any other observation is lacking. This is the reason for his silence on the magnetic aspects of the revolution in chapter 3, book VI, *de Magnete.*

What Gilbert adds to the usual Copernican arguments for the earth's revolutions is the most remarkable fact that the earth's axis of rotation is also a magnetic axis and that a terrella in the earth's field will turn its magnetic axis into its direction. He showed that the terrestrial magnetic force can turn even a very heavy floating magnetic rock with its boat. But he states that the daily rotation was chiefly caused by the influence of the sun's 'virtues' (which he nowhere describes as being magnetic), and he does not even offer a suggestion as to how an outside magnetic force could help the innate terrestrial magnetic virtue in turning the earth diurnally. He thinks that unspecified solar forces turn the earth and that the latter's magnetism - its 'primary energy' - assists in this.

The magnetic evidence he has is at best suggestive of a daily rotation:

". . . si . . . in medio quiesceret, et librata esset terra, ut certa consisteret positione [the reference is to the Aristotelean view], inutilis esset telluris politas . . ."

(if the earth rested still in the centre and was suspended so that it stood immobile in a certain position, its polarity would be useless, p.146).

Thus the existence of the magnetic axis in the terrestrial sphere, for which he has other reasons to postulate a rotation, makes this motion's reality more likely. But the existence of the terrestrial magnetic axis does not show that the earth turns for magnetic reasons. The claim of the quoted sentence is rather that the observed degree of stability of the alignment of the axis is assured for a turning earth by its magnetism.

The explanation for the revolution of the earth is partly teleological: it takes place for the sake of the equal distribution of temperature and the general good of the earth, "so the earth seeks and seeks the sun again, turns from him, follows him, by her wondrous magnetical energy" (VI.4, p.334). It was the power of the sun to which Gilbert had attributed the rotation on the preceding page, evidence that he believed the rotation to be due to a combination of solar and terrestrial forces. His ideas are speculative and his formulations are obviously less than clear.

It was also arranged for the benefit of having equitable seasons on earth that the earth's axis has an inclination of 23 degrees 28 minutes against the ecliptic as he says in VI.7., *de Magnete*. The teleological trait in this is at bottom to be understood religiously: things were arranged in this way by the creator. It is therefore a mistake to consider Gilbert's views to be less scientific in this sense than other views of the world which are based on the belief in a prescient purposive creator.

Trouble would threaten from the moon if the earth did not revolve, for the tides would become upset and the sea would rise unduly in places. The connection between the earth and the moon is particularly close and in part of a magnetic nature:

"Perveniunt ad tellurem effusae vires Lunares, fluidaque corpora agunt; perinde magneticae virtutes telluris Lunam circumfundunt; ambo utrarumque conactu conveniunt, consentiuntque motuum proportione & conformitate; magis tamen imperat tellus vincente mole. Tellus fugat Lunam & allicit; Luna suis sedibus & viis perstat, fugat tamen intra terminos quosdam, & allicit: non ut coirent corpora, quemadmodum solent magnetica, sed ut cursum repeterent"

(the effused lunar forces reach the earth and set liquid matter in motion; terrestrial magnetic virtues surround the moon in like manner; they draw each other mutually through joint action and they agree together by a proportion and conformity of motions; however, the earth rules more by an over-powering mass. The earth flees the moon and attracts it; the moon remains in its places and paths, flees, however, to certain limits and attracts: not so that the bodies come together as magnetic ones usually do but in order to make for their course again; p.187).

The idea that the tides were caused magnetically was current at Gilbert's time. According to Duhem (1977, p.233 ff) it goes back as at least an analogy to the Middle Ages when William of Auvergne compared the moon's action on the seas to the action of magnets on iron. Duhem says that a magnetic theory of the tides was generally accepted in the middle of the 16th century. Such ideas were of course particularly far-fetched at a time when it was not known that the earth had a magnetic field. Gilbert could have adopted them with seemingly much greater justification, yet he cautiously cites magnetic action still only as an analogy: after denying that the moon moves the seas through light or through rays, he says on p.307 that the tides are due to

". . . corporum conspiratione, atque (ut similitudine rem exponam) Magnetica attractione"

(a common action of the bodies [i.e. the sea, earth and the moon] and - to explain the matter by a simile - magnetic attraction).

On p.186 we read that the moon moves the seas *"actu astricae virtutis"* (through an act of an astral virtue). This again could not be magnetic in origin, for the spring tides were, according to Gilbert, caused by the additional influence of the sun for which he - as already stressed - nowhere postulates magnetic virtues.

E. The forces between the globes.

The influences of the celestial bodies on one another, which I will consider again in the next chapter, are by no means magnetic, with the important exception of those of moon and earth, but due to unspecified virtues peculiar to the respective globes. Gilbert does not consider the force of the magnetic virtue of the earth on the moon and that of the moon on the earth, through the vacuum in between, in any detail. But his hypothesis that magnetic forces act between at least two celestial bodies is of great importance in suggesting a bridge across the gulf between the terrestrial and the celestial realms characteristic of Aristotle's cosmology. This bridge has particularly attractive features: the magnetic force or its effects were phenomena completely accessible to experiment on earth; and then the earth itself turned out to be a magnet. Even though the force's effects on the moon was a matter of speculation only, the indisputable fact that the very same force inherent in some small stones and pieces of iron was a property of a whole planet which it surrounded, extending as far as one could tell at least some way into space, meant that some terrestrial physics at least should perhaps be directly applicable to the rest of the universe. This, it seems to me, must have contributed to the downfall of the Aristotelean cosmology in the thinking of many astronomers of Gilbert's time and its further influence is evidenced by the use of magnetic forces by Kepler and others.

The action of the powers of the heavenly globes on the earth shows itself in other ways, for example in the case of the precession of the equinoxes:

"nam praecessio aequinoctalium conversionum, ab inflexione quadam axis terrae fit: & poli telluris non vere & praecise alligati sunt punctis, in

caelo & aethere destinatis, nec stellis aut astris polaribus; sed constantiam habet terra a proprius viribus, quae cum moventium globorum potestatibus ante multa secula, & ab ipsis primordiis mundani systematis, perpetuo faedere conspirant."

(for the precession of the aequinoxes comes from a certain inclination of the earth's axis and the poles of the earth are not really & precisely bound to fixed points in the heavens and the aether nor to stars or the pole stars. But the earth has constancy from its own forces which work together by a perpetual compact with the powers of the moving globes since aeons and from the very beginnings of the world system; p.136.)

The general underlying principle concerning the motions of celestial bodies is that "whatever in nature moves naturally, the same is impelled by its own forces and by a consentient compact of other bodies" (VI.3, p.322). The harmony of the periodic motions of the celestial bodies is thus accounted for by the postulated mutual influences, the sun providing the strongest force:

"Sol praecipuus in natura actor, ut erronum promovet curses, sic hanc telluris conversionem incitat"

(the sun, the principal agent in nature, as he promotes the courses of the planets, so does he incite this revolution of the earth; p. 142.)

In the *de Magnete* he had a similar sentence in which he had added that the sun does this "through the effused virtues of its orbes and through light". Gilbert was obviously more careful in his suggestions in this part of *de Mundo* in not speculating on the nature of the forces.

Although each of the bodies had forces *sui generis*, these had the effect of together ordering their positions and causing their movements through space. This is a very important advance on the view widely held in the 16th century, viz. that circular motion was 'natural' to heavenly bodies in the sense that one did not need to assume the existence of any particular forces to account for it (the creator had perhaps set the system in motion and no particular forces were needed for its continuation). To Gilbert circular motion was also natural yet forces were required. Its 'naturalness' seems to be connected to the absence of friction and other direct material restraint so that the body can follow the compound of its own forces and of those of the distant globes. I will return to the question of natural motion briefly in the next chapter.

Gilbert's revolutionary discovery of the earth's magnetism made the idea that a celestial body could be a source of force quite natural to him. Kepler was much impressed by Gilbert's discovery and postulated a special force, the *species motrix*, to drive the planets, augmented by additional magnetic forces to account for the variations of their distances from it. Gilbert was too careful to assume that magnetism had any part in the motions of the celestial bodies except for those of earth and moon.

Chapter Ten.

Gravity.

A. Forces and effluvia.

The last chapter touched on questions concerned with gravity, but in Gilbert's scheme the motion of the heavenly bodies and the other manifestations of gravity are not really properly connected as we will see, although for him, too, attraction plays a role in all of them. The concept of 'gravity' or 'gravitation' as the comprehensive cosmological force which causes free fall, represents the pull of one heavenly body on another, and holds each of them together, is of course not really applicable before the time of Hooke and Newton. Gilbert considers all these manifestations of gravity in some detail, but he does not say that they are due to magnetism, as alleged by many commentators. There exists considerable confusion about Gilbert's thoughts on gravity. It is interesting to examine these details and follow the development of the ideas on this subject since Copernicus.

Copernicus had said that the revolving earth did not disintegrate because revolution was natural to a spherical body (cf. Copernicus, 1947, bk. I., ch.8, p.13). Gilbert is not satisifed with this, but is looking for a specific force or mechanism to account for the earth's cohesion. To what then was this due? There was first of all the force which kept the "foundations of the earth . . . conjoined, connected, held together". This occurred largely magnetically. "So let not Ptolemy of Alexandria, and his followers and our philosophers, maintain that the earth will go to pieces, neither let them be alarmed if the earth spins round in a circle" (II.23, p.142). As the core of the earth consisted of the primary magnetic material, Gilbert had a substantial basic force which held the centre together. As to the general coherence of the rest of the earth, he is very brief, saying in II.2, p.97 only: "The matter of the earth's globe is brought together and held together by itself electrically".

He then needed to explain free fall on earth and elsewhere and was concerned to show that there was no natural place for bodies in the centre of the earth, the Aristotelean centre of the universe, toward which they would fall. This was a job to be done by every Copernican; for with a moving earth, the problem of free fall required a solution very different from Aristotle's. There had to be a direct relationship between the falling object and the body of the earth, cutting out the importance of the latter's supposed central position. Gilbert's discovery of terrestrial magnetism had a most important effect on this problem. On Aristotle's scheme of free fall, the earth did not have to exert any force on a falling body or provide any other mechanism to cause the fall which was one towards the centre of the universe and in a sense one only incidentally toward earth. Therefore, the idea that the earth as whole might exert a force was alien to Aristotle. Later scientists could not show that there were any terrestrial forces until Gilbert discovered that of magnetism. This, then, changed the position at a stroke. If the earth had a magnetic force, it might seem much more likely that it could also have an attractive one. Gilbert was therefore in the best position to provide support to the Copernican system by a straightforward postulation of a gravitational attractive force. He was, though, still too deeply immersed in traditional thought to do so outright and assumes that all matter naturally seeks to unite again with the heavenly body it originated from. However, he also importantly postulated two forms of attractive force in addition: one of a quasi-electrical nature and one to act across empty space on the other heavenly bodies. This results in a somewhat confusing array of forces and effects.

He defines gravity after various general remarks on the motion of the traditional elements and of light and heavy bodies:

"Terra in loco suo non manet propter gravitatem, ut antea docuimus: ita neque corpora confluunt ad terram, nisi quae ab ea egressa fuerint. . . Est igitur gravitas corporum inclinatio ad suum principium, a tellure quae egressa sunt ad tellurem"

(The earth does not stay in its place because of weight as we have taught before: so bodies do not come together to the earth unless they had originated from it . . . therefore gravity is the desire of bodies for their origin, of that which came from the earth for the earth, p.47.)

