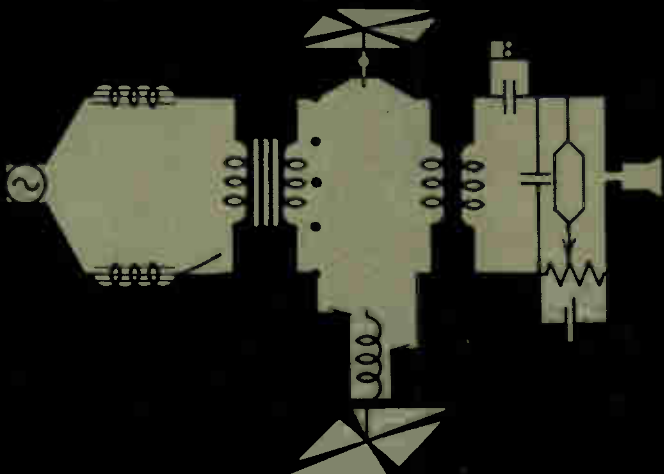
Syntony and Spark

THE ORIGINS OF Radio



Hugh G. J. Aitken

A volume in Science, Culture and Society:
A Wiley-Interscience Series edited by Bernard Barber

It began with the ideas of harmony and resonance—the concept of "syntony." What it developed into—modern communications technology and the radio industry of today—is the subject of Professor Aitken's lucid essay, SYNTONY AND SPARK—THE ORIGINS OF RADIO.

Tracing the development of radio from the work of the 19th-century pioneers, Hertz, Lodge, and Marconi, the author reveals how technology mediates between science and economics, transforming ideas and discoveries into goods and services. He shows, too, how technological innovation is basically a process of information transfer. We learn that through technology, information flows in two directions: forward, from pure science into practical application, and also backward, from application to pure science. How these realms enrich each other and what it means to work at their interface is crucial to an understanding of the origins of new technology, and of its impact on society.

Professor Aitken documents his book from the writings of the men who made fundamental contributions to the emergence of radio technology, and for added insight draws on commentaries by those who came later, weaving into his narrative an analytical model of the process of technological change itself. To help the reader gain better grasp of the technologies discussed, the book contains more than 30 diagrams and illustrations of early radio apparatus.

For students of economics, history, engineering, or business, SYNTONY AND SPARK—THE ORIGINS OF RADIO offers an unusual examination of an extraordinary innovation, with almost incalculable implications for today's world of science, industry, and commerce. For people who make radio their profession, for those who find it a satisfying hobby, or for those who just use it as a means of entertainment, the book provides an illuminating view of where radio came from, how it grew, and how it changed our lives.

About the Author

HUGH G. J. AITKEN is Professor of Economics at Amherst College. He holds both a First Class Commercial Radio Telephone Operator's license and an Amateur Extra Class radio license. Dr. Aitken received his Ph.D. in economics from Harvard University in 1951 and has taught in California, Canada, and New England.

SCIENCE, CULTURE AND SOCIETY: A WILEY-INTERSCIENCE SERIES is a series of books by social scientists, scientists, and humanists who examine the evolution, function, structure, and activities of the scientific enterprise and its relationship to other disciplines throughout history. Factual and objective, the series is addressed to informed readers and students, as well as to scholars, researchers, administrators in all fields of science, and in many fields of the humanities and public affairs. By making available the results of unusual investigation, thoughtful analysis and responsible speculation on topics of continuing significance, we expect SCIENCE, CULTURE AND SOCIETY will provide knowledge not only for its own sake, but for the encouragement of progress and societal wellbeing.

—Bernard Barber, Series Editor

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From the Foreword by Bernard Barber—

"I cannot forbear to mention the charm of this book, over and beyond its analytical sophistication and its empirical solidity. The reader will find delight, in a way that is not often the case in scholarship of this kind, in Aitken's fond and intimate knowledge of his subject. He makes us feel that he not only understands the work of Clerk Maxwell and Hertz, but could also have been a pioneer radio operator for Marconi. There is charm for our minds also in Professor Aitken's discussion of the concept of syntony, so fundamental to understanding the development of radio technology. It is more than a lovely word rediscovered for us; it is an idea of general significance for fields so diverse as music, art, science, and religion, indeed for any type of idea-system. It stretches our minds and gives us a new way of seeing not only the world of radio but the world more generally. Syntony, harmony, congruence . . . we are led on to see new patterns in new places and to have new joys of discovery."

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Can you send forth lightnings, that they
may go and say to you, "Here we are"?

The Book of Job, Chapter 38, Verse 35

Foreword

In a world where the powerful social forces of science, technology, and the economy are all too often oversimplified and stated doctrinairely, Hugh Aitken's book has a special virtue; it gives us an account of the complex interrelationships among these three forces. Aitken treats each as a complex and partially autonomous system in its own right. But he also sees how they interact among themselves and with other elements of our total society. Professor Aitken has no preference for complexity for its own sake. But if that is the way the development of science, technology, and the economy occurs in reality, that is the way our analysis must account for it.

It is another special virtue of Professor Aitken's book that his case is made both *positively*, not negatively and merely in opposition to other views, and *empirically*, not merely in abstract and deductive terms. His analysis is built with impressive solidity on the positive and empirical base of a detailed and loving description of the development of the theory and technology of electricity and radio from Clerk Maxwell's work in the 1860's, through the contributions of Hertz and Sir Oliver Lodge, to Marconi's enterprises in the early twentieth century. His historical tale is informed by and supports his analysis of the complex reciprocal relationships among science, technology, and society, yet it has its own compelling interest simply as the story of great creative innovations that have affected the lives of all of us.

Finally, I cannot forbear to mention the charm of this book, over and beyond its analytical sophistication and its empirical solidity. The reader will find delight, in a way that is not often the case in scholarship of this kind, in Aitken's fond and intimate knowledge of his subject. He makes us feel that he not only understands the work of Clerk Maxwell and Hertz, but could also have been a pioneer radio operator for Marconi. There is charm for our minds also in Professor Aitken's discussion of the concept of syntony, so fundamental to understanding the development of radio technology. It is more than a lovely word rediscovered for us; it is an idea of general significance for fields so diverse as music, art, science, and religion, indeed for any type of idea-system. It stretches our minds and gives us a new way of seeing not only the world of radio but the world more generally. Syntony, harmony, congruence . . . we are led on to see new patterns in new places and to have new joys of discovery.

BERNARD BARBER

New York, New York
August 1975

Acknowledgments

Most of the work for this book was done during the academic year 1973–1974, when I was on leave from my teaching duties at Amherst College. I must begin, therefore, by expressing my thanks to the President and Trustees of the College for granting me that leave, to the John Simon Guggenheim Memorial Foundation for providing the fellowship that enabled me to devote my time entirely to research, and to my departmental colleagues for the uncomplaining courtesy with which they shouldered the additional duties imposed by my absence.

The reading, thinking, and writing were carried out almost entirely in the Frost Library of Amherst College. To the staff of that library, and particularly to its Interlibrary Loan Department, I owe a considerable debt. Their efforts to procure the information I needed were untiring. But more than that: their calm confidence that, if information existed in published form, they could get it for me, encouraged me to venture into explorations that otherwise I might have shirked.

A number of friends were good enough to read parts or the whole of the manuscript and give me their critical advice. I should mention particularly Professor Joseph H. Taylor, Jr., of the Five-College Astronomy Department at the University of Massachusetts; James Perry, chief engineer of radio station WTTT; the Reverend James H. Clark of Grace Episcopal Church in Amherst; Provost Emeritus Frederick C. Terman of Stanford University; and Elliot Sivowitch of the Smithsonian Institution. Their advice was invariably candid and constructive; if I did not always follow it, this book may well be the poorer.

I must also express my indebtedness to the many individuals, most of them unknown to me except by call sign, whom I have met and conversed with "on the air" and who, by their comradeship over the years, have helped me to feel a member of the fraternity of commercial and amateur radio operators, a fraternity whose origins date back to the years with which this book deals and which still maintains its sense of identity. Through them, more than through any printed words, I have been able to acquire some sense of radio technology as a way of life, and of the distinctive cast of mind and thought it gives to those who practice it.

And lastly I should thank my wife, Janice, who not only typed the final version of the manuscript and caught many errors of omission and commission but also sustained me throughout with her courage and confidence. After 20 years of marriage it makes little sense for my name to be on the title page unaccompanied by hers: anything of worth that has been done we have done together. She knows this, but perhaps it is well to make it a matter of public record.

HUGH G. J. AITKEN

Amherst College
Amherst, Massachusetts
August 1975

A Word to the Reader

If you are principally interested in the story of the origins of radio technology, you may prefer to start this book with Chapter 2 and end it with Chapter 5, omitting the Prologue and Epilogue completely. These two chapters are intended for the reader with a specialized professional interest—an interest in how science, technology, and economic affairs influence each other and how we should go about analyzing their relationships. I believe personally that the narrative sections of the book are more interesting if one reads them with these general problems in mind. You may not agree or, if you agree, you may not care for the particular way in which I have formulated the issue. The story can, in any event, stand on its own merits, and you should feel free to ignore the analytic discussions and concentrate on the narrative if you feel so inclined.

H. G. J. A.

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SYNTONY AND SPARK—
THE ORIGINS OF RADIO

ONE

PROLOGUE

How new things happen is a puzzle that has aroused the curiosity of man since first he gave thought to the world in which he lived and to his place in it. It is the perennial concern of philosophers, scientists, and historians. Writers and artists must live with the search for creativity every day of their lives, must learn how to tap its springs and give it expression in words, music, sculpture, painting, or whatever is their chosen mode. No one, indeed, is absolved from the task of coping with novelty: if it is not our role to bring it about, it is nevertheless our fate to live with its effects. And those effects are frequently so disconcerting, so unpredicted, and so inescapable that we feel impelled to come to terms, emotionally and intellectually, with the new things that are the source of disturbance.

Catastrophic novelty—the earthquake, the pestilence, the sudden failure of crops or water, the disaster that comes without apparent cause—is one of the sources of religion, posing as it does a challenge to man's irrational conviction that the world he inhabits must make sense to him. Such events do not "make sense" in human terms. Their causes are unknown, their purposes hidden. We strive to understand them, and as our knowledge increases much that earlier was a mystery becomes comprehensible and even amenable to human control. But there is always a residue of the unexplained, of new things that seem to have no cause, no purpose, no reason. If these new things are rationalized, it is in terms of higher powers and transcendant purposes, at which man can only guess. The response is therefore propitiatory, by sacrifice and prayer, by confession of sins known and unknown. Those who, like Job, would argue with the deity, demanding to confront their adversary so that they may hear his indictment in terms comprehensible to them, receive no direct answer. Job, prototype of the man who insists on understanding what is happening to him, finds peace and an end to his troubles only when he accepts that the reasons for undeserved catastrophe are not for him to know.

Systematic novelty evokes a different response. An eclipse of the sun may cause terror when it is unexpected. But when men begin to keep records of the event, to predict the time of its next occurrence and to speculate on physical causes, it has moved out of the realm of religion and into that of science. Regular recurrence may not, in the final analysis, remove any of the mystery—there are those to whom each returning spring is a new miracle—but it does enable the mind to grapple with the event in secular and not sacred terms. How new things happen becomes a problem open to scientific analysis.

Even more clearly should this be true, it would appear, when systematic novelty results from man's own actions, from his own creating of things that are new. These actions may, and indeed typically will, have unanticipated consequences. Nevertheless, they should be amenable to analysis. There are means and ends, processes and purposes. Actions are undertaken by certain individuals in pursuit of certain goals, using certain instrumentalities, and typically in situations that involve a mixture of cooperation and competition with others. There is nothing here that rules out investigation by the normal canons of scientific inquiry, nothing that would seem to require us to invoke the mystical or transcendental.

And yet the sources of creativity have not in fact proved easy to uncover. How new things happen—the discoveries, inventions, and innovations that reshape our lives in what is now a seemingly permanent revolution—is not a question that social and behavioral scientists can claim to have solved, or even with a few courageous exceptions to have reduced to a form in which hypotheses can be put to the test. Meanwhile the flood of new things—new ideas, new information, new products, new processes, new forms of art and experience—pours over us. It becomes hard to find a firm place on which to stand and take our bearings. The future engulfs us before we have perceived the present or assimilated the past. Even man-made systematic novelty—the creations of our own curiosity and intelligence—comes to seem like built-in catastrophe.

The temptation is, then, to see something mystical in creativity, to argue that, no matter how closely we analyze the context in which creative acts occur, there will always be an irreducible residue that evades systematic explanation. This must be so, we tell ourselves, because in the nature of the case a creative act cannot be completely explained in terms of prior circumstances. If it could, it would not be truly creative. Creativity, the appearance of something truly new, necessarily involves a leap beyond anything that the "givens" in a situation can explain.

Now, for scientists to say, "We do not yet know," in answer to a problem posed to them, is no reason for embarrassment. For behind the surface humility of the statement lies confidence that eventually, through science, we can know. But to say, "We shall never know," is a different matter entirely, for implied by that response is the belief that some matters by their very nature are forever closed to human understanding. This is not something that, in our role as scientists, we should be too ready to admit. To assert that there is in creativity something necessarily beyond explanation may well be a premature and unnecessary retreat to mysticism.

Not that the problem is easy to solve. The question is whether it is possible to construct a theory that will serve to explain how and when new things happen. Can we build a model that will make such events understandable when they have happened in history, and predictable and possibly subject to control when they happen in future? An honest answer, at the moment, must be in the negative. None of the social sciences is yet able to offer such a model. In psychology and anthropology, in the history of invention and discovery, there are clues, but one looks in vain for a general theory. This is ironic, for creativity has been the essence of the human experience so far. It is, however, a hard nut for social and behavioral scientists to crack. Equilibrium models are easier to build; gradual and continuous change is simpler to handle mathematically. Unfortunately, it is the discontinuities in history, and in one's own life experience, that cause the trouble.[1]

Lacking a general theory, we must proceed by steps, using as our guides the generalizations that previous workers in the field have suggested. Two of these are particularly relevant. First, there is general agreement that creativity is best analyzed as a process, not as a set of isolated, distinct events. Barnett, for example, defines innovation as "the reorganization of a configuration of ideas"; the essence of change lies in the restructuring of the parts so that a new pattern results. To understand creativity we should look not at the separate parts but at the process by which they become fused into a new configuration.[2] Second, novelty always emerges out of the familiar. If we look only at the end product we may be struck by its apparent "distance" from its antecedent components; but this impression usually vanishes on closer examination. George Sarton, dean of historians of science, expresses it this way: "When one considers carefully the genesis of any discovery one finds that it was gradually prepared by a number of smaller ones, and the deeper one's investigation, the more intermediary stages are found."[3] Creativity implies a new configuration. *Ex ante* this typically appears as a discontinuity, a leap into the unknown. But to the historian, analyzing the process after its closure, the new configuration seems to emerge not by one single leap but a series of incremental, distinguishable steps.

Neither of these propositions seems, on the face of it, very profound or helpful. Taken together, they suggest a particular point of view and an avenue of analysis. The first statement directs our attention to the way in which novelty emerges rather than to the particular features of the new things that result. These results, the particular outcomes of the creative process, are very diverse. What does a new symphonic form have in common with a new antibiotic or a new mode of political organization? If we are looking for the common characteristics of creativity it is not likely that we will find them by analyzing the characteristics of these end products. Much more probable is it that we will find them located in the process by which creativity happens. It would be well, then, to look less at inventions and more at inventing; less at discoveries and more at discovering. And since inventing and discovering are activities carried out by individuals and groups that are part of organized societies, acting in socially defined ways and seeking socially valued objectives, it will clearly be a social process that we are investigating, and our methods had better be chosen accordingly.

The second proposition reminds us that nothing is wholly new; novelty emerges out of the known, the familiar. Its origins are to be looked for in preexistent data, and the degree to which it is new is to be gauged by the extent to which it goes beyond any ways in which those data have previously been put together. Creativity does indeed involve an act of insight, a shift of perspective that makes the familiar strange and the strange familiar. But the process starts from combinations of known elements; its immediate stimulus is a perception of incompleteness in those combinations, often under the stress of a problem that demands solution; its essential characteristic is the sudden "seeing" of what is required to complete the incomplete pattern; and its necessary sequel is critical revision to integrate the new configuration more thoroughly into what was known before. At both ends of the process the emergence of something new is rooted in a field of the known and the familiar.[4]

When we speak of "discoveries" we think of science or of exploration. When we speak of "inventions," devices like the steam engine or the telephone come to mind. And when we talk of "innovations" we associate the word with new forms of organization, new methods of production, or new modes of behavior. These semantic differences are certainly useful but they must not obscure the common feature that underlies them. This feature is creativity, the emergence of novelty in ways of thinking, ways of acting, and ways of perceiving. This phenomenon cuts across all our conventional categories of human experience. Any model we devise to explain it or understand it must be one of very general applicability. The idea of creativity as a process by which novelty emerges out of recombinations of given elements is a first step toward such a model. Whether in the arts or in science and technology or in the business of everyday life, the best clue to understanding how new things emerge is to watch how elements of the known are combined and recombined in new patterns, how new features are added to fill perceived gaps, and how new patterns are modified and revised so that they interlock with the familiar. The kaleidoscope turns gradually, but suddenly the elements of the image shift and fall into a new design. The result may appeal or not; it may prove useful or useless, interesting or dull. But the process has been creative, irrespective of our liking for the outcome.

It may well be true that the artist is more sensitive to the shape of things that are "struggling to be born" than is the scientist. If we wish some insight into modes of human experience that are not yet fully realized but still clouds on the horizon "no bigger than a man's hand," possibly it is to contemporary literature, drama, music, and poetry we should turn, rather than to forecasts of future scientific or technological breakthroughs. In the arts the imagination works under fewer constraints. Requirements of verifiability, skepticism, objectivity, and disinterestedness check and channel creativity in the sciences, but not in the arts. And artists, seeking the widest possible audience, are more likely to express themselves in a vernacular than are the scientists, who necessarily use a special-purpose vocabulary unintelligible to laymen or to scientists in fields other than their own. For artists past forms are fetters from which they must struggle to liberate themselves; for scientists they are structures on which to build. For these reasons, in the constantly shifting modes of artistic expression we may sometimes catch a glimpse of the future earlier than it shows itself in the more disciplined advance of science and technology.

The processes by which novelty emerges in science are in important ways distinguishable from parallel processes in other fields. The differences arise principally from differences in the nature of the outputs that are desired. The output of science is of course knowledge, but knowledge of a particular kind. In the words of Robert K. Merton, it is "that particular kind of knowledge which springs from and returns to controlled experiment or controlled observation."[5] The crucial element is the emphasis on control, on the processes by which science discriminates between desirable and undesirable, true and false, good and bad outputs. To maintain these controls science has developed a rigorous methodology of testing and proof; a set of standards by which new discoveries are appraised; an internal social structure one of whose functions is to monitor conformity to these standards; and an ethos or set of internalized values which guides behavior and limits deviance. These are the elements we refer to when we speak of scientific creativity as being subject to discipline. There are disciplines in the arts too, but not of this type.

Science is that sector of society which specializes in the systematic production of new knowledge. The essence of creativity, here as elsewhere, lies in the combination of bits of information into new patterns. Sometimes the elements are already at hand; only the combination is new. But sometimes, to complete a pattern perceived as incomplete, new information is seen to be necessary; in such a case novelty is not just a new combination but involves also the deliberate search for an element of information not available but recognized as required. In either case the outcome of the creative process is a new structure of knowledge: a generalized conceptual scheme. Such generalized conceptual schemes, produced according to accepted canons of inquiry and appraised according to accepted canons of verification, are the sole output of pure science, strictly defined. Advance in pure science is a matter of developing conceptual schemes of ever greater explanatory power, a process that includes extension, revision, and testing, an "inherently endless process of establishing provisional truth." Applied science, in contrast, is science "devoted to making conceptual schemes instrumental to some other purpose than that of the pursuit of conceptual schemes as ends-in-themselves."[6] Advance in applied science is gauged by the degree to which such social purposes are served.

Merton has suggested that historians of the future will find it strange that so few social scientists of the twentieth century could bring themselves, in their work, to treat science as one of the great social institutions of the time. "Long after the sociology of science became an identifiable field of inquiry," he observes, "it remained little cultivated in a world where science loomed large enough to present mankind with the choice of destruction or survival."[7] If we think of science as that sector of society which has the particular function of generating systematic new knowledge, then indeed it is ironic that we should have such meager understanding of how "new things" in that sector occur. And it is even more ironic that our understanding of how new information generated by science becomes translated into new technology, and then into the structures, devices, and processes of everyday economic life, still barely transcends the level of glib and ill-supported generalizations. If the "new things" of science, technology, and economic life often seem overwhelming and uncontrollable, the reason may well be that we have, in modern societies, created highly efficient structures for the generation of new knowledge without seriously attending to the processes by which new knowledge is put to use. These processes are social processes, and they relate particularly to the ways in which new information generated by science is screened, filtered, and transformed as it is converted first into new technology and then into goods and services and new patterns of economic behavior. There may be some excuse for our failure to grapple with the problem of creativity itself; it is harder to justify our failure to come to grips with these simpler questions of how new information is transformed and utilized.

It is clear that the relationships of science to the wider society of which it is a part are not random or haphazard. In some situations science flourishes and shows high productivity; its practitioners are esteemed and its ventures receive generous support. At other times and places the reverse is true. In the same way, there are situations in which new discoveries made by science are transformed with little delay into feasible technology; and there are other situations in which such transfers take place slowly if at all. Clearly these differences call for explanation in terms of the ways in which science, as a specialized social subsystem, is integrated into the wider social structure. Some forms of integration generate large flows of resources into science and large flows of information out of science; others do not. How can these differences be described and explained?

We have already stressed that, although the creative process in science must surely resemble in many respects its counterpart in other fields of behavior, nevertheless it operates under special constraints. The purpose of these constraints is to confine scientific output to new knowledge of particular types, eliminating outputs that have not been unambiguously produced according to recognized canons of inquiry and validation. Seen in this light, these constraints define the nature of the scientific enterprise; they set it off from other modes and forms of human action; and they necessarily interpose barriers to the free flow of information into and out of science. There are some kinds of information that scientists, as scientists, will not and cannot hear, because it has not been produced under conditions that qualify it as scientific evidence; and there are some kinds of information that scientists, as scientists, will not and cannot produce, because to produce it and certify it as scientific information would mean violation of the rules of science. None of this, of course, is arbitrary or without function: these constraints exist as the result of a long process of historical evolution, the very process that has brought modern science into being and endowed it with its remarkable productivity.

Merton has described the institutional imperatives of pure science under four headings which, with some quibbling over terminology, are now generally accepted. These are: universalism, "communism," disinterestedness, and organized skepticism.[8] Together, they form the ethos of modern science. They are reinforced by sanctions imposed and effectively enforced by the scientific community itself, and by internalized values which, transmitted by precept and example, fashion the "scientific conscience." By universalism is meant the rule that claims to truth, no matter what their source, are to be tested by preestablished, impersonal criteria, in particular by their consonance with controlled observation and with previously confirmed knowledge.

Adherence to this canon rules out acceptance or rejection of claims on the basis of criteria such as the particular ethnic, national, or social origin of the claimant. "Communism," in the special sense in which Merton uses the word, refers to the rule that the findings of science are to be treated as products of social collaboration. Title to them is to be vested in the community as a whole, knowledge of them is to be dispersed as widely and quickly as possible, and the equity interest of the individual producer is confined to a claim to priority of discovery. Disinterestedness refers not to the motives of the individual scientist, which may be highly personal and idiosyncratic, but rather to the requirement that scientific findings be submitted to the impartial scrutiny of the scientist's peers, to be appraised by them irrespective of its source. This is the requirement that gives to science its rigorous self-policing character, limits the occurrence of fraud, and denies the propriety of appeals to authority as a guarantor of truth. Lastly, organized skepticism requires the examination of all the available evidence and suspension of judgment until claims to truth are tested by recognized criteria; and it denies that any beliefs at all are exempt from scientific scrutiny, no matter how dogmatically they may be expressed or how firmly they may be held.

Even to list these structural characteristics of science suggests that in many respects there are likely to be discrepancies between the scientific way of thinking and acting and that which is characteristic of other sectors of society. The universalism that science requires, for example, may well conflict with the particularistic discriminations that are so commonly found in attitudes to race, religion, national origin, or social class. "Communism," in Merton's sense, has on more than one occasion proved hard to reconcile with the requirements of national security. And the mandatory skepticism of science, the requirement that all beliefs be doubted, brings science into conflict with the value systems by which other people live, the myths that give meaning and purpose to their lives. These potential incongruities have frequently been noted, and there is no question that they go far to explain why science, in some times and places, enjoys large support and high esteem, while in others it has to make do with a starvation diet and lives only on sufferance.[9] Science cannot change its ethos without jeopardizing its productivity. It cannot adopt a new set of rules to guide its search for truth and still expect the search to be successful. Yet adherence to that ethos and to that set of rules is a major reason why science is vulnerable.

It is not, however, the only reason. Science produces, not things that people can touch, smell, and taste, but systems of ideas: generalized conceptual schemes that are valued partly for the range of their explanatory power but partly also for their elegance and beauty. These idea-systems are pure science's only product; but they are not a product that nonscientists are competent to appraise. The value of science's output, in other words, can be directly estimated only by scientists themselves; and a judgment as to the level of support that science should receive must be made either by taking on faith the judgment of those who will themselves receive the support, or alternatively by some other criterion of only indirect relevance. Typically this alternative criterion is a technological one. Science is "valued," that is to say, not in terms of its own outputs, but in terms of the outputs of the technological system, the devices and processes that, in some degree, embody scientific knowledge and give it physical form. To the extent that social support for science is not just an act of faith, it is based on this technological criterion. Readiness to accept the authority of science rests on its "daily demonstration of power" through technology.[10]

Science, in short, is valued socially largely in terms of its technological byproducts This can be a source of strength or of weakness, since these byproducts can be valued either positively or negatively. Hence the profound social ambivalence toward science; and hence too the fragility of science's social support. When the technology is valued highly, science reaps the benefit; when the technology is disliked, feared, or distrusted, science suffers the loss. And this situation is inescapable, given science's insistence on autonomy, its repudiation of control by any outside agency, its dogged adherence to its own distinctive canons of thought and action, and its unintelligibility to the general public. Social support for science depends heavily on the production of outputs which are other than the outputs scientists themselves value most highly. And over these outputs, their particular form, and their social impact, scientists exercise very little control.

An analysis along these lines has obvious implications for any historian who wishes to understand changes in the social role of science in the past. And it is not without relevance to those concerned with the position of science in the present and the near future. Either of these interests would suggest that one critical area of interaction is the translation of scientific discoveries into technological innovations. For it is as a result of this process of translation that science impinges on the economic system and on everyday life. And it is to a significant extent in terms of the favorable or unfavorable evaluations of the resulting technology that science itself is judged.

Our present concern, however, is with a somewhat different question. We have asked how new things happen. And we have suggested that one of the springs of creativity can be found in the process by which bits of information are combined and recombined into novel forms. From this point of view an understanding of the processes by which new information is transferred from science into technology, and from technology into economic life, is clearly indispensable. Likewise, we need to know something about the reciprocal relationships, for it would be naive indeed to suppose that the advance of science is in no way influenced by inputs of new information from technology, or the advance of technology in no way influenced by inputs from the economy. The interrelationships are complex. We do not imply that technological creativity arises only from new inputs of information from science. Nor is it suggested that economic creativity is solely a passive absorption of new information from technology. Our hypothesis is more modest: it is merely that, in the combination and recombination of elements that is the essence of creativity, inputs of new information are frequently catalytic. They are the spark that starts the fire, the snowball that sets off the avalanche. Science is that sector of society which specializes in the systematic production of new knowledge. The processes by which this information is made available to other sectors of society are clearly of vital importance. In particular, they must form an essential element in any comprehensive theory of technological change.

What factors are conducive to a smooth and easy interchange of information between science and technology, and what factors are likely to impede the flow? It is clear that, in terms of their characteristic values, science and technology are in many respects congruent. Both are highly rational systems of action; in this respect they stand in sharp contrast with religion and art. They are both highly utilitarian, in the sense of being oriented to "this-wordly" rather than transcendental goals. Both are highly universalistic and individualistic, repudiating authority and the particularities of race and religion as validators of truth. And both are imbued with the idea of progress, taking it almost for granted that today's science, like today's technology, is better than yesterday's, and that tomorrow's will be better still. Thus, even if the ethos of technology is not identical with the ethos of science, there are many parallels between them and no differences in orientation, it would seem, massive enough to check transfers of information.

When one looks at their social structures, much the same impression emerges. Science and technology both show a highly specialized division of labor. Although both have hierarchies of status and reputation, in both there is high mobility and the avenues of ascent are relatively open. And though status in both, as one would expect, brings authority and power, their systems of governance, by which power is legitimized and rules of behavior maintained, are relatively democratic. In short, although neither in their characteristic values nor in their social structures are science and technology identical, yet they are, as it were, "mirror-image twins," resembling each other more closely than either resembles other parts of society.[11]

There are, however, differences, and there is reason to believe that these differences are of some importance in understanding the exchange of information between the two systems. In the first place, there are differences in the degree to which each system "lives up to" its own canons of behavior. Even in pure science, where respect for the ideals of the system is very high, conformity to those ideals is not complete. In applied science this is even more true, and in technology still more. As the pressure to serve practical needs builds up, and as the rewards for successfully satisfying them become larger and more immediate, so does deviation from the nominal standards of behavior become more probable. To be blunt about it, fraud is more common in technology than in pure science; so is the abuse of authority, the tendency to evaluate truth-claims in terms of their source rather than their intrinsic merit, and the propensity to allow self-interest rather than disinterestedness to influence judgment.[12] This is not, of course, because individuals working with technology are intrinsically less moral, high-minded, or altruistic than are pure scientists; it is because the "rules of the game" are subtly different, the pressures on the individual differently phased, and the sanctions that are supposed to insure conformity to the nominal values of the system are more ambiguous and less effectively enforced.

Technology, too, tends to be somewhat more "traditional" in its attitudes than pure science, in the sense that there tends to build up in each field a body of technological lore, of accepted ways of getting things done, which new entrants must show they have mastered before their own originality is allowed scope. Combined with this traditionalism, however, there is in all technologies a great deal of "cut-and-try" rule of thumb pragmatism, a willingness to make do with quite low-level theories provided that the job gets done. It is this mixture of traditionalism and pragmatism that gives the world of the engineer its characteristic tone and temper, recognizably different, by all who have experienced it, from the world of the pure scientist.

There are other differences. Consider, for example, the question of property rights. It is true that the everyday language of scientists is permeated with words that suggest that the idea of property is by no means alien to them. As Merton puts it: "Borrowing, trespassing, poaching, credit, stealing a concept which 'belongs' to us—these are only a few of the many terms in the lexicon of property adopted by scientists as a matter of course."[13] But what kind of "property" is this? Certainly not the rights of exclusive use that are ordinarily implied. In science the concept of property is narrowed down to an emaciated shadow of what it means in the courts and the marketplace. It refers in fact to one type of claim and one type of right only: the claim to priority of discovery and the right to have priority recognized. This is, of course, precisely why priority of discovery is, in the world of the scientist, a focal point for so many anxieties, tensions, and rivalries, and why such pains are taken to establish priority: it is the only kind of exclusive right to the products of their labor that scientists, if they follow strictly the ideals of their calling, are permitted to claim. The discovery is not the individual's alone. It is the product of communal, cooperative effort; it belongs to no individual but to science as a whole. To quote Merton again: "Once he has made his contribution, the scientist no longer has exclusive rights of access to it. It becomes part of the public domain of science. Nor has he the right of regulating its use by others by withholding it unless it is acknowledged as his. In short, property rights in science become whittled down to just this one: recognition by others of the scientist's distinctive part in having brought the result into being."[14]

Claims to priority of discovery and disputes over those claims are, of course, by no means unknown in the world of technology. In that world, however, priorities (at least in capitalist societies and in many semisocialist ones also) are backed up by the patent system. It is no longer just pride or professional reputation that is at stake but marketable rights. A discovery in technology, in short, is a commodity in a sense in which a discovery in science cannot be. It can be bought and sold; rights to its use can be leased; income can be derived from its possession; and capital can be invested in its production in the rational expectation of a profit. The outputs of the technological system, in other words, are institutionally defined in such a way as to make their absorption into economic use easy and rapid. The values and behavior patterns typical of technology reflect this fact. Scientists who patent their discoveries too often or too readily are likely to find themselves despised by their colleagues and edged out of the inner circles of their guild.[15] If, in contrast, inventors fail to patent their discoveries promptly, we tend to regard them as abnormal; their behavior calls for special explanation.

One way of expressing these differences is to say that science and technology use different criteria of choice. Science, as we have seen, jealously guards its autonomy. Its output consists of pure idea-systems that have no necessary or obvious relationship to particular economic or social needs. The advance of knowledge is its own justification. If choice must be made between alternative courses of action, the criterion is the relative probability of generating new and more powerful conceptual schemes. For this reason science resists, in normal circumstances, any external attempts to direct its course unless these directions are validated by scientists themselves; and it has institutional defences and a set of shared beliefs which normally make such resistance effective.

In technology the insistence on autonomy is much less pronounced, though not completely absent, and the presence of "self-steering" processes is less obvious. Technology is infinitely more exposed to the pressure of social needs, as they are reflected through the economic system, than is science. Its products are designed and created specifically to meet such needs, and the hope of profiting thereby is an entirely acceptable motive for its participants. The signals transmitted by the price system (or, in centrally directed economies, by the priorities of the planning authority) are clearly received throughout the world of technology and serve as its primary criteria for allocating scarce resources.[16] Technology, to oversimplify, is always for hire; pure science, if it is true to its own ideals, never is. The difference is not one of morality. The plain fact of the matter is that the rules of the game which have been found historically to be best suited for progress in science are not identical to the rules of the game best suited for the advance of technology. The autonomy of the technological system shows itself in resistance to the dictates of the price system in particular instances: for example, the insistence that a particular course of action is improper by technological standards, no matter what the economic rationale for it may be. Such instances are common, but they are a far cry from the unqualified general repudiation of outside "steering" that characterizes science.

We have been concerned with the factors that influence the two-way exchange of information between science, technology, and the economic system. To the extent that, between any two of these systems, there are pronounced differences in structure, in values, or in norms of behavior, to that extent transfer of information will be difficult. We have pointed out that, between science and technology, there are such differences, existing side by side with undeniable similarities. And in the same way, though in far less pronounced form, there are differences in structure and values between technology and the economic system. The existence of these differences does not mean that the transfer of information is impossible; it means that the transfer must be handled in particular ways, that it is not automatic or mechanical, that it is in fact a distinct and identifiable function. Information that is generated within one system exists in a particular coded form, recognizable by and useful to participants in that system. If it is to be transferred from one system to another —say from science to technology, or from technology to the economy, or in the reverse direction—it has to be translated into a different code, converted into a form that makes sense in a world of different values.

These transfers take place continuously in any modern, commercialized society. History provides an ample portfolio of cases where this was not so, where flows of information from one system to another were reduced to trickles or disappeared entirely. It is no accident that in such cases development within each system typically slowed down or ceased, for the exchanges of information and resources are not without function. Whether we are concerned with understanding such "negative" cases in the historical record or with the contemporary world in which the flood of new things has become almost overwhelming, we must attend not only to developments *within* each of our three systems but also to the transactions *between* them, to what goes on at the interfaces where science meets technology and where technology meets the economy, and to the ways in which, at these interfaces, information is scanned, filtered, and translated.

In the modern world these interface processes have become institutionalized. The applied sciences serve as translators between pure science and technology. "Research and development," corporate and governmental, perform the analogous function between technology and the economy. And the reverse flows of information, particularly those from the economic system into technology, have become highly organized. Social needs, as these are imperfectly and partially reflected in the price system and government budgets, steer the course of development within technology, and guide the way in which the outputs of new information from science are screened for possible technological use. The processes have become very complex and highly bureaucratic, largely because of economies of scale in the processing of information. We have evolved, to manage the creation of new things, a communications system of enormous complexity. To some signals of need it responds efficiently and with alacrity; other it hardly seems to hear.

In the pages that follow we shall be dealing with a simpler era, one in which particular individuals can be identified as having played critical roles in these processes of information transfer, and we shall concentrate on a single case study—the origins of electronic technology and the birth of radiocommunications. This makes it easy and convenient to use the narrative form, the historian's traditional mode. It should not be necessary to add a caution that this in no sense implies a "heroic" approach to the subject. The individuals we shall be dealing with—Heinrich Hertz, Oliver Lodge, Guglielmo Marconi, and a host of others—were actors in a drama of large-scale social change, and to some degree their personal characteristics and circumstances influenced the way that drama worked itself out. But I am not writing biographies. I am interested in these individuals only to the extent that they can be shown to have played significant roles in a major historical process; the process that created a new means of communication and a new economic resource out of a conceptual advance in pure science—Maxwell's theory of the electromagnetic field. The roles these individuals played are to be thought of in terms of the kinds of information transfers we have already discussed. Hertz, Lodge, and Marconi dominate my story because in this case they dominated the way new information was transferred from science into technology and thence into economic use, and—equally significant—the way information was fed back from economics to technology and thence into science. These men were the translators, the individuals who moved information from one system to another, interpreting it and changing it in creative ways as they did so.

Where should such a story begin? With Oersted, Ampère, and the discovery of electromagnetism? With Faraday and Henry and the discovery of inductance? Or perhaps still further back, with Volta and Galvani and the earliest experimenters with the "electrical fluid"? Any starting point is to some extent arbitrary.

I choose to start with James Clerk Maxwell, Faraday's admirer and successor, and with the year 1865, when Maxwell published his historic paper, "A Dynamical Theory of the Electromagnetic Field."[17] I leave it to the historians of science to explore the intellectual origins of this scientific classic; for our purpose what counts is what it meant for the future.

Maxwell, whenever possible, insisted on a mechanical model for every phenomenon he investigated, something that could be visualized as physically real. In his paper on the electromagnetic field, however, he abandoned these aids to thought. He presented instead a mathematical model devoid of any "physical scaffolding."[18] And he had been led to the construction of this model by a purely mathematical difficulty: an apparent contradiction between the known laws of electricity and magnetism and the basic physical law of continuity, stating that electric charge could be neither created nor destroyed. This contradiction could be removed and consistency in the system of equations regained by the insertion of a new term in Ampère's law of electrodynamics—a term that had the characteristics of an electric current, except that it flowed in an insulator (i.e., in space) instead of in a conductor. Maxwell called it a displacement current. Along with such a current, if it fluctuated with time, there had to travel a fluctuating magnetic field. Together they formed what Maxwell called the electromagnetic field. How such a current could flow and in what medium such fields could travel was not clear. Mathematically, however, it was a solution to the difficulty, and one not without elegance.[19]

The resulting model was stated as a system of equations. Its physical counterpart was obscure, it owed its origin to a mathematical deduction, and there was no experimental evidence whatsoever at the time to confirm it. Indeed it was by no means clear that it could be verified. It was precisely this lack of experimental evidence and the difficulty of visualizing Maxwell's model in physical or mechanical terms that led eminent scientists —von Helmholtz and Lord Kelvin, for example—to reject it.

The implications of the model were, however, intriguing. They could be interpreted as stating that electromagnetic fields could be propagated through space as well as through conductors; that, if so propagated, they would travel as waves with the velocity of light; and that light itself *was* electromagnetic radiation, within a certain narrow range of wavelengths. If Maxwell's model were valid, optics and electromagnetism would merge, and science would have at its disposal a single conceptual scheme with vastly greater explanatory power than either of its distinct and separate antecedents. But there were difficulties. Maxwell's field equations had an enormous number of solutions; their very generality, which was later to make it possible to apply them to a wide range of problems, at first inhibited agreement on whether and how they could be tested. And there were profound philosophical problems. How could matter act where it was not? If there were waves, what were the waves *in*? In what medium did they travel? The old preferences for action at a distance, as with gravity, or action by impact, as in the corpuscular theories of light, did not easily yield to the newer notions of fields traveling with finite velocity through empty space.[20]

It required 23 years to produce experimental support for Maxwell's model. In 1888 Heinrich Hertz, a young German physicist, announced that he had succeeded in generating electromagnetic waves, in detecting them, and in measuring their velocity. As predicted by Maxwell's equations, that velocity was, within the limits of experimental error, the same as the velocity of light. The evidence was striking. To those who had for years wanted to accept the Maxwellian model it was convincing. To those who had resisted, it was difficult to explain away. Hertz's experiments were easily duplicated; there was no question that electromagnetic energy could be propagated through space, and that the rate of propagation was finite.

To later readers of Hertz's reports the most striking features of his experiments are their intellectual brilliance and their physical simplicity. The simple, of course, is not always the obvious. Three main problems had previously plagued those who wished to put Maxwell's model to the test. The first and least serious was how to generate an electromagnetic wave. A feasible technique had been suggested by G. F. FitzGerald, the Irish physicist, in 1883: electricity could be stored in a capacitor, such as the Leyden jars commonly in use at the time, and then suddenly discharged across a spark gap into a conductor. The result should be a rapidly accelerating and then decelerating pulse of electric current; if the theory were correct, this should generate an electromagnetic wave, the length and frequency of which would be determined by the size of the capacitor and the amount of inductance in the circuit. The practical problem was to devise equipment that would generate, not a single pulse, but a whole series, so that the experimenter would have some chance of measuring the waves.

The second problem was more serious. Granted that the waves could be generated, how were they to be observed? How could they be detected and measured? If Maxwell's theory were correct, light waves were electromagnetic waves. To study the behavior of light the experimenter could avail himself of that wonderful detector, the human eye. The whole science of optics, indeed, had been built on the capabilities of that detector. But the range of wavelengths the eye could detect was narrow—not more than one octave. What was required was an analogue to the human eye, a device that could respond to much longer wavelengths and lower frequencies.

The third difficulty was a related one, and in some ways it was the least obvious and the hardest of all. It was necessary to generate electromagnetic waves with a length and frequency such that they could be measured within the physical confines of the laboratories in use at the time. And, once a detector was discovered, it was necessary to find some way in which the experimenter could be sure that his detector was responding only to the waves being emitted and not to any other electric disturbance. The analogies that came to mind in connection with these allied problems were from the science of acoustics rather than optics. What was required was a detecting device that would *resonate* when electromagnetic waves of a certain frequency impinged on it, much as a tuning fork of a specific pitch would resonate when an identical fork nearby was made to sound. Maxwell, and Faraday before him, had known of the theory of electrical resonance; they were well aware that a low resistance circuit containing inductance and capacitance had a specific frequency at which it would resonate, that frequency being determined by the product of its inductance and capacitance. Hertz shared this knowledge and made good use of it in designing his radiating device. He knew, or thought he knew, the frequency of the waves it would emit. But not even Hertz seems to have fully appreciated how critical to the success of his experiments the theory of resonance and the choice of a particular wavelength were to prove. To use the parlance of a later time, his apparatus had to be tuned. His radiator had to radiate waves of known frequency, so that the length of the wave could be measured and its velocity determined. And his detector had to be tuned to the same frequency as his radiator if it was to detect the emitted wave and respond to fluctuations in its strength.

Engineers and scientists still refer to this phenomenon as resonance, and it has remained fundamental to all work in electromagnetic radiation. Tuning was the word Marconi preferred, and it has become part of everyday language. Oliver Lodge liked to talk of syntony: when two circuits were brought into resonance with each other they were, so to speak, of equal tension, like two strings on a lute or guitar. Syntony was a good word, with a respectable history in aesthetics and musical theory, and it is unfortunate that it fell into disuse.[21]

Maxwell's model was a discovery in pure science. There could hardly be a clearer example. As Hertz pointed out, Maxwell's theory was not merely *stated* as a system of equations; it *was* that system. The validation or testing of the model had been the sole purpose of Hertz's experiment. There is no hint of any concern with technological or economic implications. Nevertheless, when Hertz published his findings in 1888, he had done more than provide experimental evidence for Maxwell's model. He had also, incidentally, created the basic elements for a new technology—the technology of spark radiotelegraphy. The first step in the transfer of knowledge from science to technology had been taken.

The man who completed the transfer was Oliver Lodge. Later we shall look at Lodge's experimental work in detail and compare it with Hertz's. They had been working along the same lines, and they had identical scientific objectives. There were, however, certain significant differences in their experimental techniques, in particular the fact that Lodge used long wires to guide the electromagnetic waves he generated whereas Hertz, after experimenting initially with long wires, had turned to direct radiation from an antenna. Lodge's technique had, however, the incidental advantage that it brought very clearly into view the phenomenon of electrical resonance. He was working with "closed" circuits which tended to "ring" like a bell when a pulse of electricity excited them into oscillation; Hertz had used "open" circuits in which the oscillations, because they lost energy through radiation, were rapidly damped. The phenomenon of resonance or syntony, in consequence, which had been of only incidental importance to Hertz and which indeed had caused him some confusion, was at the very focus of Lodge's thinking. And it remained so. If Hertz, as a byproduct of his scientific work, invented the basic technology of spark telegraphy, Lodge, also as a byproduct of his work, discovered the technology of tuning.

Through the work of Hertz there had been created a system of laboratory equipment which could be used to generate, radiate, and detect radio waves. Its function had been to test a theory of light. In Lodge's hand this laboratory equipment, refined and made more sensitive, became by 1894 a technological system that could be, and in fact was, used as a means of sending messages over distances. By that date Lodge had at his disposal all the technological items necessary for a feasible communications system; and he knew and had publicly demonstrated how these items could be put together and made to work. Later refinements there were to be in plenty; but the initial transfer from science to feasible technology had been completed.

Despite these facts, Lodge showed at that time no signs whatever that he was aware of any potential commercial uses for his system, nor that he would have taken steps to explore such uses had he been aware of them. In particular he did nothing to stake a claim, through patents, to the circuits and equipment he had devised. The power levels he had used for his demonstrations were trifling; the distances covered were only a few hundred yards. For Lodge the equipment was a curiosity, an intriguing set of devices to add interest to a public lecture. He was to change his attitudes later, after others had shown the way; but in 1894, when Lodge first demonstrated radiocommunication, there was no thought of commercial development. The transfer from science to technology had been effected; the transfer from technology to economic use had not yet begun.

All this changed in 1896, the year in which Marconi arrived in England. Marconi brought with him not only a working radiocommunications system—embodying refinements in detail but essentially the same as Lodge's—but also a determination to put the system to practical use and, if possible, make money from it. For the historian there is, with Marconi's arrival, the feeling of entering into a different world—the world, not of the scientist but of the engineer and the entrepreneur. Marconi was no scientist in any ordinary sense of the word, but he was a gifted and creative technologist and an uncommonly shrewd businessman. For this reason the formation of the Marconi's Wireless Telegraph and Signal Company in 1897 is the first episode in the *economic* history of radio. With that step the transfer from technology to the economic system began.

These are the bare essentials of the story we shall be analyzing in this book. Rather strict limits, self-imposed, have been set on the coverage in space and time. The work of men like Fessenden, Stone, and De Forest in the United States; Slaby, Arco, and Braun in Germany; Ducretet in France; Popov in Russia; and Poulsen in Denmark receives only incidental attention. And I end this story in 1914. Precisely when the first age of radio technology ended is a question on which reasonable people may differ, but a good case can be made for the date I have chosen. By 1914 the use of spark discharges to generate radio waves was clearly becoming obsolete. New techniques were at hand, techniques of generating continuous waves instead of the pulse trains generated by the spark gap. Arc transmitters were in use, the radiofrequency alternator was available, and the triode vacuum tube was just over the horizon. By that date, too, the transfers of information between science, technology, and the economy which Hertz, Lodge, and Marconi had initiated had become regularized and vastly enlarged in scope. A new industry had been created. Supporting it and supported by it there was a new technology; and underlying the whole was the progress of physical science, largely following its own internal logic of development but also furnishing new knowledge to technology and occasionally learning from it. Man now knew that the radiofrequency spectrum existed; he had developed ways of gaining access to it and of locating himself in it; and he was beginning to grasp the fantastic range and variety of its possible uses.

This new knowledge, without which there could be no "electronic age," stemmed from the work of the men who appear in this story and from the new information they generated. Maxwell's theory of the electromagnetic field provided the conceptual scheme that made all else possible. Hertz devised the technology to test and validate Maxwell's equations. Lodge refined that technology and showed how it could be used to transmit information. Marconi found for the new technology an economic "place" where it could fit, survive, and grow. Spark discharges gave these men access to the electromagnetic spectrum; syntony enabled them to use it, locate themselves in it, stake claims to it, and convert it into an economic resource.

Notes

1. For a survey of the literature in psychology see Frank Barron, *Creative Person and Creative Process* (New York: Holt, Rinehart and Winston, 1969). Serious interest in the psychology of creativity stems from the pioneering work of J. P. Guilford, and it is interesting to note that Guilford's interest in the problem arose from his dissatisfaction with the use of standard "intelligence" tests in the selection of combat crews for the U.S. Air Force in World War II. His classic article, "Three Faces of Intellect," *The American Psychologist*, Vol. 14 (1959), pp. 469–479, should be consulted for the concept of "divergent thinking," in which "the product is not completely determined by the given information." The distinction between creativity and intelligence, as conventionally measured, is heavily stressed in Jacob W. Getzels and P. W. Jackson, "The Highly Intelligent and the Highly Creative Adolescent," in Raymond G. Kuhlen and George G. Thompson, Eds., *Psychological Studies in Human Development* (New York: Appleton-Century-Crofts, 1963), pp. 370–381. Clinical studies such as Ann Roe, *The Making of a Scientist* (New York: Dodd, Mead, 1952) are very suggestive; the older literature on "genius," such as Francis Galton, *Inquiries into Human Faculty and its Development* (New York: Dutton, 1907) and William James, *Principles of Psychology* (New York: Holt, Rinehart and Winston, 1890), is still worth consulting; and for those who doggedly believe that there is much in creativity that will forever escape the psychometrician's net, there is Arthur Koestler, *The Act of Creation* (New York: Macmillan, 1964) and Jacques Maritain, *Creative Intuition in Art and Poetry* (New York: Macmillan, 1955). A somewhat irreverent approach to what he calls the "wondrous buzz and clatter of naturally occurring thought upon creative matters" can be found in Herbert F. Crovitz, *Galton's Walk: Methods for the Analysis of Thinking, Intelligence, and Creativity* (New York: Harper & Row, 1970).
2. H. G. Barnett, *Innovation: The Basis of Cultural Change* (New York: McGraw-Hill, 1953), pp. 181–224. Compare Morris I. Stein, "Creativity and the Scientist," in Bernard Barber and Walter Hirsch, Eds., *The Sociology of Science* (New York: Free Press, 1962), pp. 329–343.
3. Quoted in Bernard Barber, *Science and the Social Order* (Glencoe, Ill.: Free Press, 1952), p. 197.
4. The approach adopted here is essentially that of my former teacher, Abbott Payson Usher. See his *A History of Mechanical Inventions* (Cam-

bridge, Mass.: Harvard University Press, rev. ed., 1954), especially Chapter 4.

5. Robert K. Merton, *Social Theory and Social Structure* (New York: Free Press, rev. and enlarged ed., 1957), p. 531.
6. Barber, *Science and the Social Order*, p. 95.
7. Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review*, Vol. 22 (1957), pp. 635–659.
8. Merton, *Social Theory and Social Structure*, pp. 553–560. Compare Barber, *Science and the Social Order*, especially pp. 62–95. Barber, with good reason, prefers the term "communalism."
9. See especially Merton, *Social Theory and Social Structure*, Chapters 15 and 16.
10. Merton, *Social Theory and Social Structure*, p. 534.
11. This analysis closely follows Barber, *Science and the Social Order*, pp. 62–94. The phrase "mirror-image twins" is from Edwin Layton, "Mirror Image Twins: The Communities of Science and Technology in Nineteenth Century America," *Technology and Culture*, Vol. 12, No. 4 (October 1971), pp. 562–580.
12. And, an economist must add, the same generalizations may be made with even greater force when comparing technology and the economy.
13. Merton, "Priorities in Scientific Discovery," p. 455, fn. 19.
14. Merton, "Priorities in Scientific Discovery," pp. 455–456; compare Barber, *Science and the Social Order*, pp. 91, 152-153, and 197.
15. Unless, that is, there are special reasons for patenting, as for instance when a discovery in medical science is patented, instead of being published in the normal way, in order to protect it from commercial exploitation.
16. This does not mean that the rate, timing, or character of technological change are uniquely determined by "demand-side" forces. For further discussion, see pp. 302–12 below.
17. Maxwell submitted this paper to the Royal Society on 12 October 1864; it was read on 8 December of that year and published in the *Philosophical Transactions* for 1865.
18. Morris Kline, *Mathematics in Western Culture* (New York: Oxford University Press, 1953), p. 318.
19. Useful sources for the layman are Morris Kline, *Mathematics in Western Culture*, pp. 308–318, and Kline, *Mathematics and the Physical World* (New York: Crowell, 1959), pp. 316–362. Maxwell's field equations in Cartesian coordinates can be found in K. D. Froome and L. Essen, *The Velocity of Light and Radio Waves* (New York: Academic Press, 1969), p. 16.
20. For the background to these problems see Mary B. Hesse, *Forces and Fields: The Concept of Action at a Distance in the History of Physics* (New York:

Nelson, 1961), and Edmund T. Whittaker, *A History of the Theories of Aether and Electricity: The Classical Theories*, 2 vols. (New York: Harper & Row, 1960).

21. Some writers tried to make a distinction between tuning and syntony. See, for example, A. Frederick Collins, *Wireless Telegraphy: Its History, Theory and Practice* (New York: McGraw, 1905), p. 165: "The word *tuned* designates an oscillator so proportioned that its electrical dimensions correspond exactly to the frequency of the oscillations set up in it, and the term *syntonized* indicates that the coefficients of the oscillator are of the same value as that of the resonator operated in conjunction with it."

TWO

SYNTONY

In 1888 Heinrich Hertz, professor of experimental physics at the Technical High School in Karlsruhe, Germany, generated a string of sparks across the secondary winding of a transformer, radiated the resulting electromagnetic waves from an antenna, reflected them from a metal sheet suspended at the far end of his laboratory, and measured the distance between their crests with a simple receiver composed of a loop of wire with a small gap across which sparks were visible. By so doing, Hertz became the first man to measure the velocity of a radio wave, confirming in the process the predictions of James Clerk Maxwell's theory of electromagnetic radiation.

Hertz's discovery of the radiofrequency spectrum of electromagnetic radiation was in many ways analogous to the discovery of a new continent. To be sure, what was discovered was not territory in the geographic sense, and the resources made available for human use were suitable not for settlement, farming, or mining, but primarily for communication. Nevertheless, the analogy is suggestive. Here were new resources—invisible resources, to be sure—whose existence had previously been a matter of speculation only; resources, indeed, that mankind had never before known how to use and whose value was to remain conjectural for many years after their initial discovery. These resources, furthermore, when their economic and military uses came to be appreciated, were to become the object of competitive struggles for exclusive possession and occupancy, just like the colonial empires carved out by the European powers in North America in the seventeenth century or in Africa in the nineteenth. Granted that the radiofrequency spectrum had potential value for human use; granted that at any given time it was limited in capacity; granted that use of it by one limited its use by others; granted these facts, certain issues were bound to arise. Could the radiofrequency spectrum become private property? If not, how were rights to its use to be acquired? Who controlled access? Could access ever be exclusive? Could jurisdictions be established and, if established, could they be defended? Who was to assign rights of use? And how were these rights to be enforced? In all these respects the problems presented by the discovery of the radiofrequency spectrum, a resource created by science and technology, came to have, as the nineteenth century neared its close, clear similarities to the problems faced earlier in the opening up of new continents.[1]

There was, however, one major point of difference. When rights of exclusive occupancy of territory are established—as for example by a treaty of peace between two nations, or by the grant of land by a government to a settler, or by the purchase of a piece of suburban real estate—reference is made to known surveyor's landmarks and to known units of measurements. When a homesteader in the American West was granted his quarter section, he knew where it was, how to find it, and how to mark off its limits. There might be dispute over the deed, and surveyor's marks have been shifted now and then, but in principle the territorial rights assigned were definite and unambiguous. The reason for this state of affairs was a historical one: the tremendous social and political importance that clear definition of title to land had held for generations in Europe and still held in America; the long development of techniques of land measurement and surveying from their ancient origins in Egypt; and the fact that, through time, there had come to exist commonly understood and widely accepted units of length and area. Techniques for the resolution of territorial disputes had been successfully evolved, partly by law, partly by science and technology. Geometry, the surveyor's chain, and the law of trespass combined to make institutional definition of property rights in land feasible.

With the radiofrequency spectrum the matter was different. Here there were, initially, no recognized boundaries. The upper and lower frequency limits of practical use were unknown. Finding one's location in the spectrum was a question for laboratory research of considerable delicacy. How much "room" there was in the spectrum, into how many "places" it could be divided, were matters for the sheerest speculation. Indeed, for the scientific discoverers of the new continent, whether it had any practical use at all was very much an open question, and not one in which they had much interest. As far as legal institutions were concerned, there were none relevant, nor would there be until science and technology solved the prior problems of how to make allocation and exclusive occupancy technically possible. A new continent the radiofrequency spectrum might be, but it was a continent whose nature and dimensions could be grasped only by the scientifically trained intellect, one in which there were no familiar landmarks or units of measurement, one where place, occupancy, and possession had to be given meanings different from any they had had before.

To bring this continent into practical use, to tame it for man's purposes, required therefore a series of technological advances before legal codes and governmental regulations could begin to grapple with it. Science had made the discovery, but technology had to translate it into terms with which lawyers, bureaucrats, and businessmen could deal. Basic to this was the establishment of some means by which rights of use could be allocated and protected. This was to involve, in time, international treaties, conferences, conventions, and a host of regulatory agencies, all concerned with the allocation of rights to the radiofrequency spectrum. But before any of this legal apparatus of allocation and regulation could be created, technology had to solve the problem of how location in the spectrum could be determined and how particular locations could be found and kept. Legal rights could not be established until technology discovered what kinds of rights could exist.

Of central importance to this technological process was the development of the concept of syntony. Today the word is hardly ever used. We speak of "tuning" instead. Out language is the poorer as a result, because "syntony" carries with it none of the associations of melody or tunefulness that "tuning" has. Syntony, or what we call tuning, is what makes it possible to locate a radio transmitter or receiver at a particular frequency or wavelength in the radiofrequency spectrum and at none other.[2] It is the concept that makes place and rights of occupancy possible.

People without specialized technical knowledge probably associate radio primarily with AM (amplitude modulation) or FM (frequency modulation) entertainment broadcasting. The receiver they use has numbers on a dial or scale of some sort, which will probably be marked from 540 to 1600 kHz (kilohertz) in the case of the AM band and from 88 to 108 MHz (megahertz) in the case of the FM band.[3] When they "tune in" their chosen station, they find it by referring to these numbers, even though they may be quite unable to give a sensible description of what the cryptic "kHz" or "MHz" mean or what is going on behind the dial when they turn the knob. That place on the dial is where the station *is*; and it is there because in the United States the Federal Communications Commission has issued to its owners a license stating that, for a specified term of years, it is authorized to be there, broadcasting between certain hours with a specified power and sometimes with a specified pattern of antenna radiation. The license, in fact, will state much more than that. It will also, for example, require that the electromagnetic radiation from the transmitter not depart from the specified frequency by more than a stated amount. This not only imposes on the station a requirement for strict control of frequency; it also limits its permissible bandwidth of emission, or the "room" that it can occupy on the spectrum.[4] Summed up and specified in the license, in short, are rights, duties, and limits of occupancy closely analogous to those involved in a grant of land. Spectrum space is being allocated rather than territorial space. This is possible because there have come into being circuits and components that permit the attainment of precise syntony, or tuning, both at the receiver and at the transmitter.

The ordinary citizen, if he or she has given the matter any thought, probably knows that television programs also are transmitted by "radio"—AM radio in the case of the picture, FM radio in the case of the sound. The TV receiver, however, will have a dial or other indicating device marked, not in kilohertz or megahertz, but by a sequence of apparently arbitrary numbers, running (in the United States) from 2 through 12 in the case of the VHF (very high frequency) stations and from 14 through 83 in the case of the UHF (ultra high frequency) stations.[5] These channels represent specific frequencies and bandwidths allocated by the FCC. Ordinarily the TV receiver will have one knob marked "Fine Tuning"; this enables the user to bring the receiver into exact syntony with the signal radiated from the transmitter.

If these imaginary individuals are among the increasing number who are licensed to use Citizens' Band radios and therefore have the privilege of transmitting as well as receiving radio signals, they will be familiar with numbers running from 1 through 23 on the dial of their transceiver, and they will know (or should) that these refer to channels in the 27 MHz band of the spectrum on which they are permitted to operate, provided (among other things) that the output power does not exceed a certain limit and also that the frequency of the transmissions is controlled by a quartz crystal. That crystal, with its associated circuitry, provides a convenient device for attaining syntony at the stipulated frequencies. If the CB operator is on speaking terms with a licensed amateur, he or she will probably find the latter referring to the CB frequencies as being on the "11-meter band" which once "belonged to" the amateurs, and may well be reminded that amateurs, unlike CB operators, are not restricted to crystal control but may vary their frequencies at will, provided only that they remain within the segments of the radio spectrum allocated to them. These references to the old 11-meter band lost to licensed amateurs are echoes of a frontier skirmish over disputed territory now long past but not forgotten. Comments on the limitations of crystal control reflect the pride that licensed amateurs take in their freedom to syntonize their transmitters to chosen frequencies at their discretion, albeit within very narrow limits.

Wherever, in short, ordinary citizens come into contact with radio, they come into contact, whether they know it or not, with the concept of syntony and with the circuits and hardware that have been devised to achieve syntony. Radio engineers, radio astronomers, owners of broadcast facilities, and the staffs of radio regulatory agencies live with syntony as a fact of everyday life, though they may never use the word. All users of the radio spectrum, from high-powered national propaganda transmitters to the two-way radio in a police cruiser or the youngster down the block who was given a walkie-talkie for Christmas, are able to function only because of the command we have achieved over the technology of syntonic circuits. Occupants of the radio spectrum, whether transmitters or receivers, depend on syntony to find the place on the spectrum where they wish to be or are required to be. Agencies responsible for regulating spectrum use depend on syntony to define the rights of use and occupancy that they allocate, for without knowledge of location and means of determining location there can be no protection against trespass or interference. Effective use for human purposes of the radiofrequency spectrum, that new continent whose existence was predicted by Maxwell and experimentally confirmed by Hertz, depended on the development of a technology of syntony.

* * *

The rate at which this electromagnetic continent has been occupied and put to human use has depended on the rate of movement of two frontiers: an extensive frontier and an intensive one. Movement of the extensive frontier has been contingent upon the advance of knowledge of how to generate, propagate, and receive radio transmissions at wavelengths previously unused. There are several curious aspects, historically, to the movement of this frontier, as we see later in this story. In particular, it is strange that the first deliberate and successful laboratory use of radio waves was at what we now call very high frequencies while the first successful commercial exploitation was at the other end of the spectrum, at the low and very low frequencies. It is as if the first explorer of a new continent had sighted land at one latitude, but the first colonization had taken place somewhere quite different. Movement of the extensive frontier has, since the days of Marconi, been largely a question of pushing into higher and higher frequencies, until today we are using frequencies on the edge of the infrared sector of the spectrum and, indeed, learning through the laser how to use light itself as a multichannel carrier of information.[6] Ahead of this extensive frontier have pushed the scientists, the experimenters, and the amateurs, modern analogues of the explorers, frontiersmen, and *coureurs du bois* of the North American West.

Meantime, to the rear of this advancing extensive frontier, there has developed an intensive frontier, as users of the radiofrequency spectrum have learned, painfully and often under duress, how to cope with increasing density of occupation. Techniques that were prodigal with spectrum have been abandoned, and techniques that conserved spectrum developed and adopted. Just as, in the case of geographical frontiers, extensive cultivation gives way to intensive as land rents rise and population begins to press on limited area, so in the case of the radiofrequency frontier spark transmissions have given way to continuous wave telegraphy, double sideband voice transmissions to single sideband, and wideband FM to narrowband. Each occupant of the spectrum has had to learn how to live in less space; technical advances in the "state of the art," reinforced by government regulation, have made possible denser occupancy without a corresponding increase in interference, the electromagnetic analogue of trespass.

Historians of the radiofrequency spectrum have noticed that these two frontiers have tended to move in an alternating rhythm.[7] The extensive frontier shifts to open up new spectrum for occupancy. Density of use builds up behind that margin, and the intensive frontier adapts to make room for the larger population. The analogy with the development of newly settled lands is striking and not coincidental. Another way to express the same relationship is to say that technological change in radio has followed a zigzag course, its thrust being in one period to move the extensive margin out, thus making more space available, but in the following period to move the intensive margin, making it possible for available space to be occupied more densely. But the rhythm of alternation has not been even; the zigs and zags have not been equal. Technological breakthroughs and the development of new institutions for spectrum allocation have sometimes made it easier to shift the intensive frontier than the extensive, and sometimes the reverse.

At the heart of the technological challenges presented by both radiofrequency frontiers has been the problem of syntony. New areas of the spectrum can be brought into use only if it is possible to tune transmitters and receivers to the new frequencies and to maintain an assigned or chosen frequency with reasonable stability. Lacking this, the new territory is not usable space but wilderness, an empty land in which there can be no rights of occupancy because there can be no assigned locations and no sure limits. Greater intensive occupancy of existing spectrum is possible only if bandwidths of transmission are narrowed, undesired or spurious frequencies of transmission eliminated, and assigned frequencies maintained with a high degree of precision. The denser the occupancy, the greater is the value of territorial rights, the less tolerable is trespass, and the more firmly must encroachment be resisted. This is why the technological history of radio has been in such large measure a history of the search for more precise control of frequency, for narrower bandwidths, and for greater selectivity. At the core of these problems is the theory and design of tuned or syntonic circuits.

* * *

The word "syntony" was introduced into the language of wireless communication by Oliver Lodge, whose patents on syntonic telegraphy were, many years after his death, adjudged by the courts to be fundamental.[8] It is tempting to speculate as to possible connections between Lodge's early recognition of the importance of resonance in electronics and his well-known later interest in spiritualism, but his writings give scant support for this notion and if there was any connection it was not at the level of consciousness.[9] The analogy that Lodge was using was an acoustic one, as indeed was true of most experimenters of his day. Electromagnetic waves were thought of as vibrations or undulations in the aether, much as sound waves were vibrations in the air, except that, first, their lengths and frequencies were very different, and, second, electromagnetic waves were transverse to the direction of propagation while sound waves were longitudinal. When, therefore, a transmitter and a receiver were so adjusted that there was a maximum transfer of energy between them, they were said to be syntonized, or resonant to waves of the same length, like two tuning forks of the same pitch or two piano strings of the same length and tension. This is what the word meant etymologically and in musical parlance. Several of the oldest Greek musical tuning systems, in fact, were known technically as syntonic, and one at least of these—Ptolemy's fourth diatonic scale—had a significant influence on musical theory as one variety of "just intonation."[10]

The word has, however, other usages, and we do not have to dig very deep into the history of Western thought to discover that the ideas of syntony and resonance have played significant roles in a wide variety of contexts. The explanation may well lie in the fact that regular recurrence or periodicity must be one of the earliest and most fundamental of all human experiences. The unborn child in the womb shares the mother's heart beat. The infant follows a rhythm of hunger and feeding, sleep and waking. And all living things recognize the regular cycles of nature: day and night, the phases of the moon, the tides, the sequence of the seasons. While there is life there is the perception of periodicity. From this perception there develop the ideas of cyclic change, rhythm, harmony, and resonance—concepts so deeply embedded in human thought that it is well-nigh impossible to separate the regularly recurrent from the sensuously pleasant, the intellectually satisfying, and the aesthetically beautiful. This is why mathematics, the most abstract of the sciences, and music, the most abstract of the arts, are and always have been so intimately related; the link between them is harmony.[11] A pure musical tone is, mathematically, a perfect sine wave. Feed the tone through a microphone into an oscilloscope and you can see its mathematical form: the path followed by a quantity that varies with time as does the trigonometric sine of an angle varying from zero to 360 degrees. It will have a definite frequency, or rate of recurrence. Tones that are related to it harmonically—that is, tones that we experience as "in harmony" or in resonance with it—have frequencies that are all multiples or submultiples of that fundamental. The mathematical relationship is the formal expression of the aesthetic sensation; it is, so to speak, its formal cause. Harmony, resonance, syntony, or tuning—these are all words we use to describe the relationship between systems that vary together cyclically, that influence one another even though not in contact, that retain their separate identities while sharing a common mode of behavior.

"Harmonics" is no longer thought of as a necessary part of a liberal education, as it was in the Middle Ages when, with arithmetic, geometry, and astronomy, it constituted the *quadrivium*, the more advanced group of the seven liberal arts. But the central ideas that called for attention in the study of harmonics have proved themselves vital and enduring, even though in more recent times they have been overshadowed by mechanistic modes of thought. Their origins are visible in Pythagorean philosophy; and their continuing force can be clearly seen in such later cosmological images as Kepler's "music of the spheres," or in theological metaphors like the "ecclesiastical music" of Thomas à Kempis.[12] In some versions of nineteenth-century aesthetic theory syntony or resonance was conceived of as the ideal relationship between man and nature, between the artist and his world, between the microcosm and the macrocosm. Ruskin's sneers at the "pathetic fallacy" in which romantics like Schiller and Wordsworth indulged were really aimed at an aesthetic theory that thought of the poet and nature as responding to each other in sympathetic harmony. And had not Alexander Baumgarten, inventor of the term "aesthetics," insisted in 1735 that thematic harmony between the representation and the thing represented was the very essence of aesthetic perfection?[13] Western thought, with its typical insistence on sharp polarizations and discontinuities—between man and God, man and nature, man and woman, man and machine—created for itself the problem of bridging those discontinuities, and the concept of harmony or resonance or syntony was one way of doing this, though by no means the only possible one. Psychologists today still speak of persons of syntonic disposition, meaning by this individuals who are temperamentally responsive to their environment and its demands—who are, in short, in tune with their world. The relation of syntony is not one of resemblance or verisimilitude, nor is it one of complementarity in the yin/yang sense; it is a matter of mutual resonance in which a real transfer of energy, a real reinforcement of response, takes place. This is what people mean when, in the popular idiom of today, they speak of a place or an event as having "good vibrations." It is what young people in love mean when they think they can communicate with each other by a look, a glance, or a touch. It is what the psychologist Jean Piaget refers to when, in his classic survey of the language and thought of the child, he tells us that, in a child's characteristic long monologues, "His activity . . . is bathed in an atmosphere of communion or syntonization," so that to speak to himself or to speak to his mother appear to him to be one and the same thing.[14] And this is, of course, reminiscent of many reports of intense mystical experience, particularly that involving prayer and the sensation of union with God.

The concept of syntony and the closely related idea of resonance have, it is clear, an array of meanings and relevances. We cannot stop to explore them here, fascinating through the task might be. Our main interest is in a rather narrow part of the history of technology. And yet surely it is good to remind ourselves that technological ideas are not born and do not live in a world of their own, distinct and somehow isolated from other aspects of cultural history. Technology is as much an expression of man's creative spirit as poetry, sculpture, or folklore. The ideas of resonance, of harmony, of sympathetic vibration, of what Lodge called syntony, have always had the power to stimulate man's imagination when he asked himself how energy could pass from one thing to another when they did not seem to be connected, when they were not related as part to whole, when there was no physical or mechanical linkage between them. Oliver Lodge, when he described his transmitting and receiving circuits as syntonic, was not inventing a new word nor attaching a strained meaning to an old one.[15] Syntony in the strict sense was exactly what he meant, and the scientist in him responded to the semantic appropriateness of it. But Lodge was also a scholar and a dreamer, and the aura of associations that surrounded the word must have appealed to his imagination.[16]

There are four-and-twenty ways, said Kipling, of composing tribal lays; and every single one of them is right. We may be sure, likewise, that there is no "one best way" to study or write the history of technology. One strategy, however, seems to have been too little tried. We have had many histories of machines and processes, and it is undeniably true that these artifacts have their genealogies and that much can be learned by analyzing how a device or a process comes into existence and then is developed and refined, partly by what seems to be its own inner technological logic, partly in response to the demands made upon it. In the same way we have many histories of inventors, the individuals who built the machines and devised the processes. It is good to be reminded, frequently and forcefully, that technology is created by man for man, even though sometimes it seems to take on a life of its own, spawn consequences that were no part of the original plan, and control its own creator.[17] We need to know more than we do about the human side of technological creativity, and the biographical approach is one way to give coherence and continuity to the story. To all of this there can be no objection.

But the history of technology is not just the history of machines and the history of heroic inventors. It is also the history of ideas—the intellectual inventions conceived by man and given shape and form in the devices man builds. These scientific and technological ideas have a creative force of their own, an impact on history that is distinguishable from that of the particular individuals who held them or the particular devices that gave them physical shape. They appear and reappear, changing their meaning in different contexts, often used almost as metaphors, but serving always to organize and give meaning to information that would otherwise remain disjunct and without structure. As it is in the history of philosophy, literature, and art, so it is in the history of science and technology: general ideas are the vehicles of creativity, the organizing devices that make new combinations possible. The history of technology, seen in this light, becomes part of intellectual history and acquires a general cultural significance that is less apparent when attention is fixed on the machine, the physical embodiment of the idea, or on the particular individual who undertook to translate concept into hardware.

A few of these ideas—that of energy, for example, or of work, or of inertia—have proved pivotal on more than one occasion to scientific and technological advance. The idea of syntony merits inclusion in that group. As we have seen, its origins lie far back in the history of acoustics and musical theory, in the analysis of harmony and the design of musical instruments. During the nineteenth century, through the work of Faraday, Maxwell, Hertz, and Lodge, it acquired a vastly enlarged meaning. Electrical circuits, scientists discovered, could be resonant. The essential building blocks here were the concepts of inductance and capacitance. Inductance referred to the ability of a circuit to impede changes in current. Capacitance referred to its ability to store an electrical charge. Any circuit containing inductance and capacitance, if its resistance was low enough, was resonant at a particular frequency. Like a string on a guitar or the column of air in a flute, it could be made to vibrate. The current it carried could be brought into oscillation, and, if the resonant frequency were high enough, the oscillations could be radiated through space in the form of electromagnetic fields. These fields could be detected if they impinged on a second circuit resonant to, or in syntony with, the first.

Between electomagnetic radiation of this kind and the movement of sound waves through the air there were vast differences, as all scientists knew. But, when they faced the problem of detecting electromagnetic radiation, it was the acoustical analogy that came to their aid. Detector and radiator, receiver and transmitter, had to be in harmony; their circuits had to be resonant to the same electrical "pitch" or to some harmonic of it; they had to be in syntony. Lacking this, the "displacement currents" that Maxwell had predicted could never be more than a mathematical construct, for there could be no means of detecting them, discriminating among them, or measuring them.

It was no accident, therefore, that when Heinrich Hertz turned his attention to the experimental testing of Maxwell's model, the first and critical step on the road to success was his observation that, when two circuits were of the same electrical dimensions, like two tuning forks of the same pitch, a pulse of electricity in one could evoke a similar pulse in the other. Syntony was the key to the electromagnetic spectrum, as earlier it had been the key to musical harmony.

Notes

1. On the issues involved in spectrum allocation, see Harvey J. Levin, *The Invisible Resource: Use and Regulation of the Radio Spectrum* (Baltimore: Johns Hopkins University Press, 1971). By the radiofrequency (RF) spectrum we mean that portion of the electromagnetic spectrum usable for radio communication. As a rule of thumb this may be taken as the frequencies between 10 thousand cycles per second and 300 thousand million cycles per second. In these pages I shall follow the modern convention and use the term "Hertz" for one cycle per second. Thus the RF spectrum lies between 10 kilohertz (kHz) and 300,000 megahertz (MHz).
2. Thus the German language, which uses the verb *stimmen* with reference to tuning a musical instrument, uses *einstellen* for tuning a radio, meaning literally "to place in a specific location." In recent years German youth has introduced *eintunen* to refer to what our idiom calls "tuning in," for example with drugs.
3. On an older receiver the corresponding scales will be marked in Kc (kilocycles) and Mc (megacycles), and sometimes also in meters to indicate wave-

length. The standard broadcast band is, more precisely, between 535 and 1605 kHz, and the FM broadcast band is between 88.1 and 107.9 MHz.

4. The broadcast band is divided into 106 channels, each of which is 10 kHz wide. Each channel is designated by its center frequency, and the carrier signal of each AM station on the band must be maintained within 20 Hertz of that center frequency. The maximum audio modulating frequency permitted an AM broadcast station is 5 kHz, so that with 100 percent modulation the full channel bandwidth is occupied.

5. In the United States the frequencies between 806 and 890 MHz, formerly designated UHF television channels 70 through 83, have been reallocated to the land mobile services. See James M. Moore, *Radio Spectrum Handbook* (New York: Howard W. Sams & Co., 1970), pp. 151–152.

6. Lawrence Lessing, "Communicating on a Beam of Light," *Fortune*, March 1973, pp. 118–205.

7. See Levin, *Invisible Resource*, pp. 15–26.

8. These were United Kingdom Patents No. 11575 and 29069, issued in 1897 and 1891 respectively, and No. 11348, issued in 1901. The United States patent was No. 609,154, issued in August 1898. Marconi's basic tuning patent was British Patent No. 7777, filed in 1900 (U.S. Patent No. 763,772, issued in 1904). The Lodge patent (U.S. No. 609,154) was upheld by the United States Supreme Court in 1943, when the original Marconi four-circuit tuning patent was held invalid. See W. R. MacLaurin, *Invention and Innovation in the Radio Industry* (New York: Macmillan, 1949), p. 45; W. H. Eccles, *Wireless* (London: Butterworth, 1933), pp. 71–82; W. J. Baker, *A History of the Marconi Company* (New York: St. Martin's Press, 1971), pp. 54–56 and 134; and *infra*, pp. 163–68.

9. Note, however, the words in which Lodge, in his autobiography, describes how he was first introduced to psychic research. While lecturing to a class in mechanics at University College, London, in the mid-1870's, he became acquainted with a student named Edmund Gurney who had been systematically collecting evidence on psychic phenomena in the belief that this evidence could be "rationalized and brought under a coherent scheme." Lodge continues: "This notion was, apparently, that a vivid impression made upon one person could reverberate and be received by sufficiently sensitive people at a distance." See Sir Oliver Lodge, *Past Years: An Autobiography* (London: Hodder and Stoughton, 1931), pp. 270–271. Compare Joseph McCabe, *The Religion of Sir Oliver Lodge* (London: Watts, 1914).

10. J. Murray Barbour, *Tuning and Temperament: A Historical Survey* (East Lansing: Michigan State College Press, 1953), pp. 15–21. For an introduction to the mathematics of musical scales and tuning, see David E. Penney, *Perspectives in Mathematics* (Menlo Park, Calif: W. A. Benjamin, 1972), pp. 52–81.

11. "Music is the pleasure the human soul experiences from counting without being aware that it is counting." Gottfried Liebniz, as quoted in Morris

Kline, *Mathematics in Western Culture* (New York: Oxford University Press, 1953), p. 287.

12. For Kepler's insistence that the distance of the planets from the sun or their periods of revolution *had to be* related by some principle of harmony, see Morris Kline, *Mathematics and the Physical World* (New York: Crowell, 1959), pp. 117–118. The alternative title of Thomas à Kempis's *Imitation of Christ* is *musica ecclesiastica*, which, as one intelligent editor has commented, refers not to the music of the prose but to the melody of the doctrine itself. See Irwin Edman, Ed., *The Consolation of Philosophy* (New York: Modern Library, 1943), p. vii.

13. Alexander Baumgarten, *Reflections on Poetry*, trans. by Karl Aschenbrenner and W. B. Holtke (Berkeley and Los Angeles: University of California Press, 1954), p. 5.

14. Jean Piaget, *The Language and Thought of the Child* (London: Routledge, 3rd ed., 1965), p. 243.

15. As Scott Buchanan has reminded us, "technology" itself is a Greek word, "not just stolen from the Greeks and reclaimed to fit a scientific novelty" but "a part of their discussion of the human arts." So it was with "syntony": not a straining after fancy words, but a natural extension of classical usage. See Scott Buchanan, "Technology as a System of Exploitation," in Melvin Kranzberg and William H. Davenport, Eds., *Technology and Culture: An Anthology* (New York: Schocken Books, 1972), pp. 132–143, at p. 133.

16. Relevant in this connection is Agnes Arber, "Analogy in the History of Science," in M. F. Ashley Montagu, Ed., *Studies and Essays in the History of Science and Learning* (New York: Henry Schuman, 1947), pp. 219-233.

17. For a series of perceptive insights into the significance of the Frankenstein myth, see Bruce Mazlish, "The Fourth Discontinuity," in Kranzberg and Davenport, *Technology and Culture*, pp. 226–227.

THREE

HERTZ

Introducing in 1893 the publication of his collected scientific papers, Heinrich Hertz wrote:

> Since the year 1861 science has been in possession of a theory which Maxwell constructed upon Faraday's views, and which we therefore call the Faraday-Maxwell theory. This theory affirms the possibility of the class of phenomena here discovered just as positively as the remaining electrical theories are compelled to deny it. From the outset Maxwell's theory excelled all others in elegance and in the abundance of the relations between the various phenomena which it included. The probability of this theory, and therefore the number of its adherents, increased from year to year. But as long as Maxwell's theory depended solely upon the

probability of its results, and not on the certainty of its hypotheses, it could not completely displace the theories which were opposed to it. The fundamental hypotheses of Maxwell's theory contradicted the usual views and did not rest upon the evidence of decisive experiments. In this connection we can best characterise the object and the result of our experiments by saying: The object of these experiments was to test the fundamental hypotheses of the Faraday-Maxwell theory, and the result of the experiments is to confirm the fundamental hypotheses of the theory.[1]

What was the nature of this test? Hertz was not the first person to generate radio waves.[2] Anyone who had ever generated a spark, even by such a trivial act as stroking a cat's fur on a dry day, had done that. Nor was he the first person to detect them. Joseph Henry, for one, had detected spark transmissions over a vertical distance of 30 feet as early as 1842; and there had been others.[3] But these events, however one may characterize them, were not tests of Maxwell's theory. Here is how Oliver Lodge saw the matter, addressing the Royal Institution in the year of Hertz's death:

Maxwell and his followers well knew that there would be such waves; they knew the rate at which they would go, they knew that they would go slower in glass and water than in air, they knew that they would curl around sharp edges, that they would be partly absorbed but mainly reflected by conductors, that if turned back upon themselves they would produce the phenomena of stationary waves, or interference, or nodes and loops; it was known how to calculate the length of such waves, and even how to produce them of any required or pre-determined wave length from 1000 miles to a foot. . . . All this was known, I say, known with varying degrees of confidence, but by some known with as great confidence as, perhaps even more confidence than, is legitimate before the actuality of experimental verification. Hertz supplied the verification.[4]

"The actuality of experimental verification"—the phrase accurately describes what Hertz accomplished. But what exactly was verified? Stated as simply as possible, three related hypotheses were involved: that electromagnetic fields could be generated by the acceleration of electrical currents, as for instance when a spark jumped across a gap; that these fields could be propagated through space; and that their velocity of propagation was finite—specifically, that it was identical to the speed of light. At stake were older and more familiar concepts: the idea of instantaneous action at a distance, which Newton had postulated for the force of gravity; and the corpuscular theory of light, which implied that light traveled in the form of material particles. Continental scientists in particular had long been reluctant to abandon these ideas. When Hertz undertook to test Maxwell's hypotheses, he was testing a theory that his academic superiors in Germany, and in particular his former teacher, von Helmholtz, would not have been sorry to see disproved. The velocity of propagation was the critical issue. Maxwell's theory implied that all forms of electromagnetic radiation, including light, traveled through space in the form of transverse waves, and that the wavefront moved with a finite velocity. Most physicists who accepted Maxwell's theory thought of these waves as taking place in an imponderable aether which existed in empty space as well as in all ponderable bodies that occupied space. For if there were no aether, and if one accepted the axiom that matter cannot act where it is not, how was the disturbance transmitted?[5] Some, including Hertz, had doubts about the concept of the aether, but that did not affect the crux of the matter. Were the effects of an electromagnetic disturbance, such as that created by an electric spark, propagated across space at a finite velocity, or were they felt at a distance instantaneously?

Lord Kelvin, when he was asked to write the preface to the English edition of Hertz's papers, suggested one significant change in the title. The German edition had been entitled *Untersuchungen uber die Ausbreitung der Elektrischen Kraft.* In the English edition this became *Electric Waves, being Researches on the Propagation of Electric Action with Finite Velocity Through Space.* The emphasis was on the last four words of the subtitle, taken from Hertz's original paper, "On the Finite Velocity of Propagation of Electromagnetic Actions," published in 1888. Hertz himself, relating the history of his experiments, was quite explicit that this was the critical issue. Von Helmholtz had stated three assumptions that required confirmation before Maxwell's theory, for all its plausibility and elegance, could be fully accepted. "I saw no way," wrote Hertz, "of testing separately the first and the second hypotheses for air; but both hypotheses would be proved simultaneously if one could succeed in demonstrating in air a finite rate of propagation and waves."[6]

How could one prove, experimentally, that electromagnetic radiation traveled across empty space at a finite velocity? The speed of light had been measured, with some precision. But to assert that this showed that all forms of electromagnetic radiation traveled at the same velocity would beg the question. It would assume precisely what needed to be demonstrated, namely that light was a form of electromagnetic radiation, differing from other forms only in its wavelength. To measure directly the velocity of the waves which, according to Maxwellian theory, were supposed to be propagated from an electric spark, or from any acceleration of electrical charge, required instruments that were not readily available. Specifically, it required a transmitter that could generate electrical oscillations at a known frequency, an antenna that could radiate those oscillations, a receiver that could detect them, and an experimental setup that could, as it were, "freeze" the waves emitted by the transmitter, so that their length could be measured. If one knew the length of the waves—say from one crest to the next—and the frequency with which the waves were generated, it was easy to find the velocity with which they traveled; it would be the product of the other two numbers.

Experimental practice in optics showed how radiated waves could be "frozen" and their wavelengths measured. If the radiated waves were made to impinge on a reflector, they could be directed backward along the path of transmission. The incident and the reflected waves would interfere with one another and produce a series of "standing waves" whose length—the distance between crest and crest—would be precisely half that of the original wave. Anyone who has stood by a swimming pool and watched waves bounce back from the end wall has seen much the same thing; so has anyone who has wondered at the spectrum of colors on the margin of a thin film of oil. Phenomena like these were the basis for the science of interferometry, originally evolved for research in the theory of light by Thomas Young, Fresnel, Fizeau, and others.[7] Now Hertz was to use interferometry at wavelengths much longer than light.[8]

* * *

The use of interferometry at radio frequencies was probably Hertz's most original contribution to experimental technique, but it was not his only one. Methods for the deliberate creation of electric discharges had been worked out some time before. Here the essential devices were the Leyden jar and the induction coil. The Leyden jar, a glass jar with metal foil lining its inner and outer surfaces, was a device for the storage of electrical charges, ancestor of present-day electrolytic capacitors. Invented, it is said, by Pieter van Musschenbroek in the early eighteenth century, it remained for many years the only available means of storing electrical energy until the invention of the voltaic cell in 1800. For Hertz's purposes the essential point was not so much the mere availability of the device but rather his knowledge of what happened when the electricity stored in a Leyden jar was suddenly released. This was easily accomplished, by connecting a wire to the outer surface and a second wire to the inner one. When the free ends of the two wires were brought together, a spark jumped from one to the other. This spark represented, of course, a sudden rush of electrical current—precisely the kind of acceleration of current flow that, according to Maxwell's equations, would generate electromagnetic radiation.

Furthermore, Hertz knew, the discharge from a Leyden jar was oscillatory. What appeared to the eye to be a single spark, when the two terminals of the jar were brought together, was in fact an oscillatory discharge, as each pole switched rapidly from positive to negative potential and back again. The oscillation was, to be sure, rapidly damped and therefore seemed like a single spark, but in fact there was a whole series of discharges. Various experimenters had proved the point—Joseph Henry in the United States, Sir William Thomson and Silvanus Thompson in England, von Helmholtz in Germany—and Bernhard Feddersen in 1857–1858 had even photographed the discharge to show that it was periodic and not merely intermittent.

The practical import of this was that Hertz knew that he had a method of creating electrical oscillations. Whether he could radiate them effectively, and detect them when radiated, was quite another matter. The inner and outer foil surfaces of the Leyden jar did not radiate well. The jar provided all the capacitance required, but there was little inductance and little radiating area. This, however, was easily remedied. Suppose one connected the inner and outer foil surfaces together by a loop of wire—a loop that was continuous except for a small spark gap at the midpoint. Now one had a circuit containing both inductance and capacitance—the inductance of the wires, and the capacitance of the Leyden jar. And it was a circuit that could radiate, for the surfaces of the Leyden jar had now been "opened out," as it were, by their connection to the two lengths of wire. At what frequency would it radiate? That could be determined by estimating the inductance and capacitance of the circuit and multiplying them together. In fact, if one wanted very rapid oscillations rather than slower ones, it might be possible to eliminate the Leyden jar completely, and rely on the distributed capacitance and inductance of the two wires.

It was through these small, incremental, one-step-at-a-time changes that Hertz's transmitter came into existence. The inner and outer foils of the Leyden jar became the two arms of a dipole antenna, separated by the spark gap. Since this antenna had both inductance and capacitance, it was a resonant circuit. Supplied with energy sufficient to sustain a stream of sparks across the gap, it would oscillate. And it would radiate these oscillations at a frequency, or set of frequencies, determined by its inductance and capacitance. What began as a Leyden jar, a simple means of storing electrical charges, ended up as a transmitter, a means of radiating electrical waves into space.[9]

Leyden jars could easily be charged by any of the classic "frictional electricity" machines, descendants of the prototypical piece of amber rubbed with fur.[10] But a better technique was available. This was the induction coil, generally known then as the Ruhmkorff coil after the French physicist who had perfected it. Joseph Henry, with his discovery of self-inductance, had laid the theoretical foundations for this device. Essentially it was a transformer, much like the ignition coil in automobiles today, having a primary winding with relatively few turns of wire and a secondary winding with many turns. Application of a pulsed voltage to the primary winding created a much higher pulsed voltage in the secondary—high enough, in some models, to create a spark 16 inches long with no more than a few voltaic cells and some form of mechanical interrupter or "make-and-break" in the primary circuit. Fizeau, the French physicist, had introduced in 1853 the practice of shunting a capacitance, such as a Leyden jar, across the secondary terminals to increase the strength of the discharge; most experimenters in the 1880's were using this modification.

So much for the transmitter and antenna: simple but effective. Hertz's receiving apparatus, likewise, could hardly have been more simple in design or structure, though this physical simplicity disguised conceptual breakthroughs. Shortly after his arrival at Karlsruhe in 1885 to take up his new appointment, Hertz had found, while rummaging around the equipment available for his laboratory, a pair of what were called Riess or Knochenhauer spirals. These were flat coils of wire or metal strip, with adjoining turns insulated from each other by sealing wax and with a spark gap connected across each coil. They were used in lectures to demonstrate inductance. What particularly attracted Hertz's attention was the fact that the discharge of even a small induction coil across one of the coils was enough to cause a perceptible spark across the other. One of the coils was, in fact, acting as a radiator, the other as a receiver. Hertz knew that the frequency of oscillation of such a circuit was determined by its capacitance and inductance, and he proceeded to vary these two elements in an attempt to maximize the observed effect—the spark in the receiving coil. In effect, he was tuning the coils to resonance. By reducing the capacity across the transmitting coil (Hertz removed the conventional Leyden jar completely), he was able to produce and sustain oscillations at much higher frequencies and also to elicit a stronger spark in the receiving coil. Since the two coils were of exactly the same physical dimensions, elimination of the Leyden jar conventionally shunted across the transmitting coil brought them into syntony at the same frequency.

All this may have begun as more or less idle toying with laboratory hardware but it ended up as systematic manipulation of variables known from theory to be relevant. The critical point seems to have been reached when Hertz removed the lumped capacitance of the Leyden jar across his radiating coil, thus raising the frequency of oscillation substantially. At these higher frequencies and shorter wavelengths the relatively small coils he was working with could be brought into resonance without difficulty. Once this was achieved Hertz was quick to notice the existence of peaks and nulls in the response of his receiving coil. At this point, as he later wrote, he became convinced that he had discovered "a clear and orderly phenomenon." He had produced sustained and stable oscillations at frequencies much higher than any previously at the disposal of physicists. He had transmitted them across short distances. And, perhaps most important, he had learned the importance of resonance. Response in the receiving circuit peaked sharply when transmitting and receiving coils were both tuned to the same frequency.[11]

* * *

The two foil surfaces of the Leyden jar, opened out and transformed, became Hertz's radiating dipole antenna (see Fig. 3.1). The Knochenhauer spiral, reduced to its bare essentials, became his receiver or resonator, in the basic experiments a simple loop of wire with its ends separated by a small gap across which the spark could jump (see Fig. 3.2). Both receiving and transmitting antennas had a fundamental resonant frequency, determined by their electrical parameters. Some of the transmitting antennas were equipped with spheres or plates of sheet metal at each end, which could be moved in and out along the arms of

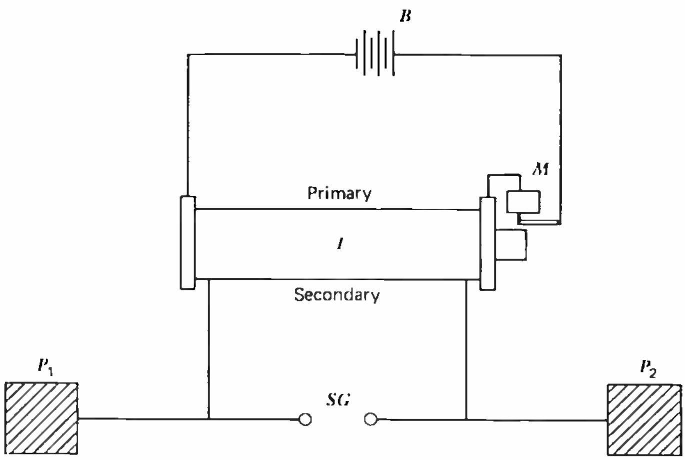


Figure 3.1 The Hertz oscillator. I: induction coil. B: battery. M: magnetic make-and-break. SG: spark gap. P_1, P_2: metal plates.

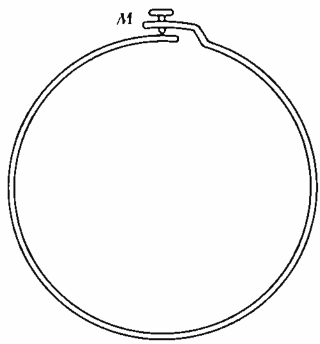


Figure 3.2 Hertz's circular resonator. M: micrometer screw for measuring spark strength.

the dipole, thus changing its electrical length and therefore its frequency. Similarly, more complex versions of the receiving loop were furnished with a micrometer to adjust and measure the spark gap, and small tabs of metal were soldered to the ends of the loop to bring it into exact resonance; sometimes, too, a lens was attached so that small sparks could be observed more easily. But in its basic and most effective version the apparatus was the essence of simplicity: a spark gap transmitter energized by an induction coil and radiating from a dipole antenna, and a loop receiving antenna, without rectifier or amplifier, with nothing but the presence of a spark to show when energy was being received. Resonance or syntony between the two circuits was what made experimentation possible.[12]

Implicit in the way in which Hertz set up his apparatus was an important decision, one that in retrospect appears to have been critical for success. This was the decision as to what frequencies to use. We must be clear as to why this was important. Hertz was proposing to do something that no one had done before: to measure directly the length of a radio wave. His experiments were to be conducted in a laboratory of limited size. Specifically, it was a lecture room 15 meters in length and 14 in breadth, with a row of iron pillars running down each side. After Hertz had set up his apparatus the available effective length was only 12 meters. The rows of iron pillars acted almost like reflectors as far as electromagnetic action was concerned, so that the effective space on each side of the measurement base line was only about 4 meters. Hertz had, therefore, an effective area of not more than 12 meters by 8 meters. The room also held an iron stove that came within 1.5 meters of the base line.[13] It was Hertz's intention to radiate waves from an antenna at one end of this room, reflect them from a large sheet of metal at the other, and measure the standing waves that resulted, using his loop receiving antenna to tell him, by the strength of its sparks, where the crests or nodes of the standing waves were. The distance between two successive nodes would be one half wavelength.

There had to be, in the distance between the radiating antenna and the reflecting sheet, at least two nodes; it would be preferable if there were several, for measuring the strength of sparks was a tricky business and it would be better if a number of measurements could be averaged. The frequency of oscillation, therefore, had to be such that at least one half wavelength, preferably more than one, could fit into the length of the laboratory. This meant—to slip into modern terminology—that Hertz had to work in the VHF (very high frequency) region of the radio-frequency spectrum if he was to take the measurements he wanted. It was essential that he work with short wavelengths. This was a matter that Hertz himself understood very clearly. For example, commenting on one puzzling result he obtained—a difference in the apparent velocity of propagation along wires and through space—he noted that the discrepancy tended to disappear as the frequency increased, and he made the correct inference. His measurements were not being taken in "free space" but in a physically confined area. The longer the wavelength, the more probable it was that measurements of the standing wave would be distorted by the physical limits of his laboratory. The room was "smaller" at long wavelengths than at short ones. "Decisive experiments for long waves," he commented, "seem to me to be still wanting. . . . A definite decision can only be arrived at under more favourable conditions. More favourable conditions here mean larger rooms. . . . I again emphasize the statement that care in making the observations cannot make up for want of space. If the long waves cannot develop, they clearly cannot be observed."[14]

Hertz, in short, established his experimental beachhead in a highly favorable region of the radiofrequency spectrum. It was this choice of electromagnetic *locale* that made his measurements possible. Lower frequencies would have implied longer wavelengths, and these he could not have detected within the spatial limits of his laboratory. It was precisely because they had been working with much slower oscillations and much longer wavelengths—because, in short, they had been exploring a different region of the spectrum—that other experimenters had failed. How large a jump in frequency Hertz made is not easy to determine, but it was substantial. Poincaré states that Feddersen, the German physicist, obtained oscillations with a period "of the order of 10^{-4} seconds" in the course of his research on discharges from Leyden jars, carried on during the 1860's, and these seem to have been regarded as very high frequency oscillations by the standards of the time.[15] A period of one ten-thousandth of a second would mean a wavelength of 30 kilometers (48.39 miles).[16] The kind of laboratory equipment Hertz was using and the kind of measurement he intended to make would have been inconceivable at such frequencies. In that region of the spectrum, with the equipment and laboratory space available, the standing waves that made measurement of wavelength possible would not have been detectable.

Precisely what frequencies Hertz did use is not, however, easy to determine. He used several transmitting dipoles of different sizes: a relatively large one saw most use in his *experimentum crucis* on the velocity of propagation, while a much smaller one was used for his later work on the reflection and refraction of radio waves. The dimensions of these transmitting dipoles give us one set of data from which the frequencies used can be inferred, though not without uncertainty. To each of these transmitting antennas there corresponded a receiving antenna: the sizes of these receiving "resonators" give us a second set of data. Thirdly, Hertz's papers give his own estimates of frequency and his measurements of the standing waves. These, however, present problems of their own.

Some commentators start with the transmitting dipoles, on the assumption that, from tip to tip, these must have been one half wavelength long in order to resonate at their fundamental frequency. The assumption is, in principle, correct, but what is required is a measurement of their electrical length, not their physical length. The two are the same only if the antenna is in free space, far from the earth, the supporting structure, and all other objects. This was clearly not the case in Hertz's laboratory. Furthermore, Hertz's large dipoles had spheres or sheets of metal attached to their arms—holdovers, probably, of the inner and outer foils of the Leyden jar that was their technological ancestor—and these bodies of metal added substantial extra capacitance (or "end loading") to the dipoles. It would have been simpler if Hertz had dispensed with them entirely, for they added nothing to the efficiency of the antenna. When in place, their effect on the antenna's resonant frequency is conjectural. Lastly, for reasons that will become clear in due course, Hertz's transmitters radiated not on one frequency only, but on a number of frequencies simultaneously. For these reasons, determinations of the frequencies used by Hertz that are based solely on the physical dimensions of his transmitting antennas must be treated with suspicion.

For what it is worth, however, Hertz's first or low frequency oscillator is described by Appleyard, who inspected and photographed the original apparatus at Karlsruhe, as being composed of two copper wires, each of them one meter in length and having at its far end a sphere of sheet zinc 30 centimeters in diameter. He gives its resonant frequency as "about a hundred million oscillations a second," which would mean a wavelength of 3 meters.[17] Poincaré, however, who is usually reliable in these matters, gives the overall length of this dipole as 1.5 meters between the two zinc spheres, and gives 50 MHz as the fundamental frequency, or a wavelength of 6 meters.[18] Hertz's own account supports neither of these authorities. His article, "On Very Rapid Electric Oscillations," published in 1887, describes a dipole 2.6 meters long with zinc spheres at each end 30 centimeters in diameter oscillating at 5.35 MHz, or a wavelength of 5.6 meters.[19] The classic article, "On the Finite Velocity of Propagation of Electromagnetic Actions," specifies a "primary conductor" 60 centimeters long with two square brass plates at each end 40 centimeters on the side, which may or may not be the same as the dipoles referred to by Appleyard and Poincaré.[20] This radiator was also used in his work on reflection and refraction, but then it was supplemented by a much smaller oscillator only 26 centimeters long from tip to tip, interrupted in the middle by a spark gap whose poles were spheres 2 centimeters in radius. If the gap were 4 centimeters, this would give an overall half wavelength of 30 centimeters, corresponding to a frequency of 500 MHz. This is confirmed by Poincaré.[21]

On the basis of this evidence we may tentatively conclude that Hertz was operating on frequencies between 50 and 500 MHz (wavelengths between 6 meters and 60 centimeters), in what we now call the VHF and UHF segments of the radiofrequency spectrum, regions which today are used for television broadcasting among other purposes. Descriptions of his receiving apparatus generally support this finding, though not unambiguously. For the experiments on velocity of propagation, for example, Hertz used as his "secondary conductor" or receiving antenna a circle of wire 35 centimeters in radius (a circumference of 2.199 meters).[22] If this were resonating as a quarter-wave antenna, the wavelength would be just under 8.8 meters and the frequency 35 MHz, or somewhat lower than we would surmise from the dimensions of the transmitting dipole. Hertz, in fact, explicitly states in the text of his paper that the quarter wavelength in these experiments was 2.4 meters; in a cautionary note added later, however, he warns that the wavelength may have been smaller.[23]

It is salutory to remember that, while it is easy for us today to state that a dipole of such-and-such a length would resonate at a fundamental frequency of so many megahertz, secure in the knowledge that the product of frequency and wavelength is a constant equal to the speed of light, no such glib solution was available to Hertz.[24] A measurement of the rate of propagation of electromagnetic waves, indeed a demonstration that there existed a finite rate of propagation, was the goal of his work, not something he could assume in his calculations. For him, measurement of frequency and measurement of wavelength had to be independent. Frequency measurement required calculation of the inductance and capacitance of his radiating dipole, followed by application of a formula derived by von Helmholtz from Maxwell's equations.[25] On this basis, Hertz estimated the wavelength used in his first experiments on very high frequencies as 5.31 meters, or a frequency of 56.497 MHz. For his experiments on the velocity of propagation, using a somewhat smaller dipole, he worked with a frequency which he initially estimated to be 35.7 MHz but later amended to 50 MHz.[26] Finding that these frequencies gave negative results during his work on reflection and refraction (the wavelengths being too large for his reflectors and prisms), he shifted to a wavelength of about 30 centimeters, this time estimating his approximate wavelength from the overall dimensions of his short dipole.[27]

Hertz was not the first explorer to be somewhat unsure of his location, nor should we exaggerate the importance of the matter. It is clear that he was using electrical oscillations very much higher in frequency than any that had been used in controlled laboratory experimentation before. And it is also clear that this choice of frequency was critical to the success of his measurements. These are the important issues, not the precise wavelength used. For many of his experimental objectives an order of magnitude was enough and a precise knowledge of frequency was not necessary. Many approximations were involved, not only in estimating the inductance and capacitance of his radiating antenna but also in measuring the length of the standing waves with his simple loop receiver. What impresses one about Hertz's work, in fact, is precisely its experimental audacity. The very formula he used to estimate the self-inductance of his radiating system contained an undetermined constant.[28] In the critical experiment on the velocity of propagation he was able to detect, in the length of his laboratory, only two nodes in the standing waves. And some of his results—for example, that the rate of propagation along wire was lower than the rate through air—were decidedly puzzling. But these were, in a sense, side issues, anomalies and unresolved difficulties that would be tidied up later. The major goal was, after all, not to measure the velocity of propagation, nor to show that it was equal to the speed of light, but to prove that it was finite.