This also applied to the sun and moon and the other 'primary orbs': objects originating from it would seek to reunite with the main body. Gravity here then seems to refer to free fall.

On p. 115 he sets out some of the wider underlying ideas concerning relevant aspects of his cosmology:

"Rerum igitur conditor, ne omnia in omnibus essent, & confunderentur, non singulis partibus primariis loca, circa quae, aut in quibus, conglobantur, & haerent, sed corpora ordinavit primariis virtutibus praedita, quibus mutuo disponunt sese, & per intervalla in mundo ordine mirabili combinantur. Non enim aut propter centrum, aut locum, aut gravitatem permanet, nec circumfuso pendet in aere tellus, ponderibus librata suis, *ut poeta cecinit, & credunt Philosophi nonulli. quod si libratio circa centrum aliquid facaret, adjuvaret potius motum circularem; sed gravitas nihil urget."(Gilbert's emphasis)*

(The creator of nature, to prevent everything being mixed up with everything else, did not appoint locations for the individual primary parts around or in which they accumulated and adhered. But he provided bodies furnished with primary virtues through which they ordered themselves in mutual relation to each other and combined in intervals in space in a wonderful order. For not on account of a centre or a place or gravity does the earth remain or hang in the surrounding air balanced by its weight, as the poet sang [this must refer to Ovid, Metam. I.11] and many philosophers believed. For if the arrangement around a centre does anything, it should rather support the circular motion; but weight drives nothing.)

He had already said on p. 61 that *"fixae stellae in determinatis locis permanent, sed non a loci natura: locus enim nec ens est, nec efficiens causa . .* " (the fixed stars stay in determined places, but not because of the nature of the place: for place is not an entity nor an efficient cause).

This may be contrasted with Aristotle's remark in "On the Heavens" (310b 2-5):

"If the earth were removed to where the moon is now, separate parts of it would not move towards the whole, but towards the place where the whole is now [i.e. the centre of the universe]".

As mentioned, Gilbert's view is built on that of Copernicus:

"Now it seems to me gravity is but a natural inclination, bestowed on the parts of the bodies by the creator so as to combine the parts in the form of a sphere and thus contribute to their unity and integrity. And we may believe this property present even in the Sun, Moon, and the Planets so that thereby they retain their spherical form notwithstanding their various paths" (Copernicus, op. cit. I, ch.9, p.15),

But Gilbert adds the action of forces, or at least 'attraction by virtues', to Copernicus' scheme:

"Partes vero primariorum globorum integris alligatae sunt, in illos naturali desiderio incumbunt. Quicquid enim terreum est, in terrae globum confluit: ita Soli homogeneum, in Soli corpus, Lunaria omnia in Lunam, & sic de caeteris corporibus universi. Singulae autem eorum partes suis totis adhaerent, nec inde sponte moventur; si vero inde motae fuerint, non solum eo redite nituntur, sed globorum virtutibus alliciuntur advocanturque. quod si non fieret, & si partes sponte separarentur, nec

redirent ad sua principia, mundus totus brevi tempore dissiparetur, confundereturque. Non autem est appetitus aut inclinatio ad locum, aut spatium, aut terminum; sed ad corpus, ad fontem, ad mátrem, ad principium, ubi uniuntur, conservantur, & a periculis vagae partes revocatae quiescunt omnes. Ita tellus allicit magnetica omnia, tum alia omnia, in quibus vis magnetica primaria desiit materiae ratione; quae inclinatio in terrenis gravitas dicitur."

(The parts of the primary globes are really bound together into wholes, to which they incline by a natural desire. Whatever is of the earth, comes together in the globe of the earth: so whatever is of the sun, in the sun's body, all lunar substance in the moon, and so with the other bodies of the universe. Single parts of these moreover stick to their wholes, nor do they move thence of their own accord; if they should indeed be moved from there, not only do they strive to go back thither, but they are attracted and summoned back through the virtues of the globes. If this did not happen and the parts separated spontaneously nor went back to their origins, the whole world would be dissipated in a short time and thrown into disorder. Nor is there an inclination to a place or space or limit, but to a body, the origin, the mother, to the beginning where they are united, preserved and all wandering parts called back from dangers, and where they rest. So the earth attracts everything magnetic, as everything else in which the primary magnetic force is absent due to the material make up. This inclination to earth is called gravity, p. 115).

So Gilbert, as already mentioned, had two factors which caused bodies to fall: their own desire and also the earth's attraction. This latter he said, acted through a form of electric action. He had found that electrics attracted everything, as did the earth (i.e. objects of terrestrial origin). Electric attraction was due to an effluvium. The earth also had an effluvium, the air (the other globes had effluvia of their own):

"Air, too (the earth's universal effluvium), unites parts that are separated, and the earth, by means of the air, brings back bodies to itself; else bodies would not so eagerly seek the earth from heights." (II.2, p.92).

In the *de Mundo* he states that bodies within the shells of the effluvia surrounding the globes move back *"ad globos"*. Therefore

". . . si extra effluvia telluris pars telluris longius fuerit, ad tellurem non delabitur; non aliter atque electrica ultra sua effluvia allicere corpora non possunt"

(if a part of the earth is further outside the effluvia of the earth, it does not fall to earth, just as electrics cannot attract bodies outside their effluvia, p.50).

But if the effluvium provided the mechanism or force for gravity in the sense described, it is not clear what the desire for unity with the star or planet contributed. There was after all no desire in small bodies to *return* to the excited electric from which they had of course not originated.

Anyway, the bodies' innate desire for unity and the globes' quasi-electrical attraction caused free fall. Yet Gilbert also postulated attractive forces (in the modern sense) between the heavenly bodies which acted across empty space. It therefore appears that he could have managed very well without the effluvia to explain free fall by leaving these attractive forces to do the work in free fall as well. His reason for not doing so may have been that he thought that the attractive forces between the globes would cause them all to fall into one another if they alone could cause free fall. This left the strange situation of the globes's attractive forces being able to help in keeping them in their paths relative to one another but not sufficient to make a stone fall down to any of them.

In space, on the other hand, bodies moved in circles:

"Motus vero circularis globorum primariorum est vera naturalis incitatio, & in vacuo fit nullo renitente corpore.";

(in fact the circular motion of the primary globes is the true natural motion and takes place in the vacuum without resisting matter, p.50)

Was the help of the globe's effluvium then perhaps needed because free fall was non-natural because non-circular? Copernicus had thought only circular motion to be natural (as against Aristotle who allowed rectilinear motion as natural for heavy bodies near the earth):

"therefore there is no rectilinear motion save of objects out of their right place, nor is such motion natural to perfect objects . ." (Copernicus, I. ch.8., p.14), and "we must admit the possibility of a double motion of objects which fall . . . in the Universe, namely the resultant of rectilinear and circular motion" (ibid.; this must refer to the fact that the falling object would take part in the earth's revolution but fall vertically from the earthbound observer's point of view).

Gilbert's pronouncement on this aspect of free fall is simpler, and he says in VI.5, p. 341 that a heavy body falls down in a straight path which is not a "composite . . [of] . . coacervation and a circular motion" (thus referring only to the terrestrial observer's perception).

In any case, as his conception of 'natural motion' included the action of forces from one globe to another, it certainly had a different character from Copernicus'.

B. Historians on Gilbert's views.

Many historians have been confused about Gilbert's views on gravity, magnetism, and the motion of the heavenly bodies. Burtt (1980, p.165), describes Gilbert's ideas:

"The earth and every other astronomical body send out these magnetic effluvia to certain spatial limits, and the surrounding incorporeal ether thus composed shares the diurnal rotation of the body. Beyond this ethereal vapour there is void space, in which the suns and planets, meeting no resistance, move by their own magnetic force".

And Butterfield has:

"The force of magnetic attraction was the real cause of gravity, said Gilbert, and it explained why the various parts of the earth could be held together . . . At the same time, this attraction was not regarded as representing a force which could operate at a distance or across a vacuum - it was produced by a subtle exhalation or effluvium, said Gilbert . . . That gravitational pull towards the centre affected not merely bodies on the earth, he said, but operated similarly with the sun, the moon, etc., and these also moved in circles for magnetic reasons. Magnetism, furthermore, was responsible for the rotation of the earth and the other heavenly bodies on their axes" (op.cit., p.140-141).

Marie Boas, too, thinks that Gilbert believed gravity on earth to be due to magnetism:

"Very interesting is his discussion of gravity [in the *de Mundo*], which he believed to be caused by magnetic attraction and hence to diminish with distance from the centre of the earth" (Boas, 1958, p.458; cf. also her 1962, p. 195)

It is puzzling that so many commentators have thought that Gilbert postulates a magnetic force of free fall and of gravitational pull not only between earth and moon but between (and on) all celestial bodies. Gilbert refers throughout *de Mundo* to the forces possessed by the celestial globes as being of their own type, peculiar to each body:

"Sunt enim in astris formae primariae, singulares & propriae; sicut in tellure, magnetica praepotens & egregia",

(there are indeed in the stars primary, singular and specific forces; as in earth the distinguished ruling magnetic one, p.80).

On p.146 he says with reference to the other celestial bodies

". . . *qui etiam suis polis firmantur,* licet non magneticis, *sed suarum naturarum distinctis viribus propriis* (my emphasis).

(which are also firmly held through their poles, *even if these be not magnetic* but by the proper distinct virtues of their natures).

Roller reads Gilbert as expressing the belief "that each planet, star and satellite has its own proper magnetic form. . ." (Roller, op. cit., p.163) and "that each of the major objects in the universe possesses its own peculiar magnetic properties, form" (p.153). Does this amount to a postulation of various different types of magnetism, something Gilbert does not consider anywhere in his books? Such a reading of Gilbert's views could only be correct, therefore, if we assume that Gilbert thought that any formal property, which gives rise to forces, was magnetic. But this disagrees with the quotations just given, especially with the definite *"licet non magneticis"* from p.146.

Gilbert in fact does not make "a philosophy out of the observation of a loadstone" as Bacon claimed, and in assessing Gilbert's scientific standing it is most important to keep to the limits he set to the scope of magnetic phenomena. When considering his cosmology we should remember, by the way, that he himself was aware that he moved in the realm of speculation in many respects. He says in the "Author's Preface", *de Magnete* (p.1):

"After the magnetic experiments and the account of the homogenic parts of the earth, we proceed to a consideration of the general nature of the whole earth; and here we have decided to philosophize freely, as freely as in the past, the Egyptians, Greeks, and Latins published their dogmas."

He did speculate freely on some of the questions but he was careful to avoid postulating a uniform magnetic force which indiscriminately governed all heavenly bodies. As there were no primum mobile, spheres, or alloted spatial positions for the stars and planets, they had to be governed by forces. But there was no evidence for the assumption that these were in general of a magnetic nature and his scientific caution prevented him from making the claims which too many of his critics ascribe to him.

Chapter Eleven

The Origin of Gilbert's work.

A. Gilbert and Norman.

Before we can assess Gilbert's overall position in the science of his time, we need to evaluate the origin and novelty of Gilbert's magnetic researches by further considering their relationship to Norman's and Porta's work; that to Peregrinus' results has been examined earlier. Gilbert's debt to his predecessors is not that of a recipient of facts, experimental results, theories or hypotheses he might simply have taken over and developed further. It concerns exclusively suggestions for numerous experiments, some of which were most important. Gilbert - as stressed before - repeated all his predecessors' magnetic experiments, however strange they may have appeared to him (such as, for example, that of rubbing magnets with garlic). In this sense, therefore, it may be said that he owed a debt to anybody whose claims concerning magnetism he had become aware of. That his greatest debts are due to Peregrinus, Porta, and Norman is a consequence of the interest and range of their work.

Peregrinus' researches were probably those of the greatest basic value to him because of the suggestions for the use of the spherical magnet. He would have been able to manage without Porta's opinions, although the latter reported very many experiments. He would perhaps even have discovered the dip of the needle because he did not rely on ready balanced compasses but magnetised his own needles and might well have noticed the effect consequent upon magnetisation. From the fact that a magnet floating on a little raft did not move northward he would have concluded that the needle was only turned, not attracted, to north, which was one of Norman's important results (the arrangement of floating the magnet being due to Peregrinus). Whether he would have hit upon Norman's idea of suspending a wire in cork under water, thus showing that the needle was not attracted in the direction of the dip, seems much less certain, it being a particularly ingenious experiment. Gilbert's debt to generations of navigators with respect to the behaviour of the compass is obvious.

Considerations of the importance of the help he received from the suggestions for experiments from his predecessors are, however, speculative. It is very likely that his work would have taken much longer without them. But Gilbert was also the heir to many mistaken reports, whose examination was time consuming, but nonetheless perhaps instructive. It seems in any case that the whole body of earlier work was of the highest value to him. Yet when Zilsel, whose views I will consider below, said "altogether, the impression of Gilbert's originality is considerably impaired, when he is confronted with his sources and especially Norman" (Zilsel, op.cit. p.25), he was mistaken.

Although both of Norman's most important experiments have been described already, it will be necessary to examine his other work in a little more detail if we want to assess a claim such as Zilsel's. Norman's pamphlet is written in an attractive tone of modesty, combined with pride in the abilities and achievements of the "mechanitians and mariners". These he defends against exhortations by the learned not to "meddle with" experiments and calculations with the compass and the finding of the longitude, because ". . . there are in this land diverse

Mechanitians that in their several faculties & professions have the use of these Artes at their fingers endes" (op. cit. p. 2). He says that he grounded his arguments "solely upon experience, reason and demonstration". These remarks are important. As we have seen, they were echoed by Gilbert and express the development of the experimental spirit and practice of the time.

Norman surveys the different types of magnets and the locations they are found in. He then characterises magnets and their properties briefly, describing correctly the basic phenomena of attraction, repulsion, induction, and the two poles. He says that the magnetic virtue is distributed in spherical form around the stone (ch.VIII). The magnet, when floated in a dish on water, will "directly show the line of variation, or imagined Attractive point" (ch.I). Even near magnetic mines (as on Elba) the compass is not drawn or changed (ch.II). This may refer to first-hand knowledge he acquired in his twenty or so years at sea.

He then describes his discovery of the dip of the steel needle upon magnetisation, with its angle of 71 degrees and 50 minutes for London (ch.IV), one of the most important magnetic discoveries thitherto. Such a needle, stuck through a cork and submersed and suspended in equilibrium in water, dips after removal, magnetisation and reinsertion. But it does not move translationally either down or up (ch.VI). There is a "point respective" in the earth to which it points. This lies on the straight line of the needle's direction and could be found as the cutting point of the lines of various angles of dip on different places on earth (the different degrees of variation would have to be taken into account on drawing these lines; ch.VII). Norman makes, however, no attempt to locate the point, probably because he was not able to obtain many values for the dip before publication of his booklet. He shows in detail the construction of his inclinometer and gives an illustration of it. From this the angle of inclination could be read off directly because the needle on its horizontal axle points to a scale of angles. It should be used in the direction of variation. The power to show the point respective is in the stone only. Whether it was important to Gilbert's work that Norman considered the point respective to lie in the earth, thus moving the focus of attention from the heavens where Peregrinus had placed it, is uncertain; for Gilbert's wide ranging work would almost certainly have focused on the earth anyway.

Norman informs his readers on other matters: variation is not proportional to any change of position on earth. It behaves quite strangely and cannot be known in advance (as we have seen, Porta claimed otherwise). The common compass is set for some specific variation. At the Azores a newly magnetised - i.e. uncorrected - compass does not point to geographic north. Variation is constant in every place with time and the mariner should make lists of the degrees of variation "in case he comes there another time". Norman exhorts sailors to use only navigational maps which were drawn in accordance with the compass employed, so that the variational allowances of both agree. Clearly Gilbert is deeply indebted to Norman and other navigators for these ideas.

It is interesting to follow Norman's reasoning concerning the compass needle briefly, for it provides a beautiful and perfectly described example of remarkably sound testing of two or three specific working hypotheses in very early science. The discovery of the dip was an accidental result of his practical concerns as a compass-maker. (He had to apply counterweights to the south pointing end of the needle after magnetisation, or else shorten the north pointing half which could be time-consuming and wasteful.) He describes hypotheses which offered themselves to explain the dip. One was that a ponderable substance be transferred from the stone into the needle which causes the north pointing end to become heavier. The first test is to weigh the needle carefully before and after magnetisation. There is no difference. He also reasons that if a weighty matter was responsible for the dip, the needle's end would lower itself by 90 degrees (with the needle on a horizontal axle).

Magnetisation of the needle with the other end of the stone makes no difference to the dip either. Then he examines whether there is a pull of an attractive point in the earth, or the heavens, on one end of the needle. The experiments of floating the needle on, as well as below, the surface of water should decide this. The result is that the needle is not moved translationally but only turned. This falsifies the hypothesis. He is left with the idea of the existence of a point respective and does not know how to proceed further.

Norman likens the distribution of the magnetic virtue to that of smells which can pass from object to object. As he knew that the virtue passes through solid objects, this simile opens further problems, which he, however, did not pursue. An explanation in terms of a sympathy could have been left sufficiently vague to allow for anything and everything but it would not have satisfied the practical compass maker. The advance of Norman's work on Cardan and Fracastoro is striking and the superiority the artisan has here over more speculative writers - even if they conducted some experiments - is obvious. To Norman magnetism was a practical matter of daily experience, something that had to follow rules, which he in turn had to take account of in his work. He saw that the phenomena on which these rules depended could be further investigated and did so in a spirit of curiosity.

His work at a time when fabulous stories about the magnet abounded and were accepted as fact, is an excellent example of a form of early research into specific questions which looked for physical explanations. But although he proceeded in this in a sound scientific manner and with great ingenuity, and although his interest in the wider questions concerning magnetism was obviously aroused, he was no scientist. Apart from following up the questions I mentioned in an exemplary way he did not pursue any researches we know of, although he said in his booklet that he would like to do so. Most of the various facts concerning magnetism he refers to will have been known to him as a matter of course from his daily work and he puts them before his readers for their information. He appends certain speculations to their expositions and to those of his experiments, but he does nothing toward a further elucidation of the properties of magnetism (he mentions, for example, how the virtue surrounds the magnet but he does not investigate the field as Gilbert was to do).

Zilsel (op. cit. p.23-4) is therefore deeply mistaken when he says about Norman:

"Except for the Latin erudition, the quotations and polemics, and the metaphysical philosophy of nature, he has everything that is peculiar to Gilbert; . . . as to scientific value. Norman's attitude does not compare at all unfavourably with Gilbert's. Far reaching theories are lacking in his book; but is Gilbert's metaphysics of 'distinguished spherical form' that brings about magnetism a useful scientific explanation?"

B. Gilbert and Porta.

We now come to Porta's importance for Gilbert's work. Porta took over Peregrinus' results but reported on many more magnetic effects than the latter. Porta will also have learned magnetic facts from Sarpi. (The great Venetian statesman, scholar and scientist Paolo Sarpi - whom Galileo was to call "my father and my master" - wrote a treatise on magnetism which was unfortunately destroyed by fire before it could be published. Porta gratefully acknowledged Sarpi's instructions on magnetism in the foreword to book VII of the *Natural Magic*.)

This book, though it contains a few fairly elementary errors, and is much less systematic than Norman's much more limited work, is without equal as a compendium of magnetic experiments and their reults assembled by a single writer before Gilbert. The latter, though he repeated all experiments relating to Porta's claims, hardly ever gave him credit for correct observations but criticises him frequently. However, he says in I.1. that Porta was, "a philosopher of no ordinary note" who

"makes the 7th book of his *Magia Naturalis* a very storehouse and repertory of magnetic wonders; but he knows little about the movements of the loadstone, and never has seen much of them . . the book is full of most erroneous experiments . . . still I hold him worthy of praise for that he essayed so great a task (even as he has essayed many another task, and successfully too, and with no inconsiderable results), and that he has given occasion for further researches."

The relationship between the two and Gilbert's treatment of Porta's work will require at least brief examination, because it obviously affects the assessment of the originality of Gilbert's work. Porta himself accused him in some strong words of plagiarism (in the guise of the translator of the Italian edition of the *Magia Naturalis*; cf.

Muraro, op. cit. p.143), mentioning Gilbert's 'theft', and the injury done to 'his author'.

Roller surprisingly does not mention Porta at all, even though he gives a chapter of his book over to a survey of the history of magnetism before Gilbert. This is a serious defect in a book which attempts to assess Gilbert's work.

Benjamin, who does deal with it in some detail, says "Porta's writing bears all the ear-marks of the compiler" (op. cit. p.231). He thinks that Porta got the knowledge from Sarpi he did not copy from Peregrinus from Sarpi. But the evidence is against his reading, even though Porta learned perhaps much from Sarpi. Benjamin himself mentions that he was "the author of many discoveries".

M. Boas says about Porta:

"he had some real comprehension of the role of experiment in investigation, . . . he concerned himself with more important and complex properties of the loadstone which he tried, often ingeniously, to test experimentally . . . Porta's optical marvels are more often than not perfectly respectable experiments . . . his pneumatical experiments are also perfectly sound examples of simple engineering . . ." (M. Boas, op. cit. p.188)

These are very apt remarks, and Porta's familiarity with the technology of his time shows itself for example in his proposal to construct a machine for raising water based on Cardan's suggestion of creating a vacuum by condensing steam.

Luisa Muraro, who has written the most recent appreciation of Porta I know of (Muraro, 1978), states that Porta experimented widely and she sees him as the first pioneer of the experimental method in his time. Porta refers to experiments throughout the *Natural Magick*, especially in the section on magnetism, i.e.book VII (all the following references to chapters of Porta's work are to this). In the foreword he says: "In a few days, not to say hours, when I sought one experiment, others offered themselves, that I collected almost 200 of principal note". He also refers to literary sources of knowledge of magnetism, and his procedure in the book is to let his own experiments correct the authority of various famous writers, something Zilsel maintains was first done by Gilbert. In ch. II he says that "my opinions are based on some experiments, others' depend only on words and vain cavils" (this remark, like some of the following, sounds exactly like some of Gilbert's); in ch.V (with respect to the new positions of the polar axes after splitting of loadstones) he writes: "who will believe it unless he tries it"; in ch.VII: "where we have not reason to direct us, experience shall prove it"; in XXVII: "I have for a long time endeavoured to make iron hang in the air and not touch the loadstone . . . But I say it may be done, because I have now done it".