And this he did, in one of the most remarkable series of experiments in the history of physics. By the end of 1887 he had published his paper, "On Very Rapid Electric Oscillations," describing his experimental apparatus, the theoretical basis for estimating periodicity, and the sharply peaked frequency response curves that resulted from tuning transmitter and receiver to resonance. At this stage he had not yet hit on the idea of using interferometry to measure wavelength directly. This came in 1888, and it is a matter of some interest that it began with a disappointment. It occurred to Hertz that waves could be propagated both along wires and through air. In straight stretched wires very distinct standing waves were produced, and direct measurement of wavelength and therefore of velocity was easy. Now, if waves from the same oscillator were propagated simultaneously through the air and also, over a somewhat longer path, through the wire, the two waves would interfere with one another and their phases could be compared. If both were prop-

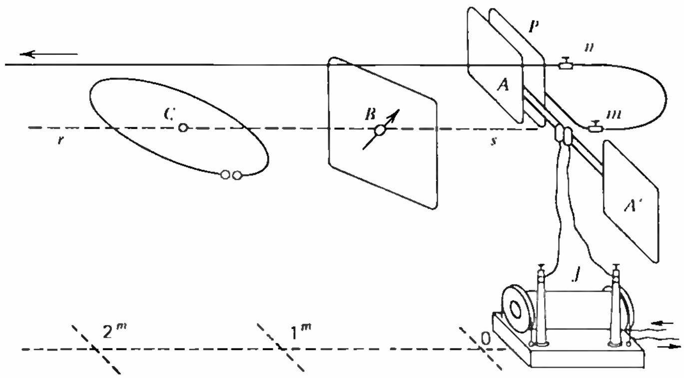


Figure 3.3 Comparison of velocity of propagation in space and along wires. From Hertz, Electric Waves.

agated with the same finite velocity, the phase difference would remain constant (see Fig. 3.3). Here, then, was (in Hertz's words) "a simple qualitative experiment which . . . could be finished within an hour" and would lead directly to the goal. The experiment was tried; it failed. The phase of the interference was different at different distances and—a finding that must have been even more disturbing—"the alternation was such as would correspond to an infinite rate of propagation through air."[29]

Careful rechecking of these results proved this initial interpretation too extreme. The rate of propagation through air and along wires did seem to differ, but not by an amount that could be reconciled with an infinite rate of propagation through air. Encouraged, Hertz simplified his apparatus and his method. He dispensed completely with the long wires. Instead he measured the wavelength in air directly, bouncing the radiation from his oscillator off a reflecting sheet and measuring the length of the standing waves that resulted (see Fig. 3.4). With a radiated frequency initially estimated at 35.7 MHz, he detected two nodes or crests in the standing waves and found the distance between them (one half wavelength) to be 4.8 meters. A wavelength of 9.6 meters and a frequency of 35.7 MHz meant a velocity of 3.42

x 10^8 meters per second. The velocity of light was 3.00 x 10^8 meters per second. The orders of magnitude were the same; the discrepancy was easily explained by experimental error. Most important, the major hypothesis had been proved: the velocity of propagation of electromagnetic disturbances through space was finite.[30]

* * *

The importance of these results was immediately recognized by the scientific community on their publication in 1888, though the conclusions to be drawn from them remained, particularly in Germany, matters of acute controversy. Hertz's later work on the reflection and refraction of radio waves at very high frequencies demonstrated in a striking way that such waves behaved just as did visible light, after due allowance was made for the difference in wavelengths. From the theoretician's point of view this was merely further confirmation of what he had already proved: the validity of Maxwell's equations. But recognition was not confined to scientists. Hertz's experiments were treated as news and received wide publicity in newspapers, mag-

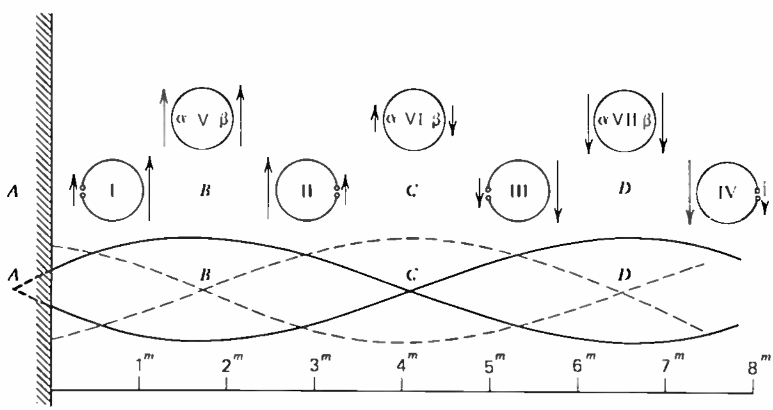


Figure 3.4 Measurement of standing waves with a loop resonator. From Hertz, Electric Waves.

azines, and public lectures. It was after reading of them in a magazine, for example, that the young Marconi turned his mind toward "wireless." The context and significance of Hertz's work were clearly not the same for the general public as for scientists. Hertz achieved fame among nonscientists not because he had experimentally verified Maxwell's equations but because he had shown how to communicate across distances without wires. To this aspect of the matter Hertz himself had paid no attention whatsoever; his scientific papers give no hint that he was even aware of it. As a scientist, his concern had been with a problem posed by other scientists, a problem that could be *seen* as a problem only by those conversant with physical theory. The creation of a new technology of communication had been no part of his plan.[31] Yet this was to be the outcome. From the work on velocity of propagation was to grow the spark era of long-distance wireless. And when, in the 1920's, the radio industry rediscovered the short waves, it was to Hertz's work on reflecting antennas that it turned. We are today, indeed, only beginning effective commercial occupation of the UHF segment of the radiofrequency spectrum, the area Hertz was exploring in 1888.

Our interest here is not in the history of electromagnetic theory and we have no reinterpretation to offer of Hertz's contribution to that history. Our concern is with the technology of radio communication, and in particular with the technology of syntony. This implies, among other things, concern with matters that were of only indirect and instrumental importance to Hertz and that have little relation to his reputation and fame as a scientist. If, therefore, we concentrate our attention on issues that caused Hertz difficulty, problems that made his work less tidy, elegant, and complete than he would have liked, it is not in order to derogate his accomplishments but because these issues were to prove important, technologically, for later workers.

There were three such areas of difficulty, and each of them can tell us something of the problems that the emerging technology of radio communications was to face. The first relates to the measurement of the frequency at which Hertz's transmitters radiated. We have already seen that this depended upon a calculation of the inductance and capacitance of his dipole antennas, and we have noticed that the typical Hertzian dipole carried spheres or sheets of metal at the ends of its two arms. Hertz was not wholly at ease with the formula used for calculating antenna inductance, but he satisfied himself that the range of possible error would not seriously affect his conclusions. On the problem of antenna capacitance he showed no such uneasiness, expressing confidence that "the capacity of the ends of the conductor consisted mainly of the spheres attached to them" and that it would be an acceptable approximation to take as his measure of capacitance the radius of either of these spheres.[32] This was a serious error, first pointed out by Henri Poincaré in 1891 and admitted with disarming candor by Hertz in the introduction to the edition of his collected works published in the following year. The total difference of potential between the two spheres was indeed what Hertz had estimated it to be; but this meant that the difference of potential between each sphere and the surrounding space was precisely half that amount, and it was this latter figure that the formula called for. Consequently, in the formula for resonance used by Hertz, the capacitance was overestimated by a factor of 2 and the resonant frequency underestimated by the square root of 2. This "fatal mistake," as Hertz called it, was made not only in the 1887 paper "On Very Rapid Electric Oscillations" but also in some subsequent ones. Its practical import was that all of Hertz's calculations of the frequency at which his transmitting dipoles were radiating were in error.

In view of this fact it is hardly surprising that none of Hertz's calculations of the velocity of propagation worked out at precisely the speed of light. It made no difference, of course, if all one wanted to prove was that the velocity of propagation was less than infinite. But the error did introduce into Hertz's experimental reports a need for approximations and a reliance on "orders of magnitude" that cannot have been to his taste.

Hertz's dipoles were tiny antennas in comparison with the massive arrays that were to follow. Yet even on that manageable laboratory scale and under carefully controlled conditions, a scientist known then and respected ever since for his experimental technique could seriously misjudge the frequency at which his antenna was radiating. The error was a harbinger of what was to follow, when experimenters less scrupulous than Hertz, with ambitions more pragmatic, and working under economic pressures from which he was free, were to pump electromagnetic energy into space at frequencies which they could barely guess at. What was required was not a scientific breakthrough: there was nothing wrong with Hertz's formula. The problem was a practical one: the development of a technology which would make it possible for the frequency of a radiated signal to be measured and maintained.

The second set of problems grew out of the immediate environment in which Hertz's experiments were carried out: the lecture room, 15 meters long by 14 wide and 6 high, which served as his laboratory. For delicate radiofrequency measurements these conditions were far from ideal, a fact of which Hertz was no doubt aware. What he did not fully appreciate was the extent to which the particular characteristics of this environment affected his measurements, at the frequencies which he intended to use. The rows of iron pillars which ran down each side of the room were each, during the experiments on velocity, less than one half wavelength away from the base line along which measurements were taken. The iron stove that provided welcome warmth in a Karlsruhe winter was only 150 centimeters away. What effect did the proximity of these large masses of metal have on his measurements? In retrospect one can see that the effect was considerable, and that it varied with frequency. Only in this way can one account for the large discrepancies Hertz found between velocity of propagation along a wire and through space—discrepancies which were contrary to theoretical expectations and which at one time induced him to suspend his experiments completely. The fact that the discrepancies tended to grow smaller as the frequency was increased might have given a clue, for at higher frequencies the pillars and the stove were farther away, if we take one wavelength as our unit of distance. But, at the time, Hertz found his anomalous results discouraging and inexplicable. As he later wrote, "It is not easy to point to any disturbing cause which could imitate in such a deceptive way the effect of a difference in velocity . . . While performing the experiments, I never in the least suspected that they might be affected by the neighboring walls."[33] As for the stove, which must certainly have distorted the standing wave measurements, "I did not think at the time that at this distance it could interfere at all."[34]

The purpose of these comments is not to find fault with Hertz's experimental technique. With more than eight decades of scientific and technological advance between his time and ours, that would be a childish game indeed. The point is, rather, that the conditions which affect the propagation of radio waves, and in particular their absorption and reflection, were not self-evident; even less was it self-evident that these propagation conditions varied with frequency. By the end of his tragically brief scientific career Hertz had learned this lesson. What one could do with radio waves, the distance over which one could transmit them, the extent to which they could be concentrated in narrow beams, the degree to which they were absorbed by the earth, the sea, or (as was later learned) by the ionosphere—these depended very directly on their frequency. The capabilities of the radiofrequency spectrum as a resource depended on where you were in it. What was true in one "place" was not true in another.

All these matters, which are now the commonplace lore of the radio engineer and technician, were lessons that had to be painfully learned. Essential to this learning process was a technology that permitted users of the spectrum to find and keep the location best suited to their purposes. The advance of the extensive frontier depended upon step-by-step acquisition of knowledge of what each newly opened-up segment of spectrum was good for. It is ironic that the segments which Hertz explored and whose capabilities he had begun to learn were soon to be completely abandoned, as the pioneers of commercial exploitation, seeing no immediate prospect of profit in the very high frequencies, pushed their frontier into the very long wavelengths in the belief that lower frequencies meant greater distance.

The third area of difficulty exposed by Hertz's experiments concerned the impossibility, in principle as well as in practice, of tuning a spark transmitter to a single frequency. Maxwell's theory predicted that electromagnetic waves would be emitted whenever electrical charges were accelerated. The source of such waves therefore had to be an oscillating current. In Hertz's system oscillating currents were generated by the string of sparks, excited by the induction coil, which jumped across the gap in the radiating antenna. These oscillations were not continuous; they were transient. The electromagnetic radiation so generated was not a continuous wave but a string of pulses, each pulse consisting of a highly damped sine wave. In a pulse of this type the amplitude of each successive swing is less than the amplitude of the preceding one by a constant ratio. The amplitude of the waves in each pulse, in other words, decreases logarithmically (see Fig. 3.5). The waves that Hertz detected and sought to measure were composed of a series of pulses of this type.[35] He had no means of generating continuous sine waves, nor was technology to provide such a device, at radio frequencies, for many years to come.

If we think of how the Hertzian apparatus developed, we recall that the starting point was the discovery that the spark discharge from a Leyden jar capacitor was oscillatory. It was analogous to a tuning fork which, struck once, emits sound waves whose amplitude decreases as the vibrations of the fork grow weaker, losing energy by friction and radiation into the air. Adding an induction coil, Hertz found a way to generate sparks in a continuous stream, but each spark was still a damped pulse, losing energy by ohmic resistance and by radiation into the elec-

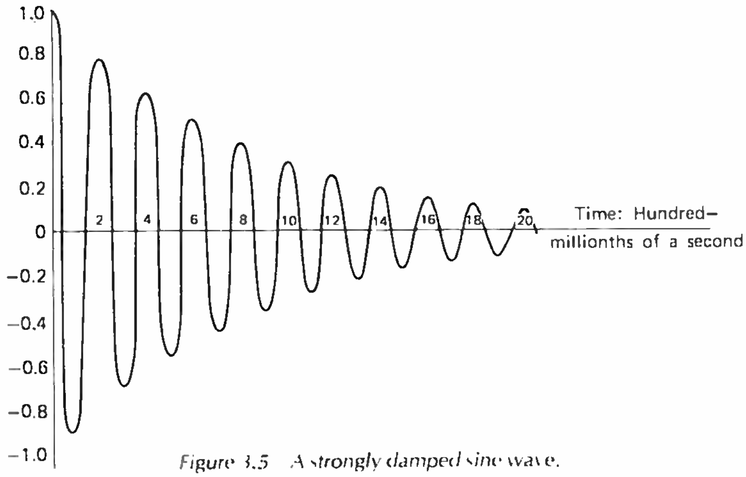


Figure 3.5 A strongly damped sine wave.

tromagnetic field. Adding a dipole antenna, he had a way of radiating these pulse trains into space. But what was radiated was not a continuous electromagnetic wave, oscillating at a single frequency. None of Hertz's transmitters, in fact, radiated a single-frequency wave only. When he measured the length of standing waves, he was measuring the strongest signal present, not the only one.[36]

Hertz first encountered this problem by accident. Experimenting in 1888 with the propagation of electric waves along wires (he was still trying to explain the supposedly lower propagation rate), he observed the "singular phenomenon" that distinct nodes could be observed on the wires even when the receiving loops in use were much too small to resonate at the frequency he thought he was using. In fact, half wavelengths as short as 24 centimeters could be observed, despite the fact that he was using as his radiator one of the larger dipoles with a nominal wavelength of several meters.[37] Hertz noted the phenomenon, arrived at what was in fact the correct explanation, and moved on to other matters. A couple of years later, however, two Swiss physicists, Sarasin and de la Rive, published a paper giving their own interpretation of what they called "multiple resonance." According to their theory, the radiation emitted by a Hertzian oscillator was not a single wave but a highly complex one, resulting from the superposition of an infinity of simple vibrations. It was, in short, like white light, which has a continuous spectrum, rather than monochromatic light with its single frequency.

Hertz's explanation differed. Multiple resonance, he said, arose from the fact that each pulse was highly damped. Multiple resonance indeed there was; the spectrum of radiation emitted, however, was not continuous. Rather, it was made up of a large number of distinct and related frequencies—not like white light, but more analogous to a piano string which, when struck, emits not just a fundamental note but also harmonics.[38] This explanation was confirmed and elaborated by the mathematician Henri Poincaré. A train of damped sine wave pulses was a complex vibration, the components of which were pure sine waves, and the frequencies of the component waves were related to each other as the terms in a Fourier series.[39] Thus if the fundamental frequency were a wave of the form $a_1 \sin t$, the complete spectrum of radiation emitted would be described by the series:

$$a_1 \sin t + a_2 \sin 2t + a_3 \sin 3t + \ldots$$

The more strongly damped the original pulse, the larger would be the coefficients a_2, a_3, and so on, in the infinite series and the stronger would be the harmonics of the fundamental frequency. At one extreme, if the degree of damping (the logarithmic decrement, or "log dec," as later radio operators were to call it) were zero, the wave emitted would be a continuous wave at only one frequency. At the other extreme, if the degree of damping were high, a large number of harmonically related frequencies would be radiated in addition to the fundamental, and these harmonics would extend, with diminishing amplitude, into regions of the spectrum far removed from the fundamental frequency.

What this meant in practice was that it was inherently impossible to tune a spark transmitter to a single frequency. The damping factor could be reduced by various means and (as was later learned) resonant circuits could be inserted between spark and antenna to reduce the strength of the harmonics. But, by its very nature, a spark transmitter could not emit a single frequency only. A true continuous wave transmitter, with no modulation, would radiate only on one frequency: it would, in principle, have only one "place" on the spectrum and would occupy no "room" (bandwidth) at that place. This a spark transmitter could never do: it was inherently a dirty radiator, polluting the spectrum with radiation that was unnecessary, wasteful of energy, and damaging to the interests of other users.[40]

Hertz, who had no interest in commercial exploitation of his discoveries, pursued these implications of spark transmission no further. Characteristically, he considered the Sarasin–de la Rive theory more "interesting" than his own, even though he disagreed with it. Poincaré and Vreeland, however, writing in 1904, after powerful spark transmission had become standard radio practice, saw the implications clearly: "With strongly damped oscillations, strong resonance and sharp tuning are both impossible, and we miss the two great advantages of a good syntonic system: strong response to a feeble signal, and the ability to distinguish one set of signals from another."[41]

In time the technological innovations necessary for the generation of a true continuous wave signal would be developed: the arc transmitter, invented by the Dane, Valdemar Poulsen, in 1903 and in commercial use by 1911; the high frequency alternator, conceived by Reginald Fessenden and perfected by E. F. W. Alexanderson in 1915; and the triode vacuum tube oscillator, discovered almost simultaneously by Armstrong, Meissner, Franklin, and Round in 1913–1914. These devices, no less than spark transmitters, would depend on resonant circuits to sustain oscillation, to determine their output frequencies, and to filter out undesired harmonics. Unlike the spark transmitter, however, they had the massive advantage that they generated not a train of damped pulses, but a true continuous wave, one which could be interrupted, if desired, for telegraphy, but which could also be modulated by audio frequencies for the transmission of voice or music, something no spark transmitter could do.[42] With the advent of these devices the first age of radio technology drew to a close.

That age had dawned in Heinrich Hertz's laboratory. Hertz's equipment furnished the basic technology that made radiocommunication possible. The string of sparks spitting across the terminals of an induction coil; the simple dipole antenna, radiating radio waves into space; and the resonant loop of wire that served both as receiving antenna and as detector. In terms of Hertz's immediate objectives as an experimenter in pure science, this apparatus served its intended purpose: it made possible the first and crucial empirical verification of Maxwell's model of the electromagnetic field. As a communications technology, however, it had serious limitations. Some of these could be remedied without undue difficulty: the need for greater sensitivity in the detecting apparatus, for example, was clear, and a solution was quickly found. But others were inherent in the nature of the spark discharge itself. The sharply damped pulses of the spark train—the "whip-crack effect" of which Marconi was later to speak with such pride—were prodigally wasteful of spectrum space and flagrant sources of interference when more than a single station was trying to transmit. Technical refinements in spark gaps might minimize these inherent drawbacks of spark transmission, but they could never eliminate them completely. As occupancy of the spectrum became more dense—a problem accentuated by the rush to the longer wavelengths where bandwidths were larger and channels fewer—spectrum conservation and elimination of harmonic radiation became more urgent. "Places" in the spectrum had to be allocated; trespass, in the form of interference, had to be prevented; transmitter and receiver had to be able to find each other with confidence and certainty. No complete solution to these problems was possible as long as spark remained the only feasible technology for generating radio waves; even the advent of continuous wave generators merely reduced their urgency, without eliminating it. Partial solutions, adequate to permit the commercial development of the new technology, depended on the idea of syntony and on the development and adoption of syntonic circuits, sharply resonant at specific frequencies. Lacking these, commercial exploitation of the radiofrequency spectrum was impossible.

Notes

1. Heinrich Hertz, *Electric Waves, being Researches on the Propagation of Electric Action with Finite Velocity Through Space*, authorized English translation by D. E. Jones (London: Macmillan, 1893), pp. 19–20.

2. The word "radio" is an anachronism in this context. It was not, of course, used by Hertz himself, nor by Lodge or Marconi. Apparently it first crept into American usage in the decade before World War I; in the United Kingdom it never has supplanted the older "wireless." My excuse for using the word is that I wish to refer to electromagnetic waves at frequencies lower than infrared light, and there is no other single term to describe this segment of the spectrum. If, for example, I were to say that Hertz was the first to measure the velocity of propagation of electromagnetic radiation, I would be wrong, because the speed of light had been measured before Hertz's time. I hope, therefore, that readers will be indulgent if I use the terms "radio" and "radiofrequency spectrum" in a somewhat unhistorical context. We must remember that the continuity of the electromagnetic spectrum, from radio waves up through visible light, which we now take for granted, was a matter of theoretical speculation only in Hertz's time.

3. See, for example, Elihu Thomson, "Curious Effects of Hertzian Waves," *The Electrical Engineer*, Vol. 18, No. 322 (4 July 1894), and Charles Susskind, "Observations of Electromagnetic Wave Radiation before Hertz," *Isis*, Vol. 55, No. 32–42 (March 1964).

4. Oliver Lodge, "The Work of Hertz—I," *The Electrical Engineer*, Vol. 18, No. 322 (4 July 1894), pp. 6–7.

5. See Hertz, *Electric Waves*, p. 21; Loyd Swenson, *The Ethereal Aether* (Austin: University of Texas Press, 1972), p. 27; and Edmund T. Whittaker, *A History of the Theories of Aether and Electricity*: 2 vols. (New York: Harper & Row, 1960).

6. Hertz, *Electric Waves*, p. 7. Note that "air" and "empty space" were used interchangeably in this context.

7. On interferometry, see Max Born and Emil Wolf, *Principles of Optical Electromagnetic Theory of Propagation, Interference and Diffraction of Light* (London: Pergamon, 1959), and S. Tolansky, *An Introduction to Interferometry* (London: Longmans, Green, 1955). There were also acoustical interferometers, measuring wavelength by beat notes. For the central role of interferometry in "aether drift" experiments, see Swenson, *The Ethereal Aether*, *passim*.

8. Radio waves range no higher than 3×10^{12} Hz in frequency, corresponding to wavelengths of about 0.01 centimeters. Light waves, in contrast, have frequencies of the order of 10^{14} Hz, corresponding to a wavelength of approximately 10^{-4} centimeters or between 0.4 and 0.8 microns. For the reader without scientific training, the best simple explanation of Hertz's use of interferometry is in W. H. Eccles, *Wireless* (London: Butterworth, 1933), pp. 22–41. Chapters 22, 26, and 27 in A. B. Arons, *Development of Concepts of Physics* (Reading, Mass.: Addison-Wesley, 1965) are also very helpful.

9. A dipole or Hertz antenna, as its name suggests, is an antenna composed of two elements placed end to end with a gap in the middle. An example familiar to most readers will be the "rabbit ear" antennas sometimes found on the rear of home television receivers. As a transmitting antenna the dipole is normally current-fed at the center. Its resonant frequency is determined primarily by its length. Note that, in contrast to a Marconi antenna, neither side of a Hertzian dipole is grounded.

10. Bern Dibner, *Early Electrical Machines* (Norwalk, Conn.: Burndy Library, 1957).

11. See the frequency response diagrams in Hertz, *Electric Waves*, p. 45. In the section on "Resonance Phenomena" on these pages the English translation reads: "it seemed to me that the existence of such oscillations might be proved by showing, if possible, symphonic [sic] relations between the mutually reacting circuits" (p. 42). This is clearly a mistranslation or typographical error. The German original has "resonanzartige Beziehungen," which would be correctly rendered as "syntonic relations."

12. Some popular illustrations of Hertz's apparatus show the receiving loop as held in the middle by the observer's hand. This would have thrown it out of resonance and eliminated the spark. Hertz's resonators were held by wooden clamps and stands, and care must have been taken to eliminate hand capacitance. Compare, for example, W. R. Maclaurin, *Invention and Innovation in the Radio Industry* (New York: Macmillan, 1949), p. 17, with the pictures of Hertz's original apparatus in Rollo Appleyard, *Pioneers of Electrical Communication* (London: Macmillan, 1930), pp. 119–120.

13. Hertz, *Electric Waves*, pp. 108–109, 125, and 272, fn. 12. For the significance of the stove, see p. 69 below.

14. Hertz, *Electric Waves*, p. 14.

15. H. Poincaré and Frederick K. Vreeland, *Maxwell's Theory and Wireless Telegraphy* (New York: McGraw, 1904), p. 23.

16. Readers who find it awkward to think in metric terms may bear in mind that the speed of light is very close to 186,282 miles per second. An oscillation with a period of one thousandth of a second would therefore form a wave with a length of a little over 186 miles from crest to crest.

17. Appleyard, *Pioneers*, p. 119.

18. Poincaré and Vreeland, *Maxwell's Theory*, p. 37.

19. Hertz, *Electric Waves*, p. 42.

20. Hertz, *Electric Waves*, pp. 108,126.

21. Hertz, *Electric Waves*, p. 173; Poincaré and Vreeland, *Maxwell's Theory*, p. 37.

22. Hertz, *Electric Waves*, p. 126.

23. Interpretation of Hertz's results at this point is not made easier by an error in the English translation, which states (*Electric Waves*, p. 133, top) that 4.5 meters was the wavelength for this apparatus. The German edition gives 4.5 meters as the *half* wavelength. See Heinrich Hertz, *Untersuchungen uber die Ausbreitung der Elektrischen Kraft* (Leipzig: Barth, 1894), p. 142.

24. And it is also salutory to be reminded by a present-day expert in antenna theory that "there is still no complete analysis of the electrical properties of Hertz's dipole or, in fact, of many other geometrically comparable end-loaded radiators." See R. W. P. King, "The Linear Antenna—Eighty Years of Progress," *Proceedings of the Institute of Electrical and Electronic Engineers*, Vol. 155, No. 1 (January 1967), p. 2.

25. Hertz's version of the formula was $T = \pi\sqrt{(PC)}/A$ where T is the half-period of oscillation, P a measure of inductance in centimeters, C a measure of capacitance in centimeters, and A the speed of light in centimeters per second. A better-known version of the formula for the frequency of a damped resonant circuit had been presented by William Thompson in 1853 and later confirmed by B. W. Feddersen's photographs of oscillatory discharges. The modern form is the familiar $F_r = 1/2\pi\sqrt{(LC)}$ where F_r is the resonant frequency in Hertz, L the inductance in henries, and C the capacitance in farads. See Hertz, *Electric Waves*, p. 51.

26. Hertz, *Electric Waves*, p. 109 and p. 272, fn.13.

27. Hertz, *Electric Waves*, p. 173.

28. Hertz, *Electric Waves*, p. 51.

29. Hertz, *Electric Waves*, p. 8.

30. Interpretation of Hertz's experimental results is not easy, and the unwary can be deceived by the scientific usage of the time, which often referred

both wavelength and period to half of a full sine wave, not the full alternation as we do today. Measurement of standing waves in the critical experiment on velocity of propagation gave a half wavelength of 4.8 meters, but Hertz stated that by an "indirect method" he had obtained 4.5 meters as the half wavelength for the same apparatus. Later he suggested that the position of one of the nodes might have been altered by "general conditions of the surrounding space" and in consequence "we might obtain much smaller values for the wavelength." The half-period of the waves was calculated at "1.4 hundred-millionths" of a second originally; after his calculations were corrected by Pioncaré, however, Hertz amended this to "almost exactly one hundred-millionth of a second." If this is correct, the half wavelength should have been 3 meters, not 4.5. I can find no warrant in Hertz's papers for the flat statement by Arons that Hertz found the velocity of propagation to be exactly 3×10^8 meters per second. See Hertz, *Electric Waves*, pp. 133, 272, fn. 13, and 274, fn. 19; compare Arons, *Concepts of Physics*, pp. 659–660.

31. One piece of evidence which has been repeatedly quoted to prove this point does not in fact prove it at all. This is a letter sent by Hertz in December 1889 to one Herr Huber, a civil engineer in Munich, who had apparently written to inquire about the possibility of using electromagnetic waves to transmit audible sounds. Hertz's reply begins by clarifying an elementary point—the rays and waves used in his experiments are both electric and magnetic, not one or the other—and then goes on to say: "However, the vibrations of a 'Transformator' or telegraph are far too slow." An audible tone of 1000 cycles per second would have a wavelength of 300 kilometers, and it would require "a mirror as large as a continent" to reflect such a wave. What Hertz is saying here is that waves of *audible* frequencies could not be radiated without gigantic antenna systems (and, he might have added, very low efficiency). His remarks were not directed to the question of using high frequency radiation for signaling but to the feasibility of radiating audio frequencies directly (i.e., without using them to modulate a radiofrequency wave, as we do today). The letter is reproduced by Rollo Appleyard, *Pioneers of Electrical Communication*, pp. 338–339, who seems to have originated the confusion. It was then reprinted, in one of his few careless moments, by Rupert Maclaurin, *Invention and Innovation in the Radio Industry*, pp. 15–16, and the error repeated.

32. Hertz, *Electric Waves*, p. 50.

33. Hertz, *Electric Waves*, p. 9.

34. Hertz, *Electric Waves*, p. 272, fn. 12.

35. In a typical Hertzian oscillator, the oscillations decayed to one-tenth of the initial amplitude in about nine swings.

36. This is why the dimensions of the receiving loop may be a better guide to the frequencies measured than the dimensions of the transmitting dipole.

37. Hertz, *Electric Waves*, pp. 15–16.

38. An alternative analogy would be with the earth itself, which rings like a bell after the pulse caused by a large earthquake. The earth's fundamental period of oscillation is 53 minutes per cycle. See C. L. Strong, "The Amateur Scientist," *Scientific American*, Vol. 229, No. 5 (November 1973), pp. 124–129.

39. Any periodic function is a sum of simple sine functions of the form $a \sin bt$, and the frequencies of these sine functions are all integral multiples of the lowest frequency. This "law" we originally owe to G. S. Ohm, who based it on physical studies. The mathematical statement we owe to Ohm's approximate contemporary, Joseph Fourier, who announced his theorem in 1807. See Morris Kline, *Mathematics and the Physical World* (New York: Crowell, 1959), pp. 306–308.

40. Anyone who has experienced interference to radio or television reception from automobile ignition systems should appreciate the point without further elaboration.

41. Poincaré and Vreeland, *Maxwell's Theory*, p. 205.

42. Reginald Fessenden in December 1900 transmitted speech over a distance of one mile, using a spark transmitter. Results were, however, unsatisfactory, and for his later work on speech transmission Fessenden relied on high frequency alternators. See Maclaurin, *Invention and Innovation in the Radio Industry*, pp. 59 and 64.

FOUR

LODGE

In August 1888, Oliver Lodge, professor of experimental physics at University College, Liverpool, published in the *Philosophical Magazine* an article on lightning conductors, in the course of which he reported the results of his experiments with discharges from Leyden jars and the standing waves that such discharges could set up in long wires. While the article was in page proof, he added to it a short postscript:

> I have seen in the current July number of Wiedemann's *Annalen* an article by Dr. Hertz, wherein he establishes the existence and measures the length of aether waves excited by coil discharges; converting them into stationary waves, not by reflexion of pulses

> transmitted along a wire and reflected at its free end, as I have done, but by reflexion of waves in free space at the surface of a conducting wall. . . . The whole subject of electrical radiation seems working itself out splendidly.[1]

Rumors of Hertz's breakthrough had reached Lodge the previous spring. If he believed them, he did nothing to rush his own results into print. Nor, when confirmation appeared in the pages of the *Annalen*, did he hurry to his Liverpool laboratory to check the results and revise the formal paper he was readying for delivery the following autumn. On the contrary, he continued placidly on his vacation in the Tyrol, accompanied by his literary colleague, Andrew Bradley, to admire the scenery and chat about Hegel. And from that retreat the postscript was written.

This was entirely characteristic of Lodge, a man of amiable and noncompulsive temperament. Nevertheless, it is hard to believe that news of Hertz's success was received without some personal chagrin. Surely not far from his mind, amid the magnificent scenery and learned discussions of the dialectic, was a dawning realization that he had suffered a major defeat. A rival scientist, a younger man, and one with no special advantages of location or equipment, had anticipated him in reaching the goal to which, as a young student, he had decided to devote his scientific career: the production and detection of Maxwell's electric waves.[2] And Hertz had done it in an infinitely more dramatic and imaginative way than had ever occurred to Lodge, measuring the waves not as they traveled along wires but in free space.

It had been a very close thing, as major breakthroughs often are in science. Lodge formally presented the results of his own research to Section A of the British Association for the Advancement of Science in the fall of 1888. It became clear, to those present at the meetings who understood what was going on, that Lodge's experimental verification of Maxwell's equations had been carried out at almost exactly the same time as Hertz's and was, in principle, no less conclusive. The experimental techniques, to be sure, were more conventional, and in particular Lodge's use of long wires as wave guides, but the theoretical implications were identical. To a mathematician like Oliver Heaviside, indeed, the two sets of experiments amounted to the same thing.[3]

Lodge, to his credit, never expressed resentment at this turn of events. Quite the contrary. Supported by his friend, G. F. FitzGerald, the mathematical physicist, he took the lead in publicizing Hertz's work in England, in translating his articles, and in paying public respect to the brilliance of his research. Years later, with radio a technological reality and a flourishing industry, he was to state flatly, "Maxwell and Hertz are the essential founders of the whole system of wireless."[4] And when in 1888 he wrote cheerfully that electromagnetic theory was "working out splendidly," he meant exactly that.

Hertz died of blood poisoning in 1894, at the tragically early age of 36, his research on radio waves at ultra high frequencies still in progress. Lodge, in contrast, lived to the age of 89, and when he died in 1940 his scientific accomplishments were far behind him. By that time he was one of the grand old men of British science, knighted by King Edward in 1902, and full of all the honors that the scientific societies of his native country could bestow. For some years, it is true, he had been regarded with a kind of patronizing condescension by the younger members of the scientific establishment. It was easy, in that positivistic age, to make fun of his profound interest in psychic research, and his unshakable belief in the physical reality of the aether had by then come to seem dated, out of fashion, an uncomfortable reminder of past simplicities. But in 1888 there was no condescension. Lodge was recognized then as one of the most promising of England's younger physicists: no great mathematician, to be sure—he had come to the subject too late for that and had never gone through the Cambridge discipline—but a gifted and imaginative experimenter in the Faraday-Maxwell tradition. He had, in particular, a sure grasp of Maxwell's electromagnetic theory of light.[5]

As was true of Hertz, Lodge approached this task through observation of electric discharges—by studying sparks, to put the matter simply. In 1881 he had been appointed to the chair of experimental physics at the newly established University College of Liverpool. No laboratory was available there, and no equipment. The only building the new College possessed was an abandoned insane asylum. But Lodge was determined to have a teaching laboratory, one in which students would learn to perform experiments for themselves. There were few models for such an unconventional establishment in Britain, apart from Sir William Thomson's laboratory in Glasgow. In Germany there were several. So he set off for the Continent, to tour universities and purchase a few items of equipment. The pilgrimage proved profitable. He met a number of leading German scientists for the first time, including an obscure but pleasant young man named Heinrich Hertz who was working as demonstrator for the great von Helmholtz at Berlin. And he managed to procure some first-class equipment, notably at Chemnitz, where a certain Professor Weinhold presided over "a sort of technical institute" and, to supplement his income, manufactured laboratory fittings and apparatus. Weinhold's equipment, to Lodge's critical eye, seemed excellent: "not made to sell, but to use." He bought a number of Leyden jars which he considered "exceptionally well made, with no chains, wooden lids, and other gimcrack arrangements, such as were usual in this country."[6] Confident that he now had at least the rudiments of a physical laboratory, Lodge returned to Liverpool, there to conduct his classes with blackboard and chalk until his assigned space—the former padded cell of the asylum—was ready for use.

These details are anecdotal, and probably no great weight of interpretation should be placed on them. But for a man like Lodge, an experimenter to his fingertips, who could in one breath apologize for his mathematics and boast of having "well-controlled muscles, deft fingers, and good eyesight," the laboratory devices he had to work with were no trifling matter. They were the tools of his trade, the means by which he translated conjectural idea into testable reality, as important to him as plane and chisel to a carpenter or micrometer and depth gauge to a machinist. The costs of the continental expedition were met out of his own pocket. Nothing would have been easier than to stay in England, help his wife find a suitable house in Liverpool, and content himself with the equipment that British instrument-makers—no mean craftsmen themselves—could provide. But it was to Germany he had to go, not only to examine laboratories but also to secure scientific hardware of the quality he demanded. The Chemnitz Leyden jars, in particular, were to play a critical role in the work that lay ahead. We shall meet them again and again, until they become familiar friends. The meeting with Hertz, on the other hand, had no enduring consequences. Lodge tells us that they were kindred spirits and that Hertz "did the honours" in Berlin because von Helmholtz was too busy with his lectures to spend much time with the young English visitor. There is no record of later correspondence or exchange of ideas until after Hertz's results were published.

* * *

Lodge's research usually had a practical side to it. His first project at Liverpool, for example, was on dust. One aspect of this was pure science: the analysis of the apparent black smoke that arose from a hot body placed under the beam of an electric light. Lord Rayleigh had recently done some work on this; it was known that what seemed to be black smoke was really a dust-free space. The problem was to explain how this dust-free space was created and maintained—a problem of some theoretical complexity, as it turned out, but also one with obvious practical relevance.[7] His next project showed the same characteristics. The secondary or lead-acid storage battery, one that could be recharged many times, had only recently been discovered. Little was known about optimal charging and discharging rates. Lodge became adviser to the Electric Power Storage Company and, working closely with the firm's engineers, was able to eliminate much of the uncertainty and lay down standards of technical practice. The practical implications were publicized by a series of articles in the *Engineer;* the scientific issues involved led Lodge to the preparation of a lengthy paper on "The Seat of the E.M.F. in a Voltaic Cell" which he presented to the British Association, meeting in Montreal, in 1884. The paper excited a vigorous exchange of views with such eminent physicists as Willard Gibbs and Sir William Thomson, and added considerably to Lodge's scientific reputation in the process.[8]

Clearly Lodge was a man who liked to keep a foot in both the engineering and the scientific camps. The projects that typically interested him were those that stemmed from a technological difficulty of some sort, but that also, for their solution, demanded an extension or elaboration of physical theory. It involved, therefore, no departure from his normal pattern of research when, early in 1885, he was asked by the secretary of the Society of Arts to present a series of public lectures on lightning conductors. The subject was of more than academic importance. Lightning rods had long been familiar as a means of protecting buildings. With the spread of the telephone and telegraph networks they had acquired a new importance, for a lightning strike could cause serious interruption to a wired communications system. During the 1870's lightning rods had proliferated all over Britain like dandelions after a rainstorm, as the Post Office authorities and cable companies grappled with the problem. Their success had been limited; it was common knowledge that none of the ordinary devices gave more than partial protection, despite much experimentation with ingenious designs and carefully prepared ground connections.

The problem lay not in the design and installation of the rods, but in an inadequate understanding of the lightning discharge itself and therefore of the electric currents such a discharge could generate. Engineers, for the most part, clung to the opinion that the best procedure was to place pointed rods as high in the air as possible, and to connect them to ground by a thick wire or cable having low resistance to direct current. If this did not give protection, nothing would. Many such rods had been installed. In Britain alone the Post Office was reckoned to have set at least half a million of them in place, and there was a certain "civil service interest" in maintaining that the money had not been wasted. There remained, however, too many documented cases where a direct lightning strike had ignored completely the low resistance path provided for it by a considerate bureaucracy, to follow instead a radically different and unpredictable path to ground, often with disastrous consequences. There was, therefore, a puzzle of practical importance, one for which the "practical men," led by William Preece, chief electrician of the Post Office, and the laboratory scientists, led by Lodge, found themselves advocating quite different solutions.[9]

For the scientist, particularly one interested in electromagnetic theory, the problem had interesting implications. It had long been known that a lightning flash was a release of energy on a massive scale. When in 1752 Benjamin Franklin flew his kite in a Philadelphia storm and, at considerable risk to his own life, charged a Leyden jar from the kite string, he was confirming the theory that the energy involved was electrical, of the same type as it was then possible to produce from friction machines. A lightning flash, in brief, was a spark, an electrical discharge, analogous to the spark that could be drawn from a charged Leyden jar, only on a much larger scale.

Lodge knew, as did most of his scientific contemporaries, that a spark discharge was oscillatory—not a single flash, as might appear to the naked eye, but a series of rapidly alternating flashes between one pole and the other, like the vibrations of a length of spring steel strip that is flexed and suddenly released.[10] The same thing happened, he thought, when lightning flashed between two clouds or between a cloud and the earth. Sometimes, to be sure, the electrical charges might leak away gradually, as a Leyden jar might gradually lose its charge.

But at other times the electrical discharge would be violent, releasing massive amounts of energy in a very short space of time. The problem was to understand why, when such a very large and sudden discharge took place, the resulting currents did not always follow the path of least resistance.[11]

The answer lay in the concept of inductive reactance: the opposition offered by an electric circuit, not to direct current, but to changes in current. Originally introduced by Sir William Thomson in 1853 under the name of "electro-dynamic capacity," the concept of inductance was still not widely understood even in the scientific fraternity. Engineers accustomed to working with direct currents tended to dismiss it as a chimera. As late as 1888 William Preece spoke of it publicly as a "bugaboo." Oliver Heaviside had done much to stress its practical importance, notably in connection with problems experienced in very long telegraph lines such as the Atlantic cables, but it was still a strange and dubious notion to most people who thought of electricity in terms of flows of direct current and saw no reason why rapidly alternating currents should behave any differently. Not until Sir William Thomson, in 1889, lent his authoritative support to Heaviside's findings did it win some acceptance by the engineering profession.[12]

Lodge, explaining its relevance to lightning conductors, put it this way: the old idea was that there was, in a thundercloud, a certain amount of electricity stored like a fluid. To protect property and lives from lightning, a "drainpipe" had to be provided that would make it possible for the electric charge to flow gently and easily from cloud to earth. This meant, in practice, low resistance wires, cables, or metal rods, and a good connection to ground. The problem was visualized, in other words, as a matter of direct current flows. And there, said Lodge, lay the error. Conventional lightning conductors were probably better than none at all, but they could never give complete safety. People had to get used to the idea that they were "living always between the coatings of a large condenser or Leyden jar." Ordinarily the distances were too great and the differences in electric potential too small for a discharge to take place. But every so often, for atmospheric reasons, potential differences increased, the insulating dielectric broke down, and a lightning strike took place. What happened then was no gentle flow of electricity but a violent pulse, a sudden acceleration of current. Circuits that had low resistance to direct current might well, if they contained kinks or loops or sudden bends, strongly oppose such sudden pulses, because of their self-inductance. In such a case (as Lodge expressed the matter), "Ohm's law, and conductivity are simply not in it"; what was needed was low reactance, not low resistance. Small wonder that the lightning often jumped from the low resistance cables prepared for it to the alternative low impedance paths presented by walls, chimneys, columns of warm air, or people. The central point was that accelerating currents behaved differently from currents that flowed uniformly at a constant speed. It was all implicit in Clerk Maxwell, albeit somewhat obscure.[13]

Also implicit in Maxwell's equations was a proposition that had a host of scientific and technological implications: the theorem that inductive reactance was a function of frequency, or the *rate* at which currents changed direction. A coil of wire, for example, would oppose a high frequency alternating current more strongly than a low frequency one. The converse was true of its twin brother, capacitive reactance: the impedance offered to an alternating current by a capacitor such as a Leyden jar was an inverse function of frequency. A capacitor that would oppose a low frequency current strongly would offer but slight impedance to a high frequency one. Lodge did not spell out these functional relationships in connection with lightning conductors, since they were of only indirect relevance, but they were to become central to his thinking before long. Inherent in these concepts and their relationships to frequency was the whole theory of syntony. Appropriate choice and arrangement of inductive and capacitive reactance in a circuit could render it resonant at certain frequencies but not at others.

The research project on lightning gave Lodge considerable satisfaction. It provoked a grand argument, in which he revelled. It had practical importance for everyday life, which he considered not at all undesirable in a scientific inquiry. And it had some intriguing and unexpected quirks to it—results that he had not expected and that called for some hard thinking. Investigation of these anomalies was to take him away from lightning conductors and back to his original quarry: Maxwell's electromagnetic waves.

* * *

For experiments in connection with lightning Lodge had used his beloved Chemnitz Leyden jars. Charged by a friction machine or by an induction coil, these could produce, on a miniature scale, the "sudden rush of a considerable quantity of electricity" which was a lightning flash. There was nothing novel there; even the oscillatory nature of the spark was "old hat" by then. But Lodge wanted to show that the presence of inductance in the circuit could make the discharge from the jar follow a high resistance path even when a low resistance path was available. This led him into unexplored territory.

Figure 4.1 is the layout of Lodge's initial experiment on what he called the "alternative path." It would be possible, of course, to translate these laboratory arrangements into the symbols of modern schematic diagrams, with idealized capacitors in place of Lodge's Leyden jars and idealized inductances instead of his loops of wire. There is some advantage, however, in seeing them as Lodge himself saw them and as he presented them to his readers.

The knobs marked A are the terminals of a Voss machine, a device for generating electricity by friction. These are connected to the inner terminals of two Leyden jars; the outer surfaces of

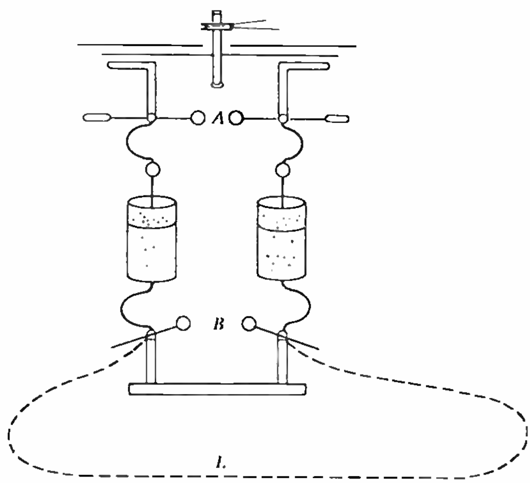


Figure 4.1 The "alternative path" experiment. A: primary spark gap. B: secondary spark gap. L: wire loop.

the jars are connected to an adjustable spark gap, B. The two terminals of the spark gap are connected together by a long loop of wire, L, having very low resistance to direct current. The electricity stored in the Leyden jar capacitors, in other words, was given a choice of two paths to follow: one, between the knobs of the spark gap, where the direct current resistance of the air gap would approach infinity; the other, around the loop of wire, the resistance of which was very small.

If all that was involved was resistance to direct current, no sparks would ever appear at B. Why should the current ever jump a high resistance gap when it had an alternative low resistance route available to it? Lodge was easily able to show, however, that vigorous discharges could be produced at the spark gap even when the direct current resistance of the wire loop was as little as 25 thousandths of an ohm.

The experiment was the essence of simplicity. The quantitative results were illustrative only. Nothing had been "proved" except that, where sudden pulses of current were involved, resistance might be of little moment, and inductance, or what Lodge liked to call "inertia," everything. It made an easy and effective demonstration for a public lecture. There was, however, an interesting anomaly to be noted: the discharges at the spark gap, B, were significantly stronger than any sparks that could be produced at A, the output terminals of the Voss machine. Where was the energy coming from that produced these more powerful discharges?

To explore this question Lodge made an important modification in his circuit. Up to this point he had been working with a continuous loop—actually about 40 feet of wire suspended around his laboratory on silk ribbons. Now he cut this loop at its far end and inserted the spark gap at that point. And he began changing the length of the loop and the size of the Leyden jars in the hope of finding the combination that would create the longest and most powerful spark (see Fig. 4.2). What he was after was, in his own words, a way of producing the strongest possible "recoil kick," as the wave front produced by the discharging Leyden jars traveled along the wires and was reflected from the far end.

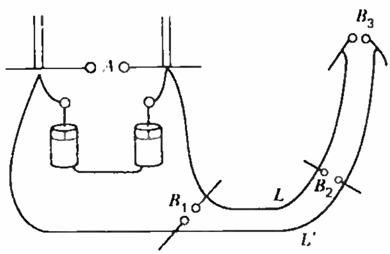
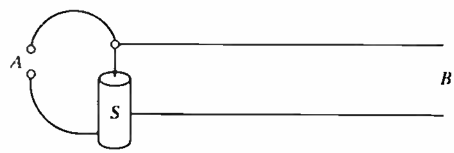


Figure 4.2 The "recoil kick" experiment: I. A: primary spark gap. B_1, B_2, B_3: positions of secondary spark gap. L, L^1 wire loop.

Lodge had a gift for exposition. His scientific lectures to the lay public were popular, partly because of the clarity and drama of his demonstrations, but partly also because, for such an audience, he avoided formulas, mathematical symbols, and unfamiliar concepts. We must remember, however, that Lodge was also a first-class physicist, one of the few men in Britain at that time who understood Maxwellian electromagnetic theory and worked comfortably with it. Lodge's talk about electrical inertia and momentum and the "recoil kick" should not deceive us into underestimating the sophistication of his analysis. And the pragmatic, "Why don't we try this now?" way in which he described his popular demonstrations should not mislead us into supposing that he did not have a shrewd idea of what he was up to. He was too good a scientist to publish his laboratory notebook.

What Lodge was doing in these "alternative path" and "recoil kick" experiments was designing resonant circuits that could be excited into oscillation by spark discharges. In the popular lectures on lightning, simple physical analogies were used: "The electricity in the long wires is surging to and fro, like water in a bath when it has been tilted."[14] But to a more serious audience the calculations of inductance, capacitance, and period of oscillation were made explicit. In Lodge's last publication before he learned what Hertz had been up to, the experimental arrangement is shown in its bare essentials and the theoretical issue stated succinctly (see Fig. 4.3).



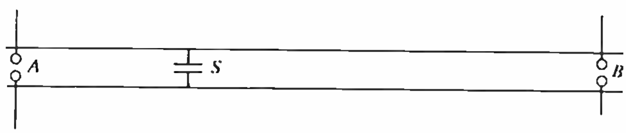


Figure 4.3 The "recoil kick" experiment: II. A: primary spark gap. B: secondary spark gap. S: capacitor.

> The jar discharges at A in the ordinary way, and simultaneously a longer spark is observed to pass at B at the far end of the two long leads . . . . The theory of the effect seems to be that oscillations occur in the A circuit . . . with a period $T = 2\pi\sqrt{(LS)}$, where L is the inductance of the A circuit and S is the capacity of the jar.[15]

Optimal conditions for producing the "recoil kick" obtained when the *A* and *B* circuits were resonant at the same frequency:

> The best effect should be observed when each wire is half a wave-length, or some multiple of half a wave-length, long. The natural period of oscillation in the wires will then agree with the oscillation-period of the discharging circuit, and the two will vibrate in unison, like a string or column of air resounding to a reed.[16]

Therein lay the explanation for the longer spark in the loop circuit: the reinforcement of response that came from resonance. Later Lodge was to call this "syntony," but in 1888 he preferred the more familiar word.[17] Push a child's swing gently and, if your pushes are timed just right, you can soon get it swinging to the sky. Blow gently across the aperture of a flute and, if you know what you are doing, you can produce a note of surprising purity. So it was with these simple electronic circuits. Inductance and capacitance—the two components of what we now call reactance—in the loop circuit had to be so adjusted that it was resonant at the frequency of the spark circuit. Small pulses of current from the primary circuit would then result in large voltage and current swings in the secondary or loop circuit. Since the ends of the loop circuit were "open," no current could flow there: it was therefore a current minimum and a voltage maximum. Hence the powerful sparks when the primary circuit was pulsed into oscillation at its fundamental frequency, the frequency at which each of the two wires in the secondary circuit was precisely one half wavelength long.

Lodge's quantitative results make it clear that he was able to achieve a clear resonance peak even in these early experiments. There were many resistive losses in the circuit, the exciting pulse was highly damped, and the resonance point as a result not very pronounced but it was there. The wires he was using in this first experiment were each 95 feet in length, indicating a resonant frequency of about 5 MHz. Later he was able to produce waves only a few centimeters long and, abandoning the use of a spark gap as detector, demonstrated the presence of standing waves by carrying out the experiment in a darkened room, so that the voltage peaks showed as a visible glow or brush discharge. But he never, until after learning of Hertz's work, gave up the use of wires: the creative leap of imagination which led Hertz to "open up" his Leyden jar and turn it into a dipole radiating in free space did not occur to Lodge. Given another few months, it might have; that we shall never know. The fact of the matter is that, until he learned from Hertz, Lodge remained wedded to wires—to what Heaviside, with his usual insight, referred to as miniature telegraph circuits.[18]

Lodge knew very well, however, that he had succeeded in generating, detecting, and measuring electromagnetic waves. The oscillations, he declared, "disturb the surrounding medium and send out radiations, of the precise nature of light." The wires served merely as guides; the waves were not *in* the wires but in "the surrounding medium," or what he called the aether. Measure the wavelength, multiply by the calculated frequency of oscillation, and the result should be the speed of light, as Maxwell's equations predicted. And so indeed it turned out. Unlike Hertz, Lodge could detect no anomalies that might suggest a lower velocity of propagation along wires: "within the limits of accuracy of that kind of observation" his results agreed with the known velocity of light in air.[19] A mathematician like his friend FitzGerald might worry lest an experiment that relied on wires might leave some skeptics unconvinced, but Lodge had no such concern. He soon had the authority of Heaviside to back him up. The proof was conclusive, said Heaviside; Hertz's results were "better and more striking evidence," perhaps, but Lodge's were simpler and easier to demonstrate. Generous in recognizing

Hertz's brilliance Lodge might be, but he did not underestimate his own work. He had done what he had set out to do: provide experimental evidence for Maxwell's theory.

* * *

Not many years before, he had been told on high authority that what he proposed to do was impossible. In 1878 Lodge had visited Dublin for the British Association meetings. There he made the acquaintance of G. F. FitzGerald, professor of experimental philosophy at Trinity College and a highly respected theoretician.[20] FitzGerald was busy preparing a paper on Maxwellian theory which he showed to Lodge. At that time (so Lodge tells us) it bore the title, "On the Impossibility of Originating Wave Disturbances in the Ether by Means of Electric Forces."[21] Before it appeared in print the prefix "im" was prudently dropped, perhaps at Lodge's suggestion, but the thrust of FitzGerald's argument remained decidedly negative. Disturbances such as light that were propagated through space in time could not be connected with electric currents; more probably they had to do with "the relations of matter and ether." Maxwell's "displacement currents," he suggested, "however these may be produced by any system of fixed or movable conductors charged in any way, and discharging themselves amongst one another . . . will never be so distributed as to originate wave disturbances propagated through space outside the system."[22] A second paper, following within a year, reinforced this disconcerting verdict by elaborate mathematical analysis. The production of waves by electrostatic or electromagnetic systems, he now asserted, was ruled out by Clerk Maxwell's own assumptions. The Maxwellian model, said FitzGerald, "excluded the possibility of wave production."[23]

Coming from a man who was one of the leading physical theorists of the English-speaking world, already the equal of von Helmholtz in reputation, these strongly worded conclusions could not be shrugged off.[24] Lodge in particular, who considered FitzGerald his "special friend" and who had no very high opinion of his own mathematical abilities, found FitzGerald's verdict profoundly discouraging.[25] The check, however, proved only a temporary one, for within two years the Irish theorist was faced with the embarrassing necessity of reversing his earlier views. He had, it appears, come across an alternative solution to Maxwell's equations, formulated some years before by Lord Rayleigh in the course of his work on acoustical theory, and the practical implications were quite different. Specifically, Lord Rayleigh's solution implied that "a simply periodic current would originate wave disturbance such as light." There was nothing necessarily inconsistent between Maxwell's equations and the idea that energy could be radiated by the propagation of electromagnetic waves through space. The alleged impossibility was possible after all, and FitzGerald was reduced to apologizing in print for venturing to investigate matters when he was ignorant of what had already been done.[26]

Did FitzGerald's misleading analysis delay Lodge in his search for methods of generating and detecting Maxwellian waves? It is not inconceivable. A month or two would have made the difference. Without this discouragement from a friend whose analytical ability he trusted, Lodge might well have pushed ahead with greater determination and, as a result, made the experimental breakthrough before Hertz did. On the other hand, Lodge had a lifelong habit of "hesitating" when on the verge of something important, a trait of which he was well aware and which he had good reason to regret.[27] We shall be on firmer ground if we remind ourselves that in 1882, when FitzGerald reversed his earlier uncompromising stand, Lodge was only in the second year of his new appointment at Liverpool, with his "bare-bones" laboratory barely established. He had had little opportunity to undertake serious experimental work.

By 1882 the question was at least clearly defined: which formulation of Maxwell's equations was to be preferred, FitzGerald's or Lord Rayleigh's? On purely mathematical grounds there was no way to decide; both were partial, not general solutions. Choice between them had to be based on physical experiment. FitzGerald suggested the form such experiments might take, but he remained skeptical of their feasibility. It would be necessary, he said, to generate alternating currents of very high frequency and to have some means of detecting them at a distance. By "discharging condensers through circuits of small resistance," it might be possible to generate such currents, but for the problem of detection he saw no solution.[28]

This is where matters stood on the theoretical front when Lodge began his experiments on lightning conductors. There was now reason to believe that experimental production of electromagnetic waves might not be impossible after all, and that if such waves could be generated, detected, and measured, a theoretical issue of first-class importance could be resolved. Lodge had already made some tentative moves in that direction—FitzGerald in fact referred to them in his 1882 paper—but they had been fruitless. What Lodge had been attempting then was the direct production of light at the specific frequencies, and those frequencies only, necessary for them to be detected by the human retina. He called this an attempt to synthesize light, and he failed. It is clear from his comments, however, that he knew where the problem lay. Radiation of electromagnetic waves at visible frequencies required some means of creating oscillatory currents "in a body as small as a molecule," and this he could not do. It would be necessary in future to generate alternating currents at frequencies that were experimentally feasible with the apparatus available: Leyden jar capacitors and inductances made of wire. This meant waves much longer than those ultra short waves called light that the human eye could detect, but waves that would travel at the same rate as light and would in fact be identical with light except in the one matter of wavelength. And this in turn meant that the central difficulty became the development of some kind of detector that would, at those frequencies, take the place of the human eye.

* * *

When he began his lightning conductor experiments in 1887, therefore, Lodge had two scientific issues on his mind. The relationship between them was not clear to him at the time; he saw it only in retrospect. One was a practical matter. He had accepted an invitation to lecture to the lay public on a matter of general interest about which a great deal of nonsense was being talked: lightning conductors. For this purpose he needed a few simple experiments that would make clear to observers who were not technically trained the nature of a spark discharge and the difference between resistance and reactance. The other issue was more purely intellectual: the realization, fed by FitzGerald's *volte-face*, that physical theory had reached a critical juncture, one in which there was a clear need for a decisive experiment that would determine whether electromagnetic waves could in fact be generated, radiated, and detected.

We have Lodge's word for it that the connection between the lightning conductor experiments and the problem of detecting electromagnetic waves was not seen beforehand. "I happened to be experimenting on lightning conductors," he told the British Association in 1888, "and somewhat to my surprise . . . I hit on an arrangement which, without any thought or scheming at all, gave me evidence of the very waves I had been thinking so much about, and enabled me to measure their lengths, though not in a previously planned-out way."[29] This was the "recoil kick" experiment we have already discussed. Resonance in the secondary circuit was indicated by a voltage peak at the far or open end, as shown by spark strength. Measurement of the length of the transmission wires gave a measurement of wavelength which, multiplied by the known frequency of oscillation of the primary circuit, gave a rate of propagation equal to the speed of light. Lodge was using interferometry, just as Hertz did, but in a less deliberate and self-conscious manner. Hertz hung the large sheet of metal at the end of his laboratory with the definite intention of reflecting waves from it; Lodge in contrast found the presence of the "recoil kick" an anomaly to be explained.

There were other differences, perhaps more significant, between the ways in which Hertz and Lodge saw the problem facing them. For Hertz the crucial issue was measurement of the rate of propagation. Lacking this, the theory of direct action at a distance, instead of wave propagation in finite time, could still be seriously entertained. The devising of a detector and a radiating antenna gave him no particular difficulties. If we may judge from his published articles, the move from a closed Leyden jar to an opened-out dipole, from transmission along wires to transmission through space, was not a matter which he personally thought of as involving special insight or inspiration. The idea of using a half-wave resonant ring antenna as a detector was not one to which he gave particular emphasis. For Lodge, on the other hand, the rate of propagation was something as good as known, unless the whole of Maxwellian theory were to be discarded, and that was not a prospect he could seriously entertain. If electromagnetic waves existed at all, their rate of propagation would have to be the same as that of light because, in all respects except frequency, they *were* light, and only the limited bandwidth of response of the human retina—barely one octave in the spectrum—prevented humans from seeing them. He took the Maxwellian paradigm for granted: it was the conceptual frame of reference in which he worked, the context within which experiments were devised and executed, not a matter that could seriously be called into question.[30]

Neither Hertz nor Lodge, it must be emphasized, had at this time (1887–1888) any conception whatsoever of using their apparatus or their conclusions for signaling. Improving the technology of communication was no part of their objectives. In the case of Hertz we can be sure of this because he never mentioned the possibility in his published work, and no one who knew him has suggested that the idea was ever seriously entertained. Elaborate explanations are not required. Hertz was a young scientist intent on investigating the electromagnetic theory of light. Any involvement in commercial exploitation of his work, if it had been suggested, would have seemed to him a distraction and a nuisance. Immeasurably more significant among his personal concerns, during the few years of life left to him, were the continuation of his research on the ultra high frequencies and the difficulties he was meeting in winning acceptance for his theories among his senior German colleagues.

Concerning Lodge we must be more careful, because Lodge did, before many years had passed, become deeply involved in the technological development and commercial use of "wireless," to the extent of taking out patents and becoming partner in a syndicate to manufacture and sell transmitting and receiving equipment of his design. With Lodge, then, questions of timing and circumstance, of the context in which the shift from pure science to technology and commerce took place, become central as they never did with Hertz.

Wireless telegraphy certainly had no place in Lodge's thinking at the time of the "alternative path" and "recoil kick" experiments of 1887–1888. As far as those experiments were concerned, Lodge was clearly a product of the age of cable telegraphy. He believed that wire wave-guides were necessary if electromagnetic waves were to travel. As he said himself, "I had not the faith to look for them in space without guidance."[31] For all his immersion in Maxwellian theory, he did not find the shift to thinking in terms of radiation in free space easy. There is not one of his experiments on electrical discharges before 1888 that does not clearly show its ancestry in the technology of wired telegraph circuits. Even his use of reflectometry was thought of as a way of avoiding the use of "excessive lengths of wire."[32] His original intention had been to inject a pulse into one end of a long telegraph circuit and measure how long it took to emerge from the other end—an idea that might well have been suggested by Heaviside's insistence that signals in the Atlantic cable, because of inductance, traveled as waves, not according to the heat-flow model suggested by Thomson.[33] As a matter of theory, Lodge insisted that waves traveled in the aether, not in the conductor; in practice, the thought of dispensing with the conductor did not occur to him.

Everything changed after he heard what Hertz had done. He saw immediately the essential difference. Hertz's circuits were designed to radiate; as for his own, it was almost as if they had been specifically designed not to. The faith that, by his own confession, he had earlier lacked was now unnecessary; he knew what to do. Wires were dispensed with and, in an intensive series of experiments and demonstrations, Hertz's tests were repeated, his results confirmed, and his apparatus improved. The imaginative breakthrough which, up to that point, Lodge had been unable to make for himself he now exploited with vigor and enthusiam. He knew very clearly that a decisive corner had been turned—decisive, that is, for physical theory, if not yet for practical use. "We have now," he wrote in the closing months of 1888, "a real undulatory theory of light, no longer based on analogy with sound, and its inception and early development are among the most tremendous of the many achievements of the latter half of the nineteenth century. In 1865 Maxwell stated his theory of light. Before the close of 1888 it is utterly and completely verified. Its full development is only a question of time, and labour, and skill. The whole domain of Optics is now annexed to Electricity, which has thus become an imperial science."[34] Lecturing to the Royal Society in March of the following year, he closed with a simile that may well have struck his listeners as extreme but that eloquently expressed his own sentiments: "One feels like a boy who has been long strumming on the silent keyboard of a deserted organ, into the chest of which an unseen power begins to blow a vivifying breath. Astonished, he now finds that the touch of a finger elicits a responsive note, and he hesitates, half delighted, half affrighted, lest he be deafened by the chords which it would seem he can now summon forth almost at will."[35]

For all the imagery, there was still no mention of signaling, of communicating through space without wires. This further breakthrough seems to have waited on two events: first, the invention of a more efficient detector; and second, a remarkable public lecture that explicitly envisaged the use of Hertzian waves for communication.

* * *

Lodge had been clear, both in his work and in his comments on Hertz, that the principal difficulty lay in devising an efficient detector. In his "recoil kick" experiments he had, like Hertz, relied on visual estimates of spark strength to tell him when the twin wires were in resonance. Later, in a darkened room, he had used the visible glow or corona at voltage peaks. Both of these had been means of "finessing" the lack of a detector. Describing Hertz's experiments in 1888, it was not the dipole radiator he emphasized but the circular resonator: "The step in advance which has enabled Dr. Hertz to do easily that which others have long wished to do, has been the invention of a suitable receiver."[36]

The irony of this is that Lodge had already discovered, during his lightning rod experiments, the principle that was later to furnish the first practical, commercially usable detector of radio waves: the coherer. He noted it at the time but passed over it, seeing no immediate use or interest: "I came across a curious effect . . . whereby a couple of little knobs in ordinary light contact, not sufficient to transmit a current, became cohered or united at their junction whenever even a minute spark passed, and thus enabled the passage of a current from a weak E.M.F. [voltage] through a galvanometer, until they were broken asunder again, which a light tap sufficed to do."[37] This was in 1889; there is nothing to suggest that at this date Lodge saw that the effect could be used to make a usable detector. The phenomenon had, in any case, been noticed many times before and in a variety of forms.[38] Perhaps most common was the observation that finely divided metal powders or filings, enclosed in a tube or even in the form of a paste, might show very high resistance to small voltages, to the extent of being practically insulators, but might change to a state of very low resistance when a higher voltage was applied or when a spark discharge took place near them. Such a "coherer" (the name was Lodge's invention) acted essentially as a switch. Its normal state was "off"; when a voltage higher than a certain threshold value was applied, it switched to "on". Then it had to be returned to its "off" state again by mechanical action, usually by tapping or shaking.

If the scientific community had been more receptive to the reporting of unusual results, Lodge might have had this detector already available to him when he began his experiments. Ten years before, in 1878, the Anglo-American physicist David E. Hughes, inventor of the carbon microphone, had run across the same effect.[39] Indeed he had, according to his later account, actually used it to detect electromagnetic radiation. Failing, however, to convince his scientific contemporaries of the significance of what he had done, Hughes became discouraged, declined to publish his results, and continued his work in private. Lodge first heard of his work 20 years later. Nor was Lodge aware of the work that Professor Edouard Branly of the Catholic University of Paris was doing on coherers at almost exactly the same time as his own work on lightning rods. Branly's results were published in French in 1890 and reports of them appeared in the pages of the (London) *Electrician* in June and August 1891. After experimenting with a great variety of materials, Branly presented specifications and designs for a practical coherer in what was to become its classic form: a tube containing metallic filings loosely packed between metal plugs.[40] He did not, however, suggest its use as a detector of Hertzian waves, nor did he patent the device (see Fig. 4.4).

It is clear from Branly's paper that he had only the vaguest of conceptions of why his coherer behaved as it did. It was easy to guess how, if a high voltage were applied, a current might pass between the two plugs by sparking across the loose filings. That did not explain, however, how the conductivity persisted after the voltage was removed. The switch, as it were, insisted on staying "on" until the filings were once more shaken loose. All Branly could suggest was that perhaps "the insulating medium is transformed by the passage of the current and . . . certain actions, such as shock and rise of temperature, bring about a modification of this new state of the insulating body."[41] Nor did British scientists, when informed of Branly's findings at meetings of the British Association and of the Physical Society in the following year, fare much better. Certainly the conductivity of the filings changed radically when an electrical machine or Leyden jar was set to sparking a little distance away. But was it the light from the spark that caused the change, or was it perhaps invisible radiation—the newly discovered Hertzian waves?

The coherer was, if we may stretch a point, the first solid-state device used in electronics, antedating the crystal detector, and it is not surprising that its action was imperfectly understood. Indeed, some uncertainty persists today.[42] As a detector of radiofrequency energy it proved awkward and temperamental. It was highly inconvenient to have to return the filings to their original non-conducting state by mechanical means (usually a clockwork or electric "tapper") after each incoming dot or dash was received. In addition, as Poincaré and Vreeland put it, the coherer at best "lent itself reluctantly to tuning."[43] The threshold voltage at which it began to conduct was always uncertain. In addition, the capacitance of a coherer was unstable and it was therefore difficult to incorporate it into a tuned circuit. If one tried to "swamp" the capacitance of the coherer by paralleling it with a larger capacitance, the sensitivity of the device was greatly

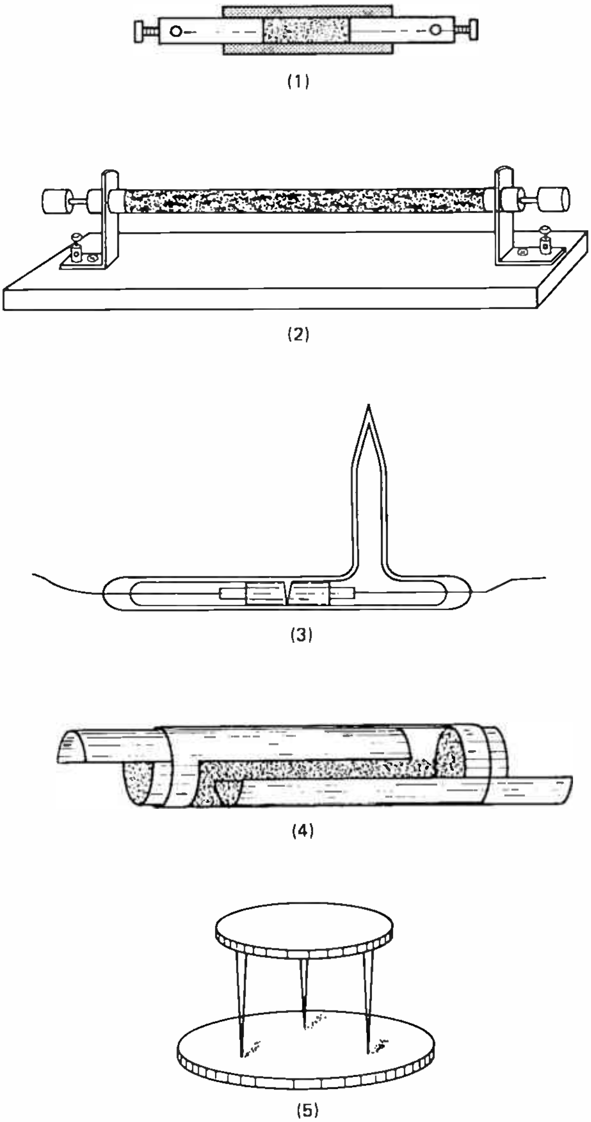


Figure 4.4 Types of coherer. (1) Branly (1890). (2) Lodge (1894). (3) Marconi (1896). (4) Popov (1896). (5) Branly tripod (1902).

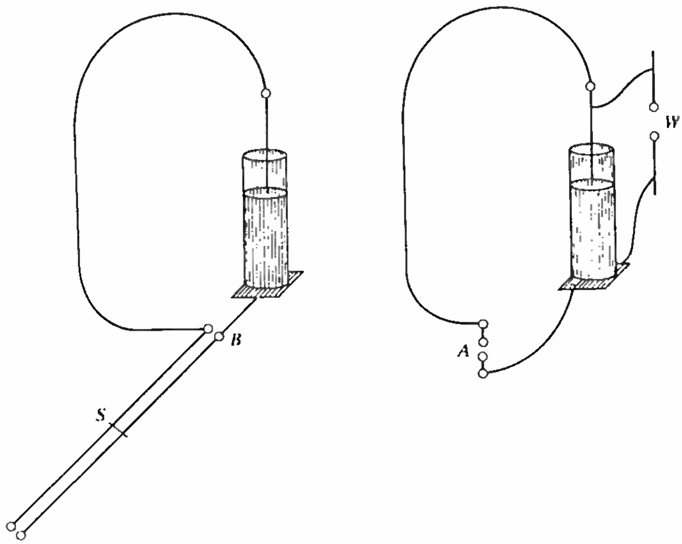


Figure 4.5 The "syntonic Leyden jars" experiment. A: primary spark gap. B: secondary spark gap. S: sliding contact. W: Wimhurst machine or induction coil.

reduced. And, of course, although this was not a problem at the time, the Branly coherer could never demodulate a signal: it was always either on or off, either conducting or nonconducting, and it was incapable of extracting from a radiofrequency signal any information other than whether the signal was present or not.[44] For transmissions in Morse code, which is essentially a matter of switching a carrier wave on and off, this was no limitation. Indeed, for the highly damped, broad-banded pulsed signals of a spark transmitter, the coherer was a crude but effective detector. If it was difficult to tune a coherer receiving circuit, it was no less difficult to tune a spark transmitter. The two devices fitted each other well enough, forming a technological system that was awkward, inconvenient, and prodigal of spectrum, but that none the less worked.

Discovery of the coherer principle made it possible for Lodge to extend his earlier experiments on resonance. One public demonstration, later to become famous as the "syntonic Leyden jars," was first presented at a lecture at the Royal Institution in

March 1889. The apparatus was simple, and the link to the earlier "recoil kick" experiment very apparent (see Fig. 4.5). What was new was the deliberate tuning of one circuit (by the sliding contact, S), so as to throw it into or out of syntony with the other.[45] No coherer was used in this first version; later a more elaborate—and, for a public lecture, far more dramatic—arrangement was used in which both circuits could be tuned and a knob coherer was shunted across the second Leyden jar (Fig. 4.6). When the first circuit was excited into discharge and the second circuit tuned to resonance, the coherer closed, ringing an electric bell. With this more sensitive and efficient detector, resonance effects could be shown very clearly.

Lodge's friend and fellow scientist, Silvanus Thompson, later

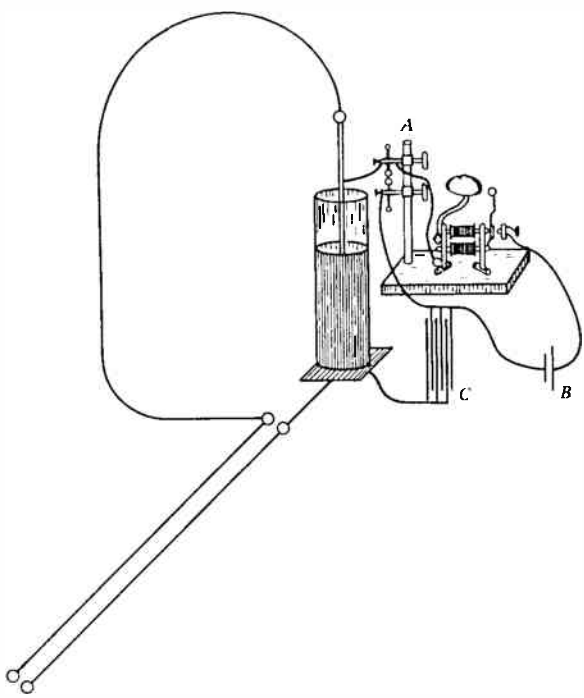


Figure 4.6 Syntonic Leyden jar with coherer and bell circuit. A: coherer. B: battery. C: fixed capacitor.

stated that in his judgment this simple experiment did more than any other to "fix in the minds of those who were studying this branch of physics the essentials of resonant action."[46] The reason was that the results seemed contrary not only to common sense but also to the expectations of anyone thinking in terms of direct current. Here was a Leyden jar—a capacitor for storing electrical charges—apparently short-circuited by a wire joining its inner and outer surfaces. How could such a capacitor ever hold a charge? How could such a circuit ever create a spark? The intellectual problem was identical to that presented earlier by Lodge's work on the "recoil kick" and, even before that, by his research on lightning conductors. Without a grasp of the concept of resonance, the matter was incomprehensible.

Lodge himself emphasized, in his comments on this experiment, one crucial feature which suggests the direction of his thinking at the time. If the discharge from the first Leyden jar were highly damped, he pointed out, so that oscillations died out after only a few cycles, it was very easy to induce a response in the second circuit. The dimensions of the circuit—the capacitance of the jar and the inductance of the loop—were, within broad limits, noncritical. Both could be varied considerably and still show a response.[47] But if, in contrast, the first circuit were what Lodge called a "persistent vibrator," giving some 30 or 40 swings before damping became evident, tuning of the second circuit became critical and even slight variations in inductance or capacitance were enough to throw it out of resonance.

The basis for these findings should be clear from our analysis of Hertz's frequency measurements. The more highly damped the oscillations, the less critical the tuning, because the damping itself generated harmonic radiation at frequencies covering a wide range of the spectrum. But this is not quite the way Lodge expressed it, and his way of phrasing the problem is more suggestive of the directions his later work on the design of tuned circuits was to take. The normal Hertz dipole, said Lodge, is an open circuit and an excellent radiator of energy; for this reason its oscillations are rapidly damped and impossible to tune to a precise frequency. In the syntonic Leyden jar experiments, in contrast, closed circuits were used. Such circuits were necessary if oscillations were to be sustained, but they were intrinsically poor radiators and "not adapted for action at a distance."[48]

What Lodge was pointing to was an emerging dilemma in circuit design: circuits that radiated well were highly damped, broad banded, and impossible to tune precisely; syntonic circuits that were highly selective in their frequency response, on the other hand, could not be made to radiate efficiently. Was there some way in which this dilemma could be resolved? Not, according to Lodge, with the methods of generating Hertzian waves then available. "The two conditions, conspicuous energy of radiation and persistent vibration electrically produced, are at present incompatible. Whenever these two conditions coexist, considerable power or activity will, of course, be necessary in the source of energy. At present, they only coexist in the sun and other stars, in the electric arc and in furnaces."[49]

Implicit in the way Lodge stated the problem of "incompatibility" was a challenge, already partially recognized, to his own creative intelligence: how to couple the radiating efficiency of "open" circuits to the narrow frequency response and sustained oscillations of "closed" ones. At first a problem of theoretical interest only, this was soon to become a matter of urgent practical concern, for the radiofrequency spectrum was not for many years more to remain free of jurisdictional claims. Before long the necessity for precise syntony was to move out from the laboratory environment of Leyden jars and loops of wire and into the competitive world of high-powered transmitters, elaborate antennas, and rivalry for long-distance traffic.

But this was in the future. In the early 1890's no one, except perhaps David Hughes, working in obscurity, was yet thinking in terms of using Hertzian waves for communication. It was recognized by many people that existing communication technologies had limitations. Communications with offshore lighthouses and islands, with ships at sea, and with moving objects such as railroad trains on land—problems such as these threw into sharp relief the economic and technical limitations of cable technology, and men like William Preece of the Post Office and Henry Jackson of the Royal Navy were well aware of them. But what experimentation was being done with electrical means of solving these problems involved inductive circuits, not radiation, and effective ranges proved to be small.[50] And yet, in 1892, all the individual elements necessary for a workable system of radio communications were at hand: the Hertzian spark oscillator; the ungrounded dipole antenna, which could be made highly directional by parabolic reflectors such as Hertz had used; the Lodge-Branly coherer; and Lodge's syntonic circuits. What was lacking was the vision, the conception of something possible, and the determination to translate that vision into reality.

* * *

The man who provided the vision, who jolted some of his contemporaries into thinking of familiar things in an unfamiliar way, was William Crookes, scientist extraordinary, speculative genius, and brilliant public lecturer. Crookes, later knighted and at the close of his career a somewhat controversial president of the Royal Society, was a man who enjoyed visions. He believed that they were necessary, both for the conduct of science and for the uses made of science. History knows him mainly as inventor of the first high vacuum cathode ray tube; the man who, but for an unfortunately timed visit to South Africa, might have beaten Roentgen to the discovery of X rays; perhaps also as the discoverer of the element thallium, as one of the first advocates of the use of artificial sources of nitrogen for fertilizer, as the deviser of the ingenious and perplexing "light mill" that you can still see whirling away in the optician's store window any day when the sun is shining. Crookes was all of these things and more; for our present purposes what counts most is that, in 1892, he was both a scientist of established and impeccable reputation and also a man who had not the slightest hesitation in speculating publicly about what scientific discoveries meant, what they implied for the future, and how they could be exploited for practical ends. A dreamer he was, of course—a visionary, almost a mystic. But he was also intensely ambitious, hardheaded about what worked and what did not, and quite unabashed in his belief that scientific research should be put to work and, if possible, used to make money.[51]

The article Crookes published in *The Fortnightly Review* in February 1892 was modestly entitled "Some Possibilities of Electricity" and began modestly enough with a few conventional comments on atoms, the aether, and the need for continued experimentation to find out what electricity really was.[52] By the second page, however, Crookes had plunged his readers into the new world of Hertzian waves—"etherial vibrations," he called them—stressing what to laymen was probably the hardest concept to grasp: the continuity of the electromagnetic spectrum. Vision, the perception of light, depended on the fact that the human eye was an efficient detector of a certain range of frequencies. But this range was narrow; it was a mere promontory, as it were, on the outlying tip of a vast continent. Now for the first time, thanks to the work of Hertz and Lodge, men were beginning to understand how vast that continent really was, and what a miniscule portion of it they had previously known. A "new and astonishing world" was opening up, a world with "an almost infinite range of etherial vibrations or electric rays, from wave-lengths of thousands of miles down to a few feet."

And what could it be used for? For communication, almost certainly. It was hard to conceive, wrote Crookes, that this new world "should contain no possibilities for transmitting or receiving intelligence."

> Rays of light will not pierce through a wall, nor, as we know only too well, through a London fog. But the electrical vibrations of a

> yard or more in wave-length of which I have spoken will easily pierce such mediums, which to them will be transparent. Here, then, is revealed the bewildering possibility of telegraphy without wires, posts, cables, or any of our present costly appliances. Granted a few reasonable postulates, the whole thing comes well within the realms of possible fulfilment.

What was lacking to make the idea practical? Scientists already knew how to generate waves of any desired length, and how to radiate them into space. Hertz had shown how they could be focused and reflected, so that a "sheaf of rays" could be emitted in any desired direction. And, with a suitable detector, they could be received at a distance, so that "by concerted signals messages in the Morse code [could] pass from one operator to another." All that was necessary was to improve devices that already existed: simpler and more certain ways of generating waves of specific desired length; more delicate detectors "which will respond to wave-lengths between certain defined limits and be silent to all others"; and better means of "directing the sheaf of rays in any desired direction" so that power would not be unnecessarily dissipated by radiating the signal in all directions at once. Granted these refinements, direct person-to-person communication by etherial vibrations was simple:

> Any two friends living within the radius of sensibility of their receiving instruments, having first decided on their special wave-length and attuned their respective instruments to mutual receptivity, could thus communicate as long and as often as they pleased by timing the impulses to produce long and short intervals in the ordinary Morse code.

Lack of secrecy in such communications could be handled either by using highly directive transmissions, if both transmitter and receiver were in fixed locations, or by precise tuning if they were not, for the spectrum was wide and the number of possible frequencies very large:

> I assume here that the progress of discovery would give instruments capable of adjustment by turning a screw or altering the

> length of a wire, so as to become receptive of wave-lengths of any preconcerted length. Thus, when adjusted to fifty yards, the transmitter might emit, and the receiver respond to, rays varying between forty-five and fity-five yards, and be silent to all others. Considering that there would be the whole range of waves to choose from, varying from a few feet to several thousand miles, there would be sufficient secrecy; for curiosity the most inveterate would surely recoil from the task of passing in review all the millions of possible wave-lengths on the remote chance of ultimately hitting on the particular wave-length employed by his friends whose correspondence he wished to tap.

And, if all else failed, messages could be sent in cipher.[53]

Crookes stressed that this was no mere dream of a visionary philosopher. The knowledge required, even in rudimentary form the instruments themselves, were already in existence. The thing was feasible; he himself (referring cryptically to David Hughes's experiments) had participated in actual transmission of messages over short distances. Oliver Lodge had shown how, by proper choice of inductance and capacitance, waves of any desired frequency could be generated, and "the discovery of a receiver sensitive to one set of wave-lengths and silent to others is even now partially accomplished." The future might hold greater wonders and more remarkable achievements—cheaper sources of electricity and some means of taming alternating currents, so that mankind might no longer be "haunted by the steam-engine with its clouds of smoke and its heaps of cinders and ashes," were particularly to be hoped for—but as far as telegraphy without wires was concerned, the means were already at hand.

And there was more: electricity to stimulate the growth of crops; electricity to light homes without wires; electricity to control the weather; perhaps even electricity, of a sort, to explain telepathy. It was all very clever of that brilliant Professor Crookes, and without doubt it provided material for many well-informed conversations. *The Fortnightly Review* was good at that sort of thing. But did it, in the long run, make any difference? Did anyone, and in particular any of the men then working with electromagnetic radiation, think of "etherial vibrations" after reading Crookes's article differently from the way they had thought of them before? Did it lead anyone to do anything he would not otherwise have done? Or was Crookes just synthesizing and dramatizing, in the way he knew so well, ideas that were "in the air," the gossip and guesses of faculty common rooms and association tea parties? How can we be sure?

Of course we cannot. We can notice, however, that Crookes's article was read very widely—and more than that, attended to and remembered—both in Europe and in the United States. There is hardly one figure important in the early days of radio who does not at some point in his memoirs or correspondence refer to the article of 1892 as having made a difference. Not, perhaps, in any very specific way: there had been talk of telegraphy without wires for many years before Crookes, and inductive telegraphy, as we have already seen, had its believers and its advocates, some of them in positions of responsibility. The knowledge and devices that Hertz, Lodge, and Branly had generated would not have been permitted to lie fallow for long. There were too many people experimenting with them, trying new types of spark gaps and improved coherers, seeking greater distances—men like Jackson in England, Popov in Russia, the young Marconi in Italy—for that to be conceivable. Nevertheless, the Crookes article was both timely and catalytic. The year 1892 does mark a watershed. Before that, experimentation with electromagnetic waves was essentially a matter of validating Maxwellian theory; after, it became a matter of devising signaling systems, of inventions and patents, of developing a commercial technology. The change is evident in the way people like Lodge acted and talked. It is evident also in an accelerating shift to new regions of the radiofrequency spectrum, away from the ultra high quasi-optical frequencies that the scientists, thinking in terms of the theory of light, had chosen, and down to the medium and low frequencies that at first seemed to promise greater distance. Crookes's "new world" was ripe for exploration and exploitation.

Exploitation, however, implied profits, and profits depended on finding a market in which services, and the equipment to provide services, could be sold. Where were these markets? To that question Crookes offered no answer beyond the vague assertion that wired telegraphy was "costly" and the implication that telegraphy without wires would be less so. Nor did he emphasize the specific limitations of existing means of communication—the gaps, as it were, that the telegraph and telephone, the newspaper, magazine, book, and postal service failed to fill. These gaps, if they could be isolated, would provide points of entry for the new technology of "wireless," just as the limitations of river navigation had stimulated the emergence of canal technology, and the limitations of canals the emergence of the railroad. There was none of this in Crookes; what he had to display were the wonders of science, not the business prospects of new technology.[54]

* * *

Here, then, was a vision of the possible. The scientific knowledge and the necessary devices were already available. What stood in the way of prompt development? Why was there still delay? Oliver Lodge, trying after the event to rationalize how an unknown foreigner, Marconi, had come to dominate within a few short years the whole field of wireless telegraphy in Britain, reached for two explanations. First, he said, commercial exploitation of wireless telegraphy was no proper business for a scientist, at least in Britain. There were properly constituted authorities for that purpose:

> Numbers of people have worked at the detection of Hertz waves with filing tube receivers, and every one of them must have known that the transmission of telegraphic messages in this way over moderate distances was but a matter of demand and supply . . . . There remained no doubt a number of points of detail, and considerable improvements in construction, if the method was ever to become practically useful; but these details could safely be

> left to those who had charge of the Government monopoly of telegraphs, especially as their eminent Head [William Preece] was known to be interested in this kind of subject.[55]

But also, he said, he and his colleagues had been blind and stupid: blind not to see the commercial potential, and stupid not to try for higher power and greater distance:

> Signalling was easily carried on from a distance through walls and other obstacles . . .[but] stupidly enough no attempt was made to apply any but the feeblest power so as to test how far the disturbance could really be detected.[56]

> The idea of replacing a galvanometer . . . by a relay working an ordinary sounder or Morse was an obvious one, but so far as the present author was concerned he did not realize that there would be any particular advantage in thus with difficulty telegraphing across space instead of with ease by the highly developed and simple telegraphic and telephonic methods rendered possible by the use of a connecting wire. In this non-perception of the practical uses of wireless telegraphy he undoubtedly erred. But others were not so blind.[57]

There is a certain disarming candor about these remarks, a candor disconcerting to any who would cast Lodge in the role of inventor of radio. Why telegraph with difficulty without wires when you can telegraph so easily with them? Why indeed? The question should at least remind us that the commercial potential of wireless telegraphy which was so obvious to Marconi was not self-evident to others. Lodge's explanations, nevertheless, are incomplete. The fact that, in Britain, the Post Office held a statutory monopoly over land telegraphy does not explain a failure to file patent applications. Lodge himself was no stranger to commerce and industry. His research projects on dust precipitation, induction, and lead-acid batteries had all had a "practical" aspect, and all led to commercial use, though not at his hands.[58] Possibly Lodge assumed that in this field there was nothing to patent; all the information necessary for the construction of a practical wireless telegraphy system had already been published in the scientific journals and elsewhere and was therefore public property.[59] There was, however, nothing to prevent him from applying for patents on important and perhaps indispensable improvements, such as syntonic circuits, for example, and in this way acquiring marketable rights of some value. This is what Marconi did in 1896 and what Lodge belatedly did the year after. In 1892 Lodge was, in plain fact, in an excellent position to establish a dominant position in the commercial development of wireless telegraphy, had he wished. He did not do so. It was no proper business for a scientist. For that matter William Preece, who had a clear sense of the need for wireless telegraphy and some public responsibility in the matter, was in an excellent position to take the initiative in bringing together a syndicate of scientific and entrepreneurial talent under government auspices to oversee the conversion of scientific knowledge into technological reality. He did not do so. Lodge and Preece had crossed swords years earlier in the controversy over lightning rods, and some of the wounds still smarted. Preece waited for the arrival of an outsider, someone not part of the British scientific establishment, before committing his prestige and bureaucratic authority to the task of development.

Lodge, for his part, busied himself with his teaching, his new researches on aether drift, and almost incidentally, the refinement of his electrical apparatus. An improved coherer and spark gap were shown in the course of a lecture on "The Work of Hertz" at the Royal Institution in June 1894. Later that month, at a "Ladies' Conversazione" of the Royal Society in London, a small portable receiver was demonstrated. With this, using headphones, Lodge claimed that ranges of half a mile had been obtained at Liverpool. For the London demonstration a mirror galvanometer was substituted for the headphones so that those present might conveniently witness reception of the waves. A paragraph in *Nature* described his equipment as follows:

> Prof. Oliver Lodge, F.R.S., exhibited a compact and sensitive detector for electric radiation, and a spherical radiator of short

> Hertz waves. The apparatus consisted of a small copper cylinder containing a piece of zinc and sponge, forming a battery, a coil and suspended needle-mirror, forming a galvanometer, and a ball contact or "coherer," or else a tube of filings, in circuit with the other two. Electric surgings in the air, or in a scrap of wire pegged into the lid, increased the conductance of the circuit. A light tap on the cylinder reduced it again. A handy lamp and scale enabled the deflexion of the needle to be seen. The surgings could be excited by giving sparks to an insulated sphere not far off, especially if the knobs supplying the sparks were well polished.[60]

Much the same apparatus was used at the Oxford meetings of the British Association in August 1894, but under more demanding circumstances. The induction coil and spark gap were set up in a room in the Clarendon Laboratory, while the receiving apparatus was in the lecture theater of the Oxford Museum, a separate building. The signals had to cross the back yard of the Laboratory and the front yard of the Museum, a distance of some 180 feet, and pass through two stone walls.[61] A siphon recorder was on hand to provide a record on tape of the signals received, but a "dead-beat" galvanometer of the type normally used for submarine cable telegraphy was used for most of the demonstrations. Lodge later described this as "a very infantile kind of radio telegraphy" but nevertheless insisted, not only that Morse code signals could be sent by these means, but that at the Oxford meetings they actually had been. The point was an important one, for within two years there was to be a rival claimant to priority. Lodge's description of the demonstration is worth quoting at length:

> The sending instrument was a Hertz vibrator actuated by an ordinary induction coil set in action by a morse key. This apparatus was in another room, and was worked by an assistant. The receiving apparatus was a filings tube in a copper hat, in circuit with a battery, actuating either a morse recorder on a tape, or, for better demonstration to an audience, a Kelvin marine galvanometer .... When the morse key at the sending end was held down, the rapid

> trembler of the coil maintained the wave production, and the deflected spot of light at the receiving end remained in its deflected position so long as the key was down; but when the key was only momentarily depressed, a short series of waves was emitted, and the spot of light then suffered a momentary deflection. These long and short signals obviously corresponded to the dashes and dots of the morse code; and thus it was easy to demonstrate the signalling of some letters of the alphabet, so that they could be read by any telegraphist in the audience—some of whom may even now remember that they did so.[62]

On the face of it, this seems conclusive evidence. All the elements of a telegraphy system were there: exciter, Morse key, and receiver. We are not told what antennas were used, but if the apparatus were indeed the same as that used with the portable receiver of the previous June, the "dumb bell" oscillator used in the exciter and a short piece of wire attached to the coherer would have been adequate. Lodge, then, it would seem, can be credited with the first demonstration of practical wireless telegraphy.

But is this really so? The question turns partly on whether actual signaling was carried out. Lodge assures us that it was and appeals to the memory of surviving contemporaries for corroboration. At least one of these contemporaries, however, contradicts him. J. A. Fleming, a scientist whose personal and professional reputation was at least as high as Lodge's, tells us in unmistakable terms that no signaling was attempted at Oxford in 1894, nor was it even mentioned. The critical passage occurs in Fleming's authoritative *Principles of Electric Wave Telegraphy*:

> although replete with interest, the lecture, as originally delivered [at the Royal Institution], contained not even a hint of a possible application of these electromagnetic waves to telegraphy. The lecturer throughout fixed the attention of the audience on the similarity between the effects produced with these waves and those better known effects produced by rays of light . . . . These experiments and some variations of them were repeated at the meeting of the British Association of Oxford in the following

> autumn, but here again no mention of the application of these waves to telegraphy was made, the object of the experiments being to illustrate an electrical theory of vision, and to expound the properties of the electric waves.[63]

The fact that Fleming was, at the time he wrote these words, in the employ of the Marconi Company and therefore concerned to defend the priority of Marconi's achievements is worth noting, but does not in itself speak to the question of what actually happened at the Oxford meetings. The matter is not unimportant for—quite apart from questions of prestige—Lodge's lecture to the Royal Institution, his demonstration at the Royal Society, and his formal presentation at the British Association meetings were reported in the United States as well as in Britain, and in that country were accepted by the courts as evidence of prior discovery.[64]

Fleming is certainly correct in saying that the June lecture to the Royal Institution dealt with the electromagnetic theory of light, not with telegraphy. This was to be expected in a presentation devised as a memorial to Hertz. The published version contains no clear reference to signaling.[65] The case is different with the presentation to the Royal Society's soirée. Lodge later claimed explicitly that this was the first public demonstration of wireless telegraphy. If he is correct, much popular mythology stands in need of revision. Difficult though it may be, we must learn to think of radio as born not on the rolling hills of the Marconi estate outside Bologna, nor on the barren heath of Salisbury Plain where Preece carried out his Post Office tests, and certainly not on the windblown heights of Signal Hill in Newfoundland, but rather amid the teacups and genteel chatter of a Ladies' Conversazione on a June evening in London. The gain in accuracy is certainly bought at the expense of historical drama.

For the uncertainty that surrounds his demonstrations in the summer of 1894 Lodge is himself largely to blame. Six years earlier he had given the first word of his research on electromagnetic waves in an article on lightning conductors, and by so doing blunted for ever his claim to simultaneous discovery with Hertz. Now in 1894 he introduced his signaling apparatus to the British Association as a contribution, not to the technology of communications, but to the theory of vision. He presented two papers to the conference, one entitled "On Experiments Illustrating Clerk Maxwell's Theory of Light," the other, "On an Electrical Theory of Vision." Only the titles appear in the official record; for the contents we are dependent on secondhand reports, and on Lodge himself.[66] Both papers were presented to a joint session of physicists and physiologists—not normal practice in the British Association. Both were received with "hearty and prolonged applause."[67] As for what they contained, the first appears to have been a straightforward demonstration of the quasi-optical behavior of electromagnetic waves at ultrahigh frequencies: reflection, refraction, polarization, and so on. The second paper contained Lodge's original notions, the ideas with which he hoped to titillate the physiologists. He had shown how a coherer could detect electromagnetic radiation at wavelengths substantially longer than visible light. Was it possible, he asked, that anything resembling the action of electromagnetic waves on a coherer could be supposed to take place in the human eye? Did the rods and cones of the retina act as coherer circuits, transmitting impulses to the brain? Was there, in the electrical circuits he had just shown, an analogue to human vision?

There was nothing very remarkable about these ideas. Edouard Branly had stumbled on the coherer more or less incidentally in the course of his studies of the nervous system and had speculated that neurones, relaying messages from nerve endings to the brain and back, might be analogous to the "imperfect contact" switches of the electrical coherer. The approach was novel enough, however, to stimulate active discussion among the physiologists at Oxford. It also deflected attention away from any experiments in signaling that Lodge may have introduced as prologue, more or less, to his talk on vision. The reporter for *The Electrician*, for example, who commented at considerable length on Lodge's theory of the retina, had not one word to tell his readers about any exchange of signals in Morse code.

Are we to conclude, then, that Lodge's memory played him false, or even that, in an attempt to prove his priority, he tried to falsify the record? By no means. The question turns partly on what we are to mean by signaling, and partly on how to interpret an unofficial semijournalistic report of a scientific conference. As for the accuracy and completeness of the reporting, one has only to thumb through the pages of *The Electrician* for a few more years to find the editors flatly stating, with reference to Marconi's first patent application, that "Both at Oxford and at the Royal Institution, Dr. Lodge described and exhibited publicly in operation a combination of sending and receiving apparatus constituting a system of telegraphy substantially the same as that now claimed in the patent application referred to."[68] Either editorial memories had improved in the interim, or what had seemed incidental and unremarkable in 1894 had acquired a new importance three years later.[69]

But editors of a trade journal are not the only ones able to change their minds. It would appear that Fleming's memory also was capable of improvement with the passage of time, or perhaps as commercial and scientific rivalries receded into the past. The passage we have quoted from his *Principles*, published in 1908, seems unequivocal: "no mention of the application of these waves to telegraphy was made." By 1937, however, Fleming was no less unequivocal on the opposite side of the fence. In November of that year he read to the Royal Society of Arts a lecture commemorating the achievements of Marconi. He took pains to point out in his second sentence that Marconi was "not the first person to transmit alphabetic signals by electromagnetic waves" and made it very clear, in the remarks that followed, whom he considered to have been the real pioneer: Oliver

Lodge. The precise occasion specified, furthermore, was Lodge's lecture to the British Association at Oxford in 1894. Fleming accurately describes Lodge's equipment and continues:

> he was able to transmit a dot or a dash signal and by suitable combinations to send any letter of the alphabet on the Morse code and consequently intelligible messages. He had also on his table a Morse inker (so he tells me), and could have used it with a sensitive relay to print down the signals, but as he wished the audience to see the actual signals he preferred to use the mirror galvanometer. It is, therefore, unquestionable that on the occasion of his Oxford lecture in September, 1894, Lodge exhibited electric-wave telegraphy over a short distance.[70]

Did Lodge in 1894 suggest in public that his equipment could be used for signaling? Did his lecture refer to the application of Hertzian waves to telegraphy? Did he demonstrate transmission and reception of Morse code? The answer would seem to be affirmative in each case. In this sense Lodge must be recognized as the inventor of radio telegraphy. This does not mean, however, that he showed any awareness at that time that what he had shown to be possible in the laboratory or across the quadrangle at Liverpool might also be commercially feasible or even commercially desirable. There is in fact no evidence to suggest that Lodge had in 1894 the slightest interest in commercial development. Scientific discovery was one thing: Lodge, Hertz, and Maxwell had accomplished that. Translation of scientific discovery into usable technology was another thing: Lodge had shown how that could be done. But between usable technology and commercially feasible development there was still a large gap, one which Lodge in 1894 showed no inclination to bridge. His invention, if invention it was, had not been made in response to any sense of practical need, except the need to demonstrate a scientific principle. The emphasis of both Oxford papers was on the quasi-optical behavior of Hertzian waves at ultra high frequencies, as Fleming stated. If there was "signaling" and use of the Morse code it was incidental. In view of the nature of the audience and the titles of Lodge's papers, it would be strange if it were otherwise. It is true, as Lodge later remarked, that signaling "of course, is what was being done all the time."[71] But it is no less true, to quote Lodge again with reference to his Oxford lectures, that he was "chiefly interested in emphasizing the fundamental work of Clerk Maxwell and the brilliant experimental discoveries of the recently deceased Heinrich Hertz."[72] Neither Lodge nor any of his British contemporaries, with the possible exception of David Hughes, had at the end of 1894 "actually applied these waves to anything that could be called practical telegraphy."[73] The words are Lodge's, and they say all that needs to be said. What was missing at this stage was neither the knowledge nor the apparatus. The scientific and technological groundwork had been laid. The elements still lacking were, first, perception of need, and second, the drive to convert abstract possibility into concrete reality.

* * *

That Lodge himself would ever have provided that drive is doubtful. By the early 1890's his scientific interests were already carrying him further and further away from direct involvement in wireless telegraphy. Increasingly he found himself preoccupied with the puzzling and anomalous results of the Michelson-Morley experiments in aether drift. Financed by his friend George Holt, the Liverpool shipping magnate, most of his time was spent devising and erecting a massive apparatus of rotating steel discs to determine whether matter really did carry the neighboring aether with it when it moved.[74] One has the impression that, if asked in 1894 what progress was being made with electric waves, Lodge would have responded cheerfully, as he did when he learned of Hertz's experiments, that the whole subject was coming along splendidly. But there was no urgency. Even to apply for a patent on his coherer had seemed too much of a nuisance. There was nothing really new about it, after all, and once a scientist had made his discoveries freely available through the proper channels, he had done all that was fitting.[75]

It took Marconi's arrival in England in 1896 and the filing of Marconi's first patent application to stir Lodge out of his cheerful complacency. In the meantime he acquired a new friend and adviser. Among the audience at Lodge's lecture to the Royal Institution in June 1894 had been a certain Dr. Alexander Muirhead, a fellow of the Royal Society like Lodge, but also and more significantly partner with his brother in a firm of telegraph instrument makers. Muirhead saw the commercial implications of Lodge's system immediately and lost no time in calling them to his attention. If indeed Lodge's Oxford lectures in the late summer of 1894 emphasized and demonstrated wireless telegraphy as his earlier public lectures had not, the explanation is to be found in Muirhead's intervention. The mirror galvanometer used in the Oxford demonstrations was Muirhead's contribution —a sensitive instrument that his firm had made for use with the Atlantic cable; so was the siphon recorder; and so in all probability was the Morse key. These were telegraph instruments. They had not been used in any of Lodge's previous experiments. Muirhead's meeting with Lodge, therefore, marked not only a union of entrepreneurial and scientific talent—Muirhead playing Boulton, as it were, to Lodge's Watt—but also a union of two technologies: the mature technology of wired telegraphy, and the new technology of radio.