It is unnecessary to multiply such quotations for it is in any case quite clear that the many statements on the behaviour of magnets and iron, with descriptions of certain details of the set up must be due to the experimenter himself, or else they could only have been written by an author who copied almost verbatim what he had been told in minute detail by the experimenter. There seems to be no reason to believe in the latter possibility. We must therefore assume that Porta not only advocated experimentation but performed it to a very considerable extent. That he had a penchant for manipulating magnets and iron becomes clear from his several reports of how he entertained friends by moving "armies of soldiers" (small bits of iron) on table tops with the help of magnets hidden underneath and performing other games with magnets. It is very unlikely that he played such games but did not experiment otherwise as well when he claims to have done so.

I will not give a list of all Porta's statements on magnetic phenomena here, for that would take up too much space. I have in any case mentioned many of them in earlier chapters. But we need to compare Gilbert and Porta with respect to their methods. Porta's *Magiae Naturalis libri XX* first appeared in 1589, i.e. eleven years before Gilbert's work.

Muraro (1978, ch. 5) has considered the relationship between Porta and Gilbert and analysed in some detail what Gilbert may have taken over from Porta. She finds that Gilbert refers 15 times to Porta's book on magnetism (in *"Gilbert cita Della Porta"*, Muraro, op. cit. p.149-160). In the next section (*"Gilbert non cita Della Porta"*, p.160-171) she points out many of the cases of more or less close agreement of specific experiments, even occasional similarity of wordings, in Gilbert's and Porta's books by quoting parallel passages from both works with the implication that in many instancees the later writer based himself on the earlier. However, I will not repeat her useful survey.

The question of how Gilbert's work differed from Porta's is of importance. This amounts, then, to the task of showing how Porta's fairly wide collection of experimentally based but often isolated facts differs from a scientific theory. The answer is straightforward: Gilbert looks throughout for connections between phenomena, for explanations and causes. He senses that he will need to postulate underlying theoretical entities. Their assumption guides the invention of further working hypotheses for testing and gives his work a systematic character which is also of decisive help in tracing errors.

Porta on the other hand carries out many interesting magnetic experiments, but usually leaves their results as isolated facts. He does not connect his observations or investigate further implications of an experiment. Thus, although he thinks, for example, that a diamant magnetises iron he does not experiment to find whether the former has poles, or turns like a magnetic needle. Only occasionally does he try to check on the consistency of very obviously related facts. When he does venture to provide a wider explanation, this is vitiated by his inability to see an important inconsistency as for example in his explanation of the magnetic action: he knows magnetism passes through solid objects but attributes its action to vapour and postulates (or sees) hairlike structures which pass it on. In spite of its shortcomings, however, his work was no doubt of very great importance to Gilbert, suggesting many experiments and providing another example (in addition to those of Peregrinus and Norman) of how successful and inventive experimentation should - and also how it should not - be carried out.

C. The origin of Gilbert's method according to Zilsel.

The envigorated development of science during the two hundred years or so from the middle of the 15th century on is of particular interest to historians who have seen it from varying angles. This applies to Gilbert's work more than to that of many other figures of the time. Boas, for example, places it in, or at the end of, a tradition of 'natural magic', whilst others contrast it with established humanist scholarship from which it is seen as having emerged by an adoption of a more mundane approach. Zilsel has examined the sources of Gilbert's scientific method and found them to have largely been the independent spirit and manual work of labourers, mariners, and artisans, but in this he really refers only to Norman. Zilsel's views have exercised a certain influence on historians of science and I will therefore examine them. It seems to me that his papers are vitally flawed by a one-sided approach and by important individual omissions. The most striking example of the second form of error is the fact that he completely ignores Porta, merely mentioning him as a "learned compiler of curiosities" (op. cit. p.8 & p.17 - unless otherwise stated all references to Zilsel are to this paper). My criticism will in part focus on this neglect.

Zilsel's thesis is that a close relationship with artisans, seamen and workers in iron smelters and mines at this time of early capitalism in England was a precondition of Gilbert's scientific method which overcame the distaste of the academic for the 'manual labour' of experimentation. Zilsel's suggestions are not without interest, mainly because they offer a warning that considerable care and perspicuity is needed in an approach to the history of science which concentrates on social and economic conditions. What seems to me of value in his views is an implied, strictly limited, part of his claims, viz. that academically trained men, as they became more interested in natural science, saw that they had to learn from artisans many technical facts and practical procedures which they would then have to apply in the laboratory if they wanted to experiment. (But Gilbert would not have been the first scholar to do so. We need only think of the considerable amount of practical skills Agricola, who died in 1555, must have learned from miners and other manual workers, and which he applied in his researches into metallurgy, mineralogy, glassmaking etc.)

Zilsel (1958, p.255) says that artisans had "the aim of gradually increasing knowledge through the method of trial and error. This method and this aim develop easily among craftsmen who have freed themselves from the bounds of the mere workshop tradition."

and speaks of

"the social rise of the experimental method from the class of manual labourers to the ranks of university scholars in the early 17th century" as "a decisive event in the history of science" (1941, p.30).

He also says on the same page that

"Bacon's far reaching ideas on the advancement of learning and scientific cooperation could scarcely have been formed by craftsmen, *though they were nothing but generalizations of their own practice* "(my italics).

These statements are unbelievable. The evidence he presents for the existence of 'the experimental method' amongst manual labourers is not impressive. It consists mainly of the claim that Norman was a case in point. Zilsel assumes by far too much. It is likely that artisans have over centuries and millenia slowly found by trial and error - but probably more often by serendipitous changes in procedures or materials - better ways of doing things. Artisans will indeed at various times alsohave tried out different ways of working in the hope of improving results. But even if this be so, one should not speak of their applying the 'experimental method'. If they had been in possession of this in a sense which approaches our modern understanding of it - Zilsel unfortunately does not define it - progress in technology probably would have been considerably faster. (We may speculate, for example, on a much earlier advance in the procedures of iron smelting, such as Darby's in 1709, if an experimental method had been applied to a technology by then about 2500 years old). Norman cannot be seen as a typical 'manual labourer' or artisan; he must rather be regarded as a very exceptional one.

As mentioned, Zilsel's understanding of the origins of magnetic work in the renaissance is severely prejudiced by the fact that he neglected the work of Porta. Had he taken this into account, his claims would have had to have been considerably weakened, for Porta had made many of the experiments before Gilbert. Zilsel says on p.24, op. cit.:

"Gilbert's experimental method and his independent attitude towards authority were derived not from ancient and contemporary learned literature, but on the one hand from the miners and foundrymen, on the other from the navigators and instrument makers of the period".

Such a claim should not be maintained without evidence. It is more likely that Gilbert sought information from practical men *because* he had an independent attitude toward the authority of learned writers (such as his fellow-professional Agricola had perhaps displayed 50 years earlier). However, the scientific literature of the time contains a number of calls for more experimentation, for example, as mentioned, from Digges, and from Porta who also put them into practice and provided many new results. It may well have struck an experienced doctor that these indeed were very sensible demands. Gilbert may have found during his medical work that authority based on traditional writings was often no substitute for practical experience (this may also apply to others of the early doctor-scientists). But in any case, a special explanation of Gilbert's method in socio-economical terms is not necessary unless it was admitted to be so in Porta's case as well.

Whatever the details of Gilbert's relationship with practical men were, one cannot agree with the conclusions expressed in the last paragraph of Zilsel's paper (1941, p.32). He refers here to the fact that the first machines were made of wood and that with such devices the

"Italian artist-engineers and Stevin made their studies and found quantitative relations and laws. Galileo . . . made brass balls roll down an inclined wooden grove. Not before the 18th century did iron machines . . . become the subject of calculation. In the preceding centuries, therefore, predilection for iron prevented rather than promoted application of mathematical methods. Thus England's natural, economic, and social conditions might form, not a sufficient, but a necessary condition for the characteristics of Gilbert's method . . . England, the country of iron mines and advancing navigation, produced the first learned book on experimental physics. It dealt with the mariner's compass, magnets, and iron. And for that very reason it did not introduce mathematical methods into natural science."

I find much of this self-contradictory (and this not only because Zilsel had said on the preceeding page that "iron manufacture was not yet the leading industry of England" and that "in the 16th century iron had not yet reached its dominant part in technology", although he also says that iron-making was advancing fast). It may be noted, for example, that he completely ignores the facts that Galileo spent considerable time on magnetic experiments and that Stevinus and Gilbert were working on exactly the same navigational problems. The statement referring to the difference between quantitative and qualitative researches in relation to the character of the materials used - wood and brass versus iron - seems astonishing.

He says on p.3 that Gilbert "performs measurements practically only when he deals with quantities which are important in navigation . . ". He thought that Gilbert was not interested in mechanics:

"Gilbert's pre-mechanical way of thinking and his predilection for a field where measurements are so difficult may be due to his individual characteristics. But they are connected also with the special conditions of his native country. Practically all quantitative investigations in *De Magnete* originate in nautical techniques and the work of the compass-maker Norman" (ibid., p. 31)

Zilsel had said that Gilbert did not measure magnetic and electric phenomena because this was too difficult at the time, and that these were not measured until 200 years afterwards, by Coulomb (ibid.). So one wonders that he should ask "Why did Gilbert himself never reckon, why did he come to a standstill at the first beginnigs of quantitative inquiry?" (op. cit., p.30/1).

The up-shot seems to be that Gilbert was not particularly interested in mechanics but was interested in magnetism, that he did not make measurements which were to prove too difficult for another 200 years, but did quantitative work in navigation, where it was possible and also desirable - a meagre result for Zilsel to have come to. (Gilbert's non-mechanical way of approaching magnetism was a blessing. It helped the formation of his field concept which a Cartesians approach, for example, could not come to. Hesse also holds it to "Gilbert's credit that he is not trapped into mechanism as an explanation of magnetic phenomena, as were all his 17th cent. successors until Newton"; Hesse, 1960, II, p.139)

Zilsel says that it was only to be expected that an interest in magnetism arose particularly in the England of the time because of the importance of navigation and iron-smelting. Had he taken note of Porta's experiments, he would not have been able to claim a uniquely close and novel connection between conditions in England and experimental magnetic researches. It should be remembered that a vital interest in navigation was always alive in Sarpi's Venice (as it was in Stevin's Netherlands and other countries). Iron manufacture was of course widespread and well advanced in Italy and elsewhere, for example Agricola's Germany. The considerable familiarity Porta had with practical metal work shows itself in book XIII of his *Natural Magick* which is entitled *"Of tempering steel"* (Gilbert refers to it). Here Porta describes in great detail over ten chapters what should be done to harden or soften iron and steel and describes aspects of their treatment and of iron smelting, tool manufacture and its special steels etc. Zilsel mentions Porta's description of iron- foundries as he mentions Agricola, but draws no conclusions from the work of either with respect to his own arguments.

An interest in magnetism at the time of the renaissance did not first arise in England. If we wanted to assess its development as a scientific field of interest, thus ignoring the more direct practical concern of the early Iberian seafarers with navigation and the compass, we would find it first to some extent in Italy (Cardan and Fracastorio). There follows Norman's work in England, and the independent researches of Sarpi and Porta in Italy which preceded Gilbert's in England.

Claims of close causal connections between social and economic conditions at a particular time and place and specific aspects of scientific work in general are contentious. R.K. Merton - influenced by Marx on the one and Max Weber on the other hand - proffered the sociological approach to some questions in the history of science in 1938. Zilsel followed him in his paper on Gilbert and in other articles. There has been a certain amount of controversy about this angle of investigation, but in the case of Zilsel's paper on Gilbert, the claim has not been made good. I referred to the very large number of Gilbert's experiments first reported by Porta in other chapters. If

Zilsel wanted to show the important influence of artisans and mariners in the origin of the experimental method, he would really have had to have shown it first of all with respect to Porta, but then the claim concerning the role of the specific English social conditions would have fallen by the wayside.

What distinguishes Gilbert from his predecessors are his thoroughness and ingenuity; not the fact that he experimented instead of relying on literary authority, but how well and how systematically he experimented; not that he was interested in magnetic, navigational, and certain practical matters, but his deep and properly scientific interest in a whole area of research, as well as his theoretical abilities.

Zilsel's statement that "England's natural, economic and social conditions might form, not a sufficient, but a necessary condition for the characteristics of Gilbert's method", is far fetched because he contrasts work in England with that in Italy. Yet there, where in Zilsel's view these conditions were presumably different, Porta had said that the magnetic needle is most useful, being employed not only in navigation but also when lining up sun dials, in discovery of ores in mines and the direction of veins of metals there, for determining the direction of underground passages in sieges or for digging trenches, when mapping buildings, cities and countries, lining up guns etc. (Porta, b. 7, ch.XXXVII). Gilbert gives us an almost identical enumeration in III, 17. Zilsel mentions Gilbert's list - being obviously unaware of the fact that Porta had given it before - and says with respect to it: "A rather complete assortment of the sources of his scientific achievements has been given by himself in his discussion of the pratical use of the magnetic needle" (p.25).

I have quoted Zilsel already on the theoretical aspect of Gilbert's work in the section on Norman above. He also says (1941, p. 30):

"With Gilbert, however, not much of the superiority of academic training [over what the manual labourers could contribute] as to the theoretical side of science can be noticed: his general speculations have not proved to be fruitful".

This statement is deeply mistaken. Without his theoretical abilities Gilbert could not have achieved his results, as has already been stressed. Zilsel would after all not think that science might be simply a collection of facts and experiments but need not, for example, postulate theoretical entities. Norman was not able to arrive at useful theoretical concepts, and neither was Porta. If Zilsel was claiming that it might have been better if Gilbert had just collected individual phenomena, this would show a complete misunderstanding of the character of science.

Whilst Zilsel's approach to an evaluation of the position of Gilbert's work in the science of the time seems unpromising, Kuhn draws an interesting general historical distinction between the classical sciences (for example mechanics) on the one hand and "Baconian science" on the other. He points out that the former's advance in the 17th century was due more to the adoption of new points of view than to a sudden rush of experimentation (as witnessed by the fact that many of Galileo's experiments, for example, were mere thought experiments). This remark illuminates both the history and methodology of the mechanics of the time. On Kuhn's view, Baconian-type science was initially more qualitative and directed at the collection of facts and new observations and on this reading early magnetic work falls into this category (cf. Th. Kuhn, 1977, particularly ch.3).

It would be very difficult, however, to show that this type of science was due to specific English social and economic conditions, even if two or three Italians concentrated on mechanics for a time, and some Englishmen worked in Kuhn's Baconian science. (Galileo, who achieved so much in classical mechanics, after all spent much time on magnetic experiments, as I have repeatedly mentioned. The fact that he did not reach new results in this area is probably mainly due to Gilbert's temporary exhaustion of the possibilities here.) On the other hand, like Galileo, Gilbert was keenly interested in astronomy, presumably a classical science in Kuhn's sense. After Gilbert most of the qualitative work in electricity - Baconian science - was done on the continent. Kuhn does not suggest that his bifurcation bears any relationship to sociological conditions and one could not use his ideas in support of Zilsel's.

Chapter Twelve

General features of Gilbert's scientific views and method.

A. The magnetic form.

I have tried to assess Gilbert's method and results *pari passu* with the description of his experiments and his conclusions throughout the above chapters. It now remains to evaluate his overall achievements and consider their interpretation by modern commentators. It has become clear that important details of Gilbert's work have been misjudged, but so have its more general features. His experimentation has been widely praised, but important theoretical aspects of his work have been misinterpreted or neglected. It has been criticised for a lack of mathematical considerations and for alleged magical, metaphysical and animistic tendencies. I will now examine the comments of some modern historians on these aspects, many of which have been touched on already.

The considerable advance Gilbert's conception of magnetism represents has been denied or belittled in a surprising manner by Heilbron. He is unwilling to make the necessary allowances for linguistic or descriptive difficulties concerning the formulation of entirely new ideas. He obviously enjoys Gilbert's more quaint and flowery formulations, which he cites, and he gives a slanted impression of Gilbert's views by failing to quote the much more modern ways in which he expressed himself elsewhere. Heilbron homes in on Gilbert's terms 'soul' or 'spirit' when he could just as well have quoted 'energy', or 'force', and so on. He says (p. 172, op. cit., my italics):

"But the mode of operation of the magnetic soul offered *nothing novel*. Gilbert says that it 'effuses' its power about it, 'informs' the mass it dominates, and seeks and is sought by any separated magnetic spirit within its sphere of virtue. All this suggests the medieval mechanics of the multiplication of species. The 'effused forms reached out and are projected in a sphere all round, and have their own bounds'; they operate by 'immaterial act', by 'incorporeal going forth'; and they pull iron by activating its form and inspiring it to self-motion."

These quotations are selected to support his view that "Gilbert remains peripatetic in spirit . . ." (ibid.). Yet all they may show is that Gilbert was occasionally at a loss for words to describe the field and its action. This is not surprising, considering the novelty of his conceptions. Yet from other formulations I have quoted it is clear that he often expressed himself in a much more modern manner. It seems surprising that a contemporary historian should concentrate on Gilbert's traditional formulations instead of examining the substance of his work, the variety of experiments, the conclusions drawn from them, and their implications from today's point of view.

It has been maintained that Gilbert's magnetic theory in the end relies on a metaphysical principle, that of the magnetic form, which could not be said to provide a scientific explanation of magnetism or its causes. Hesse, for example, says that Gilbert's magnetic

"form is the cause of the observable magnetic motions in the sense that it subsists only when they are present, and it is made 'manifest and conspicuous' by them. But it is not identical with them, because a quality of primacy and prepotency is ascribed to it which is metaphysical in the sense that it does not appear to add anything to the empirical content of the form. If this is the case Gilbert has not, in the modern sense, put forward any causal *explanation* of magnetic effects by speaking of a magnetic form, and yet in some sense he does regard it as a cause" (1960, II, p.134).

She thinks that the form "is metaphysical in the sense of being unfalsifiable" (p. 138). This follows her claim that

"the form is something that manifests itself in five kinds of motion observably exhibited by magnets . . . but if we ask, how the magnetic form is a theory of the *nature* of magnetism, or a *cause* of magnetism as opposed to a summary description of magnetic behaviour, it must be said that it gives a causal explanation only in the sense of giving a familiar analogy [this refers to Gilbert's likening of the magnetic form to a soul, an aspect we can ignore here], and not in the sense of being a falsifiable theory" (ibid., her italics).

These claims are a result of the fact that Hesse does not appreciate that Gilbert often uses the term 'magnetic form' instead of 'energy' or 'force'. The first expression, unless analysed as to Gilbert's meaning, seems inevitably obscure to the modern reader. Gilbert frequently simply describes or characterises magnetic energy as 'magnetic form'. I have tried to show that at other times he makes clear how the two are related, and it will be recalled that the energy is 'formal', it 'formates' and 'orders'. The term 'magnetic form' serves, then, as a summary concept expressing the fact that the energy in each part of the field has a certain degree and direction, i.e. a definite form which in turn orders magnetics that may be there.

A general appreciation of this central entity must consider whether he has given an explanation specific enough to account for its manifestations within the possibilities of early science, yet perhaps also one of sufficient generality to accomodate much of the subsequent development of the subject. The course of the latter has shown that Gilbert achieved far more than could have been expected. He understands magnetic energy to be something *sui generis*, a force not depending on, or connected with, other forces. This is the appropriate conception of the matter prior to the evolution of electromagnetism. Hesse's question about a causal explanation need not be asked about the magnetic energy.

Gilbert's five movements (coition, direction, variation, dip, and revolution) are not identical with the magnetic form, which therefore is not their summary description. They are *caused* by the form or energy. Gilbert says that they "all proceed from a congregant nature, or from verticity [Gilbert's other term describing the directed energy] . . ." (II, 1, p.73). He refers to the causes of the magnetic movements as "depending on its [the magnetic's] true form" (ibid.). By this he understands the energy's strength and direction. He does not "in some senses regard it as a cause", as Hesse says, but he clearly regards it as *the* cause of the motions. But Gilbert nowhere implies that the magnetic form only subsists when magnetic movements are present, nor can this claim be deduced from his work. After all, the form can show its ordering effects in the phenomenon of induction in the absence of any kinetic events, an aspect Hesse ignores. A magnetic motion does indeed make it 'manifest and conspicuous', but if no such motion occurs at a particular occasion, this only indicates that no suitable arrangement of magnetics exists at that time. If earlier tests had shown that a body was a magnet, its virtue would persist provided the magnetisation was known to be permanent, something that could be forecast for familiar methods of magnetisation and materials. If a particular set-up of magnetics produced certain movements, or if it failed to do so, Gilbert had an explanation within the range of parameters given by his theory. But the form or energy in magnet or iron can also be destroyed by strong heat, an effect which Gilbert correctly understands as a disordering by the temperature (cf.

II,4). This shows an extremely good and very surprising comprehension of the nature of magnetism and an understanding of the magnetic form as a physical, not a metaphysical, entity. Any identification of the magnetic form with the five motions is off the mark although the movements would display the features of the form's character in a particular place.

Gilbert's postulation of some formal arrangement of the iron's structure upon magnetisation is wide enough to cover the later understanding of the process as one of an alignment of magnetic dipoles. Though he was not able to speculate on the iron's microstructure, his conceptions could not have been more apposite. The 'formation' of the iron was a process of energising which could not be further characterised in terms of better known phenomena. But it had the regular describable and predictable physical relationship to observables which a basic theoretical entity in a qualitative theory has to possess.

It is hard to see what more might be expected from Gilbert's general conception of magnetism with respect to falsifiablity. Hesse says that ". . . Gilbert's theory of magnetism was either merely descriptive, or metaphysical in the sense of yielding no predictions. . ." (op. cit. p.141). This charge is unjustified. The theory could yield any number of qualitative or semi-quantitative predictions. One can readily find new tests for it, including highly indirect ones for the properties of the field, such as tests for the neutral points of a magnet's field in that of the earth (where the two neutralise each other so that a needle is not moved by either), an experiment which is perhaps conceivable on Gilbert's assumptions.