Later, in 1901, Lodge and Muirhead were to formalize their relationship by the creation of a "syndicate" to build and market radio equipment of Lodge's design. There seemed no need for this in 1894, and if Muirhead did indeed bring home to Lodge the practical and commercial aspects of "signalling without wires," it did not result in any prompt action to acquire patents. Lodge's first such application was filed only in 1897. There can be little doubt that it was precipitated by the publicity attendant upon Marconi's arrival in England the year before and the knowledge that Marconi had applied for a broad patent on signaling by Hertzian waves (see below, pp. 203–10). Lodge had undoubtedly been slow to see that there were commercial possibilities in the scientific work in which he had been engaged and slower still to seek property rights to his discoveries. He was reluctant, he said later, to "go through the inappropriate and repulsive form of registering a claim to an attempt at monopoly."[76] But now, with Marconi asserting claims to ideas and devices that Lodge had thought of as public property, such scruples seemed out of place.

It would be a mistake, however, to lay too much emphasis on Marconi. He may have been the catalyst, but there were other experimenters at work—Braun and Slaby in Germany, Righi in Italy, Popov in Russia, Henry Jackson in England, to mention only a few. All were using variations of the basic transmitting and receiving apparatus that Lodge had demonstrated at Oxford in 1894; a spark-excited dipole antenna for transmission; a similar antenna for reception, in circuit with a coherer, usually operating a bell or recorder of some kind through a relay, with provision for automatic "tapping back" of the coherer (see Fig. 4.7). There was considerable variation in coherer and spark gap construction, and to a much lesser degree in antenna design, but the basic circuit was the same for all. All were practical and feasible systems of wireless telegraphy, though with serious technical limitations. What distinguished Marconi from the half-dozen other experimenters whose systems in 1895–1896 were similarly on the verge of commercial acceptability was mainly his determination to achieve greater distance. If he had not been pushing so rapidly and decisively into commercial development, however, others would have done so within a few years. If Lodge was to seek property rights to any of the devices or circuit concepts he had helped to devise, he could not postpone action much longer.

Conspicuously lacking, to modern eyes, in these early transmitting and receiving circuits is any provision for tuning, for achieving what Lodge called syntony. The only frequency-deter-

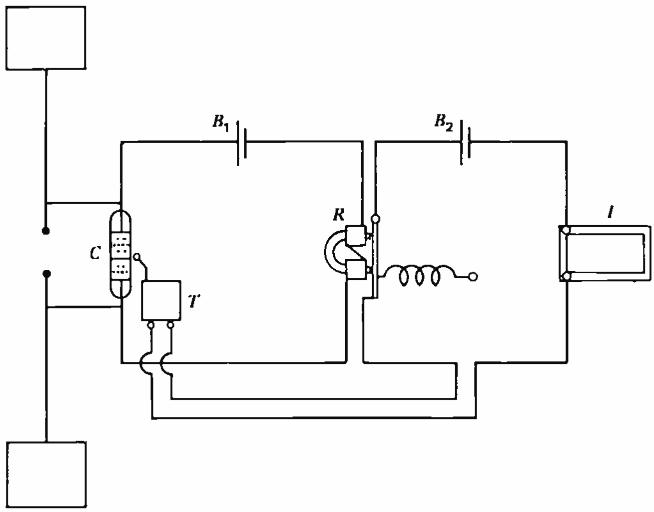


Figure 4.7 Lodge receiving apparatus, 1894. B_1, B_2: batteries. C: coherer. T: trembler. R: relay. I: inker.

mining element in both receiving and transmitting apparatus was the antenna itself, whose inductance and capacitance (in combination with stray inductance and capacitance in the rest of the circuit) would in theory determine the fundamental resonant frequency. In practice, as we have already noted, the ordinary Hertzian dipole, whether it had metal globes at the ends of its arms or the large metal sheets that later experimenters preferred, was virtually aperiodic; that is, it had no natural resonant frequency. A signal emitted from a spark-excited Hertzian dipole was very like the raucous interference that one can hear today on an AM broadcast receiver when an automobile with badly adjusted and badly shielded ignition passes by. The noise appears "all over the dial." And even if the Hertzian dipole had been designed to be more sharply resonant, there still remained the problem of the rapidly damped wave typically emitted by a spark transmitter. Precisely because such an antenna radiated energy so efficiently, it could not be what Lodge called a "persistent vibrator." Rapid damping meant the radiation of multiple harmonics of the fundamental frequency, implying once again a signal that was wasteful of energy and "as broad as a barn door."

We are not referring in this context to methods of deliberately varying the frequency of transmitters or receivers. This was to come later. The circuits we are discussing now were essentially untuned to any frequency, except to the degree that the particular antenna used might, by accident or design, discriminate in favor of some frequencies and against others. And if the signals radiated by such transmitters were untuned, then of course there was no need for sharply tuned receivers. Within very broad limits, the radiated energy would be picked up by the receiving antenna and detected by the coherer, no matter what adjustments were made by the receiving operator. It would be impossible, in fact, to avoid receiving the signal if one wished to do so, as might indeed be desirable if two stations were transmitting simultaneously.

To later engineers and radio operators these considerations added up to a problem. To early experimenters they were no such thing. They became a problem only as the radiofrequency spectrum came to be occupied, as "places" on the spectrum became scarce, and as the availability of a clear channel came to have commercial or military value. Protection against trespass is required only when there are property rights, or at least claims to territoriality, and potential trespassers. From the perspective of the pioneers in wireless telegraphy, aperiodic transmitters, highly damped wave trains, and nonselective receivers were not difficulties but almost necessities. They made it possible for transmitters and receivers to find each other in an unfamiliar world that was disconcertingly wide and devoid of surveyor's marks. Measurement of frequency was still, in the late 1890's, a laboratory operation of considerable delicacy. Methods of maintaining frequency stability were primitive in the extreme. In these circumstances, as transmitters and receivers explored the new continent, a broad signal and a nonselective receiver were not drawbacks but advantages. It was easy to get lost; communication depended upon transmitter and receiver finding each other in an electronic desert.

These were, however, in the nature of the case, transitory advantages. As the spectrum became more crowded, highly selective receivers and precisely tuned transmitters became increasingly necessary. This is why Lodge's syntonic tuning patent of 1897 was destined to become, with the passage of time, more and not less strategic to the business of radio communication. Particular devices such as coherers were quickly obsolete, but the principles of syntonic circuits never could be. They remain fundamental today, though the patents have long expired and the word itself has grown strange.

In his experiments with Leyden jars Lodge had observed that there was no difficulty in securing very sharp tuning as long as he used what he called "closed circuits." These were, by definition, circuits that radiated poorly. When excited into oscillation by the sparks from an induction coil they tended to "ring," as it were, like a bell or a tuning fork. Such "persistent vibrators" were easy to tune because they produced virtually a pure sine wave of a single frequency, with very slow damping. Open circuits, on the other hand, lost energy rapidly because they radiated it into space; their oscillations died away quickly and this rapid damping meant that sharp tuning was impossible. Hence the designer of radio circuits had to compromise: if he wanted efficient radiation, he had to sacrifice sharp tuning.

There were two ways of tackling this problem. One was to abandon spark excitation entirely and adopt instead some method of generating continuous waves—pure sine waves of a single frequency with a zero damping coefficient. This would eliminate the need for any design compromise. But how to do it? It was easy at low frequencies, using alternators like those which generated the 50 or 60 cycle current that powered homes and factories. Nikola Tesla had demonstrated remarkable results with alternating currents of much higher frequencies, but Lodge knew of no alternator capable of generating alternating currents of radio frequencies. The problem here was a technological one and was eventually solved by Fessenden and Alexanderson in the United States. It was 1915, however, before General Electric had a perfected radio alternator to market. The only other possibility Lodge knew of was the electric arc, which scientists knew to be a generator of high frequency alternating currents. This had been adapted to radio use by the Danish scientist, Valdemar Poulsen, but not until 1911 was an arc transmitter in operation. Lodge mentioned it as a theoretical possibility but did not pursue the matter. Lacking, therefore, a suitable alternator, and with the arc transmitter no more than an idea, Lodge accepted spark excitation, with all its problems, as the normal technique and designed his circuits accordingly. Not until the development of the oscillating triode vacuum tube by De Forest, Armstrong, and others in 1913–1914 did a truly efficient and versatile generator of continuous waves become available.

The alternative approach to the problem was to compromise: to search for circuits, or combinations of circuits, that offered reasonably high radiation efficiency and at the same time reasonably sharp tuning. This is the approach Lodge adopted. His syntonic circuits were essentially modes of securing an acceptable trade-off between conflicting design objectives. The way in which he did this, and the principle that underlies the patent he secured in 1897, was to require that the antenna systems of both transmitter and receiver be made sharply resonant at the intended frequency. The two antennas had to form a syntonic system. Energy would then be coupled into the antenna circuit in the case of the transmitter, and out of it in the case of the receiver, in such a way as to disturb its natural resonance as little as possible.

* * *

Oliver Lodge secured four British patents in 1897. Two of hem (Nos. 16,405 and 18,644) related to improvements in coherers and do not concern us at the moment. The other two (Nos. 11,575 and 29,069), both described as "Improvements in Syntonized Telegraphy," were to prove of long-run importance, both legally and technologically, and of these two it is the first, for which Lodge made application on 10 May, which came in the course of time to be recognized as the basic patent on tuning. It deserves, therefore, more than casual analysis.[77]

The patent is not, of course, a patent on the idea of syntony, since ideas as such were not patentable. What Lodge describes is a complete system of telegraphy by Hertzian waves. It is, however, explicitly stated to be a syntonized system; the circuits and components described take the form they do because they make syntony possible; and the specific claims with which the patent ends center on the assertion that by the means specified in the text syntonic response between transmitter and receiver can be achieved. The concept of syntony is, in short, what inspires the particulars of the patent. Marconi also, in his patent of the previous year, had described a system of signaling by Hertzian waves. The two systems differed because the concepts that inspired them differed. The two men had different ideas of what a communications system should include.

The leading idea of Lodge's system is clearly stated in the third paragraph of the application. Electrical oscillations at "a particular frequency of oscillation" were to be generated at a transmitting station. These oscillations would elicit a response, through suitable instruments, at a distant receiving station whose circuit was "capable of electric oscillations of that same particular frequency" or some multiple or submultiple of it. Other receiving stations would be unaffected, but would respond to other frequencies. Thus "individual messages can be transmitted to individual stations without disturbing the receiving appliances at other stations which are tuned or timed or syntonized at a different frequency."

So stated, the concept has a stark simplicity about it. One's first inclination is to wonder why such an obvious notion was worth mentioning. This is because we live in a syntonic age; we take for granted the idea that radio equipment is tuned, that transmitters transmit and receivers receive on specific frequencies, that not every receiver will respond to every transmitter. The concept was, however, far from obvious in 1897, and to make it the pivotal feature of a communications system implied a particular conception of what such a system should be able to do and which of its features were of prime importance. The thrust of Marconi's patent of 1896, for example, is quite different: of the 19 specific claims made in the English version of that patent there is not one that makes reference to tuning or syntony.[78] Lodge's patent application was filed before the contents of Marconi's patent were publicly known. Its purpose was not to lay claim to some detail that Marconi had overlooked. The point is a more fundamental one: Lodge regarded syntony—selective response of a particular receiver to a particular transmitter—as a necessary feature of a feasible communications system using Hertzian waves. Marconi in 1896 did not.

For this reason, to call Lodge's 1897 patent a patent on syntony is not the oversimplification it might appear to be. What is described is certainly a complete system; but the idea that gives structure to the whole is the idea of syntony. The transmitter is to emit oscillations of a specific frequency only, and the receiver is to respond only to that frequency. This has implications, in particular, for the design of antennas and for the techniques of coupling electrical oscillations into and out of the antenna. These implications are spelled out in the remainder of the patent.

Let us look first at the antennas. According to the patent, any spheres or square plates or other metal surfaces may be used to transmit or receive Hertzian waves, but in a syntonic system what is needed is a radiator or receptor that combines "low resistance with great electrostatic capacity." A particular configuration is the result. Figure 4.8 below is Lodge's pictorial representation of a pair of signaling stations, the one on the left being the transmitter, that on the right the receiver. The antenna is

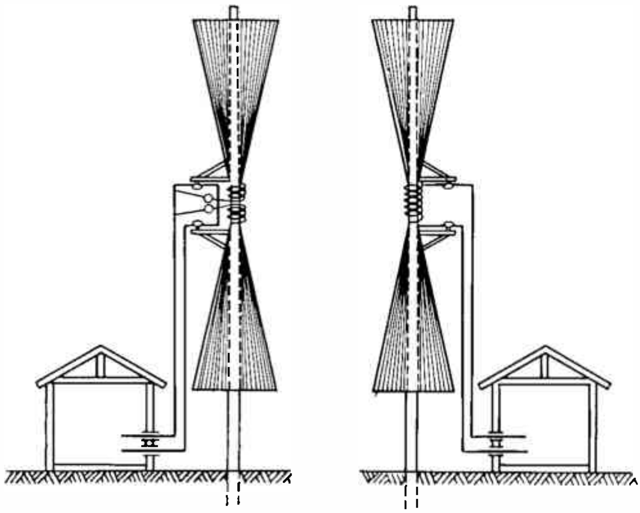


Figure 4.8 Lodge's transmitting and receiving antennas, 1897.

depicted as a pair of triangles or cones, set apex to apex on a vertical axis, each of them insulated from ground. Lodge did not refer to this as an antenna or aerial; he called it a "definite radiator" and described it as consisting of a pair of "capacity areas." These could be arranged either "as a Leyden jar" or spread out in space. They look strange to modern eyes, until one notices that the familiar "bow-tie" antenna for the reception of UHF television signals that adorns many rooftops, usually with a dish reflector behind it, is very close to Lodge's design. The key to the concept lies in Lodge's description of it as a "definite radiator." This was to be an antenna that could be tuned precisely to a specific frequency. It is quite clearly a descendant of Lodge's "syntonic Leyden jars." The inner and outer foils of the jar become the twin capacity areas of the antenna.

In his patent application Lodge stated, to cover all possibilities, that a good connection to ground could be used in place of one of the capacity areas. This would convert his design into something closer to a modern grounded vertical antenna. Elsewhere, however, Lodge made it clear that in his judgment a grounded antenna was to be avoided unless the circumstances of a particular installation made it inescapable. He had two reasons for this. First, when a grounded antenna was used for experimental purposes, it confused the theoretical issues. As he expressed it, "in my experiments . . . [I] avoided earth connexion as giving an unfair advantage from the point of view of theory. If a disturbance was detected through the earth, that wasn't the same thing as detecting it through waves in space."[79] The point at issue here was not, of course, efficiency but the "convincingness" of experiments. And in an age when many still found it difficult to believe that energy could be radiated through free space, without any intervening "ponderable matter," Lodge may well have been right. He had reason to be sensitive to experimental nuances. The Lodge who "avoided earth connexion" was the same Lodge who had used wires in his experiments on propagation and seen his results eclipsed by Hertz.

The more substantial reason for avoiding grounded antennas, however, was that they made syntony difficult. Describing antenna experiments at the Muirhead factory, for example, Lodge later reported that "What we found was that the avoidance of earth connexion assisted the definiteness and purity of the waves, prolonging the oscillations and rendering very accurate tuning possible. We found, indeed, that earth connexion spoilt the tuning by damping the waves."[80] For practical purposes the use of the earth was simpler and sometimes inevitable. And for "big distances" a grounded antenna could be very effective, since it helped to produce the "whip-crack" effect (a rapidly damped spark pulse) that was thought best for long-distance work. But if you wanted a precisely tuned wave, on a single frequency, you were better off without a grounded antenna.

As Lodge stated the matter, this criticism of ground connections was somewhat oversimplified. A grounded vertical antenna can be made as sharply resonant as an ungrounded dipole. There is nothing inherent in the ground connection to make radiation from the antenna more broad-banded than it would otherwise be. What Lodge was driving at was a rather different point: the efficiency of a grounded antenna depends significantly on the "quality" of the ground—that is, on its resistance to radiofrequency currents. Antenna designers today try to compensate for this by burying large numbers of radial wires in the ground under the vertical radiator, thus creating a standardized ground plane; in the absence of such an extensive system of radials a grounded vertical antenna can be efficient or inefficient, depending on the nature of the ground. This was a matter to which Lodge was sensitive: his early research on lightning conductors had left him highly skeptical of most conventional "good grounds."

There is nothing in Lodge's antenna, as we have described it so far, that would make it easier to tune, or less broad-banded in its frequency response, than a conventional Hertzian dipole with sheets or spheres of metal at the end of its arms, except perhaps Lodge's insistence that it have low resistance.[81] By using the "cone" configuration Lodge had distributed the capacitance along the whole length of the antenna instead of "lumping" it at the ends. But that was all. Resonance required inductance as well as capacitance. And it was what Lodge did with the inductive component of his antenna design rather than the capacitive component that represents a major innovation.

If the antennas in Figure 4.8 are examined closely, it will be noticed that the two "capacity areas" do not quite touch each other at the tips. At the transmitter they terminate in two coils of wire, each of which in turn is connected to one side of the spark gap. At the receiver there is a single coil joining the tips of the "capacity areas"; from the ends of this coil the feedline leads to the receiving instruments (see Fig. 4.9).

These coils are there to provide the inductance that converts the antennas into resonant circuits, and it was on the introduction of precisely calculated amounts of inductance that Lodge relied to tune his antennas to frequency. They are, in fact, tun-

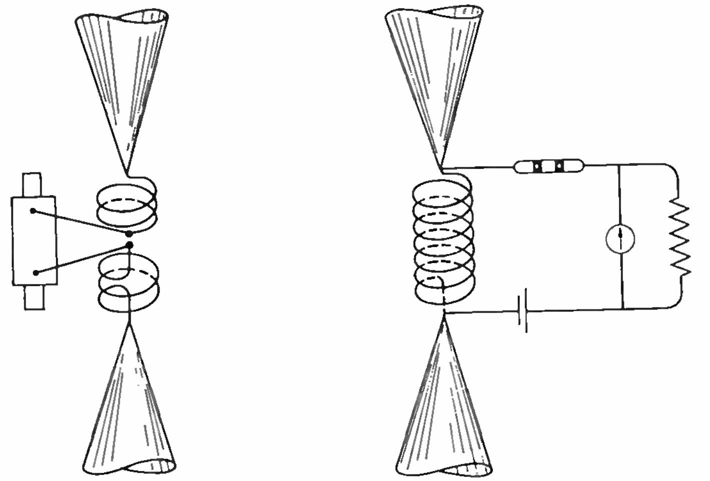


Figure 4.9 Syntonizing inductance coils. Left: transmitter. Right: receiver.

ing coils, and their presence in this patent application is one of its major claims to historical significance. We have met them before in Lodge's work. They are the analogues of the wire loops he used in his "syntonic Leyden jar" experiments and even earlier in the "alternative path" experiments that he had used to convince skeptics that the peculiar behavior of accelerating currents had to be taken into account in the design of lightning conductors. Lodge's purpose in adding these inductances to his antennas was, in fact, precisely to provide an "alternative path" for radiofrequency currents; from the spark exciter circuit into the antenna for the transmitter, and from the antenna into the coherer detector circuit in the case of the receiver. Inductance and capacitance combined to determine the resonant frequency, precisely as in the experiments by which Lodge had first detected and measured Maxwell's waves.[82]

Lodge made it explicit, in his patent specification, that the antenna induction coil—the "syntonizing self-inductance coil," as he called it—could be made adjustable at will, both in transmitter and in receiver. The "desired frequency of vibration or syntony with a particular distant station" could be obtained either by replacing one coil with another, or by using a coil with a switch designed to short out any desired number of turns. Figure 4.10, reproduced from the specifications, shows the receiving antenna coil in greater detail. In Figure 4.11 three tuning coils are shown, each connected to its own spark gap, so that one station could transmit on three different frequencies simultaneously, using the same antenna. Figure 4.12 depicts an antenna that could be used both for receiving and for transmitting, with the central inductance determining the frequency in

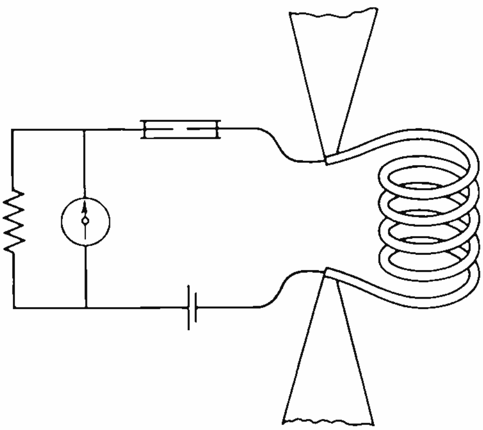


Figure 4.10 Receiving antenna inductance (detail).

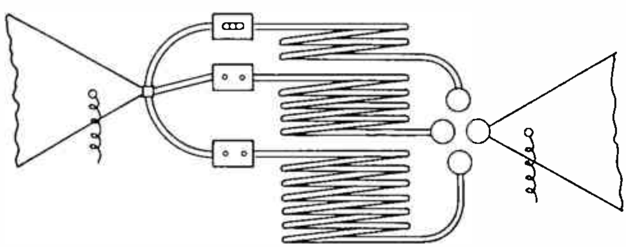


Figure 4.11 Triple transmitting inductances.

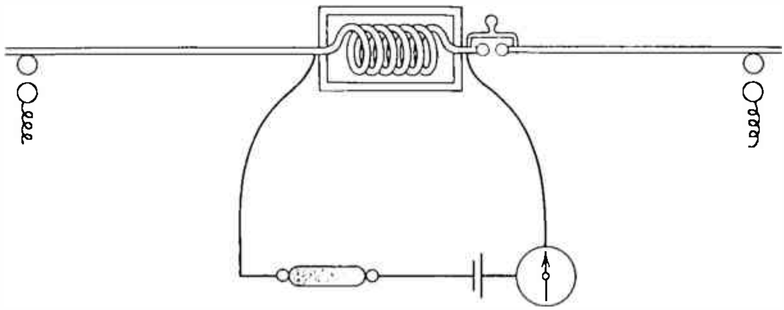


Figure 4.12 Syntonic radiator and receiver with transmit/receive switch.

each case. The small U-shaped device to the right of the coil is a transmit-receive switch, used to short out the central spark gap when receiving. When transmitting the gap is left open and the discharge is coupled from the induction coil to the antenna through the outer spark gaps.

Two of the major innovations included in this patent are already clear: the tuning of the antenna by the insertion of inductance, and the use of a variable inductance to change the tuning. The third concerns methods of coupling energy into and out of the antenna. Here Lodge's approach differs as between transmitter and receiver. At the transmitter three methods of coupling are described: first, direct coupling, in which wires are led directly from the induction coil to the spark gap and thence to the antenna; second, the use of a pair of supplementary spark gaps attached to the arms of the antenna; and third, the use of a pair of Leyden jars or other capacitors in the leads to the antenna. Lodge makes much of the differences between these methods and in his patent recommends the first—direct coupling—only when it is desired to radiate a signal that cannot be tuned out, as in distress calls. His practice did not always follow this precept. The other two are forms of capacitive coupling; Lodge called them "shock excitation" or sometimes excitation by "aerial disruption or impulsive rush" and recommended them for use in syntonic systems because "the vibrator is left to oscillate freely after receiving a blow, like a bell."[83] The simile accurately reflects the model Lodge was working with: an antenna as resonant as it could be made to be, shocked into oscillation by the spark discharge, and then left to "ring" freely at its natural frequency.

There is no mention of the possibility of inductive coupling at the transmitter. This is hard to understand, since Lodge very clearly refers to the use of a high frequency transformer when describing the receiver circuits (see Fig. 4.13). The relevant passage is an important one in the history of electronics: "In some cases I may . . . surround the syntonizing coil of the resonator with another or secondary coil . . . (constituting a species of transformer) and make this latter coil part of the coherer-circuit, so that it shall be secondarily excited by the alternating currents excited in the conductor of the resonator . . . the idea being to leave the resonator freer to vibrate electrically without disturbance from the attached wires." Once again we have the image of the freely resonating antenna. But if transformer coupling was desirable when feeding oscillations out of the antenna at the receiver, why was it not equally desirable when feeding oscillations into the antenna at the transmitter?

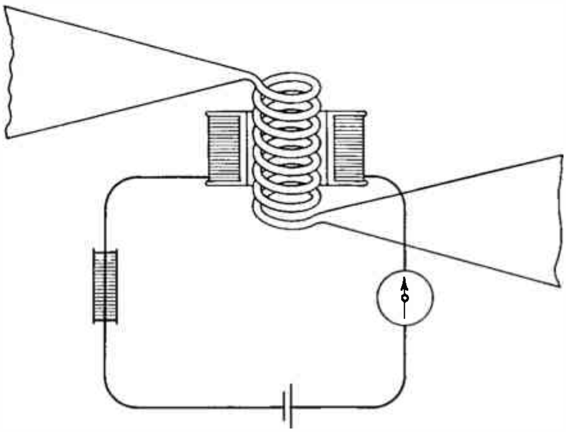


Figure 4.13 High frequency antenna transformer.

There seems no obvious answer to this minor puzzle. Nevertheless, Lodge clearly envisaged the use of the high frequency transformer, even though he did not in this patent describe its use in a transmitter circuit. This was a major contribution to the development of electronic circuits and is the third reason why his 1897 patent was to prove fundamental to later designs. There is no obvious reason why such a transformer should not be used at the transmitter, and Marconi in fact did so in his famous "four sevens" tuning patent of 1900. It may be that Lodge, seduced by his own imagery of the vibrating bell, believed that at the transmitter the antenna had to be struck violently into oscillation by the spark discharge, and that direct or capacitive coupling were the only techniques capable of doing this. At the receiver, where voltage levels were much less, inductive coupling was preferred because it imposed less loading on the antenna. Years later, discussing antenna coupling circuits in his *Talks on Radio*, Lodge explicitly granted priority in the use of inductive coupling at the transmitter to Marconi. He referred to it as the "continuous working-up method" and compared it to bringing a tuning fork into resonance by stroking it with a violin bow, instead of striking it suddenly. And he credited it with the generation of "a purer tone, more satisfactory to deal with, and easier to tune out when not wanted."[84]

To anyone familiar with the course of Lodge's scientific research up to 1897, and in particular with his experiments on syntonic Leyden jars, there would have been nothing strange or unusual about the contents of this patent. Everything follows naturally from ideas he had been working with since his research on lightning conductors. There is, of course, a new emphasis on signaling, and in consequence a new concern with efficient radiation and absorption of energy. Compromises with the "closed circuit" designs he had been working with up to that point were therefore necessary. As we have already pointed out, Lodge realized clearly that the kind of syntony possible between two coupled closed circuits could not be hoped for with open circuits that radiated energy into space or absorbed energy from it—at least, as long as spark discharges were the source of that energy. Nevertheless, syntony was the goal; selective radiation and absorption of radiation are the theme that underlies the whole patent; and the components and circuits specified are all framed in terms of that concept. As Silvanus Thompson expressed it, "What he invented was not tuning, but how to make a radiating system tunable at all. . . . He invented *tunability*."[85] The three major features of the patent were the use of syntonizing coils to tune the antennas to resonance; the use of variable inductances to alter tuning; and the use of a high frequency transformer to couple energy from the antenna to the detector circuit. All three are based on the concept of resonant circuits. All were to prove fundamental to electronic design. They remain so today.

It is important to notice that nowhere in this patent does Lodge require that any circuit other than the antenna circuit itself be tuned to the desired frequency of operation. This is true even when, as in the apparatus in Figure 4.13, two circuits are shown coupled together, one the antenna circuit, the other the detector. Lodge relied totally on resonant antenna circuits to provide syntony between transmitter and receiver.

This is rather strange, for no one had emphasized more forcibly than Lodge that an antenna which was a good radiator (or a good responder) could not be a persistent vibrator and could not be tuned to exact syntony. If that idea were in the foreground of his thinking, one would have expected him to insist that receiver and transmitter should each have at least two circuits—one a closed circuit that would be a persistent vibrator and would be tuned to a precise frequency, the other an open circuit, including the antenna and its tuning coil, that would be a good radiator or responder. Such a "two circuit" arrangement might well have struck him as a feasible method of combining the advantages of a closed circuit and an open one, providing both persistent oscillation and efficient radiation. And the high frequency transformer already specified as one possibility in receiver design could have been used to couple the circuits together. If he had followed this line of thought he would have arrived at an arrangement similar to the Marconi and Stone tuning patents (see below, pp. 247–58). It is puzzling that he did not do so, for it would have been entirely consistent with the way in which he had analyzed the problem and would have made his patent, legally and technologically, far more powerful. One cannot, however, by any stretch of the imagination, read a tuned "closed circuit" into any of these specifications.

* * *

Lodge's patent of 1897 granted him property rights in any system of signaling by Hertzian waves that used antennas tuned to resonance by an inductance, a variable inductance to change the frequency of resonance, or a high frequency transformer to couple oscillations from one circuit to another. For more than 10 years neither Lodge nor his associate, Alexander Muirhead, took any steps to prevent encroachment on these property rights by others, despite the fact that Marconi's tuning patent, granted in 1900 and used in all Marconi transmitters and receivers after that date, included all three of the features that Lodge had specified.

Why Lodge made no attempt to protect his patent rights is not easy to explain. His memoirs are not informative on the point. It is possible that he felt some conflict between his role as a scientist, committed to free dissemination of information, and the more proprietary and exclusive role in which litigation on behalf of his patents would have cast him. On one occasion he expressed this point of view forcibly:

> The instinct of the scientific worker is to publish everything, to hope that any useful aspect of it may be as quickly as possible utilized, and to trust to the instinct of fair play that he shall not be the loser when the thing becomes commercially profitable. To

> grant him a monopoly is to grant him a more than doubtful boon; to grant him the privilege of fighting for his monopoly is to grant him a pernicious privilege, which will sap his energy, waste his time, and destroy his power of future production.[86]

It is of course true that Lodge was a busy man. He had his teaching duties, with administrative responsibilities increasingly added to them; and there was his research on aether drift—an issue, as he saw it, of really fundamental importance to physical science. But there may also have been some procrastination and indecisiveness. Litigation to compel the Marconi Company to pay royalties might well have been an expensive nuisance, but Lodge's position was a strong one and, after 1901, there were his business partners, the Muirhead brothers, to share the expenses. It would have been worth trying, if only to provide a little supplementary income to the struggling Lodge-Muirhead Syndicate. It is not clear why it was not tried. Lodge's explanation in terms of a conflict with the mores of a scientist is a little too facile to be convincing.

Alexander Muirhead, as we have seen, had introduced himself to Lodge shortly after the lecture at the Royal Institution in 1894 and had provided him with some useful items of equipment for the later demonstrations at Oxford. It is possible that the patent applications of 1897 were made at his instigation. It was not until 1901, however, that the Lodge-Muirhead Syndicate formally came into existence as a limited liability company.[87] Its stated purpose was to manufacture and sell radio apparatus built from Lodge's patents, and in particular his patents on syntonic telegraphy. Commercially, it was a late arrival on the scene. By 1901 Marconi's Wireless Telegraph Company had already won a leading position in ship-to-shore communications in Britain, was reaching out for a similar position in the United States, and, with much publicity, was entering the fight for transatlantic traffic in competition with the cables. In Germany there was the Telefunken Company, using the Slaby-Arco-Braun patents and aggressively seeking to establish itself in maritime and European traffic with the strong support of the German Government. In the United States the United Wireless Company, using apparatus based on De Forest patents, operated a network of stations handling ship traffic on the Atlantic and Pacific coasts. And there were other, less formidable rivals. In comparison, the Lodge-Muirhead Syndicate was a puny infant. Its only real asset was the Lodge syntony patents, the essential features of which were already being freely used by others.

With the exception of a few licensed experimental stations, the Syndicate never became an operating company, as the Marconi Company did, in the sense of maintaining a radiocommunications service of its own. Its business was the manufacturing and selling of equipment to others. No examples of its transmitters or receivers seem to have survived. There are a few photographs; they are not very informative, as so often tends to be the case with photographs of electronic equipment. More useful are schematic diagrams of the circuits used and descriptions of the construction and functioning of particular pieces of apparatus. For information on these, the best sources are the patents themselves, an article on Lodge-Muirhead equipment published by Frederick Collins in 1903 (largely reprinted in his *Wireless Telegraphy* of 1905), descriptions of particular pieces of apparatus included by Fleming in his *Principles of Electric Wave Telegraphy* (first published in 1906), and a rather extensive presentation on "Syntonic Wireless Telegraphy" by Lodge and Alexander Muirhead to the Royal Society in 1909.

In analyzing these sources of information it is necessary to put aside any notions of mass-produced apparatus. Lodge-Muirhead equipment was built on special order for particular buyers. Improvements and refinements were added as they became available, and it is very probable that no two transmitters or receivers were ever precisely alike. Modifications were also made "in the field." The Lodge-Muirhead Syndicate did not furnish operators; it provided equipment and instructions and took pride in the fact that purchasers could install and operate the equipment themselves without further guidance. This may have been a selling point, but it also meant that individual operators were more free to adapt and modify the apparatus to suit their own ideas than was the case with, say, the more standardized Marconi equipment, operated by employees of the Marconi Company.

Lodge-Muirhead radio equipment, in other words, must be thought of as custom-built apparatus, made by a manufacturer of precision instruments to designs furnished by a university scientist. To its quality and reliability there is good testimony. Nothing is known of its cost relative to other types of "wireless."

Collins's description of Lodge-Muirhead apparatus as it was manufactured and sold in 1903 shows several changes from the designs of the 1897 patent, but they are relatively minor. Figure 4.14 shows the circuit diagrams of the transmitter. Two modifications are apparent: first, the use of high frequency transformers; and second, the insertion of capacitances in the antenna and ground leads. Collins describes the transformer as "made of a very few turns of heavy wire for the primary, which also serves

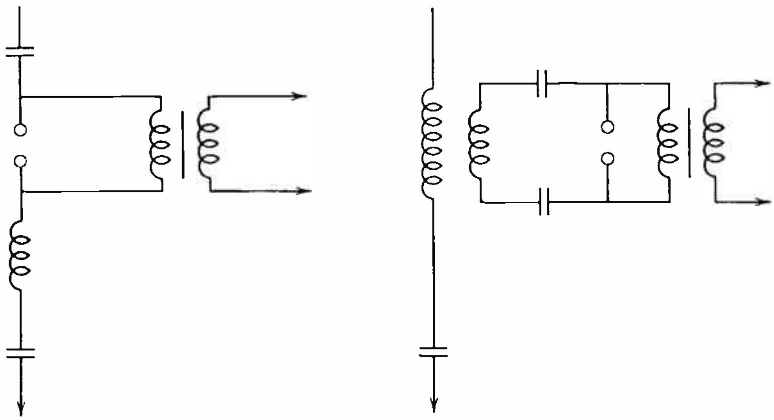


Figure 4.14 Lodge-Muirhead transmitter circuits, 1903. Left: "simple oscillator system." Right: "compound oscillator system."

as an inductance connecting the capacity areas of the resonator system . . . and a great number of turns of finer wire in the secondary."[88] And he stresses that it must be without an iron core, "as this would retard and tend to damp out currents of such enormous frequencies." In the "Simple Oscillator System," which he calls "the most efficient type," the spark gap is still connected directly between antenna and ground;- the transformer shown here is a low frequency device coupling the spark gap circuit to a source of alternating current. In the "Compound Oscillator System," however, an open (antenna) and a closed (spark gap) circuit are shown, and they are coupled together by a high frequency transformer, with low frequency alternating current fed to the spark gap circuit through a "commercial transformer." Collins does not specifically refer to the closed circuit in this case as being tuned.

Although Collins's article reproduces the illustration of the cone-shaped "capacity areas" from the 1897 patent, the schematic diagrams are labeled as having antenna and ground (earth) connections, and the text refers to one side of the open circuit as connected to ground. It is possible, though not certain, that in 1903 Lodge was having doubts about his original "capacity area" concept, which must have been decidedly awkward for use on board ship. In any event, he now took advantage of both tuning options: variable inductance and variable capacitance. Either one or two capacitors are shown in all of the 1903 "open circuits" and the text specifies that these are adjustable.

Figure 4.15 shows the receiver or "resonator" circuits. As with the transmitter, there is both a simple (direct coupled) and a compound (transformer coupled) system. The antenna is resonant in each case, as one would expect. So, less obviously, is the coherer or detector circuit. The key component here is the capacitor shunted across the battery and recording device which (in Collins's words) "permits the oscillations set up by the waves to surge through the closed circuit-resonator with a predeter-

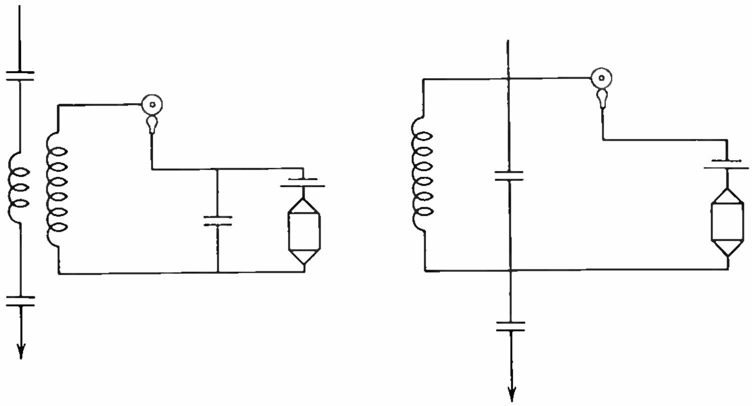


Figure 4.15 Lodge-Muirhead receiver circuits, 1903. Left: "compound resonator system." Right: "simple resonator system."

mined and definite time period until it reaches its maximum amplitude." In other words, it furnished an alternative path for radiofrequency currents and permitted Lodge to make the detector circuit as well as the antenna circuit resonant, which the battery and siphon recorder would otherwise have made impossible. Lodge's coherer patent of 1897 and the second of his two syntony patents of that year (Nos. 18,644 and 29,069) had covered this refinement.

This was, however, only one of the "ingenious applications of some absolutely novel principles" which Collins saw in Lodge's receiver designs. More conspicuous was the new "wheel coherer."[89] Lodge had always been dissatisfied with the "filings coherer" that he and Branly had devised. It was not merely a question of its temperamental performance, its susceptibility to disturbance by "atmospherics," and the need for complicated and trouble-prone gadgetry for "tapping back" or shaking the gadget after each dot or dash was received, which slowed down receiving speeds so severely. For Lodge an equally serious defect was the fact that its electrical characteristics (internal resistance and reactance) varied widely while in operation. This meant that a filings coherer could never be part of a reliable syntonic circuit, which depended on constancy of its electrical parameters to stay in resonance. Every early experimenter with Hertzian waves had good reason to search for improved radiofrequency detectors, and a great variety were in fact evolved—from Fessenden's "barretter" to Marconi's magnetic detector and finally the carbon and crystal rectifiers, immediate predecessors of the vacuum tube. But Lodge had a special reason, and for his purposes not any sensitive rectifier of high frequency currents would do.

Lodge was looking for a detector that would be mechanically sturdy, would not require "tapping back," and would have constant impedance. In 1897, in addition to the filings coherer, he had suggested use of the "point contact" type—an offshoot of his early observation that two brass knobs in light contact tended to cohere when a small spark passed between them (see above, p. 102). This, however, brought little improvement. The wheel coherer, devised and patented in 1902, was much closer to the ideal. It was self-restoring, in the sense that no mechanical "tapping back" was required. It was physically strong and stable, suitable for rough use in the field or on shipboard. And its electrical characteristics were predictable and reliable.

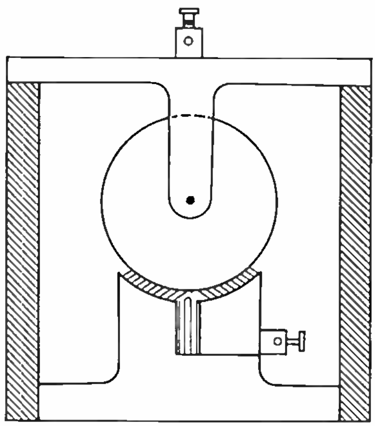


Figure 4.16 Lodge-Muirhead disc coherer.

The basic idea was simple (see Fig. 4.16). A steel disc was made to revolve slowly by clockwork. The edge of this disc was placed so that it just touched a small globule of mercury, the surface of which was covered with a thin film of paraffin oil. A dry battery maintained a potential difference of a fraction of a volt between the two surfaces. Under normal conditions there was no electrical contact between the disc and the mercury, as the oil film served as an insulator. When an electrical impulse was received from the antenna circuit, however, the oil film was pierced and a current flowed between disc and mercury, sufficient to actuate a siphon recorder or a relay. No tapping back was required, as the rotation of the disc continually returned the film to its nonconducting state. Elegant in concept, the device proved efficient, compact, and reliable in service. Lodge was proud of it, and even Ambrose Fleming, who had an understandable leaning in favor of Marconi's magnetic detector, found something to say in its favor.[90]

The next glimpse we have of Lodge-Muirhead equipment comes six years later, when Lodge and Alexander Muirhead presented a joint paper on syntonic wireless telegraphy to the Royal Society of London. Lodge, who wrote the introductory sections, was clearly uncomfortable about this paper, feeling perhaps that there was some incongruity in singing the praises of his own equipment before a prestigious scientific society. He apologizes for the "hesitation and delay" in sending in the paper; he explains that his financial interest in the apparatus has been "hitherto entirely nonexistent"; and he justifies the paper on the grounds that the measurements it reports could not have been made in a laboratory or without apparatus similar to that which he and Muirhead had developed. The thrust of the paper, however, is quite transparently not to eulogize Lodge-Muirhead equipment as such but to stress the increasing necessity for effective tuning in the design of radio equipment and the ease with which it could be secured, if only certain principles of design were respected.

Preeminent among these principles is the avoidance of earth connection. On this point Lodge shows not the slightest movement away from his earlier position: in fact, he is even more obdurate, retreating from the minor concessions to grounded antennas made in 1897. "The present trouble is caused by the utilisation of the earth as one terminal of the aerial system, both in sender and receiver. I do not expect this to be immediately admitted; but so it is—at any rate at land stations. With the use of the earth as part of the main electric vibrator no perfect tuning is possible."[91] Recognition of the "evil effect of the earth" has, he claims, become clearer since 1897; experimental results show that the influence of the earth, because of its "capricious and variable conductivity," is "wholly deleterious to accurate syntony." He is prepared to make two concessions only: when the equipment is to be used on board ship so that the hull, in direct contact with salt water, can be used as a ground; and when a "closed circuit vibrator" is used. With this latter arrangement, the oscillating closed circuit can "force the radiator to emit a tuned disturbance, even though one end of it is earthed." The simplest and most economical plan, however, is to use the same circuit—that is, the antenna—as both vibrator and radiator, and in that case it is essential that no earth connection be used.

Lodge's second concession to grounded antennas was, whether he realized it or not, a major one, for the trend of circuit design was clearly toward separating the antenna and oscillator circuits, tuning both to resonance, and coupling them with a high frequency transformer. This was the guiding principle of Marconi's tuning patent; and Lodge himself, according to Collins's account, used it in his "compound" transmitters. Lodge had, in a sense, designed himself into a corner by his insistence that the antenna had to be the frequency-determining element, for this required very sharply resonant antenna circuits and led inescapably to the conclusion that the damping effects of ground resistance had to be avoided. The addition of a second "closed" tuned circuit in the transmitter or receiver might seem an unnecessary complication and expense, but in fact it was not, since it removed the constraints on antenna design to which Lodge gave such forceful expression.[92]

The antenna described by Lodge in 1909—presumably that erected at the Muirhead factory in Kent—showed some elaboration over his earlier design but clearly reflected the same concepts. Instead of two cone-shaped capacity areas mounted vertically apex to apex, Lodge now showed an antenna made up of two large flat wire arrays mounted parallel to each other in a horizontal plane (see Fig. 4.17). Each of these was shaped, as he expressed it, like a Maltese cross: that is to say, it was made up of four wire triangles, insulated from one another except at the common vertex in the center, where the feedline was connected and, needless to say, also insulated from ground. The perimeter of each of the eight triangles was equal to one wavelength at the intended frequency of operation.

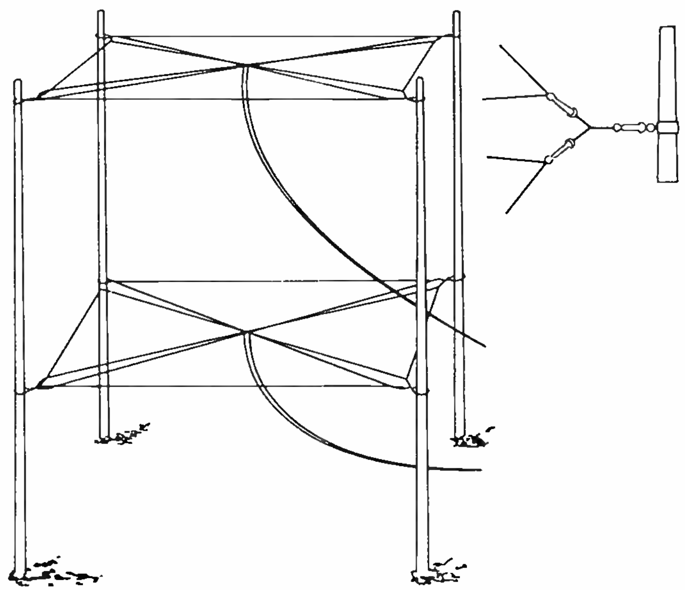


Figure 4.17 Lodge-Muirhead fixed station antenna, 1909.

Antenna design is a field in which what looks peculiar to one generation is accepted as normal by the next. At first glance Lodge's antenna of 1909 cannot but appear a very unusual and idiosyncratic device. The line of descent from the 1897 design is clear enough: we now have not two "cones" but eight, four in each plane. And the resemblance to the plates of a huge air-dielectric capacitor—which is precisely how Lodge thought of it —is evident. The antenna consists, in fact, of two flat capacity areas (the wire Maltese crosses would be the electrical analogue of flat plates), connected by long feedlines to the top and bottom of the transmitter output impedance. The frequency of operation for the tests Lodge described was 441 meters; the two antenna surfaces were between 54 and 21 feet apart vertically. This was a very small fraction of a wavelength, and it is this close spacing of the two elements that makes the configuration look strange to our eyes today. Contemporary designers would double or quadruple this spacing. Nevertheless, while such an antenna would not be notable for its efficiency as a radiator, its performance would still be passable. We would think of it as a reasonably close approximation to an "elementary doublet."[93] If the elements were spaced 47 feet apart, its radiation resistance would be a little less than one ohm; the loss resistance of the whole system would probably be not more than 10 ohms, so that an appreciable fraction of the radiofrequency energy would be radiated, rather than dissipated as heat. With even closer spacing of the capacity areas, the radiation resistance would be correspondingly less, but more than a trivial fraction of the total power supplied would still be radiated. In view of the limited knowledge of antenna theory at the time, Lodge's design was no mean achievement. The measurements reported by Lodge and Muirhead in their paper do not include field strength measurements; they do, however, indicate sharp resonance at the design frequency, which for Lodge was still the prime desideratum.[94]

The semipictorial diagram of a Lodge-Muirhead "complete installation" (Fig. 4.18) shows the receiver and transmitter circuits. The antenna is tuned to resonance at the desired fre-

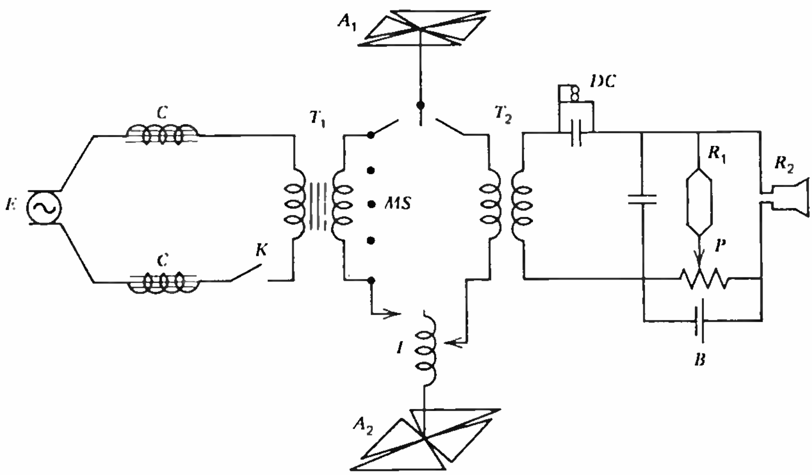


Figure 4.18 Lodge-Muirhead transmitter and receiver circuits, 1909. A_1, A_2: upper and lower capacity areas. E: alternator. C: iron-core chokes. K: telegraph key. T_1: transmitting transformer. T_2: receiving transformer. MS: multiple spark gap. I: syntonizing inductor. DC: disc coherer. R_1: siphon recorder. R_2: telephone receiver. B: battery. P: potentiometer.

quency by a small tapped inductance in the lead to the lower "capacity area"; one tap places part of this inductance in series with the primary of the receiving high frequency transformer, on the right; another connects it directly to the multiple spark gap of the transmitter, on the left. The upper "capacity area" is connected either to the receiver transformer or to the spark gaps by a transmit-receive switch. There are no variable capacitors in the antenna circuits, as there were in the 1903 equipment.

The receiver circuit, though shown with somewhat more detail, is essentially the same as the "compound resonator" described by Collins, but includes one important refinement. This is variable selectivity. The receiver is transformer-coupled to the antenna. The text makes it clear that the tightness of coupling between the windings of this transformer could be varied at will by moving the primary and secondary coils closer to each other or farther away. For very narrow selectivity—Lodge's term was "accurate tuning"—when the operator wished to receive signals from one specific transmitter only, the coils were moved far apart so that the coupling was loose. By this means, Lodge explained, "the disturbances received by the coherer can be minimized till it will only just respond." The tuned circuit was then operating "on the top of the curve." When broader selectivity was required—"if at any time it is desired to receive from other stations, or, indeed, from all stations round about"—the coils were moved closer together and the coupling tightened. A coastal station, for example, would normally be left with its receiving transformer tightly coupled, changing to loose coupling and high selectivity when communicating with one particular ship. Lodge claimed that, with loose coupling, his receivers could discriminate between signals as little as one-half of one per cent apart in wavelength—a degree of selectivity that he considered "exceedingly remarkable."

The secondary or coherer circuit of the receiver was also tuned to the operating frequency. The coil in the secondary of the coupling transformer provided the fixed inductance while a large variable capacitor in parallel with the wheel coherer made it possible to bring the circuit into resonance. Another capacitor in parallel with the siphon recorder, telephone instrument, call-up bell, and battery provided a path for high frequency currents around these components. Lodge explicitly described this section of the receiver as representing "my old syntonic Leyden jar experiment." The impulses received from the antenna system were made to "accumulate" in the tuning capacitor until they could "overflow" into the coherer. The words and imagery were those he had used 15 years before.

The salient features of the transmitter circuits are not quite so self-evident. Low frequency alternating current is generated by an alternator and fed directly into a circuit containing the sending key, a switch, two adjustable iron-cored chokes, and the primary of a coupling transformer (or Ruhmkorff coil, as Lodge still preferred to call it). This circuit is resonant, but not at the operating frequency nor indeed at any frequency related to the operating frequency.[95] If, as was probably the case, the alternator generated current at a frequency of 50 or 60 Hz, that would be the resonant frequency of this circuit. Its function was to supply energy to the spark gap circuit through the transformer, and it was only in the spark gap circuit that radiofrequency oscillations were generated. The alternator is, in short, a low frequency alternator and the coupling transformer a low frequency transformer. There are two coupled resonant circuits, but their resonant frequencies are not related. Lodge expressed it succinctly: "the frequency of this alternator is in tune with its circuit, and likewise in tune with the secondary circuit of the sending transformer T connected to the aerial. But when the spark occurs it short-circuits the aerial through the spark gaps MS and confers upon it a totally different frequency, enormously more rapid."