At the time all tests depended on the observation of movements or their absence, and even induction could only be verified by a subsequent observation of one of the motions. But the action of the magnetic form during the process of induction itself is obviously not one of any of the macroscopic movements. The magnetic form's character is specifiable (via observable consequences allowed for by the experimental possibilities) to the extent necessary for the principal theoretical entity of Gilbert's theory. It can accomodate additionally many of the further implications of a theoretical term which the progress of the subject revealed during subsequent centuries.

Its falsifiablity accepted, Gilbert's theory of magnetic energy is also seen to be sufficiently firmly embedded in the everyday physics of cause and effect not to be considered metaphysical with respect to other possible connotations of the term: proximity of magnetics, heating, the passage of time, the make-up of the magnetic etc. all affect the magnetic form in a specifiable way.

The magnetic energy is the most important theoretical entity of a well tested theory. It is of the highest importance, being the first such concept in the history of science. It gives Gilbert's work the basic characteristics of the first qualitative scientific theory of terrestrial physics in our modern sense.

B. The quantitative aspect of Gilbert's work.

Although it has been said by Galilei, Zilsel and others that measurements and mathematical treatment are largely missing from Gilbert's work (the navigational sections perhaps excepted), they are in fact not entirely absent. Indeed, some quantitative considerations and results are reported throughout. This is only to be expected, for there could not be a treatment of magnetism of this breadth without them.

What more specifically numerical or at least quantitatively related observations does he mention? There are for example those concerning the relative strength of the magnetic virtue of different loadstones, due to their particular type of material or shape (the strength of elongated magnets, for example bar magnets, is at least roughly compared with that of spherical ones). Figures (weights) are given for the lifting power of various stones and a numerical comparison is made of armed with unarmed magnets. The varying degrees of possible magnetisation of different types of iron and steel are investigated and considered as dependents of differing procedures at least in such terms as 'stronger' and 'weaker'. The effect of time on magnetic strength is taken account of. Gilbert sees a decrease in magnetic force with distance, and he tacitly accepts its comparison with the case of light, following Porta. He makes an important mistake in thinking that repulsion is weaker than attraction. I have considered this problem in detail above, but we have seen that the error proves to be of surprisingly small practical effect in his work as a whole, a consequence of its high general quality. Its importance concerns his appreciation of the overall conceptual character of magnetism which lacks the essential symmetry of the two manifestations of the magnetic force.

Gilbert clearly could have made more measurements, for example with the scales he mentions (these measured the strength of a magnet fixed to the end of one arm by showing what weight of sand at the end of the other arm would lead to the magnet's separation from a fixed mass of iron below the it). But it is far from obvious that this would have led to any new insights of importance. In the absence of any useful definition of force, it is unclear what more Gilbert could have achieved in this area. (Even after Newton's definition of force it took after all a hundred years before that of magnetic interaction was measured.) When considering Galilei's criticism concerning the lack of 'mathematics' in Gilbert, we should remember that Galileo himself arrived at no notable results, either qualitative or quantitative, in his magnetic work. And Zilsel himself had said that a quantitative treatment of magnetism was too difficult. If it had not been, we may be sure that Gilbert would have employed it, for he uses mathematics and particularly geometry, widely enough in connection with navigational and astronomical problems.

Koyrè makes some remarks concerning a mathematical treatment of Gilbert's theory which it may be useful to consider. He says, after referring to Galilei's enthusiastic approval of Gilbert's experimental results:

"Une théorie physique de la gravité existe. C'est celle de Gilbert . . . Galilei a beau admirer Gilbert, il a beau accepter sa doctrine sur la nature magnetique de la gravité, il ne peut l'utiliser parce qu'elle n'est ni mathematique, ni meme mathematisable. L'attraction gilbertienne est une force animée." (A. Koyre, 1939, p. 248-9.)

Koyrè here commits the common error of thinking that for Gilbert gravity was a magnetic phenomenon. (Later (1965, p. 173) he changed his mind about this and says: ". . . he does not explain gravity by magnetic attraction . . .".) Koyrè's claim in 1939 that Gilbert's theory was not mathematisable appears to be due to his view of its animism. But it is not at all clear that animism of the kind Gilbert held would have prevented a mathematical formulation even then; for if the *anima* acted in a completely regular and predictable way - as Gilbert indeed thought - a mathematical treatment would very well have been possible.

Hesse, who herself correctly stresses that Gilbert's conception of gravitation was not magnetic, mentions Koyrè's claim of the impossibility of a mathematical treatment of Gilbert's magnetic theory with approval (cf.Hesse, 1960, II, p.139: "it is true, as Koyrè remarks, that Gilbert's magnetic form could not be mathematised . . ."). However, she attaches no importance to Gilbert's animism and agrees with Agassi, who said that Gilbert, in endowing magnets with souls, merely stresses their self-movement and harmony (on Agassi's comments see below). Her agreement with Koyrè's statement does therefore not relate to his animistic understanding of Gilbert's magnetic theory.

Why, then, should she hold that it was not mathematisable? I cannot assume that she thought that Gilbert's appreciation of the strength of repulsion and attraction as asymmetrical would make a mathematical treatment of his theory impossible. Gilbert's theory would indeed eventually have to be corrected for the mistake of considering repulsion to be weaker than attraction. But it would not be true to say that the theory was not susceptible to at least partial mathematical formulation simply because repulsion was seen as weaker than attraction, and Hesse would no doubt have been aware of this. It appears that her views about the question are based on her appreciation of the whole theory as having a metaphysical character. But the difficulties in respect of a mathematisability of his theory are due to the lack of a useful conception or definition of force.

C. Gilbert's animism.

I now have to consider Gilbert's animism. The fundamental fact about it is that it does no work whatsoever in either his magnetism or cosmology. If all mention of 'souls' was removed from them, neither Gilbert's experimental results or formulations nor his theories would be in the slightest bit changed.

Marie Boas (1962, pp. 194-5) expresses very surprising views when she says that "Gilbert was, after all, a natural magician, not a natural philosopher . . .", and that he

". . . genuinely believed that magnetism, however subject to control by the natural magician who thoroughly understood the ways in which it manifested itself, was truly an occult force".

Unacceptable is also her statement that

"his failure to treat more adequately of theoretical subjects reveals more clearly than anything else the gap that separated natural magic from the new expertimental learning to be developed in the seventeenth century" (p.195).

This seems to be based on a misapprehension of Gilbert's work as a whole and of some important details (an example of these is her support of Bacon's stricture that Gilbert "made a philosphy out of a loadstone" by ascribing to him a magnetic theory of gravity, cf.Boas ibid.). She cannot quite make up her mind about Gilbert's standing as an experimenter, saying that "Gilbert's method was not very different from that of Porta; many of his experiments were very like those of earlier writers . . .". This, she thinks, was due to Gilbert's being in the tradition of natural magic. However, she adds, ". . . he was more ingenious, more thorough, more curious, and possessed of a better power of evaluating experimental results" (p. 191). Although she says that in his electric work "Gilbert proceeded with a thoroughness of experimental detail which is utterly captivating" (p. 192), and in the most important areas of magnetic research "he provides a wonderful array of interesting and ingenious experiments mostly original . . .", unfortunately

> "even Gilbert's experimental genius was cramped by its attempts to restrain it within the bounds of a method which, however much it endeavoured rationally to understand the way in which the forces of nature worked, and to control them, yet assumed these forces to be impervious of rational comprehension, because it knew them to be occult and essentially unknowable." (p.196)

Thus Boas finds much to praise in Gilbert's experimental work, applying to it the term 'ingenious' more than once, but she does not say in what respect his experimental genius was being cramped on the other hand. She does not appreciate the most important fact that he could not have achieved his experimental results if he had not treated adequately of theoretical questions. She thus ignores the fundamental connection between the invention of hypotheses and theories covering a subject area, and detailed and extensive experimentation concerning it. Boas should be left to wonder how an inadequate theorist could have done so much superior experimental work in a whole field of physics, particularly as the claim is that he knew the forces he was concerned with to be occult.

An inadequate theorist could either not invent hypotheses which have a reasonable connection to what he believes already to be experimental facts, or he could not see the relevance of further observations to a hypothesis, and he would be less capable of devising new tests for it. It is therefore hard to see how a scientist of low theoretical ability could have devised ingenious experiments. The ingenuity of experiments and tests must after all at least largely lie in the way theoretical considerations are employed in their devising and the appreciation of their import. Even the later partial specialisation of scientists as experimenters or theoreticians, presupposes that the former be able to deal at least *adequately* with theoretical questions. But Gilbert found only his predecessors' various separate magnetic experiments which were unconnected by any theory, which covered the field of magnetism only to a limited extent, and which contained numerous errors of observation, not to mention interpretation. No proper scientific theory in any other field of terrestrial physics existed either. Gilbert's experiments are indeed often ingenious, but his abilities as a theoretician are also quite indisputable.

It is his animism which makes Boas treat Gilbert as one of her subjects in a chapter entitled *"Ravished by Magic"*, although she had also said that "Gilbert combined rationalism and mysticism in a peculiar blend in which neither interferred with the other" (1962, p. 120). Obviously her assessment of Gilbert is less than clear. If Gilbert's mysticism did not interfere with his rationalism, how is it that his theoretical work would suffer and his experimental genius could be cramped by it?

We find Gilbert's animist remarks in various places in his books. Some quotations from *de Magnete*, book V, ch. 12, may suffice to illustrate it. He says:

"Wonderful is the loadstone shown in many experiments to be, and, as it were, animate. And this one eminent property is the same which the ancients held to be a soul in the heavens, in the globes, and in the stars, in sun and moon."(p. 308)

On earth not only man but also animals have souls,

"even God, by whose rod all things are governed, is soul. . . But in the bodies of the several stars the inborn *energy* works in ways other than in that divine essence which presides over nature." (p. 310, my italics)

(He continues to say that it works differently again in plants and animals). I have quoted this sentence because Gilbert here speaks of 'energy' instead of 'soul', showing that he equates the two or that he is not particularly firmly committed to any animism (cf. also VI,5, p. 339, my italics, where he makes this quite clear concerning the earth's magnetism speaking of "this magnetical form, *be it energy or be it soul* . . ."). The soul or energy has the teleological purposes, though, of preventing chaos and allowing of motion, seasons and propagation. In the light of this it is interesting to consider Boas' claim that Gilbert saw the magnetic and cosmological forces as 'occult and essentially unknowable'. Would they not in fact be rather better understandable in at least some important sense if comprehended as furthering teleological purposes coinciding also with human interests and concerns as Gilbert sees them? It would require a detailed argument on Boas' part to show why Gilbert, the 'natural magician', able to control magnetism and having an understanding of the ways it manifested itself and knowing its character as the earth's teleologically acting force, should think magnetism any more unknowable than would the 17th century scientist with whom she contrasts Gilbert.

There are other reasons which make Gilbert ascribe souls to the earth and sun:

"Since living bodies spring from earth and sun and are by them animate . . [here follows a description of how plants may be created spontaneously from earth without seeds but with the help of sun light], it is not likely that they can do that which is not in themselves; but they awaken souls, and consequently are themselves possessed of souls."