Lodge lays great stress on the design of the coupling transformer and in particular on the need for wide spacing between its primary and secondary windings in order to reduce capacitive coupling between the two resonant circuits to a minimum. The reason is obvious: it was essential that the spark gap circuit should oscillate independently. If we ask what determines the frequency of oscillation of the spark gap circuit and therefore of the transmitted signal, we receive Lodge's classic answer: the antenna itself, brought to resonance at the desired frequency by the tapped inductance. We are back, in other words, at a simple direct coupled spark gap circuit. There is no high frequency transformer in this transmitter. There are no filtering circuits between the spark gap and the antenna. The frequency determining parameters are the capacity areas and their associated inductances. To quote Lodge again: "It is not to be supposed that these sending alternations are of the frequency of the Ruhmkorff or of the alternator; they are of a totally different order; they are the alternations proper to the aerials . . . . The tuning of the alternator and the Ruhmkorff only enables them to be maintained."

In some respects this transmitter design resembles that which Ambrose Fleming had developed for Marconi's Poldhu transmitter eight years before (see below, pp. 262–64). Fleming's design, however, contained no less than two tuned radiofrequency circuits and two high frequency transformers between the alternator and the antenna. Lodge's contained none. It is strange to find a man who laid such stress on selectivity and precise tuning in his receivers paying so little respect to the generation of a stable narrow-banded signal at the transmitter. Lodge's response would be, of course, that high frequency transformers wasted energy; that his transmitters would radiate signals at a frequency determined by "the alternations proper to the aerials"; and that he had devoted great time and trouble to insuring that his aerials would be resonant at one frequency only. This approach, however, placed undue reliance on the electrical characteristics of a large, exposed, and complex wire antenna system to maintain a stable frequency; it ignored the probability that an antenna resonant on one frequency would also be resonant at harmonics and subharmonics of that frequency; and it overlooked the fact that a spark gap coupled directly to an antenna would inevitably radiate not one frequency but a whole series of related frequencies.

Analysis of the transmitter circuit suggests strongly that, although in his receivers Lodge may have taken great pains to reconcile syntony with spark, his transmitters hardly made that goal easier to achieve. Far less than full advantage was taken of the insights into the theory and uses of syntonic circuits that Lodge himself had made. A Lodge-Muirhead transmitter, to put the matter bluntly, can hardly have been other than a prolific source of interference. And there is supporting evidence to this effect. Henry Muirhead, testifying before the Select Committee on the Radio Telegraphic Convention in 1907, was incautious enough to boast that, when a Lodge-Muirhead station began transmitting, everybody else shut up—a claim for which he was sharply questioned by the Committee. And Lodge in 1909 admitted that, although his transmitters were low powered, "the radiation is so strong from an open circuit radiator that in places I fear we perturb the Admiralty. We can tune them out quite easily, but we are informed that our radiation cannot be tuned out, their idea appearing to be that it is too strong." This charge of course he denied, suggesting that the real problem lay in the fact that the Admiralty stations, with Marconi equipment, used an earth or sea connection and therefore could not tune their antennas properly. "Our radiation," he insisted, "is of one perfectly definite wave-length, and of that alone."[96]

It is hard to understand how a spark transmitter built according to the Lodge circuit of 1909 could radiate just "one perfectly definite wave-length," no matter how sharply resonant its antenna. A clue to the reason why Lodge, no innocent in these matters, believed that it might be so is provided by his description of the type of spark used. He insisted, as we have seen, that the spark gap circuit should be free to resonate at the frequency determined by the constants of the antenna and its tuning coil. The function of the transformer that coupled it to the alternator was merely to supply energy, not to influence the frequency. In these circumstances, he claimed, the spark became almost continuous and came to look almost like an arc, instead of a series of intermittent discharges. This he called not a "simple snap" but a "maintained spark," and he went to some pains to stress that it could be precisely tuned. "It has sometimes been said . . . that a spark is incapable of being tuned. A short snappy spark is incapable, but that is not the sort of spark that we employ. On the contrary, we use a comparatively long-continued flame-like spark, between points rather than knobs; and we take it in an enclosure, so that the ionized air escapes immediate dissipation and is for the necessary time practically a conductor." Analysis of the spark discharge by means of rotating mirrors showed it to be "a long beaded band," not a rapidly quenched discharge.

"The old snappy spark or whip-crack of Hertz . . . is unsuited for a syntonic station." Lodge's new "maintained spark," in contrast, could be tuned.

We shall later have occasion to notice, when analyzing the Marconi "disc discharger" installed at the high-powered Clifden station in 1907, references to a continuous discharge, very like an arc, that, according to Marconi and his advisers, was capable of generating a true single-frequency sine wave output. It is possible that Lodge's multiple spark gap and tuned spark gap circuit of 1909 accomplished much the same result. Marconi, to make the signal generated by his discharger detectable by coherer-equipped stations, had to deliberately break up the wave train into pulses. Lodge, in the same way and for the same reason, had to introduce what he called a "buzzer" in the primary of the coupling transformer.[97] It may be, therefore, that Marconi with his disc discharger and Lodge with his sharply tuned spark gap circuits both came close to doing what seemed impossible: the production of pure sine wave output from a spark discharge. Marconi, however, filtered the output of his discharger through resonant circuits before feeding it to the antenna; Lodge did not. A certain skepticism about Lodge's claim to be radiating only "one perfectly definite wave-length" is therefore still indicated. It is possible that Lodge's work on syntony and resonance was exploited and used more effectively by Marconi and his designers than by Lodge himself.

* * *

No complete record of sales of Lodge-Muirhead equipment has survived, but it is possible to list the most important installations made before the Syndicate went out of existence in 1911. There were, in the first place, the two stations owned and operated by the Lodge-Muirhead Syndicate itself under an experimental license issued by the Post Office. These were at Elmer's End, near Beckenham, Kent, where the Muirhead factory was located, and at Downe, a village 8 1/2 miles away. The only other installation in the United Kingdom was owned and operated by the Midland Railway, under a commercial license, for communication with their steamers running between Heysham and the Isle of Man. Early in the history of the Syndicate (before 1903) two cable ships belonging to the Eastern Extension Telegraph Company, the *Patrol* and the *Restorer*, were equipped with Lodge-Muirhead apparatus before their departure for the Far East where they were to lay a cable for the Dutch Government. These were later supplemented by two shore stations, owned by the same company, at Singapore and Hong Kong, used to communicate with the cable repairing ships. In the Caribbean, Lodge-Muirhead equipment was installed in a government station on the island of Trinidad and used to maintain a radio link with a sister station on the island of Tobago, about 30 miles away. At Lagos in West Africa the African Direct Telegraph Company maintained a station with Lodge-Muirhead equipment, under an experimental license. And lastly—an installation of which Lodge and his partners were particularly proud—the Government of India operated a radio link between the mainland of Burma and the Andaman Islands, a distance of some 300 miles.[98]

This is not a very extensive list, and pales into insignificance beside the hundreds of stations using Marconi, Telefunken, or De Forest equipment by 1911. It is possible, of course, that some installations are not included. Both Lodge and Henry Muirhead, however, when testifying before the Select Committee on the Radio Telegraphic Convention in 1907, were given ample opportunity to describe where their equipment had been successfully used. Nothing in their testimony would lead one to believe that the list should be greatly extended beyond the half-dozen installations mentioned. It seems impossible to escape the conclusion, therefore, that whatever its other merits, Lodge-Muirhead radio equipment was not a commercial success. Why was this?

A reasonably satisfactory answer has to include at least three elements: the characteristics of the product itself, the nature of the available market, and the quality of the Syndicate's management. On the first point there is little uncertainty: Lodge-Muirhead equipment was manufactured to very high standards, it was sturdy and reliable, and it could be operated by personnel without special training. There is also little room for argument about what particular communications functions it was and was not designed to perform. It was intended for low-powered operation over relatively short distances; and its principal claim to excellence lay in the selectivity of its receivers. As Lodge expressed it in 1909, "with extremely great distances Mr. Marconi has chiefly dealt. My object has been to perfect the tuning for moderate distances."[99] Testifying in 1907, he claimed low-powered operation as a virtue: "Our power is insignificant. . . . We are only using the power that is necessary to go the distance." And he invited the Select Committee to consider the advantages of a system of many small stations rather than a few powerful ones: "If their power is limited they cannot produce so much disturbance. If they are very large stations which have to shout a long way they are more likely to interfere with each other than a lot of little ones would. I would rather see a lot of little ones for coast purposes."[100]

Such a philosophy was in striking contrast to that followed by Marconi. Thinking in these terms, Lodge produced receiving equipment that would be at its best in areas where the spectrum was crowded, where many transmitters competed with each other, where interference was the operator's major problem. This was certainly not the state of affairs in Singapore, Hong Kong, Lagos, Trinidad, or the Andaman Islands. In these locations the principal strength of Lodge-Muirhead receivers, the accurate tuning and precise syntony in which Lodge took such pride, was literally unnecessary.

Where Lodge-Muirhead equipment would have been at its best was in the crowded waters of the English Channel and the Western Approaches, where a score of ship transmitters and shore stations might all be clamoring for attention in a narrow band of frequencies at the same time. And there is irony in the fact that this is precisely the market into which Lodge-Muirhead equipment was unable to make the slightest penetration. By 1903, when Lodge-Muirhead equipment began to become available, the ship-to-shore and ship-to-ship radio business in these waters was almost completely under the control of the Marconi Company, partly because it had been first in the field, partly because of highly effective publicity, and partly because of an exclusive and very advantageous contract with Lloyd's of London. Against this entrenched competition Lodge and Muirhead would in the best of circumstances have had difficulty making headway.

But the near impossibility of breaking into the marine radio market stemmed not only from economic causes. After 1904, under the terms of the Telegraph Act of that year, no radio station could be operated in the United Kingdom without a license from the Post Office. When the Lodge-Muirhead Syndicate sought licenses to set up stations on the south coast of England, their applications were denied, on the grounds—which must have seemed ironic to Lodge—that they would cause interference with already established Marconi stations.[101] The result was that, in the market for which its equipment was technically best suited, the Lodge-Muirhead Syndicate never established the smallest foothold. Only from colonial governments, not under the jurisdiction of the British Post Office and functioning in areas less dominated by the Marconi Company, did the Syndicate manage to find a toehold.[102]

Also involved is a question of commercial strategy. This is not the place to analyze how the Marconi Company managed to achieve its dominant position, but it is relevant to note that as long as it defined its business role as merely that of manufacturing radio equipment for sale to others, it likewise found its market extremely limited. Rapid growth in volume of business began only when it transformed itself into an operating company, providing a complete communications service to which others could gain access by leasing Marconi equipment and operators. If it had not taken this step, the Marconi Company might for many years have found itself facing the same kind of narrow market as did the Lodge-Muirhead Syndicate: government agencies, telegraph companies looking for a way to supplement their wired systems, possibly the Admiralty and the War Office, and not much else. It was necessary, in this early phase of radiocommunications, to provide not merely equipment but also a communications system in which equipment could be used. The Marconi Company did this; the Lodge-Muirhead Syndicate did not.[103]

To some extent the difference in strategy was a matter of timing. When the Marconi Company began building its network of coastal stations in 1900, no licensing policy had been laid down and there were no competing systems to demand that their interests be protected. By 1904 the need for a licensing system had become unmistakable, although the form it should take was still undecided. In the circumstances, and pending resolution of international disagreements, the Post Office seems to have decided that the safest course of action was inaction—that is, to issue no more licenses except for experimental purposes. An indirect effect of this was, of course, to protect the established Marconi system from competition. It is also true, however, that the marine radio service built up by the Marconi Company since 1900 had reached a respectable level of technical efficiency by 1904; that, following established company policy, no ship with Marconi equipment aboard would have communicated with a Lodge-Muirhead shore station, even if one had been established; and that the advent of a rival system would have led to greatly increased levels of interference, both accidental and deliberate.

But there were also very real differences between the two organizations and their ways of doing business. The Lodge-Muirhead Syndicate was, both for Lodge and for the Muirhead brothers, a sideline. For the one it was a doubtless interesting but surely often unwelcome distraction from teaching, university administration, and the research that was the true calling of a scientist. For Alexander and Henry Muirhead it was an outgrowth of the craft in which they excelled and in which they had made their reputation: the manufacturing of high-quality telegraphic equipment.[104] None of the principal members of the Syndicate, in short, ever gave it his undivided attention; to none was it a matter of deep personal commitment; none depended on it for a career or reputation. The contrast with the Marconi Company, and in particular with Guglielmo Marconi, its driving spirit, is striking. Technically, Lodge and the Muirhead brothers were at least as knowledgeable and well equipped as Marconi. Organizationally, they began with functioning manufacturing facilities and established business connections in the communications industry. What they did not have was his vision of the future of radio, his drive for achievement, his deep personal commitment, and his flair for publicity. Marconi would not have been at home in the little factory at Elmer's End; and one imagines that neither the Muirheads nor Lodge would have been comfortable working for Marconi.[105]

* * *

In 1901, when the Lodge-Muirhead Syndicate was formed, it had only one substantial asset: Oliver Lodge's patents on syntonic radiotelegraphy. Ten years later it had no more, except perhaps a handful of satisfied customers who, having once bought radio equipment, showed little inclination to increase their investment. There were also a very few organizations—the obscure Helsby Wireless Telegraphy Company, for example—that had gone to the trouble of obtaining licenses to use Lodge's patent and were paying royalties.[106] The leading firm in the industry, however, the Marconi Wireless Telegraph Company, not only denied that it was infringing on Lodge's patent but on at least one occasion had warned an important potential customer, the British War Office, not to buy Lodge-Muirhead apparatus on the ground that it infringed Marconi's tuning patent of 1900.[107]

Two sequences of events combined to break this impasse. First, Lodge secured an extension of his patent and began to take vigorous action to enforce his rights under it. British law made it possible for a patent holder, if he could show to the satisfaction of the courts that he had been insufficiently rewarded during the initial 14-year term of his patent, to petition for an extension for an additional 7 years. Lodge's patent would, in the normal course of events, have expired in 1911. He secured legal counsel and—as is indeed not surprising—had little trouble demonstrating that the compensation he had received since 1897 had been insignificant. The court, in the person of Mr. Justice Parker, granted him an extension. Fortified by this legal victory, Lodge began to mobilize the support of other patent-holders and put together a "fighting fund," amounting eventually to some £10,000, to finance legal action against the Marconi Company to compel it to obtain a license and pay royalties under the extended patent. The Marconi Company entered a countersuit for infringement against the Syndicate.[108]

There, for a short time, the matter rested. The Marconi Company, however, was not at this juncture in a position to allow the validity of its patents to remain for long in doubt. In the United Kingdom its position had seemed secure. For more than 10 years Marconi's possession of the "four sevens" tuning patent of 1900 had been enough to prevent a legal confrontation with Lodge and the Muirhead brothers, who had neither the funds nor the disposition for court battles that might prove long and complex. Now, with Lodge finally bestirring himself and his patent extended for another seven years, a patent position that had appeared safe suddenly seemed at hazard. Marconi's patent went beyond Lodge's, it was true; but it used circuits that were clearly specified in Lodge's patent and in that sense it was derivative. In other countries, where increasingly serious competition was being encountered, the Marconi position was, in terms of patents, even more questionable. In Germany the Telefunken organization was prepared to admit infringement of Lodge's patent but not of Marconi's. In the United States, Marconi's application for a tuning patent had at first been rejected by the Patent Office on the grounds that it had been anticipated by John Stone Stone. Eventually it was allowed, but on the sole ground that Marconi showed the use of a variable inductance for tuning the antenna circuit while Stone, in the opinion of the Examiner, tuned his antenna circuit by adjusting the length of the antenna itself. And as if this were not in itself flimsy enough grounds on which to base further litigation, there was the awkward fact that Lodge's United States patent, issued before Marconi's but apparently overlooked by the Examiner, clearly showed the use of a variable inductance.[109] Both in Germany and in the United States, in short, the patent position of the Marconi Company was beginning to seem decidedly shaky. What was needed was control of the patent which underlay Marconi's: Lodge's syntony patent of 1897.

Godfrey Isaacs, who had taken over as managing director of the Marconi Company in 1908, knew little about radio but a great deal about finance and litigation. He was determined to take action to defend the Marconi Company's patent rights—as a defensive measure against Telefunken in Europe and Australasia, more aggressively in the United States, where the shaky financial position of his chief competitors, United Wireless and the National Electric Signaling Company, held out attractive prospects of a quick takeover, if only favorable judgments in patent infringment suits could be secured. And he had other irons in the fire, potentially vital to the future of the company. In March 1910 he had submitted to the Colonial Office an ambitious scheme for linking the British Empire by an "Imperial chain" of radio stations—18 originally, in Egypt, India, Malaya, China, Australia, and Africa, as well as in the British Isles—to be erected, maintained, and operated by the Marconi Company. Preference had later been expressed in Parliament for a state-owned system, but the Marconi Company was to be the chosen instrument for its construction. By the autumn of 1911 negotiations with the Post Office looking toward a draft contract were well under way.[110]

This was, in short, a highly inconvenient time for any questions to be raised about whether the Marconi Company's patents could withstand legal challenge. In Britain any draft contract for the "Imperial chain" would have to be submitted to Parliament for approval, and it was certain that, both in committee and on the floor of the House, it would be subjected to the most critical scrutiny, as would the commercial position of the company that proposed to build it. Isaacs wanted no disconcerting questions raised at that point. Elsewhere, in the United States and Germany, he was relying on patent infringement suits to eliminate competitors, or at least bring them to terms. For that strategy to be successful, ownership of Lodge's patent of 1897, in addition to Marconi's of 1900, was essential. The Marconi Company had to have it, not indeed to operate radio stations, but to win court decisions.

Bringing the two sides together cannot have been easy. Lodge, who by this time seems to have got the bit between his teeth, was determined to prosecute his suit against the Marconi Company and, fully confident that the courts would support him, was not disposed to consider compromise or an out of court settlement. The man who played the role of honest broker was William Preece, former chief engineer of the Post Office. In this there was a certain irony, though one doubts whether either Lodge or Isaacs appreciated it. Preece and Lodge had found themselves on opposite sides of the fence many times in the past, at least since their first public disagreement over lightning conductors, and relationships had not been improved by the prominent role Preece had played in sponsoring Marconi's system and bringing it to the attention of the Post Office, the War Office, and the Admiralty. By 1911, however, Preece was no longer the staunch ally of Marconi's interests he had once been. A few years before, he had publicly characterized the Marconi Company as "the worst managed company I have ever had anything to do with"—referring not to its technical competence but to the way its managers did business. The practices he found objectionable had not changed under the regime of Godfrey Isaacs. On the other hand, Preece, through his Post Office contacts, was undoubtedly well briefed on the delicate state of the "Imperial chain" negotiations, and it is at least possible that, semiofficially, he was still representing the Post Office, and through it the British Government, in his attempt to bring order out of the confusion of patent infringement suits.

Preece it was, in any case, who brought Lodge and Isaacs together, and it was through his good offices that, on 21 October 1911, a compromise was finally hammered out and accepted by both parties.[111] Its terms were straightforward. Lodge's patents were purchased by the Marconi Company for a cash payment of undisclosed amount, and the Lodge-Muirhead Syndicate was dissolved. Lodge himself received a stipend of £1000 a year for the remaining life of the syntony patent, and accepted a nominal position as scientific adviser to the company.

Godfrey Isaacs was not a man reluctant to push a lawsuit through to final judgment, if he thought he could win. The fact that he was willing to negotiate with Lodge suggests that he and his technical advisers recognized that the Marconi Company would probably be the loser if the infringement suits were allowed to proceed. It had, in fact, already lost the major battle when Lodge's patent was extended instead of being allowed to expire. The patent of 1897 was basic to any radio system that used tuned circuits. All other tuning systems were either derived from it or duplicated it. This was realized in 1911 when the Marconi Company sought to consolidate its competitive position. It was confirmed many years later, in 1943, when, in a decision by the U.S. Supreme Court, Lodge's patent was the only one of the three principal Marconi Company patents to be completely upheld, the Marconi tuning patent, once the keystone of the company's patent structure, being declared invalid.[112] Such an outcome would not have surprised Oliver Lodge. He might have wondered, however, being at root a simple man, why, when the truths of natural science were so simple and harmonious, it took men of business almost half a century to decide who had the right to draw income from his discoveries.

Notes

1. Oliver Lodge, "On the Theory of Lightning-Conductors," *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, Vol. 26 (August 1888), pp. 217–230. Lodge later claimed that this article should have appeared in the July issue, adding that "in those days months were important." Publication in July, the month in which Hertz's article appeared, would have greatly strengthened Lodge's claim to simultaneous discovery. See Lodge, *Advancing Science* (New York: Harcourt Brace, 1930), p. 111. For a comprehensive general biography of Lodge, see W. P. Jolly, *Sir Oliver Lodge: Psychical Researcher and Scientist* (London: Constable, 1974).
2. Lodge, *Talks About Radio* (New York: Doran, 1925), p. 39. The decision had been made, according to Lodge, while he was a student at Heidelberg.
3. *The Electrician*, Vol. 21 (September 1888), pp. 607–608; Oliver Heaviside, *Electrical Papers*, 2 vols. (New York: Macmillan, 1894), Vol. 2, pp. 488–490. Compare Lodge, *Advancing Science*, pp. 112–114.
4. Lodge, *Talks*, p. 46.
5. One cannot read Lodge's articles and his autobiography without acquiring the conviction that he never seriously questioned Maxwellian theory. He knew how his experiments *ought* to turn out. This contrasts sharply with the impression one gathers from Hertz's writings; here the feeling is one of suspended judgment until the measurements are made. In Thomas Kuhn's terminology, Lodge was carrying on "normal science" within the Maxwellian paradigm; Hertz was concerned with the choice between paradigms (although in a more considered, "objective" manner than Kuhn would have us believe is normal). See Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). This may be what Lodge was hinting when he wrote that, in 1888, Hertz was "not at that time fully acquainted with Maxwell's theory" and "does

not seem at first fully to have realized what he was doing" (Lodge, *Talks*, pp. 44–45).

6. Sir Oliver Lodge, *Past Years: An Autobiography* (London: Hodder and Stoughton, 1931), pp. 153–154.
7. Lodge published his findings in the *Philosophical Magazine* (March 1884) and in *Nature*, Vol. 29, p. 610, and Vol. 31, p. 265. His discovery that the dust-free space could be greatly extended if an electric charge was placed on the object anticipated the electrostatic dust precipitators used in industry today. See fn. 58 below and also A. D. Moore, Ed., *Electrostatics and Its Applications* (New York: Wiley, 1973), pp. 180–220.
8. Lodge, *Advancing Science*, p. 88.
9. Preece, author of several books on telegraphy, electricity, incandescent lighting, and the telephone, was an early experimenter with inductive telegraphy. He joined the Post Office as electrician in 1877 and, after 1896, became an early and influential sponsor of Marconi. For a sketch of his career, see Orrin Dunlap, Jr., *Radio's 100 Men of Science* (New York: Harper, 1944), pp. 71–73.
10. That is, if the circuit resistance were low enough. High resistance in the circuit would result not in oscillations but in a "dead-beat" or highly damped return to equilibrium after the initial disturbance.
11. Lodge assumed at the time that the cloud was a conductor and that the discharge was oscillatory. Later he recognized this as an error. The lightning flash was normally an intermittent discharge in a single direction. As far as inductive reactance to accelerating currents was concerned, the difference was immaterial. See Lodge, *Advancing Science*, p. 96.
12. Lodge, *Advancing Science*, p. 94.
13. Lodge's experiments and recommendations on this subject are conveniently brought together in his *Lightning Conductors and Lightning Guards* (London: Whittaker and Co., 1892), but see also "On the Theory of Lightning-Conductors," and, for his report of the Bath meeting of the British Association which saw the confrontation between the scientists who believed in self-inductance and the engineers who did not, *The Electrical Engineer* (New York), Vol. 7 (November 1888), pp. 533–538. The controversy would make an interesting case study in the conflict between laboratory culture and shop culture to which Monte Calvert has called our attention; see his *The Mechanical Engineer in America, 1830–1910* (Baltimore: Johns Hopkins University Press, 1967).
14. Lodge, *Lightning Conductors*, p. 60. Compare W. H. Eccles, *Wireless* (London: Butterworth, 1933), pp. 34–35.
15. Lodge, "On the Theory of Lightning-Conductors," p. 227. "Six times the square root of inductance times capacitance" was Lodge's rule of thumb for calculating the period of oscillation. In view of the approximations involved in the measurement of the two variables in those days, to use 6 for 2π was certainly acceptable.

16. Lodge, "On the Theory of Lightning-Conductors," p. 228. His mention of a string is a reference to the famous "Melde experiment" in which a taut string, half a wavelength long, is attached to a vibrating tuning fork and standing waves observed in the string. Knowledgeable readers will recognize in Lodge's experimental arrangement what later came to be known as "Lecher wires," an early and quite precise method for measuring wavelengths at very high frequencies.

17. Lodge's later preference for "syntony" was based on his feeling that "the essential thing to be connoted is the synchronizing of the vibration-period of two things." See Lodge, *Modern Views of Electricity*, 1892 ed. (London: Macmillan, 1889), p. 337, and Silvanus P. Thompson, *Notes on Sir Oliver Lodge's Patent for Wireless Telegraphy* (London: Waterlow and Sons, Ltd., 1911).

18. Heaviside, *Electrical Papers*, Vol. 2, pp. 488–489, a reprint of a communication to *The Electrician*, October 19, 1888: "the experiments of Prof. Lodge, sending waves of short length into a miniature telegraph circuit."

19. Lodge, *Lightning Conductors*, p. 108. For open wire lines, the velocity factor (the ratio of the actual velocity along the line to the velocity in free space) usually averages 0.975.

20. His formal memoir, "On the Electromagnetic Theory of the Reflexion and Refraction of Light," presented to the Royal Society in 1878, is still regarded as an important contribution to classical electromagnetic theory.

21. Lodge, *Talks*, p. 40.

22. Joseph Larmor, Ed., *The Scientific Writings of the Late George Francis FitzGerald* (London: Longmans, Green, 1902), p. 92.

23. Larmor, *Writings*, p. 93.

24. For FitzGerald's status in the scientific community, see Larmor, *Writings*, p. xxiv (obituary from *Nature*, March 7, 1901).

25. Lodge, *Past Years*, p. 140 and *passim*.

26. Larmor, *Writings*, pp. 99–101.

27. Lodge, *Past Years*, pp. 111–112.

28. Larmor, *Writings*, p. 100. At the 1883 meetings of the British Association FitzGerald took the further step of suggesting the use of discharges from Leyden jars; in 1888 Lodge describes him as having "pounced on" Hertz's newly published work as the subject for his presidential address to Section A of the Association—an address which (again in Lodge's words) amounted to a "glorification" of Hertz. This is the closest to evidence of personal pique that one can find in Lodge's writings; if indeed FitzGerald's advice had resulted in diverting Lodge from his original research objectives, some small symptoms of resentment are excusable. See Lodge, *Talks*, p. 41, and *Advancing Science*, pp. 98 and 101.

29. Lodge, *Lightning Conductors*, p. 110.

30. As Lodge later expressed it, Hertz "was not then, like me, imbued with Maxwell's doctrine, and was not on the look-out for waves; but he pursued the subject with unexampled skill, and presently he found that his results could all be understood on Maxwell's principles; so that he rapidly became an enthusiastic disciple, the first, I think, in Germany" (*Advancing Science*, pp. 97 and 111). That Hertz was not "imbued with Maxwellian theory" in the way Lodge was may be true; that in 1887 he was not "on the look-out for waves" is nonsense.
31. Lodge, *Advancing Science*, p. 92.
32. Lodge, *Modern Views*, pp. 243–244.
33. Lodge, *Advancing Science*, p. 94.
34. Lodge, *Modern Views*, p. 307.
35. Lodge, "The Discharge of a Leyden Jar," delivered at the Royal Institution of Great Britain, 8 March 1889, reprinted in Lodge, *Modern Views*, pp. 359–383.
36. Lodge, *Modern Views*, p. 302.
37. Lodge, *Past Years*, pp. 182–183.
38. See the references in Lodge, "The History of the Coherer Principle," in *Signalling Without Wires* (New York: Van Nostrand, 1902), pp. 73–114, and J. A. Fleming, *The Principles of Electric Wave Telegraphy* (London: Longmans, Green, 1908), pp. 355–357. All subsequent references to these works are to the editions specified in this footnote.
39. J. J. Fahie, *A History of Wireless Telegraphy* (New York: Arno Press reprint of 1901 edition, 1971), pp. 305–311, "Researches of Prof. D. E. Hughes, F.R.S., in Electric Waves and Their Application to Wireless Telegraphy, 1879–1886." Hughes was born in England in 1831 but received his early education in Kentucky and became professor of music at the University of Kentucky. He invented a printing telegraph in 1855 that proved highly profitable, but is probably better known as the inventor of the carbon microphone, which is itself based on the principle of changing resistance among carbon granules when exposed to the fluctuating pressure of sound waves. He died in 1900, leaving a large fortune. See A. V. Howard, *Chambers's Dictionary of Scientists* (New York: Dutton, 1951), p. 230; Ellison Hawks, *Pioneers of Wireless* (London: Methuen, 1927), pp. 168–174; and Dunlap, *Radio's 100 Men*, pp. 63–64.
40. Fahie, *History*, pp. 292–304, Appendix C, "Variations of Conductivity Under Electrical Influence: Substance of a Paper by Prof. E. Branly, of the Catholic University of Paris."
41. Fahie, *History*, p. 203.
42. Readers interested in manufacturing their own coherers will find full instructions in Henry B. Davis, "The Coherer," *Popular Electronics* (May 1967), pp. 47–50.

43. H. Poincaré and Frederick K. Vreeland, *Maxwell's Theory and Wireless Telegraphy* (New York: McGraw, 1904), p. 228.

44. This was not true of point-contact types such as Lodge's knob coherer or Branly's later tripod coherer, which acted as rectifiers. Detectors of this type were used by American soldiers during World War II; a rusty razor blade, a steel needle, a coil of wire, a pair of headphones, and a substantial dose of ingenuity could be made into an acceptable receiver of strong local AM signals. Lodge himself reported that, during his experiments with the knob coherer at Liverpool, he "could pick up lots of communications from ordinary telephone lines, and used to hear people ordering potatoes for dinner, and other absurdities." Lodge to J. Arthur Hill, 5 July 1915, in J. Arthur Hill, Ed., *Letters from Sir Oliver Lodge* (London: Cassell, 1932), pp. 83–84.

45. Lodge, "On the Discharge of a Leyden Jar," reprinted in *Modern Views*, pp. 359–383.

46. Thompson, *Notes on Sir Oliver Lodge's Patent.*

47. See Lodge, "Electric Radiation," *Philosophical Magazine*, July 1889, reprinted in *Lightning Conductors*, pp. 259–260. This is why Lodge could say, "Exact timing of the receiver is unessential. If resonance occurred to any extent, so that the combined influences of a large number of vibrations were really accumulated, the effects might doubtless be great; but hitherto I have seen no evidence of this with linear oscillators; the reason being, I suppose, that the damping out of the vibrations is so vigorous that all oscillations after the first one or two are comparatively insignificant; and very bad adjustment, or no adjustment at all, will give you the benefit of all the resonance you can get from such rapidly decaying amplitudes."

48. Lodge, "The Work of Hertz," *The Electrical Engineer*, Vol. 18, No. 322, 4 July 1894, p. 7. Compare Lodge, "Electric Radiation": "The electrical surgings obtained while the Hertz oscillator is working are of just the same character as are noticed when a Leyden jar is discharging round an extensive circuit; but whereas from a closed circuit the intensity of the radiation will vary as the inverse cube of the distance as soon as the circuit subtends a small angle, the radiation from a linear or axial oscillator varies in its equatorial plane only as the inverse distance, as Hertz showed. Hence, for obtaining distant effects the linear oscillator is vastly superior" (p. 258).

49. Lodge, "The Work of Hertz," p. 7. Lodge's reference to the electric arc shows considerable prescience, for the use of this device as a generator of continuous waves was several years in the future. Lodge made some rough estimates of the power necessary to sustain continuous oscillations in a radiating circuit and found the results disconcerting. He was, of course, conceptualizing the problem in terms of sparks with a very high repetition rate and concluded that there was, in those terms, no feasible solution: "even if sparks were made to succeed one another at the rate of

1000 per second, the effect of each would have died out long before the next one came. It would be something like plucking a wooden spring, which, after making 3 or 4 vibrations, should come to rest in about two seconds; and repeating the operation of plucking regularly once every two days."

50. See Fahie, *History*, pp. 68–176, and Elliot N. Sivowitch, "A Technological Survey of Broadcasting's Pre-History, 1876–1920," *Journal of Broadcasting*, Vol. 15, No. 1 (Winter 1970–1971), pp. 1–20.

51. For further biographical information see Howard, *Chambers's Dictionary*, pp. 112-113, and *Dictionary of Scientific Biography* (New York: Scribners, 1951), Vol. 3, pp. 474–482.

52. William Crookes, "Some Possibilities of Electricity," *The Fortnightly Review*, No. 102 (new series), 1 February 1892, pp. 173–181.

53. It is interesting to note that nowhere does Crookes envisage the possibility of voice transmissions, or the use of radio frequencies for anything other than person-to-person communication. The same concern over secrecy and the same blind spot regarding broadcast communications is evident in Lodge and Marconi.

54. Seen in this perspective, Crookes's article is an early example of what has been called the "gee-whiz" approach to technology, an approach that has since become familiar to the point of tedium. The lack of any sense of economic context is noted in the text; also notable is the failure to consider any but the most immediate and "first-order" social consequences of the technological innovations discussed.

55. Lodge, "History of the Coherer Principle," pp. 84–85. The acid in Lodge's comment must have been apparent to those who knew that Preece became Marconi's principal sponsor after his arrival in England in 1896.

56. Lodge, *Signalling Without Wires*, p. 84.

57. Lodge, *Signalling Without Wires*, p. 45.

58. Commercial exploitation of the research on dust precipitation and on ignition (especially as applied to automobile engines) was undertaken successfully by Lodge's sons, not by Lodge himself. The Lodge spark plug, familiar to British automobile drivers, was evolved by his son, Alexander Lodge. Dust precipitation was developed commercially by the Lodge Fume Deposit Company, formed by two other sons. See Lodge to Hill, 11 December 1914, in Hill, *Letters*, pp. 46–48.

59. See Lodge's comments on British patent law in *Signalling Without Wires* (1902 edition), pp. 50-51: "If a scientific worker publishes in the natural way, no one has any rights in the thing published; it is given away and lies useless, for no one will care to expend capital upon a thing over which he has no effective control. In this case practical developments generally wait until some outsider steps in and patents some slight addition or modification, or else, as sometimes happens, patents the whole thing, with some

slight addition. If a scientific worker refrains from publishing and himself takes out a patent, there are innumerable troubles and possible litigation ahead of him, at least if the thing turns out at all remunerative; but the probability is that, in his otherwise occupied hands, it will not so turn out until the period of his patent right has expired." The particular reference of this passage is, of course, to Marconi's patent of 1896 and Lodge's syntony patent of 1897. See also Lodge's comparison with U.S. patent law in *Advancing Science*, p. 126, and compare the comments in Hawks, *Pioneers of Wireless*, p. 243.

60. *Nature*, Vol. 50 (21 June 1894), pp. 182–183, reporting exhibits at the annual Ladies' Conversazione of the Royal Society held on Wednesday evening, 13 June 1894. A photograph of this portable receiver may be found in Lodge, *Signalling Without Wires*, p. 33.

61. Thompson, *Notes on Sir Oliver Lodge's Patent.*

62. Lodge, *Advancing Science*, pp. 164–165. Compare *Past Years*, pp. 213-232. Photographs of the apparatus may be found in *The Electrician* (London), Vol. 39, 17 September 1897, p. 687.

63. Fleming, *Principles*, p. 424. W. J. Baker, in his *History of the Marconi Company* (New York: St. Martin's Press; 1971), p. 24, accepts this account by Fleming without qualifications. It may possibly be relevant to recall that Fleming was long afflicted with deafness and, since Lodge's papers were not printed, must have depended on what little he could hear for his firsthand knowledge of what went on.

64. Lodge, *Advancing Science*, p. 126.

65. See Lodge, "The Work of Hertz," *The Electrical Engineer*, Vol. 18, 4 July, 11 July, 18 July, and 25 July 1894. The version of this lecture reprinted in Lodge, *Signalling Without Wires* (1902 edition) contains insertions of material, particularly referring to signaling, not present in the original.

66. British Association for the Advancement of Science, *Report of the Sixty-Fourth Meeting, Oxford, August 1894* (London: John Murray, 1894), p. 582.

67. *The Electrician* (London), Vol. 33, 17 August 1894, pp. 458–459.

68. *The Electrician* (London), Vol. 29, 17 September 1897, pp. 686–687.

69. Lodge's account is also fully supported by Silvanus Thompson, the physicist, who was present on the occasion. See Thompson, *Notes on Sir Oliver Lodge's Patent.*

70. Sir Ambrose Fleming, "Guglielmo Marconi and the Development of Radio-Communication," *Journal of the Royal Society of Arts*, Vol. 86, No. 4436 (26 November 1937), pp. 42–63, at p. 46. Degna Marconi, in her fascinating if imaginative biography of her father, cites this statement by Fleming but manages to convert it into an assertion of Marconi's priority by changing "unquestionable" into "questionable." A historian, of course, must always creatively reshape documentary sources while interpreting them; one trusts that, in this case, the privilege was not abused. See Degna Marconi, *My Father, Marconi* (New York: McGraw-Hill, 1962), p. 24.

71. Lodge, *Talks*, p. 50.
72. Lodge, *Advancing Science*, p. 162.
73. Lodge, *Talks*, p. 52.
74. Loyd Swenson, *The Ethereal Aether* (Austin: University of Texas Press, 1972), p. 105.
75. Cf. Lodge *Signalling Without Wires*, p. 50.
76. Lodge, *Signalling Without Wires*, p. 50. Lodge later stated, "I was advised to oppose the granting of Marconi's first patent, which was certainly weak; but I am glad we did nothing to increase his difficulties at that early stage" (*Advancing Science*, p. 126).
77. The U.S. patent number is 609,154, application filed 1 February 1898, issued 16 August 1898. All direct quotations are from the United States version.
78. For a more extended analysis of Marconi's patent, see below, pp. 203–08. The United States version of Marconi's British patent of 1896, which lists 56 specific claims, includes in Claim No. 56 reference to a receiver "having a conductor tuned to respond" to the oscillation produced by the transmitter. There is no parallel claim in the British version.
79. Lodge, *Past Years*, pp. 232–233.
80. Lodge, *Talks*, p. 104.
81. Lodge is remembered as one of the pioneers of radioastronomy for his early attempt to detect radiation from the sun—unsuccessful because of an unfortunate choice of frequency. He should also be remembered for his remarkably prescient suggestion of the use of cryogenics to reduce circuit resistance and improve signal-to-noise ratios. "Persistent oscillation is only killed by resistance; and if a conductor could be used of infinitesimal resistance, extraordinary results could be obtained. Some day, perhaps, something could be done in that direction by immersing the set in liquid hydrogen, or even helium; for at those low temperatures the resistance of metals almost disappears. Conductors become perfect, and oscillations would work up to some approach to an infinite value, even with small stimulus." (Lodge, *Talks*, p. 139.)
82. Compare Lodge on antenna design in *Talks*. "There is no gain in mixing up capacity and inductance. They should be kept distinct and separate. The upper part of the aerial, combined with the earth below it, should have all the capacity; and the self-induction coil should have as little as possible. Then the wave length has a chance of being clear and definite" (p. 125).
83. Lodge, *Talks*, pp. 162–163.
84. Lodge, *Talks*, p. 163.
85. Thompson, *Notes on Sir Oliver Lodge's Patent*, section 27.
86. Lodge, *Signalling Without Wires*, pp.50–51.
87. The nominal capital was £50,000, of which £36,000 was issued to Lodge in

exchange for his patents, probably in the form of fully paid-up shares. See Great Britain, House of Commons, Sessional Papers, 1907: *Report of the Select Committee on the Radio Telegraphic Convention* [referred to hereafter as *Select Committee* (1907)], testimony of Henry Muirhead.

88. A. Frederick Collins, "The Lodge-Muirhead System of Wireless Telegraphy," *Electrical World and Engineer*, 1 August 1903, pp. 173–176, at p. 173.

89. Patent No. 13,521 of 1902. See Lodge, "A New Form of Self-restoring Coherer," *Proceedings of the Royal Society of London* (1903), Vol. 71, p. 402.

90. Fleming, *Principles*, p. 373.

91. Sir Oliver Lodge and Dr. Alexander Muirhead, "Syntonic Wireless Telegraphy; with Specimens of Large-scale Measurements," *Proceedings of the Royal Society of London*, Series A, Vol. 82 (1909), pp. 227–256, at p. 228.

92. It is also true, however, that the design and construction of low-loss high frequency coupling transformers caused many problems. By dispensing with the transformer in his "direct coupled" circuits, Lodge eliminated a source of serious power losses.

93. See Frederick E. Terman, *Radio Engineer's Handbook* (New York: McGraw-Hill, 1943), p. 770.

94. Analysis of this antenna system has been greatly assisted by advice from Frederick E. Terman, Provost Emeritus of Electrical Engineering at Stanford University.

95. The function of the adjustable iron-cored chokes was to make the alternator circuit syntonic—that is, to bring it into resonance. This would greatly increase the voltage supplied by the alternator to the antenna, hence the electric charge in the antenna, and hence the transmitted power.

96. Note, too, Lodge's denial of harmonic radiation: "In no case was any trace of harmonic detected: e.g. when a station was sending 300 meters and the neighboring station was attuned to 600 meters, it did not necessarily feel any disturbance. The waves emitted and received by these radiators appear to be practically pure" (Lodge and Muirhead, "Syntonic Wireless Telegraphy," p. 255). This is of course a carefully qualified statement; many designers since Lodge have made similar claims for their transmitters without adding his judicious "necessarily" and "practically."

97. Collins, "Lodge-Muirhead System," p. 176.

98. Lodge-Muirhead equipment was also used by the 2nd Army Corps of the British Army on maneuvers in 1903, Marconi equipment being used by the 1st Corps. No significant difference was found in the performance of the two systems, but the officer commanding the Royal Engineers of the 2nd Corps noted that the Lodge-Muirhead antenna was too cumbersome in packing. See W. P. Jolly, *Marconi* (London: Constable, 1972), pp. 149–150.

99. Lodge and Muirhead, "Syntonic Wireless Telegraphy," p. 240.

100. *Select Committee* (1907), testimony of Oliver Lodge.

101. Compare *Select Committee* (1907), *Report:* "Pending a settlement of the policy to be finally adopted, the Postmaster General has hitherto refrained from issuing licenses for competing stations on the south coast of England and Ireland." Henry Muirhead, in his testimony before this Committee, stated that the refusal to grant the Syndicate a license dated from 1903 (i.e., it predated passage of the Telegraph Act of 1904), and that the main reason given was "on account of interference with other installations." He added: "We say that, if you had as efficient a system of tuning as we have, that would not hold."

102. There are obviously unanswered questions in this connection. One would like to know when, on whose behalf, and how forcefully, the Syndicate applied for operating licenses. Charges were made during the Marconi Scandal of 1911–1912 that the Marconi Company had exercised undue influence over the granting of licenses and the extension of Lodge's syntony patent. These accusations were strongly denied, and there is no direct evidence to support them. It should also be noted that in 1909 the Post Office took over the coastal stations of the Marconi ship-to-shore service, and after that date the Marconi Company could have had little interest in blocking the granting of licenses for short-range low-powered stations to the Lodge-Muirhead Syndicate.

103. Compare *Select Committee* (1907), testimony of Henry Muirhead. "*Question:* Have you any stations under the control of your own company, carried on at the instance of your company, open for public business? *Answer:* No, for the simple reason that all those we have supplied machinery to have preferred to put up their own installations, and we could not do it in England because we could not get a license."

104. The list of known installations of Lodge-Muirhead radio equipment suggests that all the purchasers had one characteristic in common: they were already involved with wired telegraph systems. It seems very probable that most it not all of the sales made by the Lodge-Muirhead Syndicate were to purchasers who were already satisfied customers of the Muirhead brothers.

105. After 1911 Lodge accepted a position as scientific adviser to the Marconi Company. His duties, however, were nominal and in no sense was he an employee.

106. S. G. Sturmey, *The Economic Development of Radio* (London: Gerald Duckworth & Co., Ltd., 1958), p. 20.

107. This intervention by the Marconi Company apparently cost the Syndicate a government contract, but not a very large one. The Syndicate took legal action against the Marconi Company as a result, seeking to prevent it from threatening potential customers. The matter was still undecided in 1907. See *Select Committee* (1907), testimony of Henry Muirhead.

108. This account follows that in Sturmey, *Radio*, pp. 20–21. Lodge makes no reference to this sequence of events in his memoirs.

109. United States Reports, Vol. 326, Cases adjudged in the Supreme Court at October Term, 1942, and October Term, 1943: *Marconi Wireless Telegraph Company of America v. United States*, at pp. 16–32.

110. See Frances Donaldson, *The Marconi Scandal* (London: Rupert Hart-Davis, 1962), p. 13.

111. This account is based on Sturmey, *Radio*, pp. 20–21. Baker, in his semi-official *History of the Marconi Company* (p. 61), states that Marconi had purchased the Lodge patent before the Newfoundland tests of 1901, but this is an error.

112. *Marconi Wireless Telegraph Company v. United States*, U.S. Supreme Court, 21 June 1943.

FIVE

MARCONI

In the spring of 1896 a young Italian named Guglielmo Marconi called upon William Preece, chief engineer of the British Post Office, at his office in London. He carried with him a letter of introduction written by one of Preece's professional acquaintances, a well-known electrical engineer:[1]

> Dear Mr. Preece,
> I am taking the liberty of sending to you with this note a young Italian of the name of Marconi who has come over to this country with the idea of getting taken up a new system of telegraphy without wires, at which he has been working. It appears to be based upon the use of Hertzian waves, and Oliver Lodge's coherer, but from what he tells me he appears to have got consid-

erably beyond what I believe other people have done in this line.

It has occurred to me that you might possibly be kind enough to see him and hear what he has to say, and I also think that what he has done will very likely be of interest to you.

Hoping that I am not troubling you too much.

Believe me
Yours very truly

A. A. C. Swinton

W. H. Preece, Esq., C.B.

Marconi was at this time 22 years old. He had been in England for only a few months, but in that short time both he and his mother, who had accompanied him, had been very busy. Marconi had spent his time rebuilding his Hertzian oscillator and receiver, both damaged by customs inspectors on their arrival in England, and in preparing the preliminary specifications for his first patent application. His mother, born Annie Jameson, had been no less usefully occupied. She concentrated her efforts on mobilizing the not inconsiderable resources of the Jameson family and their many friends. Campbell Swinton's letter of introduction to Preece marked the first victory in her campaign.

What did Marconi bring to England in 1896? More precisely, what did he bring that was not already there? At one level of analysis the question is easily answered. There is a well-known photograph of the young Marconi, seated, chin in hand, behind the two pieces of equipment that he showed to William Preece.[2] It is the face, not the hardware, that dominates the photograph: the direct, almost disconcerting gaze; the controlled features; the smooth pale cheeks that look as if they had never yet felt a razor. On the table in front of him are the two pieces of equipment on which he had pinned his hopes: on his right, a dipole oscillator with two projecting brass balls; on his left, a plain black box with a Morse code sounder on top and two pieces of copper strip stretched out in front. Inside the box is Marconi's improved coherer. The two copper strips are the receiving antenna: from their dimensions it looks as if the operating frequency were in the neighborhood of 2 meters (150 MHz).

There is nothing in this equipment that is new. Swinton's letter to Preece said as much. Indeed, it did Marconi less than full justice, for the coherer was not simply "Oliver Lodge's coherer" and the oscillator was not quite the same as anything seen before in England. But the differences, significant though they were, were differences of design, not of conception. As soon as Preece set eyes on it, he must have known that he was seeing essentially the same type of transmitter and receiver as Lodge had demonstrated at Oxford two years before.

Nevertheless, Preece reacted quickly and with enthusiasm. By June of that year he had organized official demonstrations, with representatives of the War Office in attendance as well as his Post Office engineers. In July and August, with Preece's private laboratory now at his disposal as well as some of his staff, Marconi was running tests across the London rooftops. By September there were tests of distance and directionality on Salisbury Plain, this time with Navy observers present as well as representatives of the Army and the Post Office. By the end of that month Preece was ready to commit himself publicly. A lecture to the British Association introduced Marconi and his system to the scientific community. In December, with Marconi in attendance to help with the demonstrations, Preece repeated the performance for the general public at Toynbee Hall. And by June of the following year, at the Royal Institution, he gave what amounted to his personal seal of approval. "There are a great many practical points connected with this system," said Preece, "that require to be thrashed out in a practical manner before it can be placed on the market, but enough has been done to prove its value and to show that for shipping and lighthouse purposes it will be a great and valuable acquisition."[3] Coming from the chief engineer of the department of government which controlled all telegraphic and telephonic communications in Britain, this was no trifling endorsement.

It is clear that Preece had, within a relatively short time, made a substantial commitment of reputation and status to a man whom he barely knew and who had no past record of performance on which confidence could be based. What led him to do this? Were there no English Marconis, no technological entrepreneurs of a more familiar breed, with credentials more easily evaluated, to whom Preece could turn? What of his own staff, technically highly competent and able to draw on the best brains in the country for advice? What of the universities, or private business, or the armed forces? Why turn to a stranger and a foreigner? These are not idle questions. Until we ask them it is all too easy, with the glib wisdom of hindsight, to say that of course it was Marconi's obvious intelligence or the excellence of his equipment or something of that nature that tipped the scales, and we end up remarking how perspicacious William Preece must have been to spot so easily and so early the man who was destined to make radiocommunications a reality. It is well to remind ourselves, therefore, that in 1896 Marconi was in effect a nobody, a man with practically no formal education, an inventor whose equipment differed in no basic way from the already known and demonstrated "state of the art," an alien with no family connections that could not safely be ignored if one had a mind to ignore them. And Preece, for his part, was a civil servant, not a risk taker, not a speculator, a man who had little to lose if he played for safety but much if he gambled on the unknown.

We can approach the problem from two sides: first, by examining more closely what it was that Marconi had to offer; and second, by considering the situation in which Preece and the department he represented found themselves in 1896.

* * *

Marconi (so his several biographers inform us) first became convinced of the feasibility of signaling across space without wires in the year 1894. He was 20 years old at the time, son of a well-to-do Bolognese landowner and silk merchant and a Scots-Irish mother. He had had very little formal education. The family summered on their estate outside Bologna, where a private tutor was usually provided, and spent the rest of the year in Florence or Leghorn, where the boy sporadically attended local academies. Freed from the restrictions of a formal course of study, he was able to follow his own interests. The family was affluent enough that there was no need to worry about learning a trade or preparing for a profession unless he chose to do so. His father—practical, businesslike, and somewhat tightfisted—may well have preferred that he settle down to regular study, but Annie Marconi was able to neutralize these pressures. The result was that the boy was left very much to himself. He showed an early interest in physics and chemistry, and particularly in anything relating to electricity. Private studies with Vincenzo Rosa at Leghorn and later with Augusto Righi at Bologna helped convert amateurish curiosity into something closer to systematic knowledge. Inadequate preparation prevented the young Marconi from matriculating at the University of Bologna, but Righi, who was a neighbor and friend of the family, allowed him to audit his lectures and gave him access to his laboratory. When Heinrich Hertz died in January 1894, Righi wrote an obituary article for one of the Italian journals which described his experiments in some detail. It was, we are told, the reading of this article while on vacation in the Italian Alps that convinced Marconi that Hertzian waves could be used for telegraphy. On his return to Bologna he settled down to the task of showing that it could be done, using first the attic of the family mansion and then moving his apparatus outdoors where, by successfully signaling over the brow of a hill, he eventually convinced even his skeptical father that his system worked and—a relevant consideration—that it might be worth investing a little money in it.

Righi's article may well have tipped the scales and set Marconi off on what was to become his life's work, but Righi's lectures were probably of more profound importance. This was Marconi's first contact with a master scientist and his first experience of the systematic exposition of physical theory. More than that, it brought Marconi into close personal relationship with one of the few scientists in Europe who, in 1894, thoroughly grasped what Hertz had accomplished, understood his experimental techniques, and shared his vision of the direction in which research should go. In later life Marconi was to deny that he had learned anything of importance from Righi, but the evidence suggests otherwise. That a physicist of this stature, working on these problems, should be teaching at Bologna, that he should be a friend of the family, and that he should be willing to accept this ill-prepared and seemingly directionless youth into his classes and into his laboratory, was not least among the strokes of good fortune that marked Marconi's career.

What Marconi got from Righi was a practical understanding of how electromagnetic waves could be generated, radiated, and detected. That he received any encouragement in his belief that Hertz's laboratory apparatus could be used for signaling is very unlikely. Righi's research interests in 1894 were in what we have since learned to call microwave optics, just as Hertz's had been in his last years: the demonstration that electromagnetic radiation at ultra high frequencies behaved as did light. It is for his work in this area—for his demonstration of the continuity of the electromagnetic spectrum—that Righi is principally remembered in the history of science. In practice this meant that the work in Righi's laboratory revolved around methods of generating radiation at ever shorter wavelengths. This was the frontier of research and it was to this end that equipment was designed. Marconi's search for methods of signaling over distances was to take him in precisely the opposite direction—to the longer wavelengths and lower frequencies where the quasi-optical phenomena that interested Righi were not in evidence. On the one hand, therefore, it is unlikely that Righi offered Marconi any direct encouragement in his signaling experiments, though he may well have thought them not inappropriate for a young man who had neither the education nor the desire to become a laboratory scientist. On the other hand, the equipment that Marconi found and used in Righi's laboratory was not such as could be directly adapted to the function Marconi had in mind. Working with Righi, in short, gave Marconi enough in the way of knowledge and apparatus to set him on the right track, but not so much as would inhibit his own creative imagination.

The extent to which Marconi borrowed from Righi and the extent to which he invented independently is clearly evident from a brief look at Righi's apparatus. To generate radiation at the ultra high frequencies that interested him, it was in principle only necessary for Righi to reduce Hertz's apparatus in scale. In practice, considerable redesigning was involved. One innovation was an improved spark gap, composed of four instead of two metal spheres (Fig. 5.1). The outer two were connected to an induction coil, or more commonly in Righi's laboratory to a static electricity machine. The inner two, separated by only a small gap, were immersed in a mixture of oil and petroleum jelly. By using spheres of about 4 centimeters in diameter and separating them by a gap of only 1 millimeter, Righi was able to move up to a wavelength of approximately 10 centimeters (3 GHz), much higher than anything Hertz had achieved. Detection of radiation at such high frequencies meant abandoning Hertz's simple ring resonator. Rather than adopt some kind of coherer, Righi

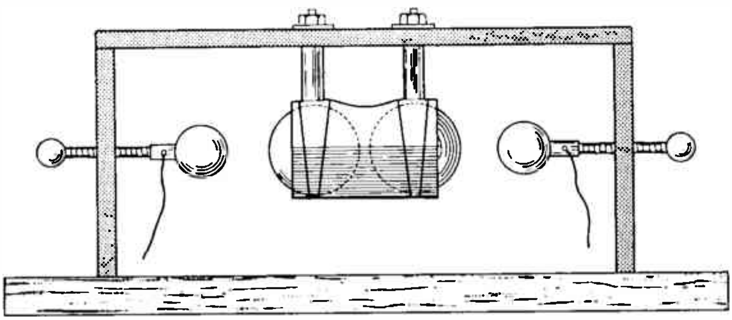


Figure 5.1 Righi spark gap.

deposited on a sheet of glass, by electrolysis, a thin film of silver in the form of a long, narrow rectangle. Across the middle of this rectangle he scribed a thin line with a diamond, cutting through the silver and leaving a gap only a few thousandths of a millimeter wide. The silver film served as a miniature dipole, the scribed line as a detecting spark gap. It was, of course, necessary to observe the sparks through a microscope, since they were invisible to the naked eye, but for laboratory purposes this was no drawback. The little device proved highly sensitive to microwave radiation and showed considerable directionality as well.

What was there here that could be used for practical signaling? Not the detector, certainly: that was a laboratory device designed for the ultra high frequencies. At those frequencies, with the equipment then in use, attenuation was high and distances short.[4] The Righi spark gap was another matter. The conception of immersing the central spheres in oil and coupling the discharge to them from the outer spheres was a sound one, producing trains of sparks of high intensity and regularity. Made larger and more rugged, spark gaps of this type became a standard feature of early Marconi transmitters.

What was Marconi to use for a detector? The obvious answer was some kind of coherer—obvious, that is, to anyone with access to scientific journals and acute enough to notice the reports of Branly's experiments that appeared in print in 1890–1891.[5] Here is another instance in which presence in Righi's laboratory and access to his library undoubtedly helped. Marconi's coherer, however, in the form in which he took it to England, differed considerably from Branly's device, and in its workmanship and specifications it bore the marks of much experimentation. In its original form the Branly coherer was a glass tube several inches long loosely filled with metal filings under atmospheric pressure. It suffered from insensitivity and unpredictability of response. Marconi's coherer, though it still left much to be desired as a detector, achieved greater sensitivity by reducing the space between the metal plugs to a small fraction of an inch, filling this space loosely with filings carefully selected for size, and heating the glass tube before sealing it, so that a partial vacuum was created in the interior (see above, p. 105, Fig. 4.4). Nothing Marconi could do could overcome the intrinsic limitations of a filings coherer. But what he could do and had done between 1894 and 1896 was develop a detector as sensitive and reliable as a filings coherer could be.

Then as later, this was to be typical of Marconi's style as an inventor. The original acts of creative insight were seldom his. Where he excelled was in the indispensable process of critical revision. Marconi's coherer was still a temperamental device, capable of driving shipboard operators in particular to distraction. It still possessed many of the drawbacks of the Branly model: for example, the need for mechanical "tapping back" that slowed down sending speeds to a fraction of those possible in cable telegraphy. It was, however, no longer a laboratory curiosity. It was a radiofrequency detector that could be standardized in production and that could take the hard knocks of day-to-day use in commercial or military service. There would be other and better detectors, but Marconi had taken the filings coherer to its technological limit.

This was no small achievement for two years of work, and if Marconi had taken to England nothing more than the Righi oscillator and his improved coherer he would have had enough to apply for a patent on "new and useful improvements" not greatly different from that which was in fact issued to him. Having said this, however, we must at once add a major qualification. The new oscillator and coherer might, in combination, have enabled Marconi to obtain results more impressive than Lodge, for example, had achieved in 1894. With them, Marconi had at his disposal a more efficient transmitter and a more sensitive receiver and, over short distances, performance would have reflected these improvements. By themselves, however, they would not have been enough to interest British officialdom and by themselves they did not explain the ranges of effective transmission that Marconi had achieved by 1896. The achievement of greater distance was principally the result of innovations in antenna design.

Anyone experimenting with the generation of electromagnetic radiation between 1894 and 1896 would undoubtedly begin with the antennas that Hertz himself had used. These were all variations of the simple linear dipole, often with capacitive end-loading in the form of metal sheets or spheres. They could be horizontally or vertically polarized. And they could be provided with reflectors, usually in the form of parabolic metal mirrors placed behind the dipole radiating element, which concentrated the radiation into a relatively narrow beam. Antennas of this basic type Marconi would encounter in Righi's laboratory, as well as in the literature on Hertz's experiments. When he began his own experiments in the attic of the Villa Grifone in the autumn of 1894, he began with the classic Hertzian dipole.

There was in principle no reason why antennas of this design could not be made to any dimensions desired. The limitations on scale were mechanical, not electromagnetic. Very large dipoles were difficult to build and difficult to support. Since the dimensions of a dipole bear a direct and simple relationship to the fundamental frequency radiated—each arm of the dipole being a quarter wavelength long—most laboratory experimenters after Hertz used wavelengths not longer than 2 or 3 meters and often much less. Righi, as we have seen, used a very tiny metal-film dipole for reception, because his interest was in microwaves. Relatively short wavelengths and relatively high frequencies—what we would now call the VHF, UHF, and microwave regions of the spectrum—had become the norm in laboratory practice after Hertz showed the way. Antenna design and construction at these frequencies presented few mechanical difficulties. A dipole and sheet metal reflector for use at a 2 meter wavelength was simple; for 200 meters a similar structure would have posed engineering problems, as well as being much too large to be used inside a laboratory. And the problems in which physicists were then interested—the quasi-optical behavior of radio waves—could best be explored at very high frequencies; at low frequencies they would not have been evident with any equipment of laboratory scale.

Later in his life, after the belated rediscovery of the "short waves," Marconi was to become greatly interested in the design of highly directional beam antennas for very high frequencies. In 1894–1895, however, what he was after was a method of signaling over long distances, and the longer the distance the greater the success, by his own standards at least. From his own accounts we know that he did not find it very difficult to signal across the length of his attic, using short dipoles as transmitting and receiving antennas. Moving his equipment out of doors, he achieved greater distances by elevating his antenna above the ground and adding large metal sheets to each arm to increase its electrical length. He soon discovered, however, that there were definite limits to what he could accomplish by further efforts in that direction. On the one hand, raising a horizontal dipole very high above ground proved decidedly awkward. One could turn it vertically and haul it up to the top of a pole; but then the feedpoint, where the spark gaps were, was out of reach and difficult to adjust. And after all that was feasible along those lines had been done, the distance gained by merely raising the dipole higher was not very much.

What was the source of the problem? The question is not hard for us to answer. Marconi, using the short dipoles he had learned about from Hertz and Righi, was transmitting at very high frequencies. We now know that radiation at those frequencies, except in unusual and transitory situations, is limited to line-of-sight transmission or slightly more. This is so for two reasons: first, very high frequency waves are not reflected or refracted by the ionosphere, so that the phenomenon of "skip" that gives great distances to the sky wave at other frequencies is not in evidence; and second, the surface wave, which at low frequencies tends to follow the curvature of the earth, at very high frequencies terminates at the horizon or slightly beyond.

Today these matters are obvious to informed laymen, if they have given any thought to why UHF television stations have shorter range than VHF ones, why international propaganda broadcasts use the "short waves" while the local AM radio station can sell its commercials only within the radius of its surface wave, why astronauts talking to ground stations must use very high frequencies, and why navy departments communicating with nuclear submarines must use very low ones. There is now, in short, a science, albeit an imperfect and highly empirical one, of radio propagation which did not exist in the 1890's. It exists today largely because Marconi and men like him insisted on pushing the frontier of radiofrequency development into regions that the science of their day had not explored.

Marconi identified success in his experiments with the achieving of greater distance. This was certainly by any reasonable standards one dimension in success, but not necessarily the only or even the dominant one. Conceivably Marconi might have concentrated his efforts on other aspects. He might, for example, have taken as his major challenge the development of techniques by which transmitters and receivers could be selectively tuned, so that each receiver responded only to the particular emission it was intended to receive. Progress in that direction would have held out some promise of enabling radio to compete with wired overland telegraphy, for the lack of "secrecy" was to be a not unimportant criticism of "wireless" in the years ahead. It was work along these lines that interested people like Lodge in England, Stone in the United States, and Braun in Germany. Alternatively, Marconi could have taken as his major challenge the achieving of highly directional transmissions—means of concentrating the radiation from his dipole into narrower beams, so that the energy radiated was not dissipated in all directions but focused along a single vector. Work along these lines might have produced a communications system with strong appeal to naval and military as well as civilian authorities, and Righi might well have felt more inclined to offer assistance. Neither of these lines of investigation held interest for the young Marconi in 1894–1895. The reflecting antennas he used showed no advance over those that could be found in Righi's laboratory or in Hertz's. As for the theory of syntony, at this stage of his career it is doubtful that he had even heard of it.

It was distance that counted for Marconi, and not only at the Villa Grifone. For the rest of his life it was to remain his technological obsession. The implications are more than just personal and idiosyncratic, for the emphasis that Marconi gave to radio, the way he defined its emerging functions, had an important, enduring, and costly influence on the way the new technology developed. We fall all to readily into the fallacy of believing that, when a new technology appears, the major purposes it will end up serving are already evident. Typically they are not. It may take generations before it becomes clear what a new technology is good *for*, what it can do better than other technologies available for use. Sometimes the uses of a mature technology are quite different from any visualized by the original inventors. In the interim, while the technology is still emergent and its range of possible uses still being explored, fixed ideas regarding what its dominant purposes should be are not uncommon.

Why did Marconi, from the beginning of his experiments, lay such emphasis on distance? Family history suggests a partial answer. Guglielmo Marconi needed money for experiments—not much, but enough to buy the wire, the copper sheets, the coils, and the batteries. His father was the only possible source, and Giuseppe Marconi was not about to part with hard-earned cash just to help his 21-year-old son make a bell tinkle at the other end of the attic. Something more impressive was in order. To signal over the brow of the hill, to a receiver that was out of sight—that was something worth attending to. So that was how it had to be: to get money out of his tightfisted father, to prove that his mother had been right in all the family battles she had fought on his behalf, young Marconi had to achieve distance. He was to spend the rest of his life in the same endeavor. It is perhaps not too fanciful to speculate that, long after Marconi's parents were dead, there was still for him a miserly and disbelieving father to be convinced, and a mother to be proved right.[6]

Greater distance from the gardens of the Villa Grifone in 1894–1895 was not to be achieved with the short dipoles, sheet metal reflectors, and battery-powered induction coils that Marconi was using at that time. A few hundred meters was the most he could attain. Consultation with Righi elicited only doubts and discouragement. Science, it appeared, had no solution, nor even a suggestion as to where to look for a solution. The situation was to become familiar to Marconi in later life. He reacted in 1895 as he would later: by determined empiricism. He had already tried to make his antenna radiate more powerfully by attaching metal plates to the arms. He had also tried raising it higher above the ground, despite the inconvenience of having coherer and spark gap out of reach. Now he combined the two approaches. He turned his antenna vertically, connected one of the metal plates to a long wire, and fixed it to the top of a high pole. The other metal plate was laid on the ground. At the receiving station the coherer was connected between the grounded plate and the end of the vertical wire; at the transmitter the spark gap was connected in the same way (see Fig. 5.2). Both coherer and spark

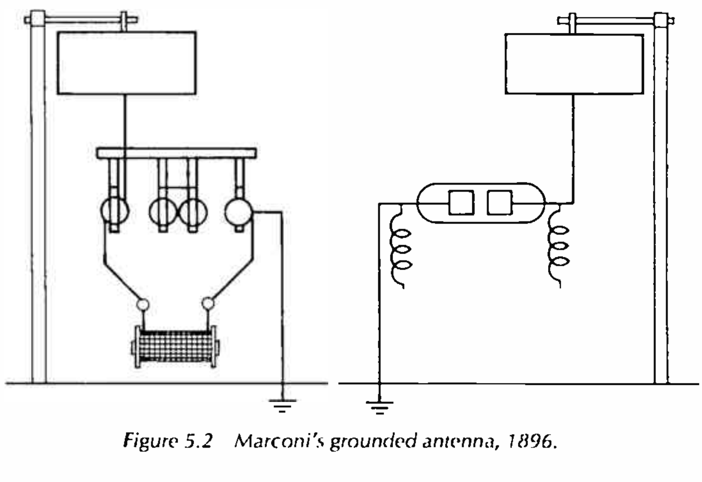


Figure 5.2 Marconi's grounded antenna, 1896.

gap were therefore at ground level where they could be adjusted without difficulty.

Here was an antenna different from any Marconi had used before. We would call it now a ground-plane antenna, or a grounded vertical. In principle it was no different from the classic dipole, vertically polarized, but now the earth served as one of the dipole's arms. Marconi found it effective. It substantially increased his range of transmission, particularly if the lower metal plate was buried in the earth. It was this antenna, with its longer range, that he used to convince his father that his experiments were to be taken seriously. It was this antenna with which he was to signal from England to France in 1898. And it was this antenna, with its vertical wire now suspended from a kite and the ground plate immersed in the ocean, that he used in Newfoundland in 1901, when he claimed to have received the first transatlantic signals.

What, if anything, had Marconi invented? He was not the first to use an antenna of this type. Franklin's kite string had served the same purpose in 1752, picking up electric discharges from thunderstorms. The lightning rods that Oliver Lodge viewed with such skepticism were essentially vertical antennas. A number of earlier experimenters, including Elihu Thomson, Thomas Edison, Amos Dolbear, and David Hughes had noted the efficiency of vertical radiators, though not in connection with telegraphy.[7] And, although Marconi was unaware of it at the time, a young Russian physics instructor at the Torpedo School in Kronstadt, inspired by Lodge's lecture of 1894, had by the spring of 1895 built and described before a scientific audience a receiver in which a coherer was connected between an "ordinary lightning conductor" and ground.[8] Alexander Popov's receiver, it is true, was intended primarily to detect atmospheric disturbances, as an aid to weather forecasting, but Popov himself was fully aware that it could be used for the reception of signals, when matched to a suitable transmitter. In any case, an antenna is an antenna, whatever may be the function of the detecting and recording equipment to which it is connected. Seen in these terms, there was little novelty in Marconi's grounded vertical.

What was new was the deliberate use of such an antenna for the transmission of signals. Although the evidence is not as unambiguous as one would wish, it does not appear that Popov used a vertical antenna for transmission; the most that he claimed personally (as distinct from the claims of his later admirers) was that his receiving apparatus was identical to Marconi's. Lodge, for his part, was highly skeptical of grounded vertical antennas, holding that they made it impossible to achieve precise syntony.[9] Lodge-Muirhead equipment as manufactured and sold never used grounded antennas, except aboard ship; the greater ease of tuning and predictability of performance that resulted from the avoidance of "ground effects" were cited as points in its favor. Such niceties were at this time of little concern to Marconi. The grounded vertical antenna gave him greater distance and that was what he was after. And it is worth emphasizing that a good measure of originality was involved. Theoreticians might have noted that a vertical wire over a ground plane made a good radiator, but no one had used it to transmit signals before. The functional similarity to lightning rods is a matter of some historical interest, but this was not the line of thought that Marconi was following. Marconi's solution to the problem of greater distance was not obvious beforehand, however much like "common sense" it may have seemed afterward. And it was this quality of nonobviousness that Ambrose Fleming, later Marconi's scientific adviser, chose to stress in his comments on the vertical antenna: "It had not occurred to Hertz, or to any other investigator, that the result of this arrangement would be to create a different type of electric wave to that generated if the oscillator were totally insulated. The novelty of such a suggestion is to be measured rather by its non-obviousness to experts than by the simplicity of the device itself."[10]

Precisely how the grounded vertical antenna produced these desirable results was quite a different question and not one to which Marconi gave much thought at the time. The diagrams he used to illustrate his apparatus show that, to form the vertical portion of the antenna, he hauled a large metal sheet, or sometimes a panel of wire mesh, up to the top of a mast and then connected it to the coherer or spark gap by a wire (see Fig. 5.3). There are times when he speaks as if it is the height of this "capacity area" above the ground that makes the difference. Sometimes, however, it is the connection to ground—the buried metal plate—that is emphasized. Antennas that operated *senza presa di terra*—without a grasp of the earth—could never achieve long distances, he believed.[11] But on still other occasions an even more cryptic explanation is hinted at, and it is suggested that waves emitted from a grounded vertical antenna are not Hertzian waves at all, but waves of a different kind. Even Fleming, who knew better, refers in the passage quoted above to "a different kind of wave," and Marconi, at least when being interviewed by the press, suggested that the waves his apparatus emitted and detected were different from those discovered by Hertz.[12]

This was, of course, nonsense, and Marconi was too intelligent to persist in any claims of that type. The fact of the matter is that, although the vertical antenna gave the results Marconi wanted, he did not at the time know why it did so. There is

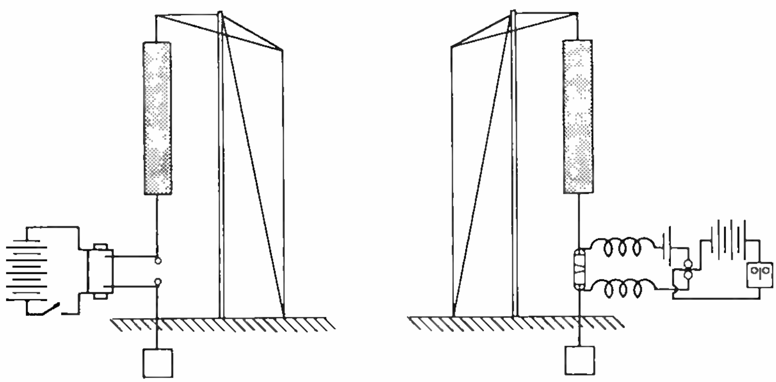


Figure 5.3 Marconi transmitting (left) and receiving (right) stations, 1896.

nothing intrinsic to a vertical antenna that gives it any particular advantage over a horizontal one so far as effective range is concerned. Much depends on the quality of the ground plane, in the one case, and the height of the horizontal antenna above ground in the other. What made the difference in Marconi's case was not the shift from horizontal antennas to vertical ones as such, but rather the shift to longer wavelengths that the vertical antenna, almost incidentally, made possible. Raising the capacity area higher above the earth did not make much difference; the length of the wire that connected the capacity area to the spark gap did. Burying one of the metal plates in the ground was not in itself an important modification; using the earth as the other half of a resonant dipole was. The essence of the change was the move to wavelengths with different propagation characteristics. By moving to longer wavelengths Marconi was migrating to an area of the radiofrequency spectrum where transmissions were no longer limited to line-of-sight range. The vertical antenna, because it could be made larger (that is, taller), resonated at lower frequencies than did the short Hertzian dipoles Marconi had used up to that point. As he moved away from the quasi-optical frequencies, so he moved away from the limitations of propagation at those frequencies. With his new vertical antenna and ground connection he was using wavelengths where the "ground wave" followed the surface of the earth for considerable distances. The critical element, however, was not the change to vertical polarization but the move to lower frequencies.

Is the matter of any importance? Not, certainly, for an explanation of Marconi's early success. The pragmatic results—and they were impressive—were what counted. And certainly Marconi was not the first innovator to have an incomplete understanding of why and how his innovation "worked." If, however, we ask what Marconi learned from this creative breakthrough, what lessons he carried forward into later work, we begin to sense the long-term consequences of the episode. What Marconi learned was that larger antennas meant greater distances. Initially this was no more than a simple rule of thumb. Marconi presented it as such in his speech on receiving the Nobel prize for physics in 1909:

> I . . . began to examine the relation between the distance at which the transmitter could affect the receiver and the elevation of the capacity areas above the earth, and I very soon definitely ascertained that the higher the wires of the capacity areas, the greater the distance over which it was possible to telegraph. Thus I found that when using cubes of tin of about 30 cms. [a] side as elevated conductors or capacities, placed at the top of poles 2 meters high, I could receive signals at 30 meters distance, and when placed on poles 4 meters high, at 100 meters, and at 8 meters high at 400 meters.[13]

Sometimes the same concept was presented as a mathematical formula: the effective range of transmission varied as the square of the height of the antenna.[14] With greater theoretical sophistication it was realized that, with each increase in the vertical height of the antenna, the fundamental resonant frequency decreased; but the practical lesson drawn remained the same—longer wavelengths meant greater distances. This was the rationale for the immense umbrellalike antennas, covering acres of ground, that were later erected at stations intended for transatlantic operation. And when such antennas reached their physical limits, when it seemed that wavelengths could be lengthened no further, there was only one way to achieve greater distance: higher power. This was, in effect, the Marconi formula for long-distance operation until the rediscovery of the "short waves" in the 1920's: larger antennas, longer wavelengths, and higher power. It was a formula whose lineage can be traced directly to Marconi's first vertical antenna.

As a formula for achieving long-distance radio communication it was woefully incomplete. From the economic point of view it was in fact a recipe for misallocation of resources, for there were other, cheaper ways of transmitting signals by radio over long distances, and these alternative methods were overlooked as a result of Marconi's headlong, and initially successful, rush to the very low frequencies. The shift to vertical antennas and longer wavelengths in 1895–1896 was indeed a technological breakthrough, but it was also the beginning of a technological fixation. Marconi learned the lesson too well. The very success of the innovation led to the emergence of a higher power/lower frequency syndrome that closed his mind to alternative approaches and encouraged him to persist in the one that had succeeded, even when further increases in power and further reductions in frequency were clearly yielding diminishing returns in terms of distance.

The episode is also significant in another sense. Up to this point Marconi's role as experimenter and innovator had been a very simple one. He was translating scientific discoveries already made by others into useful and potentially profitable devices. Analytically, he was the final step in a simple linear progression —final in the sense that with Marconi and experimenters like him (Popov in Russia; Ducretet in France; Slaby, Arco, and Braun in Germany; Stone, Fessenden, and De Forest in the United States; to some extent Lodge in England) the line of scientific advance that had led from Faraday and Maxwell to Hertz had now reached the stage of commercial exploitation. The transfer of new knowledge had been up to this point entirely one way: out of science into technology and thence into commercial use. Now, however, a reverse flow of information was beginning as Marconi, in search of an objective—greater distance—which was of little immediate concern to scientists, moved out from the area of knowledge in which the science of the time could help him and began the exploration of problems for which that science had no solution. Marconi's function now became more complex. In addition to utilizing existing scientific knowledge for practical ends, he was also generating, in a kind of feedback process, problems for science to solve and data for science to rationalize. These anomalies—pieces of information that did not "fit" the recognized theories and that called for extension or refinement of scientific generalizations—were byproducts of his main purpose. Indeed, each of them represented a difficulty he could well have done without. They were no less significant for scientific and technological change for that reason. Their recurrent appearance reflected the fact that, as technological entrepreneur and innovator, Marconi was reaching into problem areas where science had no ready answers.

This feedback process, the generation of new information from "field experience," would have been much slower to appear if Marconi had been content to continue working at very short wavelengths, for there the scientists were also hard at work and unexpected results would have been less likely. It is worth noting that Lodge, in his experiments and demonstrations between 1894 and 1896, found nothing that surprised him, no phenomena that, as a scientist, he thought anomalous or strange. Marconi, in contrast, had already by 1895 moved out of these tidy and well-tended pastures into *terra incognita*. Consider, for example, what he would have required to fully comprehend the results he was attaining with his new antenna and coherer. He would have needed a theory of antenna design; apart from the basic theory of the linear dipole, there was none. He would have needed a theory of propagation, and in particular a theory that would have enabled him to recognize and exploit the differences between the propagation characteristics of different frequency bands. No such theory existed; if it had, the whole course of Marconi's work and of subsequent radio history would have been different. And he would have needed a theory of transmission lines, such as would enable him to match his transmitters and receivers to their antennas. Here some of the empirical relationships had been worked out, but not as a systematic body of knowledge. In each of these fields Marconi's work was already generating new data and new problems.

What, then, did Marconi take to England in 1896? If the question refers to equipment, it is easily answered: he took very little that was not already there. The Righi spark gap was already familiar; it was, in any event, little different from a three-element gap used by Lodge. The induction coil, with its primary circuit interrupted by a switch or Morse key, was standard equipment in all experiments with Hertzian waves. The Marconi coherer, on the other hand, appears to have been substantially more sensitive and stable than anything of the kind used before and, as we shall see, it was the design of the coherer and its efficiency as a detector of radiation that Marconi and others

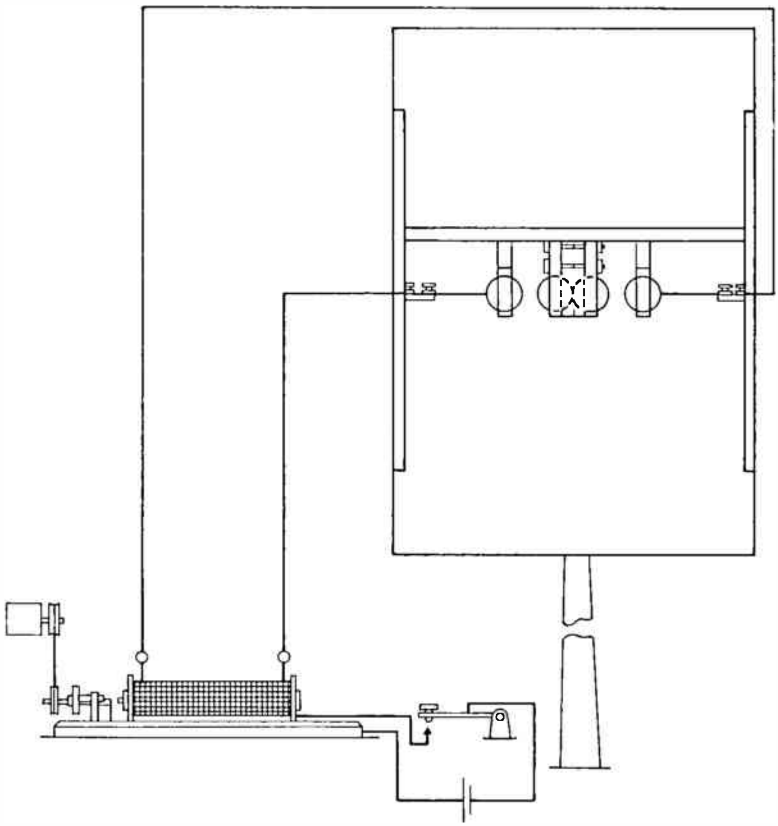


Figure 5.4 Marconi transmitter with parabolic reflector, 1896.

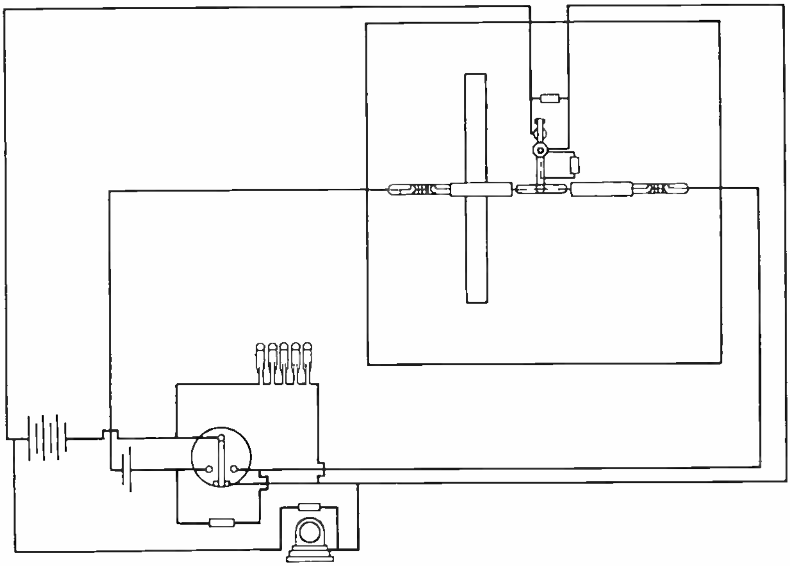


Figure 5.5 Marconi receiver with parabolic reflector, 1896.

emphasized as one of the distinguishing features of his system.

If, however, our question refers to information—the knowledge that Marconi brought with him—our answer has to be more extended. In the first place, as regards antenna design, Marconi was thoroughly familiar not only with the simple Hertzian dipole but also with the whole panoply of directional reflective antennas that Hertz had been working with in his later years and that Righi had elaborated in his UHF and microwave experiments at Bologna (see Figs. 5.4 and 5.5). The information needed for the design and construction of such antennas was readily available in the scientific journals; a careful reading of Hertz's last papers, with no more extensive research, would have disclosed the essentials. No one in Britain at the time, however, had worked with directional antennas as Marconi had, and no one knew as well as he did what could and could not be done with them in practice. Beyond this, as a potent weapon in his arsenal, Marconi had the concept of the grounded vertical antenna and the knowledge that he could with its aid transmit signals over the horizon. This was knowledge that nobody else had. Popov at this time thought of the vertical antenna only at the receiver. Lodge disliked it because it made syntony difficult. Neither they nor any other experimenters had used vertical antennas to transmit signals. Marconi had. Why he got the results he did may have been obscure, to him and to others, but of his ability in practice to achieve those results he had no doubt.

And this may point up the two most important items that Marconi took to England—pieces of baggage not as liable to damage at the hands of suspicious customs inspectors as his precious coherer and oscillator. These were, first, confidence that he could create a system of signaling by Hertzian waves that would have commercial and military, not merely scientific, value; and second, an unshakable determination to do precisely that. These qualities were new, vital, and catalytic. In a different technological context they would have been doomed to frustration, as have the purposes of many other experimenters no less intelligent and determined than Marconi. By 1896, however, the technology of radio was ripe for just the kind of entrepreneurial thrust that Marconi brought to it. Consider the others who knew as much or more about "signaling through space" as he did, but lacked his vision and his commitment: Lodge, demonstrating his laboratory curiosities to fellow scientists at Oxford, speculating about the human eye as a detector of radiation, and then heading off into his experiments with aether drift—a really serious matter for a scientist, not like the entertaining tricks that could be played with Hertzian waves; or Popov in Russia, exhibiting his ingenious apparatus for detecting the approach of thunderstorms and then (much like Braun in Germany) diverted into other work by the fascination of the newly discovered Roentgen rays; or David Hughes, who in many ways seems to have anticipated all the others, hugging his discoveries to himself rather than face further discouragement from a skeptical world; or Henry Jackson, busy with his secret trials for the Royal Navy precisely at the time of Marconi's arrival;[15] or Amos Dolbear and (a little later) Lee De Forest and Reginald Fessenden in the United States. So far as scientific knowledge and technical expertise were concerned, any of these men could have done as much as Marconi. Some of them, inspired by his example, were later to prove the point. For none of them, however, did opportunity, knowledge, and incentive combine in quite as favorable a constellation as they did for Marconi in 1896.

* * *

William Preece, discussing in 1896 what was new about Marconi's ideas, reminded his London audience of the famous anecdote of Columbus and the egg. Many others could have done it; none of them did. The point was well taken. Novelty or originality in concept were not, in any event, what Marconi himself claimed for the elements of his system. What seemed to him new and original were his "improvements," and it was to these improvements that he sought rights of ownership in his patent application of 1896. This, the first radio patent ever issued and for many years the first line of defense of the Marconi Company against challenges to its virtual monopoly of marine communications, is an intriguing document.[16]

The application was filed in two parts: the Provisional Specification, dated 2 June 1896, and the Complete Specification, filed on 2 March 1897. Neither was a public document until the patent was finally issued on 2 July 1897—an important point, since Lodge's syntony patent was filed in the interim. The Provisional Specification was sketchy in the extreme. Of its approximately 1300 words, only about 200 are devoted to the oscillator and transmitter and another 150 to the antennas. The remainder of the text, or almost three-quarters of the total, is devoted to descriptions of the coherer, its associated circuitry, and the mechanism for "tapping back." The only novelty claimed for the transmitter is a revolving contact on the induction coil. Of the three sentences devoted to antenna design, one refers to the placement of spark gap and coherer at the focal point of parabolic reflectors, one to the insertion of chokes in the coherer circuit, and the third to the use of buried plates and elevated conductors "when transmitting through the earth or water."

It is of course true that the function of a Provisional Specification was little more than to establish a filing date and get the bureaucratic machinery in motion. Nevertheless, it is a fair presumption that, even in this preliminary document, Marconi would lay greatest stress on those elements which he considered original and important. If this is so, there is no question that the design and construction of the coherer were thought of as the critical elements. The specifications of this component are detailed in the extreme. In comparison the remarks on the exciter and antennas seem casual, almost offhand. This, of course, is fully consistent with what one might infer from the publicity that attended Marconi's first demonstrations in London and the fuss made over his secret "black box" that so excited the ire of British scientists, Lodge among them. Inside the box was the all-important coherer; the box itself was there to provide a measure of shielding against stray electromagnetic fields.

The Complete Specification is a different kind of document entirely.[17] Designed to be as comprehensive as possible, it describes in considerable detail a complete transmitting and receiving system, with several alternative arrangements of the apparatus at each end, and culminates in a series of 19 claims, each specifying a particular element of what Marconi considered to be his invention.[18] We should realize that at this time in Britain, although the issue of a patent theoretically required evidence of originality and priority of discovery, in practice no rigorous proof of these qualities was insisted on. A British patent conveyed little more than a right to bring suit. When claims to property rights in inventions came into conflict, the courts would decide. It should occasion no surprise, therefore, to find Marconi claiming as the first element in his invention "The method of transmitting signals by means of electrical impulses to a receiver having a sensitive tube or other sensitive form of imperfect contact capable of being restored with certainty and regularity to its normal condition substantially as described." It would have been difficult indeed for Marconi to prove that he had literally been the first to "invent" what this claim, or in fact most of the other 18 claims, described. That, however, was hardly the essence of the matter. He was the first to claim these methods, these pieces of equipment, these circuits, as *property*, and under British patent law that was what counted.[19] In Germany and the United States the law attached greater weight to priority of discovery, and in those jurisdictions Marconi's first patent was weaker than in Britain. In American courts, for example, the fact that Lodge's lectures and demonstrations in 1894 had become public knowledge in the United States was a weighty consideration—weighty enough, in fact, to nullify the practical impact of this first Marconi patent.

Our interest here, however, is not in the complexities of patent law but in what Marconi's Complete Specification can tell us about his equipment and the knowledge embodied in it, as they existed in 1896. There is no need for us to dwell on the design and construction of the coherer. Marconi's application claims no originality for the device itself; it does stress, however, multiple refinements in detail and claims superior performance as a result. ("I have noticed that a sensitive tube or imperfect contact . . . is not perfectly reliable. My tube as shown in figure 5 is, if carefully constructed, absolutely reliable, and by means of it the relay and trembler etc., can be worked with regularity like any other ordinary telegraphic instrument.") As regards the transmitter, reference is made to the revolving contact on the "interrupter" in the primary circuit of the induction coil, and spark gaps of the Righi type are described in detail (but with no reference to the Italian physicist). Nothing in these sections of the application would come as a surprise to anyone who had followed the scientific journals or the semipopular accounts of

Hertzian wave experiments over the preceding seven or eight years. More likely to impress would be the clear evidence, in almost every sentence, of the applicant's scrupulous attention to design refinements and details of construction—the multiplicity of seemingly small changes and improvements which, when words were translated into apparatus, added up to levels of performance and reliability never achieved before.

The information on antenna design is the only material in the application that shows evidence of conceptual originality. There is no single section on antennas. The relevant data and claims are dispersed through the sections on transmitter and receiver construction, suggesting that at this stage Marconi had not come to think of antenna design as a separate identifiable problem area. Three types of antenna are depicted. In the first, which accompanies the diagrams designed to show a typical transmitting and receiving system, the induction coil and the coherer are each shown attached directly by wires to what the patent refers to as "two insulated spheres or conductors . . . fixed a small distance apart." These are depicted as suspended from a horizontal pole some distance above the ground. The second is the short Hertzian linear dipole set at the focal point of a parabolic metal reflector (the coherer being similarly located at the receiving station.) And the third is what we have referred to as the grounded vertical antenna. The diagrams accompanying the application make it plain that, at this stage, the vertical antenna was no more than a variant of the first type: one of the "insulated spheres or conductors" is taken down and buried in the earth while the other is left suspended.[20]

It is evident that Marconi had a variety of antennas to choose from. The application does not differentiate among them in terms of frequency but rather in terms of directionality and distance to be covered. The dipole with parabolic reflector is recommended for use "when it is desired that the signal should only be sent in one direction" and "in order to increase the distance at which the receiver can be actuated by the radiation from the transmitter." The antenna with two elevated plates is presented as a "modified form of transmitter with which one can transmit signals to considerable distances without using reflectors" and the comment is added: "other things being equal, the larger the plates at the transmitter and receiver, and the higher they are from the earth, and to a certain extent the farther apart they are, the greater the distance at which correspondence is possible." The grounded antenna, with one plate in the air and the other buried, is specified for use "where obstacles, such as many houses or a hill or mountains, intervene between the transmitter and the receiver" and once again it is claimed that the larger the plates and the greater the distance between them, the greater the range, other things being equal. No connection is suggested between wavelength and range.

Tuning is hardly mentioned. None of the diagrams, whether of transmitters or receivers, contains any tuned or tunable circuit elements except the antenna itself (see Fig. 5.6). The sus-

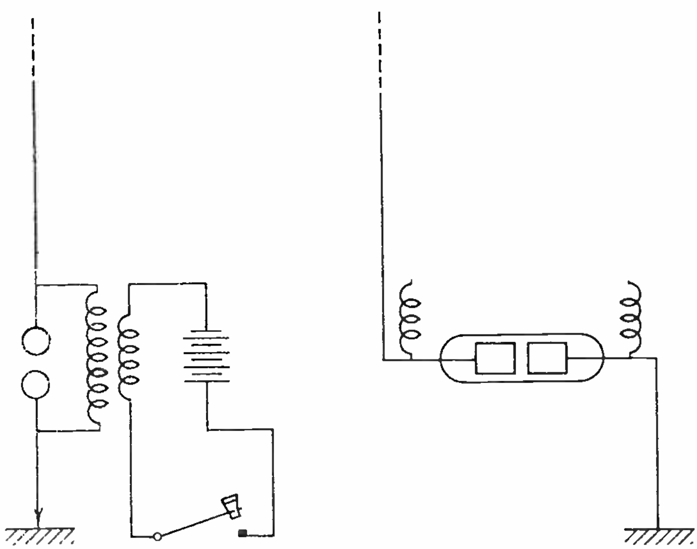


Figure 5.6 Marconi transmitter and receiver circuits, 1896.

pended plates of the receiving antenna are to be of such a size "as to be preferably tuned with the length of wave of the radiation emitted from the transmitting instruments." And the dimensions of the parabolic reflectors are stated in terms of wavelength. Marconi was well aware, in other words, that transmitters and receivers could be at least roughly tuned by altering antenna dimensions. No reference is made, however, to the problem of interference, nor to the possible desirability of tuned circuits to minimize that problem.

These, then, are the principal features of Marconi's first patent. What are we to make of them? On the one hand we have the acidulous comments of the London *Electrician,* leading professional journal of the day, which in September 1897 summarized Marconi's patent, matched its claims against Lodge's demonstration in 1894, and proceeded to blast the patent and British patent law in general:

> Dr. Lodge published enough three years ago to enable the most simple minded "practician" to compound a system of practical telegraphy without deviating a single hair's-breadth from Lodgian methods. . . . It is reputed to be easy enough for a clever lawyer to drive a coach and four through an Act of Parliament. If this patent be upheld in the courts of law it will be seen that it is equally easy for an eminent patent-counsel to compile a valid patent from the publicly described and exhibited products of another man's brain. No longer is it necessary to devise even so much as "a novel combination of old instrumentalities", and the saying *"ex nihilo nihil fit"* evidently was not intended to apply to English patents at the end of the nineteenth century.[21]

From one point of view these remarks, though overstated, are defensible. There was nothing in Marconi's patent that was new in concept, with the single possible exception of the vertical antenna used for transmission. There is no direct evidence that he learned from Lodge or even knew of Lodge's work, though it would be strange if he did not. Borrowing from Lodge, however, was not necessary. Marconi learned what he needed to know about electromagnetic radiation from Righi and, at one remove, from Hertz. He married that to the familiar technology of wired telegraphy. And he added the fruits of his own experiments with antennas. The result was precisely what the title of Marconi's patent stated: "improvements in transmitting electrical impulses and signals, and in apparatus therefor." The improvements were partly in design and construction, partly in the way the components were integrated into a system. Marconi's own statement is precise and accurate, if one gives full weight to the initial sentence:

> My invention relates in great measure to the manner in which the above apparatus is made and connected together. With some of these forms I am able to obtain Morse signals, and to work ordinary telegraphic instruments and other apparatus; and with modifications of the above apparatus it is possible to transmit signals not only through comparatively small obstacles such as brick walls, trees, &c., but also through or across masses of metal, or hills, or mountains, which may intervene between the transmitting and receiving instruments.[22]

What Marconi described in his patent application of 1896 was the technological embodiment of Maxwell's theory of the electromagnetic field, first stated some 30 years earlier. If there was any question of borrowing, of using what the *Electrician* called the "products of another man's brain," it was from the scientific tradition of Faraday, Maxwell, Hertz, and Righi that Marconi borrowed, not from Lodge or from any of the other experimenters who had interested themselves in the transformation of theoretical predictions into physical apparatus. Seen from this point of view Marconi stood at the culmination of one process and the initiation of another. He was beginning the process of commercial and military development of radio. But he was also at the culmination of the process whereby a major scientific advance was translated into practical use. Scientific theorems had already been translated into apparatus—pieces of hardware which, suitably interconnected, could be used for tests and demonstrations. Hertz, Lodge, and Righi had, following their own interests as scientists, completed that stage. Marconi carried the process a step further. He translated laboratory hardware into a technological system that could serve practical needs. With the advent of this stage the matter for the first time became of direct economic relevance, something that could be discussed in terms of costs, revenues, and competition with alternative modes.[23]

In all respects save one, Marconi's system as it existed in 1896 was at the frontier of relevant scientific knowledge. In some respects, indeed, as we have seen, it was already pushing beyond that frontier, into territory where contemporary science could offer little guidance. In general, however, it reflected the "state of the art" as of that moment. The one exception concerned tuned circuits. Here both the scientific knowledge and the technology in which to embody that knowledge were available, and in 1896 access to that knowledge was completely unrestricted by patents. Yet Marconi's application ignored the subject. The explanation is not difficult: Marconi had never encountered a situation that called for precise tuning or high selectivity. He knew that his receivers responded best when tuned to approximately the same wavelength as his transmitters. This, however, was seen as a question of antenna dimensions only. The relationships that so fascinated a speculative thinker like Lodge, the sympathetic response that carefully syntonized circuits could evoke—this was, in 1896, of no concern to the pragmatic Marconi. Such unconcern was a luxury possible only when there were no rival claimants to the radiofrequency spectrum. Marconi's system as it existed in 1896 was a practical system of wireless telegraphy only as long as there was very little wireless telegraphy.

* * *

What persuaded William Preece to support this man and his system of radiotelegraphy? Preece was no stranger to the art, no technologically innocent bureaucrat to be hoodwinked by sealed black boxes and all-inclusive patents. On the contrary, he was himself an experimenter and innovator of some note. Several years later, after his first fine enthusiasm for Marconi had cooled somewhat, he recalled with pride that he had been at work on wireless telegraphy 12 years before the Italian experimenter came to England; that in 1892 he and his men had successfully signaled across the Bristol Channel; and that in 1895, when the cable between the Island of Mull and the Scottish mainland had been broken, they had maintained uninterrupted communication by wireless telegraphy for three weeks. These had not been private experiments; they had been reported in the newspapers and had created something of a sensation. "As a matter of fact," Preece recalled with pardonable vanity, "the press made as much fuss over the success of my experiments as they did six years later over the success of Mr. Marconi."[24] Why did Preece, a Welshman with no mean opinion of himself, abandon his own experiments and willingly see his achievements consigned to the footnotes of history?

The history of technology is full of dead ends. They attract less attention than the open roads, but that is because it pleases our vanity. We prefer success to failure, achievement to frustration.[25] William Preece in 1895–1896 found himself at a technological dead end and it says much for his intelligence that he recognized the fact. It also speaks well for his ability to swallow his pride, for the decision to cut his losses and take up Marconi cannot have been an easy one. For more than 40 years, as a career employee of the Post Office, Preece had been working with a system of wireless telegraphy that was destined to lead nowhere. This was inductive telegraphy, a system by which signals could be exchanged between two locations—separated, perhaps, by a body of water—by laying out long wires in each location parallel to each other. Fluctuating currents flowing in one wire would then induce fluctuating currents in the other, although at greatly attenuated levels. There had been many experimenters with this method; Preece was one of the last of them, and he achieved more solid success than most. The techniques involved were simple, particularly for a man with access to the manpower and equipment of the Post Office. Stringing the long wires that inductive telegraphy required was not, after all, very different from the jobs that telephone and telegraph linemen did every day. And it worked. Preece had the results to prove it.[26]

It did not, however, work very well. And there were some situations in which it did not work at all. The greater the distance over which signals had to be transmitted, the longer had to be the parallel wires at each location. Preece's rule of thumb was that each wire had to be as long as the distance between the stations: to signal over 100 miles by inductive telegraphy, you would require parallel wires at each location 100 miles long.[27] If this was awkward enough in normal situations, consider the problem of communicating with a small island. Or a lighthouse, perched on some remote rock. Or a ship. Inductive telegraphy worked best when there was lots of land area over which to lay out the wires. This was, however, just the situation in which conventional wired telegraphy worked best, and with no greater cost for wire.

The problem was fundamentally insoluble, for reasons which electromagnetic theory makes clear. A varying electromagnetic field has two components of interest: an inductive field, which varies inversely with the square of the distance, and a radiation field, which varies inversely only as the first power of the distance.[28] Preece's system, like all others of the type, suffered from very rapid attenuation with distance because it relied on inductive coupling. The long parallel wires were an attempt to compensate for this, and over short distances—half a dozen miles, perhaps—the system worked well enough to be useful in emergencies. Long distances, however, were quite another matter; here only radiation—the newly discovered Hertzian waves—held out any prospect of producing at the receiver effects powerful enough to be detected and deciphered.

Whether Preece understood the physics of the matter is doubtful and probably not very important. He knew, of course, that Marconi's system used Hertzian waves; but he drew no sharp distinction between different modes of wireless telegraphy and he did not greatly stress the element of greater range. Consider his statement to the Select Committee of the House of Commons in 1906, remarks which he knew would be widely reported and commented on: "The main point in my coming here is that wireless telegraphy is a free system open to the whole world. It was not patented by the Post Office; it was developed by the Post Office, and when Mr. Marconi came he only came with a new way of doing an old thing . . . . He could not, and did not, patent wireless telegraphy, for wireless telegraphy existed then."[29] Furthermore, what attracted Preece to Marconi's "new way" was not primarily distance but the simple fact that it used less wire than Preece's method and, most important of all, that it could be used to communicate with moving ships.

What he chose to emphasize in 1897, when he lectured on his system and Marconi's at the Royal Institution, was Marconi's coherer (described as a "new relay") and the fact that "conductors of very moderate length only" were needed. Distance, to be sure, was important—Preece thought it depended on the length of the spark—but after all Marconi in 1896 had not achieved distances any greater than had Preece's inductive method. That was not the critical difference. What seems to have impressed Preece most strongly was Marconi's ability to dispense with the miles of wire that the inductive system required. It was this that made it possible to think of communicating with ships and even—Preece's great ambition—of uniting the telegraphic and telephone systems of Britain and the Continent by bridging the English Channel without a cable. And, indeed, it requires no great stretch of the imagination to sense the impression that Marconi's parabolic reflectors and short top-loaded vertical antennas must have made on an engineer accustomed to thinking in terms of miles of wire. If Preece had been able to visualize the massive antenna arrays that Marconi engineers were later to erect at Poldhu, South Wellfleet, Glace Bay, and Clifden, he might have been less enthusiastic.

Preece's reaction to Marconi's system was not merely personal. It was a question, as he conceived it, of official bureaucratic responsibility. The communications functions that wireless telegraphy was intended to perform were not frivolous incidentals that could be shrugged off and forgotten. Preece and the Post Office were under statutory obligation to oversee the development of all forms of electric communication within the British Isles and British territorial waters. They were also, it would seem, under informal but perceptible pressure from the Admiralty and from Lloyd's, the maritime intelligence and insurance association, to develop techniques of some kind for communicating with lighthouses, lightships, and ship traffic in the vicinity of British ports, particularly in the Western Approaches and the English Channel. Businessmen and financiers who, from their own memory, could vividly recall what the advent of the telegraph and telephone had meant to the internal economic life of Britain were not likely to underestimate the importance of extending that communications system into the surrounding oceans. These were communications needs that visual signaling, the landline telegraph, and the submarine cable could not meet. Preece and his department knew this, and by 1895 they also knew that the answer was not to be found in the technological system of "wireless" that they had up to that point been using.