(p. 310)

The important argument for the existence of souls in magnets and the heavenly bodies is their power of harmonious self-movement. But as this quotation shows, this is connected to the claim that plants have souls, and their spontaneous generation is evidence for the animate character of earth and sun.

Agassi correctly speaks of "the widespread myth" of Gilbert's animism (J. Agassi, 1958, p. 240). One cannot describe Gilbert's animism as a myth; though the ascription of an effect of any importance it had on his scientific magnetic and cosmological work would be a myth, and we should perhaps read Agassi in this sense.

Agassi's further characterisation of Gilbert's views seems to me to be mistaken in important respects. He says:

"Gilbert does not consider magnetism to be a terrestrial phenomenon but rather a cosmic feature which he identifies with form (in the Platonic sense), with light (in the wider neo-Platonic sense), and with force (which is his own new metaphysical notion). Thus he considers magnetism to be the cause of all celestial action." (ibid.)

Gilbert's magnetic form should not be seen as having the 'familiar Platonic sense'. The form, as I hope to have shown, represented the specific magnetic energy and is therefore not metaphysical and very different from the Platonic idea of form. I do not see that he identified magnetism with light (although, as we have seen, he compares it to light in its way of propagation through space and manifestation in affected objects). Agassi commits the old error of claiming that Gilbert considered magnetism an embracing cosmological phenomenon to which all celestial motions were due.

Connected with this is his belief that Gilbert carried out his magnetic researches in order to support Copernicanism, a view shared by Benjamin, but for which there is no evidence. If, as is most likely, the insight concerning the earth's magnetism was one of the *results* of Gilbert's researches, it is hard to see why he should have chosen magnetism to begin with to help Copernicanism. We must also not overlook the fact that Gilbert wrote on other scientific matters in the *de Mundo*. These studies on numerous questions of natural science, which were not pursued experimentally or extensively, certainly did not all have the aim of providing arguments for Copernicanism. As we have seen, Zilsel claims that Gilbert's interest in magnetism is connected with a desire to find aids for navigation. In fact we simply do not know why Gilbert concentrated on magnetism. His often expressed sense of wonder about magnetic phenomena may be sufficient explanation.

Concerning a more general aspect of Gilbert's animism, Agassi correctly says that

"by endowing magnets with souls Gilbert merely endowed them with (force and) harmony, following the Pythogorean-Platonist theory which viewed the soul (as self-moving and) as harmony" (ibid.).

This Agassi considers to be harmless, and he then draws parallels between Gilbert's and more modern understandings, such as Einstein's, of the world as being harmonious.

We can now summarise the import of Gilbert's animism with respect to magnetism and cosmology: it cannot detrimentally affect his scientific researches, for it does no work whatever at the phenomenal or even at the theoretical level and does not, for example, as we have seen, in any way hinder the development of theoretical concepts. He did not shun the necessity of giving the most thorough description of the character and causes of magnetism he was capable of because he saw a possible presence of souls or likens the forces and energies to them. The souls of the heavenly bodies and the magnetic soul of the earth and its magnetic constituents are introduced in a somewhat anthropomorphic manner principally to account for their self-movement. They appear as regular forces whose actions result in turn in perfectly regular phenomena. This regularity makes the forces and observable events susceptible to scientific study. There is no suggestion whatever that the souls at any time cause events which are outside the regularities the scientist could investigate. Gilbert after all equated the soul repeatedly with energy and might well have accepted their description as something like 'divinely determined energy', for what he was keen to add to their characterisation as energy was the aspect of harmony of God's created scheme of things.

D. Summary of Gilbert's importance.

In magnetism Gilbert took the vital step of creating the first comprehensive theory of terrestrial physics by maintaining the existence of a specific well-conceived force which filled the space surrounding the magnet in the form of the field. His postulation of the ordering of the matter of magnetics as the underlying phenomenon giving rise to this energy, though somewhat speculative, represents an ingenious anticipation of the modern conception. Gilbert set the standard for subsequent systematic, thorough. and comprehensive, experimentation. His discovery of the earth's magnetism was of revolutionary importance and his investigations leading to it show an already mature use of observation, experimentation, and arguments from analogy in science.

His much briefer electric researches, though less successful in spite of numerous new results than the magnetic work, laid the foundations for further investigations. The course of the latter showed that the shortcomings of his approach in this area were due at least in part to the considerable objective difficulties of the subject.

His support of important aspects of the Copernican conception of the universe, which is well-founded on careful consideration of the available evidence, helped its spread in England.

His discovery of terrestrial magnetism provided the impetus for a postulation of other cosmological forces, for example an attractive force, to account for the movements of the celestial bodies.

Gilbert overthrew the traditional way of collecting observations and providing metaphysical or mystical explanations for them. Where his predecessors, such as Norman and Porta, had come to a halt in face of difficulties after discovering new but isolated physical phenomena, Gilbert continued his extensive researches until a comprehensive qualitative theory emerged. He thus showed that natural phenomena are susceptible to systematic investigations and that the postulation of genuine theoretical entities could provide the necessary deep links between them. He understood that these allowed the extrapolation to further observational manifestations which could be tested for, and that the new observations would in turn provide tests for features of the theoretical entities themselves. Gilbert is therefore one of the first of the founding fathers of modern experimental science.

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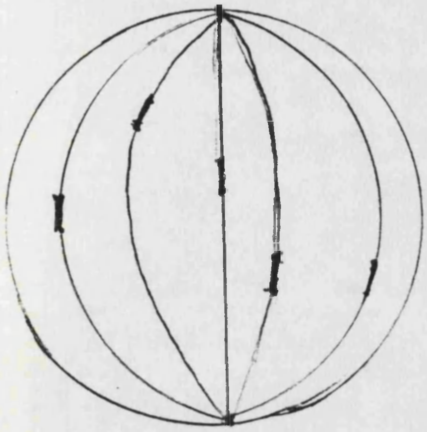

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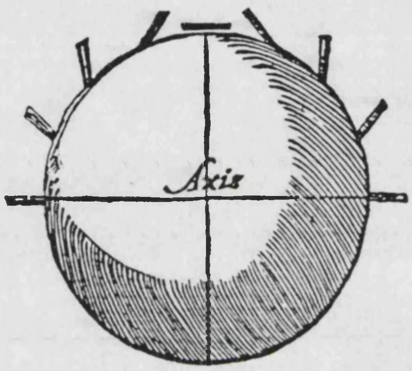

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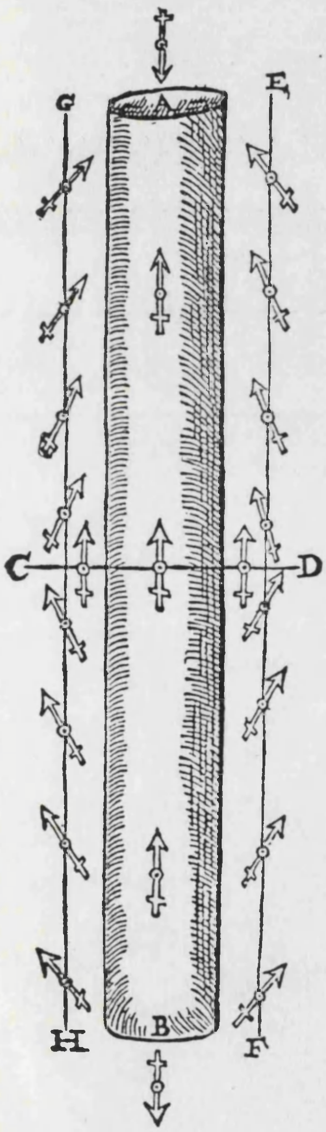

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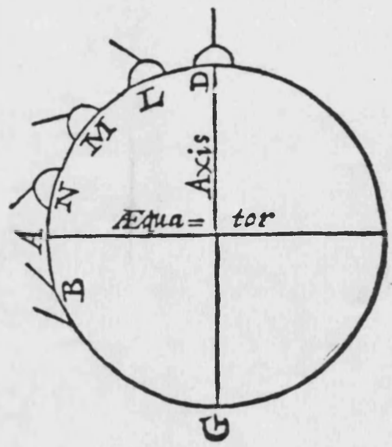

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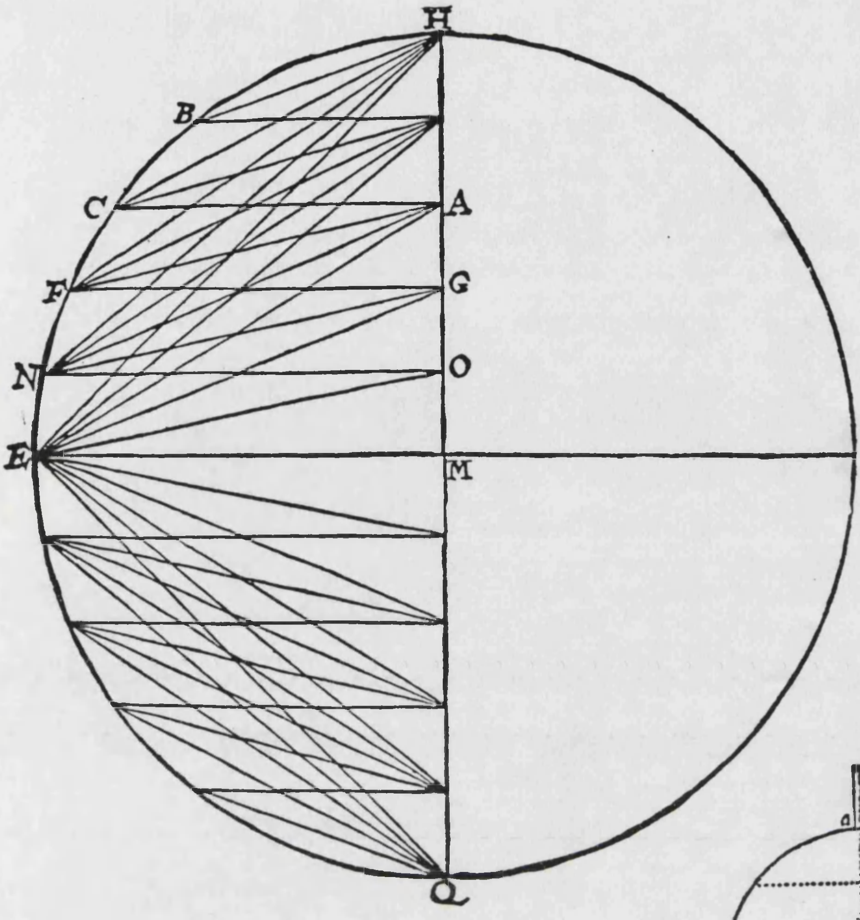

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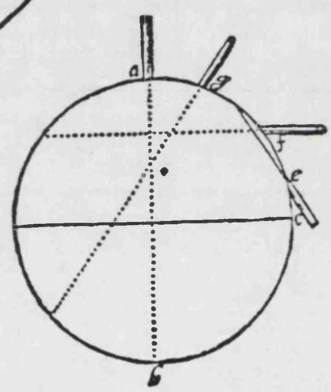

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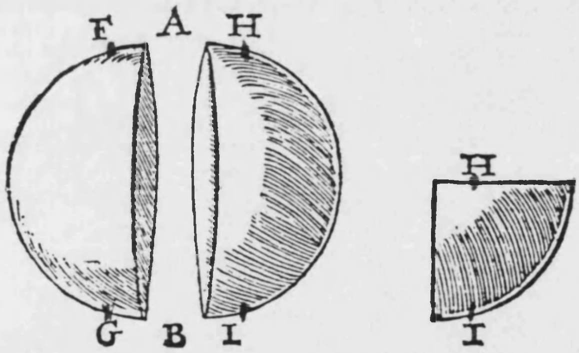

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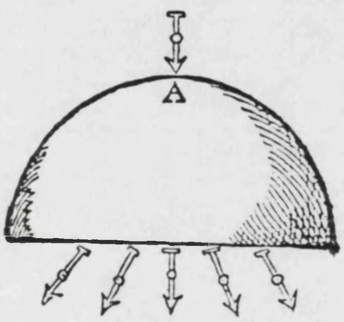

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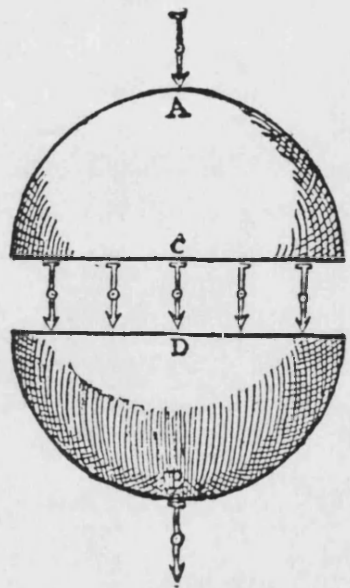

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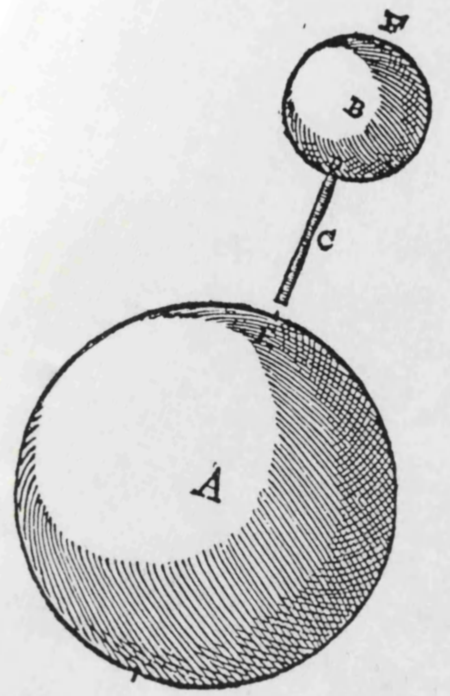

fig. 10

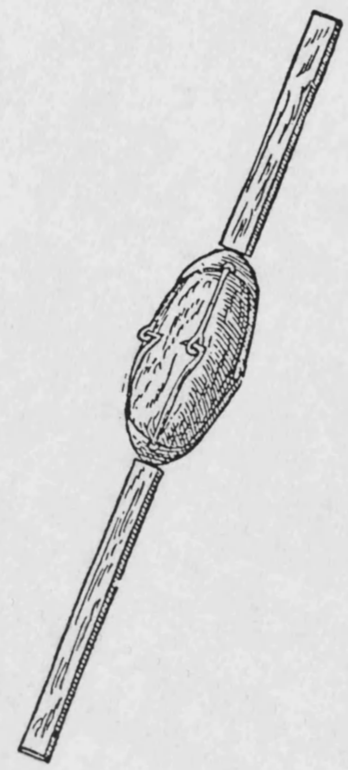

fig. 11

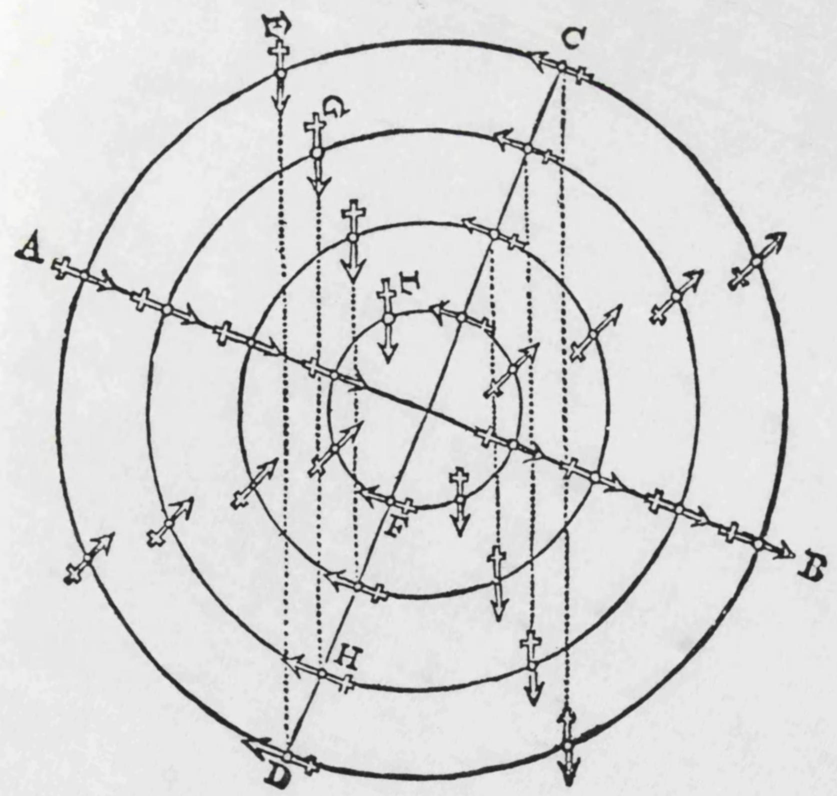

fig. 12

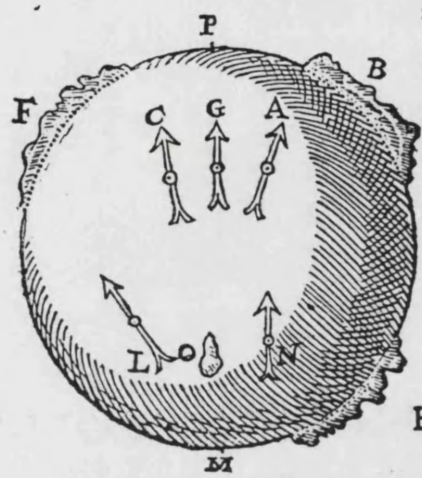

fig. 13

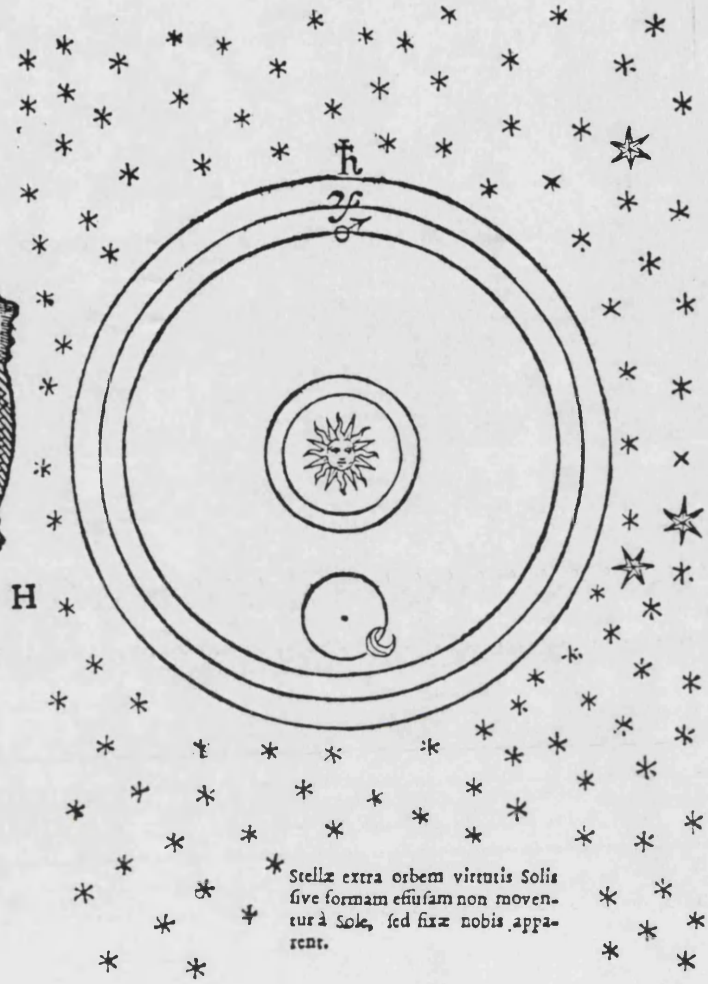

fig. 14

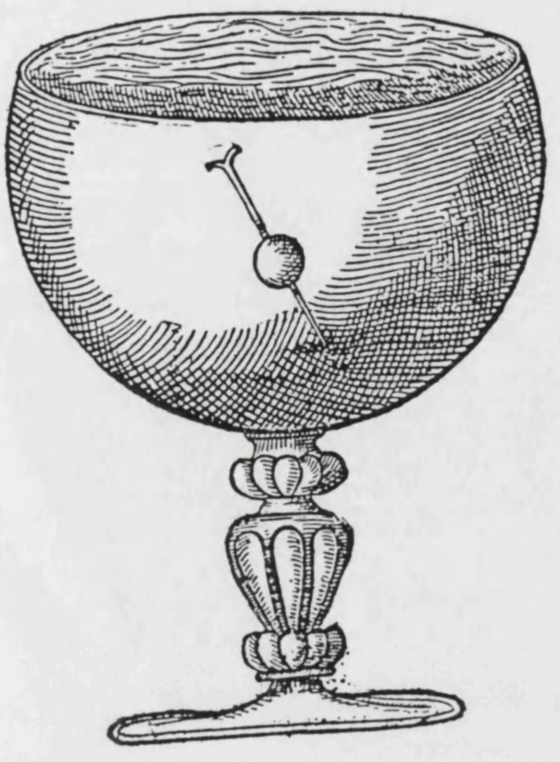

fig. 15

CORRIGENDA

p. 51, l. 3-6: read "(He had already found that any magnetic pole receives extra strength from the proximity of a strong like pole belonging to another magnet and that, on the other hand, proximity of an opposite pole weakened the attractive power of a magnet's pole (II.25, p.147))."

p. 69, l. 16-17: read "Thus a predicate must not be termed 'theoretical' if it is more observational than another and this latter is described as 'observational'."

p. 88, l. 24-25: Instead of "Here he knew that forces acted, but..." read "He did not appreciate that forces acted in this case either, and...".

p. 99, l. 4: read "(III.13, p.217)".

p. 102, l. 19: the ref. is to fig 12, not 11.

p. 159, l. 24: read "Cambridge" instead of "Canterbury".

p. 227 under "Gilbert, William 1958": the translater's full name is P.Fleury Mottelay.

top of p. 229: insert missing reference as follows: "Peregrinus, Peter de Maricourt, 1902, *Epistula de Magnete* tr. by S.P. Thompson. Whittingham & Co., Chiswick."