The initial statute that gave the Postmaster General jurisdiction over all forms of electrical communication in Great Britain had been passed in 1868. Its purpose had been to bring the development of domestic telegraph networks under public control. Extinguishing the rights of privately owned companies, however, had proved a slow process, and it was only in 1896, with the purchase of the National Telephone Company, that a true state monopoly of internal telegraphy and telephony had been achieved.[30] The fact that, in that year, Marconi approached the Post Office with a communications system unlike any the original statute had envisaged was, of course, a coincidence. Nevertheless, it presented the authorities with the need for a critical decision that could not be long delayed. Clearly "wireless" communication could not be banished from bureaucratic consideration just by ignoring it. There were too many people working in the field, as Preece well knew, in Germany and France as well as England. And there were clearly recognized communication needs to be met. Here now was an Italian with what looked like a complete functioning system, and he was offering it first (in England, at least) to the government. What should be the appropriate response? To do nothing would be to permit a new form of electrical communication to develop without the public oversight that the Post Office was charged with maintaining. To extend some kind of formal approval and then permit development by a private company would be to run counter to the policy of direct state ownership that had just reached successful completion in the more conventional fields. Seen in these terms, the obvious policy was for the Post Office to test Marconi's system and, if it seemed to perform as claimed, to adopt and develop it officially as the state system.

This, in fact, seems to have been precisely what Preece had in mind, although events were to follow a very different course. His own experiments had been undertaken in the hope that they would yield a communications system suitable for official adoption. By 1896 he knew that this was highly unlikely. In relation to certain important objectives it was in fact impossible. Marconi's system looked as if it might attain precisely those objectives. It was at least worth a trial: the loan of a few Post Office engineers, a little equipment, some readily available space. A few tests, properly observed, should settle the matter. No commitment to future development had been asked for; no government subsidy had been sought.

It is worth emphazing, however, how limited were the communications functions which Preece thought of as possibly suitable for wireless telegraphy, whether inductive or otherwise. It was a matter of small islands, lighthouses, and above all moving ships. There was no mention of competition with the established technologies of communication over land; no mention of long-distance signaling in competition with the submarine cables; and of course no thought of broadcasting in any form. Preece was concerned with short-range point-to-point signaling in a limited number of special situations where the established technologies were, for particular reasons, ineffective or, because of low traffic volume, too costly. What he saw in Marconi's system was a possible solution to a series of special problems: the problems that his inductive system had been intended to deal with.[31]

* * *

No one could accuse Preece of being less than wholehearted in his support, once he decided to give it. The necessary tests were arranged quickly and conducted efficiently. In general Marconi's equipment performed well, but there were a few difficulties. The Salisbury Plain tests in September 1896 were tests of directive antennas, and Marconi's sheet metal parabolic reflectors proved effective. Distances achieved were just under 2 miles, the wavelength being probably in the neighborhood of 2 meters (150 MHz). The next proposed test, however, Marconi declined even to attempt. This was a proposal originating with the War Office that he try to develop a transmitter that could activate either, but not both, of two receivers in a sheet metal box sunk in water a mile offshore.[32] The objective was to develop means of exploding underwater mines by remote control. Essentially it was a test of selective tuning—the kind of thing Lodge would have enjoyed tackling. That is why Marconi could not attempt it. His transmitters, with their rapidly damped sparks, were broadbanded; his receivers had no tuned circuits. There was nothing in his equipment that could even begin to approach the kind of selectivity that the War Office test would have required.

There were also anxious moments during the Bristol Channel tests in May 1897. These were tests of signaling over considerable distances over water, between the same sites that Preece had used for some of his inductive experiments a few years before. The maximum distance, between Lavernock Point near Cardiff on the Welsh side and Brean Down near Weston-super-Mare on the English one, was just under 9 miles. An island in midchannel provided a site for intermediate distances. Over such ranges Marconi knew from experience that his parabolic reflector antennas were ineffective. He used instead the arrangement that had given the longest range in Italy: the grounded vertical antenna, with one plate immersed, in this case, in the water. Even with this configuration he at first could get no results. Success came when, leaving one elevated plate on a pole at the top of a cliff, he moved the receiver down to the water's edge, connecting the two by a long wire. Later, over the full 9-mile distance, he used a kite to support his aerial wire. With this arrangement signals between the two sides of the Channel were clear and reliable. Once again Marconi's faith in the vertical antenna, and the longer wavelengths which it enabled him to use, was reinforced.[33]

Up to this point all had gone as well as Preece could have wished. The tests, however, had attracted general attention and had been widely publicized—a process that Preece himself had certainly not hindered by his own pronouncements. One interested observer, introduced to Marconi by Preece, had been Professor Adolf Slaby, of the Technical High School at Charlottenburg, Germany. Slaby's visit had been arranged through diplomatic channels; his presence, though possibly unwelcome to Marconi, could hardly have been prevented.[34] He departed greatly impressed and before the year was out was describing Marconi's achievements in glowing terms before the German Association for Industrial Development. Slaby was specific about where he thought Marconi had made his technical breakthrough. "What I saw," he said, referring to the Bristol Channel tests, "was in fact something new; Marconi had made a discovery. . . . [He] has first of all discovered for the process an ingenious device which, by the most simple means, obtained a reliable technical effect. He has further shown that telegraphy only becomes possible by connecting the apparatus with earth on the one hand, and by using long extended perpendicular wires on the other."[35] Marconi's coherer and the grounded vertical antenna were what made success possible. Out of this visit there developed first the Slaby-Arco system, high-quality radio equipment that gave the Marconi Company formidable competition, and later, in 1903, the still-thriving Telefunken system, marketed by the Gesellschaft für Drahtlose Telegraphie.[36]

German competition was, however, no more than a long-range problem. More immediately threatening to Preece's plans was British entrepreneurship. In April 1897 Marconi had been approached by H. Jameson Davis with the suggestion that, instead of working toward a contract with the Post Office, he should join in the formation of a private company to exploit his inventions.[37] Jameson Davis was his mother's cousin; it was he who had met them on their first arrival in England, who had found them respectable lodgings in Bayswater, and who had arranged for Marconi's letter of introduction to Preece. In all probability it was also Jameson Davis who had helped Marconi find a good patent lawyer. This was not, in short, a suggestion that Marconi was in any position to ignore. He was not an isolated individual but a member of a family, and that family had interests and expectations of which he had to take account.

It is important to realize that when Marconi and his mother came to England they came not as strangers but as returning members of an extended family. Annie Marconi had been born Annie Jameson. Her father, Andrew Jameson of County Wexford, Ireland, had been born in Scotland. He migrated to Ireland with his two older brothers, who established a brewery in Dublin. Andrew Jameson built a distillery at Fairfield and there began the production of what has since become one of Ireland's major exports as well as an object of veneration to all lovers of great whiskeys. As his business prospered, he bought for his residence an old manor house, Daphne Castle of Enniscorthy, and there Annie Jameson had been born. She was also related to the Haig and Ballantyne families of Scotland, creators of liquids no less admirable than that which her father made.[38]

The talents of a novelist are needed to explain how a girl with this background came to be the wife of Giuseppe Marconi, silk merchant of Bologna, and mother of Guglielmo, first entrepreneur of the electronic age. Her granddaughter tells the story this way.[39] Annie, who had a beautiful singing voice, was offered a singing engagement at London's Covent Garden Opera House. Her parents, considering this no proper vocation for a daughter of theirs, forbade it. As consolation, she was offered a trip to Italy to study *bel canto*. While staying with friends at Bologna she met Giuseppe Marconi, 17 years older than she and already a widower with one son. He proposed marriage; she was inclined to accept but thought it best to return home and ask her parents' consent. She did not get it; from the family's point of view such a marriage was totally unsuitable. Giuseppe, however, was patient, and Annie had a will of her own. As soon as she came of age, she eloped. The couple met at Boulogne in 1864 and were married. They made their home in Bologna. There were later brief visits to England—young Guglielmo even attended Rugby for a short time—and some reconciliation was reached with her parents. Not until 1896, however, when she and her son arrived in London to see what could be done about his invention, did Annie Marconi leave her husband for any extended period.

Guglielmo Marconi, as soon as he was personally sure that his invention was a technical success, had offered it first to the Italian Government, receiving in return a polite but definite expression of disinterest. It made good sense to think next of Britain. Maritime communications was clearly the field in which wireless telegraphy could find its first practical use. Great Britain had the world's largest merchant marine. It was the hub of international trade, finance, insurance, and marine intelligence. It was still the world's leading naval power. There were also, however, good reasons of a quite different character why it made sense to turn to Britain. In Britain there were important assets on which Marconi and his mother could draw, assets of family membership that would not have been available in Paris, Berlin, or New York. The Jamesons and the Haigs were rich, but not very rich. They were "county families," not aristocracy; their wealth came from commerce, not from land. They were, however, above all a tightly knit Scots-Irish clan that looked after its own. Annie Marconi, despite a questionable foreign marriage and more than 30 years of residence in Italy, was still a Jameson. She and her son were accepted; they were looked after; they were given advice and guidance when they needed it. They were enabled to draw on family resources, not by special dispensation but by right.

These resources were money, advice, and connections. Money was at first of slight importance. The Marconi family was not poor; Annie Marconi had her pride; and there were no large expenses. Advice and connections were a different matter. These Guglielmo Marconi could not do without. How do you apply for a patent in England? How do you find legal counsel who will understand what you are talking about and who can put together patent specifications that will withstand scrutiny? How, in particular, do you arrange to meet people with the power to decide? How do you get past the overworked secretaries and junior officials who always and inevitably find it easiest to say no? How do you gain entrance to the corridors of power? Without shrewd and informed advice young Marconi could have cooled his heels for years in London without once describing or demonstrating his equipment to any responsible individual. He had to have introductions to people of influence—not, at this stage, the politicians, but rather the senior officials in the civil service, the War Office, and the Navy, who controlled access to government sponsorship and who had the authority to cut red tape. Here Jameson Davis, representing the extended family of which Annie Marconi was a part, played an indispensable role.

Good advice and useful introductions, however, would have served little purpose if young Marconi had not used them well. Coached and guided by Jameson Davis he may have been, but when fielding Preece's probing questions or wrestling with balky equipment on the cliffs of Lavernock Point, family connections were not of much help. The Post Office tests were by no means a formality, and only Marconi's technical competence, ingenuity, and determination carried him through them successfully. Then, as later, Marconi impressed because he knew his job better than anyone else around; he had good hands with equipment; he could survive discouragement; and, like his mother, he had a formidable stubbornness in adversity. He never had trouble attracting fine engineers to work with him, and once they had worked with him they seldom left his service. During these critical months of 1896 and 1897 he was facing the scrutiny of well-informed and skeptical observers who cared nothing about his family background or how he had wangled his introduction to Preece. They were interested only in the technology and whether it would perform as claimed.

It was, one imagines, precisely this combination of technical competence and impeccable credentials that converted Preece into such a valuable and willing ally in 1896. Marconi did everything right. There was no importunity. There were no requests for money. There were no excuses or delays. Conversely, of course, there were no promises. Marconi accepted no government funds and he gave Preece no commitment. If Preece's lectures lent a semblance, if not the substance, of official approval to his sytem; if the government-financed tests gave publicity that could have been obtained no other way; if by mid-1897 the name of Marconi was, through Preece's efforts, known in Britain and Europe as it had never been before—that was certainly a substantial gain to Marconi and the family that was backing him. But at the same time Preece, through Marconi, was being helped out of a potentially embarrassing technical impasse and was being given the opportunity, if he acted fast enough, to acquire for his department and his government first rights to Marconi's system. There seemed, in short, real benefits to both parties; and if, until the middle of 1897, no awkward questions were raised and no uncomfortable pressures applied, that was no more than consistent with the spirit of cooperation between gentlemen and fellow professionals in which the whole affair had been conducted.

It is sometimes suggested that Marconi turned to private enterprise to finance and develop his system because the British Government was slow in making him a firm offer.[40] The facts hardly support such a view. The first explicit suggestion that Marconi should consider transferring rights to his invention to a private company instead of to the government came from Jameson Davis in April 1897. Marconi had at that time been in England only a little more than a year. His patent had not yet been approved. The Bristol Channel tests, which for the first time gave evidence of clear superiority over inductive telegraphy, had not yet been conducted. Preece was, in short, in no position at that time to enter into formal negotiations. What case could he have presented to the Postmaster General and through him to the Treasury to justify a request for funding? The fact is that Preece had moved with remarkable swiftness, and if Marconi was intent on doing business with the government he had every reason by the middle of 1897 to feel confident that an acceptable arrangement could be made. Certainly no one who heard or read Preece's address at the Royal Institution in June of 1897 could have been left in any doubt as to the solidity of his support. Preece had publicly committed himself to Marconi by the summer of 1897 as firmly and fully as any responsible civil servant could.

Matters may, however, have looked very different from the point of view of Marconi and the family representatives who supported him. The tests that to Preece were simple prudence may well have seemed to them unnecessary bureaucratic delays. Marconi's apparatus worked. If imperfect, he could improve it, given a modicum of funds and encouragement. Proper application for a patent had been made and in due course it would be issued. What reason was there to wait for the government to make up its mind? Simpler means were at hand.

What Jameson Davis proposed was that a private company should be formed with the primary purpose of exploiting Marconi's invention commercially and developing it further. He suggested, as suitable terms for Marconi personally, £15,000 in cash plus a substantial block of shares; and he promised a working capital of at least £25,000 to be spent under Marconi's personal supervision on the further development of his system.[41] Marconi responded with propriety, as Davis undoubtedly knew he would. He communicated the terms of the offer to Preece immediately. He added that he had neither sought nor encouraged it, but that the costs of developing and patenting his system were beginning to press on his personal finances. His major interest was in developing and improving his invention; Davis's offer, if accepted, would enable him to do that.

Preece did what he could to counter the move. He advised Marconi that he was not at liberty to negotiate with any other party, in view of the assistance he had already accepted from the Post Office. Marconi did not deny the assertion, but neither did he explicitly accept it. Preece advised his superiors of the turn of events, and recommended that Marconi be offered £10,000 for his patent, subject to a favorable opinion on its validity from the law officers of the Crown. He called attention to the fact that Oliver Lodge claimed priority of invention but added that he did not personally support that claim. He concluded by stating, first, that Marconi did not know of the high opinion of his system held by the Post Office, and second, that the Post Office was the only body in Britain capable of developing it. In each of these beliefs Preece was deceiving himself.

It is not clear what happened to Preece's recommendation. It required, of course, approval by the Treasury as well as a ruling from the law officers of the Crown on the patent. Preece could apply little influence in either sphere, and any signs of unseemly haste would not have advanced his cause. If a reasonable offer had been received from the government—and in view of Davis's offer of £15,000 plus securities Preece's figure of £10,000 was hardly reasonable—Marconi might well have accepted it. His inclination, both in Italy and after arrival in England, had been to work through government bureaus rather than approach private financiers. In the summer of 1897, however, time was running out both for Marconi and for Preece. On Marconi's side there were family obligations and family expectations—those expectations that, because they are never stated explicitly, are hardest of all to combat. For Preece there was the cumbersome machinery of interdepartmental memoranda and committees, an engine of administration admirably adapted for dealing with the routine and minor departures from it but ill designed for coping with adjustment to new technology.

The recommendation was still, we are told, "under consideration" when, in July 1897, the Wireless Telegraph and Signal Company Limited was incorporated.[42] To it Marconi transferred all except the Italian rights to his patent, receiving in return £15,000 in cash plus £60,000 in paid-up shares and a contract employing him as engineer on a three-year contract at £500 per year.[43] The nominal capital of the company was £100,000. The cash to purchase the remaining £40,000 of ordinary shares, out of which Marconi was paid, was subscribed mostly by the Jameson and Davis families, possibly with some Haig and Ballantyne funds from Scotland. It does not appear that any monetary capital from the Marconi side of the family was invested. The company was, in that sense, a formalization of the family syndicate that had supported and guided Marconi since his arrival in England. It was, after all, a relatively small speculation, a minor diversion of earnings from the whiskey trade. Jameson Davis, representing the interests of the family, became first managing director, with the understanding that he would be allowed to stand down as soon as the company was on its feet.

It would be understandable if Preece felt that he had been used. The public reputation of Marconi and his system was, after all, very largely the result of his work and his sponsorship. Now he had been made to seem, if not a fool, at least slow, indecisive, and dilatory, a bureaucrat outwitted by the whiskey aristocracy. Nor was he immune from more serious criticism. He had, for more than a year, permitted arrangements with Marconi to remain informal and casual, in the belief that the man he was dealing with had no other important backers and no alternative ways of getting his invention developed. Convenient as this may have seemed at the time to Preece, it played into the hands of the men behind Marconi, furnishing them with publicity that could have been obtained in no other way. As late as 1907 Preece was still being lectured on this point by members of Parliament who resented the effective monopoly the Marconi Company had by then achieved in British radiotelegraphy. This monopoly, they suspected, had been attained largely because the Post Office had initially supported Marconi and then later, when other systems became available, had refused to license them on the grounds that they would cause interference to established Marconi stations. "If, at the time when the Post Office was giving Mr. Marconi effective assistance," wrote the Select Committee on the Radio Telegraphic Convention in that year, "the government had thought it expedient to secure a right of preemption of his inventions and patents, an enterprise of national importance could have been prevented from passing into the hands of a private company and subsequent difficulties might have been avoided."[44] This, however, was easy hindsight. Preece had misread the situation when Marconi first came to him. By the summer of 1897 he had lost his chance. He had moved as fast as a man in his position could move, but not fast enough. When, a few years later, the Marconi Company offered to lease to the government rights to all Marconi patents, the price was £50,000 a year. This the government refused as exorbitant.

For at least three reasons the outcome was important. In the first place, it helped to determine how the radiocommunications industry in Britain was to develop. Under Preece's leadership policy had been evolving in the direction of government ownership. The new technology of radio was to be treated, in essence, as an extension of the older technologies of the telegraph and telephone: as a public service facility, owned and operated by a government department. In relation to internal overland communications, this concept was firmly maintained until after World War II, when private television broadcasting was first permitted.[45] In the period with which we are at present concerned, however, radio was thought of primarily if not exclusively as a maritime communications technology: communications with ships, lighthouses, and islands, possibly also transoceanic communication in competition with submarine cables. In these fields any plans to establish a government-operated system were, after 1897, set aside, leaving the field open (so far as Britain and British territorial waters were concerned) for the Marconi Company.[46] The influence of government on the organization and functioning of the industry had now to be exercised indirectly, through its licensing powers under the Telegraph Acts of 1868, 1869, and 1904. These powers were at first exercised reluctantly and inconsistently, largely because the technical issues of tuning, selectivity, and frequency allocation were little understood. Pressure for an effective government licensing system came from the requirements of international radio conventions and of military preparedness.

Secondly, the way in which Preece had been outmaneuvered, if not outwitted, left a legacy of bitterness between the Marconi Company and the Post Office which was distinctly perceptible and still capable of influencing public policy many years later. The clearest evidence of this is the extreme difficulties that were encountered in all the recurrent attempts to construct an "Imperial chain" of radio stations capable of linking Britain with her overseas dependencies and Dominions, a chain that the enemy could not cut, in the event of war, as easily as the submarine cables. This was a project of national importance; it was also a project that no organization in Britain but the Marconi Company had the technical knowledge and trained personnel to handle. Yet, until 1924, all attempts to work out contractual arrangements acceptable to the company and to the government proved futile, the most determined attempt, in 1911–1912, culminating in a major political scandal and highly unpleasant lawsuits.[47] It is difficult to avoid the conclusion that there originated in 1897 and continued thereafter a tradition of distrust of the Marconi Company that no amount of demonstrated virtuosity on the part of its technical personnel could dissipate. The Marconi Company, for its part, had its own traditional grudge against the Post Office, holding Preece responsible for Slaby's visit to England and the subsequent rise of German competition in the form of the Slaby-Arco and Telefunken systems. Mutual distrust between government departments and powerful corporations certainly does not always or necessarily militate against the public interest; in the case of the Marconi Company and the British Post Office, however, it is hard to point to any clear benefits.

Little of this resentment and distrust, it is interesting to note, was directed at Marconi personally. Preece was, needless to say, annoyed and irritated, but his respect for Marconi's technical competence was undiminished and the two men continued to collaborate and to speak well of each other.[48] Preece's successor at the Post Office, one J. Gavey, testifying before the Select Committee of 1907, used a phrase to describe the episode that puzzled his questioners. He said that Marconi had, in 1897, "joined hands with the company," as if to stress that admiration for Marconi as inventor was entirely compatible with hostility to the company that bore his name.[49] This was true also in 1911–1912, when the Marconi scandal threw British politics and journalism into an uproar. None of the allegations that were too freely exchanged at that time touched Marconi personally, and pains were taken by all but the most unscrupulous enemies of the company's management to distinguish between the man and the firm. Partly this can be explained in terms of the growing myth of Marconi as the "wizard of wireless," a man of pure science whose motives were humanitarian and not pecuniary and who was almost above personal criticism. But partly too it reflects the fact that many who found much to dislike about the Marconi Company and its policies nevertheless could not but admire the skill of its engineers and operators. Radiotelegraphy very quickly developed a fellowship of its own, as all technologies do. This was a fraternity of technicians and engineers, an exclusive brotherhood of men who built the system, understood it, and kept it operating. It had its own subculture, its own language, and its own status system, in which Marconi stood very high. Businessmen and financiers did not.

Thirdly, and most important for Marconi personally, the formation of a private company to exploit his patent provided him with a vehicle to support and finance his work in the years ahead. An agreement with the Post Office might have rewarded him for what he had done in the past; it provided no assurance of support for work in the future. With the company, in contrast, he would have a continuing relationship: its fortunes and his would be linked. He could be confident of this, not because it was a private company, but because of the kind of company it was. Because ownership and control in the critical first decade and a half of its existence were confined to a small circle of family members, Marconi was shielded from pressures for immediate returns. There would be no agitation for dividends, no risk of loss of control to outsiders. Any revenues earned would be ploughed back to finance further development. In 1937 Ambrose Fleming remarked that one of Marconi's important assets as an inventor had been that he enjoyed freedom from any necessity for income-earning work other than his work on radio.[50] This was true principally because an institution had been created with precisely that end in view. Similarly, although he was the most important human resource of the company that bore his name, Marconi was under no pressure to generate dividends. That was not the primary purpose for which the company had been created. Earnings were important—indeed, frequently critical—but as a source of funds for research and development, not as a source of income to the proprietors.[51] The company was an embodiment of the extended family to which Marconi belonged; it existed as a resource for his benefit. Returns would be in the form of capital gains in the long run, not profits in the short.

* * *

As far as Marconi was concerned, the company formed in 1897 was an instrument by which he could continue his experiments. But along what lines? In what directions? These were not questions that could be answered as if they were problems in the logic of pure scientific inquiry. Technological change in radiocommunications did indeed have a logic of its own, but it was a pragmatic logic, a matter of serving the practical needs that arose in the course of attempts to meet market demands. For Marconi the company might be a vehicle for technical experimentation; nevertheless, it was a company, a business enterprise that could survive only if it could sell products and services in the marketplace.

Commercial problems and technological problems, therefore, were bound together in close interaction. Just as the anomalies that Marconi and his engineers encountered in developing radio technology provided feedback to science, so the challenges and difficulties that the company met in trying to establish its economic viability provided feedback into the development of technology. After 1897, therefore, in contrast to what had gone before, the price system exercised a strong and persistent influence on the course of technological change in radio. Information generated in the search for revenue, in the negotiation of contracts, in the attempt to build and operate a communications system that could compete with rival modes became part of the necessary inputs for engineers and technicians as they sought to refine their equipment. Devices and methods were developed, adopted, or discarded in terms of their contributions to survival and growth in the market.

But what was the market? An answer to this question—or rather a series of answers, for no single one could be final—was essential to the company's survival. What business was it in? What business should it try to be in? Simple questions, it would seem: naive, almost. Yet answers were never self-evident. Most of the company's history from 1897 until well after World War I can be told as a process of "hunting" in the cybernetic sense, a trial and error search for solutions to these questions. In this search Marconi played a vital role, perhaps even more vital than his role in technological development, a field where he soon had competent assistants. Marconi had a sense of where markets existed or could be created. If others, including on several occasions the management of the company, did not see them, Marconi, with that flair for public relations that makes him seem the very model of a twentieth-century entrepreneur, pointed them out. If the technology to serve those markets did not already exist, he and his engineers created or borrowed it. There is no item of new technology in the history of the Marconi Company after 1897 that cannot be clearly identified as a solution, attempted solution, or partial solution to a problem presented by the needs of existing or projected markets.

Two technological problems faced the company immediately upon its formation. One was the problem of achieving greater distances, the other that of finding some way in which two or more stations could transmit at the same time without interfering with one another. The first problem attracted most immediate attention, and the reason was simple: without a demonstrated ability to communicate over stipulated distances, there was no market. The original objective of the company was to provide a radiotelegraphy service for lighthouses and lightships around the coasts of the British Isles. It was clearly necessary to show that Marconi equipment could cover the distances involved, and not just sporadically. A series of tests for the Corporation of Trinity House late in 1898 proved the point. Nevertheless, no contract was signed and in 1899 Trinity House announced that they had decided against adopting the system. Similarly in the summer of 1898 demonstrations were arranged for Lloyd's, the association of marine insurance underwriters, and a communications system was set up on Rathlin Island off the coast of southern Ireland, to relay news of passing ship traffic to the mainland. This was a strategic location for marine intelligence, covering the Western Approaches in an area where fog often prevented visual signaling from the island to the shore. Once again, there was no difficulty showing that Marconi equipment could cover the necessary distance. If any doubts on that score remained, they were removed by March 1899 when Marconi successfully passed signals across the English Channel. There was, in short, no question that the technology available to the company was adequate to achieve its original objectives, at least so far as distance was concerned. Up to the end of 1899, however, there were no contracts and therefore no revenue, and this despite much favorable publicity and a series of tests and demonstrations that had been uniformly successful.

For this there was one overriding explanation. Commercially, the company at this time visualized its function as that of manufacturing radio equipment for use by others. It was no part of its objectives, in these early days, to set up and operate a radiocommunications system itself, except for purposes of demonstration. It wanted to sell equipment. Purchasers were expected to staff and operate their own communications systems, using Marconi equipment and drawing on Marconi personnel for advice, but nevertheless as autonomous entities. In the demonstrations for Trinity House, for example, great emphasis was laid on training lighthouse and lightship crews to operate the apparatus themselves. The same was true in the Rathlin Island tests for Lloyd's.

This approach to the market problem was premature to say the least. Later, when technical knowledge was more widely diffused and there was a pool of operating and engineering talent to draw on, it might be reasonable to expect customers to set up their own communications systems, but not in 1898–1899. The Post Office could have done it; Trinity House and Lloyd's could not, or were unwilling to face the risks and uncertainties involved. As long as the Marconi Company persisted in this interpretation of its role, its potential customers were in effect limited to organizations that had or could easily recruit competent personnel. This meant, in practice, armies and navies.

These markets were by no means neglected. The first actual sales of equipment by the Marconi Company were to the British War Office in 1898 for use in the Boer War. The transaction had some public relations value but led to no large additional sales. More significant was a contract signed with the Admiralty in July 1900 which provided for the installation of Marconi equipment in 26 ships and 6 coast stations, and their maintenance for a period of 14 years, the life of Marconi's British patent. For each of these installations the Admiralty agreed to pay £3,200 plus an annual royalty of the same amount for the life of the contract, the company in return granting full use of all Marconi patents, present and future, and contracting to install and maintain the apparatus, to train Navy signalmen in its use, and to keep the Navy informed of any improvements in equipment and methods.[52] Contracts were also sought with the United States Navy Department, but with less happy results.

The Admiralty contract promised some needed cash and was important for prestige reasons, but its growth potential was limited. Certainly military and naval establishments, once they learned how to use radio for strategic and tactical communications, would be large customers. There was a strong possibility, however, that they would develop equipment of their own, using the same skilled personnel that had made it possible for them to purchase Marconi equipment outright in the first place, and paying scant respect to patent claims. Were the 30-odd installations leased to the Admiralty the forerunners of larger orders in the future? Or would they serve as prototypes from which Navy designers could evolve equipment of their own?[53] Corporate survival and expansion required finding customers other than these. This meant nongovernmental customers and in the context of the time this meant private shipping interests. Private shipowners, however, were not likely to be attracted by the prospect of having to establish complete communications systems of their own, including not only shipboard installations but also a network of shore stations.[54] What they wanted was, not ownership of a communications system, but access to one. To serve this kind of a market, however, implied that the Marconi Company would have to redefine the kind of business it was in. It would have to become an operating company, providing communications services, rather than a manufacturing and sales company, providing communications equipment.

Recognition of these market imperatives came in 1900, with the formation of the Marconi International Marine Communications Company as a corporate subsidiary and the rapid construction of a string of Marconi shore stations in the years following. At the same time there came a reversal of Marconi sales policy. The Marconi Company would now sell equipment to no one. Shore stations and shipboard stations alike were to remain Marconi Company property, operated by Marconi personnel exclusively. Shipowners wishing radio facilities now leased equipment from the Marconi Company and by so doing gained access to a communications system that used standardized apparatus and standardized operating methods. Risks and uncertainties were correspondingly reduced, which was no small advantage to the shipowner. From the point of view of the company, control over both receiving and transmitting facilities implied that it was now possible to put together a completely integrated and exclusive system, one that could refuse intercommunications with any other. This was in fact the policy adopted. Except in the case of distress calls, Marconi operators were under orders to accept no messages from any station not equipped with Marconi apparatus.

The decision to set up a Marconi owned and operated system was shaped not only by the difficulty of selling equipment in the absence of such a system, but also by the Post Office's monopoly under the Telegraph Acts of all forms of "electrical" communication within the British Isles. The original purpose of these statutes had been to merge the several wired telegraph and telephone systems of Britain into a single interconnected network under government ownership. They had given no special consideration to maritime communications; there was no reason why they should. And they were of course written in terms of the existing technology of wired systems, not the as yet unborn technology of radio. Nevertheless, it was within the framework of these statutes that the Marconi Company had to operate. Clearly they ruled out any thought of an overland service competitive with the government-owned systems. Less obviously, they seemed also to rule out a private ship-to-shore service within British territorial waters, and it was precisely within these waters—particularly in the English Channel, the Bristol Channel, and the approaches to British seaports—that heavy revenue-earning traffic might be expected. The only loophole left open to the Marconi Company was the fact that the Acts did not prohibit a private company from sending messages for its own use, nor from providing the same service to others, provided that no direct charge was made for messages handled. Marconi shore stations, in other words, were free to communicate with Marconi ship stations, even within territorial waters and even though the ships themselves were owned by others. All that was necessary was that the traffic handled be, in the eyes of the law, intracompany traffic.[55]

The practical effect of the Telegraph Acts, in other words, as they affected the development of marine communications, was somewhat ironic. Rather than serve to carry over the policy of public ownership from the era of wired telegraphy into the new technology of wireless, they gave the Marconi Company additional reason to organize its ship-to-shore service as a private, closed system. The Marconi policy of nonintercommunication with competitive radio systems was not only well calculated to give it, as the leading firm in the industry, an effective monopoly; it was also the only way of circumventing the Telegraph Acts, the original purpose of which had been to assure monopoly by government.

Given this context of statute law, and given the fact that few opportunities for direct sale of radio apparatus seemed to exist, the decision to establish a Marconi communications service, open only to those who rented Marconi equipment and Marconi operators, made good sense. Indeed it is hard to conceive of any other market strategy available at that time that would have sustained the growth of the company. Indicative that the right formula had been found, and basic to the later success of the company, was the signing of a contract with Lloyd's in September 1901. Lloyd's had at this time, in all the major seaports of the world and most of the minor ones, a network of more than 1000 agents who, in addition to other duties relating to marine insurance, were especially charged with transmitting to London from their districts the latest news of ship arrivals and ship movements. Radio held out significant prospects for a vast improvement in the efficiency of this global information network, most notably perhaps in the new facility it afforded for communicating with ships on the high seas—a facility that submarine cables could never provide. It was reasonable for Lloyd's to be interested.

It was less reasonable, perhaps, for them to accept apparently without quibble the Marconi Company's demand for exclusive privileges, for the 1901 agreement was much more than a simple contract for the hire of Marconi equipment and operators. It called for the erection of a series of Lloyd's wireless stations on the coast of England and conveyed to Lloyd's the right to use Marconi apparatus at these stations; but it also bound them to use no other equipment, not to communicate with ships using other systems, and not to permit the use of any other system at, or in connection with, their signal stations. The only important exceptions made referred to stations along the coasts of the United States and Chile.[56]

What this meant in practice was that any major shipping line, if it wished to take advantage of the worldwide network of marine intelligence that centered on Lloyd's of London, had no alternative but to see to it that its vessels were equipped with Marconi apparatus. And if the shipowners themselves showed some reluctance to take this step, or to commit themselves to this particular line of equipment, pressure from marine insurance underwriters was likely to tip the scales in favor of integration into the Lloyd's-sponsored system. The critical element was, of course, the fact that Lloyd's agents were prohibited from communicating with ships using non-Marconi equipment. This requirement was not based on any technical incompatibility between, say, Marconi and Slaby-Arco or De Forest equipment, although Marconi spokesmen were capable of suggesting that it was. All systems used spark transmitters and, for detection, some kind of coherer. Nor was it a matter of using different frequencies, for standard marine calling and listening frequencies were soon established, and in any event transmissions were very broad and receivers nonselective. The reasons for stipulating that Lloyd's use only Marconi equipment and communicate only with others using Marconi equipment were not technological but economic. The Marconi Company was the first in the field; in fact, at the time the agreement was signed, it was the only company offering a marine radio service. It was in its corporate interests to deny to later entrants into the industry the right to integrate with the Marconi-established network. What technology made easy, corporate policy would have to make impossible.

The agreement was supposed to run until 1915. In fact it became null and void in 1908 when the International Convention on Wireless Communication at Sea, signed by Britain the year before, became effective. This Convention, largely at the insistence of Germany and the United States, called for unrestricted interchange of communications between all stations, regardless of the make of equipment, and thereby destroyed, as it was intended to, the virtual monopoly of ship-to-ship and ship-to-shore traffic that the Marconi Company had built up in the intervening years.[57] While it lasted, however, the contract with Lloyd's was the very cornerstone of Marconi expansion, and its signing was a master stroke of corporate strategy. It was also a source of acrimonious conflict between the two contracting parties. Why the directors of Lloyd's signed such a restrictive contract in the first place remains something of a mystery. It is true that in 1901 no other radio system was in commercial operation. That did not, however, compel Lloyd's to bind themselves exclusively to Marconi equipment for the next 14 years, nor did it require their assent to the principle of nonintercommunication with other systems when such systems appeared. They were in a strong bargaining position and could have insisted on a less restrictive policy had they wished. It appears that, in 1901, they did not so wish.

Under questioning before the Select Committee of the House of Commons in 1906, when the draft of the Convention on Telegraphy was being reviewed, the secretary of Lloyd's quoted the original agreement as stating that "Lloyd's is of the opinion that one system of wireless telegraphy should be in general use," and that that system should be Marconi's; and the managing director of the Marconi Company asserted flatly that the 1901 agreement took the form it did because Lloyd's anticipated that the government would want to take control of radiotelegraphy, and Lloyd's wanted to "join hands" with Marconi against the government. Testimony of this kind would suggest that Lloyd's had thrown its full weight behind the drive for a Marconi monopoly in marine radio. This may have been true initially, and it is plausible to believe that the directors of Lloyd's, confronted with a technology that promised great benefit but with which they were quite unfamiliar, may have opted prematurely for standardization and unified control. They did not, however, find the Marconi Company the most complaisant of bedfellows. Particular bones of contention seem to have been the question of who should have operational control of the coastal radio stations, and a recurrent suspicion on the part of Marconi executives that Lloyd's intended to turn their shore stations over to the Admiralty. Litigation in 1905 resulted in new "terms of settlement" that bound both parties to "do their best to persuade the British government not to grant licenses for the use of wireless telegraphy . . . to anyone but . . . Lloyd's and the Marconi Company." Uncomfortable with the original contract Lloyd's may have been, but they were still bound to the Marconi Company as allies in the drive for exclusive rights and the maintenance of nonintercommunication.

Without an understanding of the Lloyd's agreement it is very difficult to explain why Marconi equipment so quickly and completely came to dominate marine radio, at least in European and North Atlantic waters. The technology was simple; the apparatus was easy to build; there were no important technical secrets. Imitation and duplication of Marconi equipment, even improvement on it, presented no great problems. Within a few years De Forest in the United States and the Telefunken organization in Germany, to mention only the most conspicuous competitors, were able to furnish transmitters and receivers as efficient and operationally durable as Marconi's. Marconi's patents, it is true, were comprehensive in their claims, but they had not been tested in the courts; not until after 1910 did the Marconi Company take vigorous steps against alleged infringement. [58] It was not fear of being sued for patent infringement that deterred potential competitors but rather the near impossibility of finding a toehold in the commercial marine radio market. And for this state of affairs the agreement with Lloyd's was primarily responsible.

The market was lucrative, and it grew. There were a few pioneering installations aboard transatlantic liners before 1901. With the signing of the Lloyd's agreement the numbers multiplied rapidly. By the end of 1902 there were no less than 70 installations aboard ocean-going ships, with 25 shore stations to handle the traffic.[59] By 1907 all the large transatlantic liners carried radio installations, and all of these were Marconi. Nor were all these ships British. North German Lloyd, Compagnie Transatlantique, the Canadian Beaver line, and the Belgian mail steamers all carried Marconi equipment, as well as the Cunard fleet and, within a few years, the P. & O. and White Star lines. Foreign governments might resent their dependence on Marconi equipment; they might detest the nonintercommunication policy; and they might have good reason for wishing to encourage domestic manufacturers. But when private shipowners had to decide on shipboard installations, they almost invariably favored the Marconi Company.[60] The reasons were obvious. A ship carrying non-Marconi equipment found itself, as it were, ostracized, unable to find other vessels or shore stations that would accept its signals except in distress. A radio installation operating under such restraints was little better than no radio at all. The system was, in a sense, self-perpetuating: the more shipboard Marconi installations there were, the less sense it made to buy anything but a Marconi installation. Not until ratification of the Convention of 1907 were these arrangements broken up and other radio systems enabled to compete on their technical—or nationalistic—merits. And the determination with which representatives of the Marconi Company, both directly and through the British and Italian delegations, fought the Convention suggests the key role that the nonintercommunication policy had come to play in the company's planning.

The contracts with the British Admiralty and with Lloyd's opened up two markets with good prospects of growth. There remained a possible third: competition with the submarine cables for long-distance traffic. This was a market quite different in character from the other two. In marine uses radio was fulfilling a communications function that no other mode (except, over limited distances, flags and signal lamps) had served before. There was, in ship-to-ship and ship-to-shore communications, a "place" for radio—a gap in the existing communications technologies large enough to give the new mode a chance to establish itself. The long-distance communications market, in contrast, was adequately served, at least between major metropolitan centers, by the technology of the submarine cable: a technology of no great antiquity, it is true, for the first such cable had been opened for business only in 1866, but nevertheless one that had by 1900 been brought to a high degree of technical efficiency and in which large amounts of capital had been invested.[61]

By 1898 there were 14 cables across the Atlantic, 12 of them in operation. The companies that owned them were profitable. After a short period of price-cutting, a pool had been organized in 1888 to maintain rates at a level of one shilling (25 cents) a word: high enough to be lucrative, but not so high as to encourage the laying of new cables. At this level, although the cables carried between 25 and 30 million words annually, there was substantial unused capacity, estimated by one authority at about 50 per cent.[62] The cable companies, in other words, could have carried twice as much message traffic as they did without having to lay new cable. One implication of this was that they were in a position to cut rates substantially in the event that the new technology of radio threatened their comfortable oligopoly. If this reduction in rates led to an increase in traffic, as there was some reason to suppose it would, the companies could absorb the increase without adding new capacity or driving up their marginal costs.

In proposing to compete with the transatlantic cables, therefore, the Marconi Company was taking on a formidable opponent. This would have been true for purely economic reasons even if radio technology had been further advanced than it was at the turn of the century. In fact the primitive state of radio technology, and of the science of radio propagation, added further competitive handicaps. The demand for transatlantic communications service at this time came principally from financial and newspaper sources. What these users wanted was speed and predictability, the confidence that messages would arrive in precisely the form sent and within reliably short time limits. Even a large price differential in favor of radio would hardly compensate these users for errors or delays. In these terms the advantages were all on the side of the cables. The now-familiar technology gave assurance against errors in transmission. The excess capacity provided a measure of redundancy that minimized the risks of delays or breakdowns. And there was none of the uncertainty and unpredictability of propagation conditions that then as later afflicted long-distance radio. The only weak point in the position of the cable companies was their artificial level of prices. If radio service could undercut cable rates, it could possibly tap a completely new market: the general business or private user, reluctant to pay the cable companies' prices and willing to put up with something less than completely predictable and prompt service. Even this limited objective implied, however, that a transatlantic radio service would have to operate profitably at a rate level substantially lower than that charged by the cables in 1901. The cable companies, accustomed by now to acting in concert and possessing large financial reserves, could cut their rates far below the conventional "shilling a word" and still survive.

In view of these technological and economic uncertainties, it is remarkable that the Marconi Company, apparently at the instigation of Marconi himself, decided to proceed with the development of a transatlantic radio service. The gap that radio might fill in this area was not technological but economic, and a thoughtful analysis of the economics of the situation would have raised serious doubts as to whether any such gap existed.[63] It is tempting, indeed, for the historian to label the decision to proceed with the construction of high-powered stations for transatlantic traffic as premature, both technologically and in terms of economics. We know, after all, that it was not until almost 30 years later that long-distance radio began to make serious inroads on cable company earnings, and then it was by the use of equipment, frequencies, and modes of transmission significantly different from those available to Marconi in 1901. More recently still we have seen improved submarine cables, supplemented now by relays via artificial satellite, take over the bulk of long-distance commercial communications, with point-to-point radio reduced to a minor role. Historical hindsight, in other words, enables us to see that, when Marconi decided to enter into competition for long-distance message traffic, he was thrusting radio into a conflict for which it was not technologically ready and in which all the economic advantages rested with the alternative mode. And it is seductively easy to lecture Marconi for his technological brashness, and his financial advisers for their willingness to support him despite the miasma of technical and economic uncertainties that enveloped the project.[64]

An interpretation along these lines, however, would represent a serious misunderstanding of Marconi's personality and of his historical importance as creator of new technology. From this point of view—which, clothed in a different vocabulary, would certainly have been his—the Marconi Company existed to serve as a vehicle for his research and experimentation. Revenue indeed was necessary, and the contracts that brought in revenue, but primarily to provide funds to underwrite his work. The record does not suggest that this viewpoint was at all uncongenial to the family syndicate that controlled the company. If they had wanted dividends they would have ploughed back their earnings into the secure haven of the whiskey business instead of diverting them into untried ventures in signaling without wires. Marconi retained their confidence; he gave them no reason to believe that their investment would not eventually prove a wise one.

The fact that, when he began his work on transatlantic communications, the techniques he would require were not already at hand was not, to Marconi, a deterrent. Quite the contrary. It was what made the thing worth doing. There was a substantial body of respectable opinion that held that, because of the curvature of the earth, the project was impossible. There was, so to speak, between the west of England and Newfoundland or Nova Scotia or Cape Cod, a mountain of water; and there was no way in which Hertzian waves could follow that curvature since, as everyone knew, they traveled in straight lines like light. No scientific justification existed for believing otherwise. It was not until 1902 that Kennelly and Heaviside first suggested, as a tentative hypothesis, that there might exist an ionized layer in the upper atmosphere capable of reflecting or refracting radio waves of certain frequencies back to earth; and not until 1925, with the publication of Edward Appleton's research, did the existence and characteristics of the ionosphere become more than a matter of speculation. The point to stress is not so much that Marconi was going beyond the frontier of scientific knowledge of his day; that is obvious enough. What really requires emphasis is that the rate of advance of that scientific frontier depended significantly on what Marconi was up to. And this relationship was not the mere reporting of data that were incidentally or in a derivative way relevant to problems that scientists were already working on. Technology was a harder taskmaster than that: it posed the problems and demanded the solutions.

There were, then, two good rationalizations for the attempt to span the Altantic by radio. One was that it provided Marconi with a field for testing new techniques and equipment. The other was that it might open up a new market for radio services. It it easy, however, to overstate the degree of "newness" involved. Technologically, Marconi relied on the formula that had worked in the past: higher power, large antennas, and longer wavelengths. The technical changes were, as we shall see, almost wholly refinements of methods already known. And there was, of course, no departure from the basic technique of spark excitation. Economically too the discontinuity may have been more apparent than real. In public relations terms the idea of transmitting signals by radio from one continent to the other was dramatic and exciting. In the more sober calculations of corporate planning, however, the company had a second more reasonable and more realizable objective: the capability of providing continuous radio service to ships on the North Atlantic run. In terms of this objective, the function of the high-powered stations erected at Poldhu, Glace Bay, and on Cape Cod was to furnish radio coverage of the major sea approaches to Europe and the east coast of North America. And, as a glance at a globe (rather than a map on Mercator's projection) will show, they were strategically located for that purpose. They were not well located for interconnection with the domestic landline telegraph networks, as they should have been if intercontinental signaling had been the prime objective. Competition with the cables for point-to-point service was an intriguing long-term objective and it made good newspaper copy. But it was not the only goal. The attempt to establish reliable direct communications by radio between the new stations in Europe and North America provided an excellent test of their capabilities and gave Marconi and his engineers a proving ground for new techniques. The immediate economic rationale of the stations, however, was to achieve radio coverage of the North Atlantic shipping lanes, and in this sense their construction was a logical extension of the ship-to-shore service that already provided the bulk of Marconi business.

* * *

Market expansion created technological problems, and these in turn called for technological innovation. The major problems were two, and in Marconi's thinking they were closely related: first, the need to achieve greater distance; and second, the need for a tuning system, so that receivers might be made more selective and transmitters less broadbanded. Greater distance had always been Marconi's prime objective, but by 1900 the need for more efficient tuning could no longer be ignored. As transmitters and receivers increased in number, Marconi's earlier circuits, with coherer or spark gap connected directly to the antenna, were no longer adequate.

That interference between stations was becoming a serious problem is clear enough. Several years later a former chief engineer of the Post Office was asked by a parliamentary inquiry what were the major defects of Marconi's system at the time it was first introduced. He replied: "It was so constructed that every receiving station within range of a transmitting station could read all messages from that center, and it was impossible for more than two stations in a given area to interchange signals at a given time without mutual interference."[65] This statement was meant literally: when the witness said "every receiving station within range" he meant precisely that, not just receiving stations tuned more or less to the same frequency as the interfering transmitter. And the belief that this state of affairs was inescapable—that, at given power levels, only one station in a region could transmit at a time—was widespread. In 1901 Michael Pupin, professor of electrical mechanics at Columbia University, was asked what he thought of Marconi's claim to have received signals across the Atlantic Ocean; he replied that it was a remarkable achievement, but people should remember that of course there could never be more than one station in England transmitting to America at a given time.[66] Pupin was a physicist and undoubtedly knew of Lodge's experiments with syntony; his comment should remind us of the intellectual leap that was necessary to understand what syntony could mean for radio. We live today in an age that takes tuning for granted. Transmitters are supposed to have a certain place in the radiofrequency spectrum and they are supposed to be in that place and not in any other, even if their function is no more grandiose than opening a garage door. It is as hard for us to place ourselves in a presyntonic age as it would be for a modern navigator to place himself in a world without compass, sextant, or even astrolabe, where the very nature of spatial extension was only dimly understood.

Lack of effective syntonic circuits in receivers and transmitters had results that were sometimes disconcerting, sometimes comic, and always bad for the marketing of radio equipment. One of the earliest and most effective forms of public relations for the new art of wireless was the reporting of yacht races. In 1898 Marconi accepted an assignment from the Dublin *Daily Express* to report the progress of the Kingstown Regatta; messages were to be transmitted from a boat following the yachts to the offices of the newspaper, thus enabling it to report the results far ahead of its rivals. The affair went off very successfully, the Marconi Company earning a fee and some useful publicity. There were no other radio transmissions in progress in the area. In the following year Marconi contracted to cover the America's Cup races in the same manner for the *New York Herald* and the *Evening Telegram*, and once again the results were excellent. In 1901, however, events took a different turn. The international yacht races in that year were reported by no less than three competing radio services: Marconi, reporting for the Associated Press; the Wireless Telegraph Company of America, using De Forest apparatus, under contract to the Publisher's Press Association; and the American Wireless Telephone and Telegraph Company, with equipment designed by Harry Shoemaker, which had no contract but decided to set up a transmitter anyway. The result was a complete fiasco. Neither the De Forest nor the Marconi boats were able to send any information to their shore stations, which were reduced to the ultimate journalistic recourse of fabricating race reports out of whole cloth. This was, furthermore, after Marconi had devised syntonic circuits and introduced them into his equipment to reduce the risk of interference.[66]

Contracts were involved in this problem as well as publicity. Inability to reject interference was one reason, though not the only one, why the Marconi Company lost the opportunity in 1899 to become a prime supplier of radio equipment to the United States Navy. The chance to secure such a contract arose partly from the favorable impression made by the performance of Marconi equipment in the America's Cup races of that year. After the races were over, Marconi was invited to install his equipment on United States naval vessels for official tests. He was at first reluctant, claiming that he had brought only short-range equipment with him and that it did not include his most recent advances. Eventually, however, he agreed to proceed, with the stipulation that he could not guarantee the results of the tests. His caution was well founded. Results were generally satisfactory, though not as good as had been obtained during tests for the Royal Navy the previous July, but in one respect the Marconi equipment performed dismally. As the Navy's official historian puts it, "The results of the interference tests were perfect. That is, the interference was perfect."[68] It proved impossible for two ships to communicate while a shore station was transmitting, or for one ship to receive the shore station when another ship began sending. Marconi put the best face on it he could, explaining that he did have a device for preventing interference but that it was not yet patented and in any case he had not been told before leaving England that tests of this nature would be called for. As all three stations were operating on the same frequency, however, it is doubtful whether even the syntonic circuits that Marconi was working on at the time would have helped much.[69]

Marconi's approach to syntony was indirect. He was, of course, conscious of the problem of interference. He was also aware that his transmitters were wasting their power by dissipating it over too wide a band of frequencies. And he was sensitive to the charge that messages sent by radio could never be secret. All of these considerations pointed in the direction of syntony. What first led Marconi to tackle the problem of designing syntonic circuits, however, was the insensitivity of his receivers. In his original circuit the coherer was connected directly between antenna and ground. Now, the coherer was a voltage-actuated device. It responded to differences in potential between its terminals. Placed directly between the base of a vertical antenna and ground it was at a point where voltage differences were minimal, for the distribution of charges in a vertical radiator is such that currents are maximum and voltages minimum at its base, while the reverse is true at its tip. The coherer, in other words, was in precisely the wrong place. Small wonder that it was an insensitive detector.[70]

There were various ways of tackling this problem, once it was recognized and correctly diagnosed. Slaby, who was probably the first to see it, adopted the simplest possible solution, moving his coherer away from the base of the antenna and inserting it in the feedline (or "syntonic side wire," as it was called) a quarter wavelength away from that point, where there was a voltage maximum. This was the basis of the Slaby-Arco receiving antenna, patented in October 1900 (see Fig. 5.7).[71] Marconi

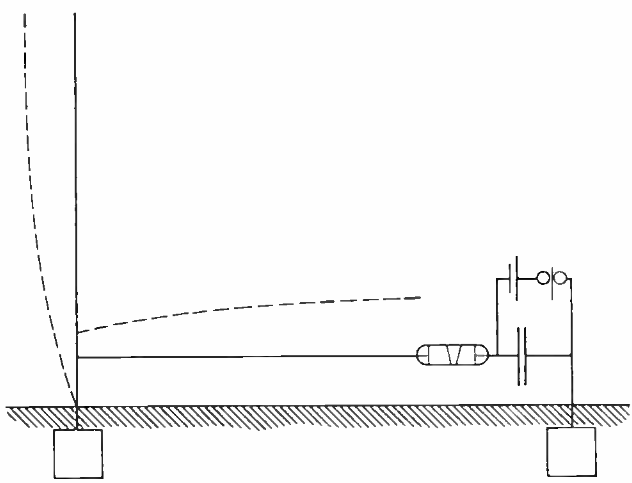


Figure 5.7 The Slaby-Arco receiving antenna with syntonic side wire. (Dotted lines represent voltage amplitude.)

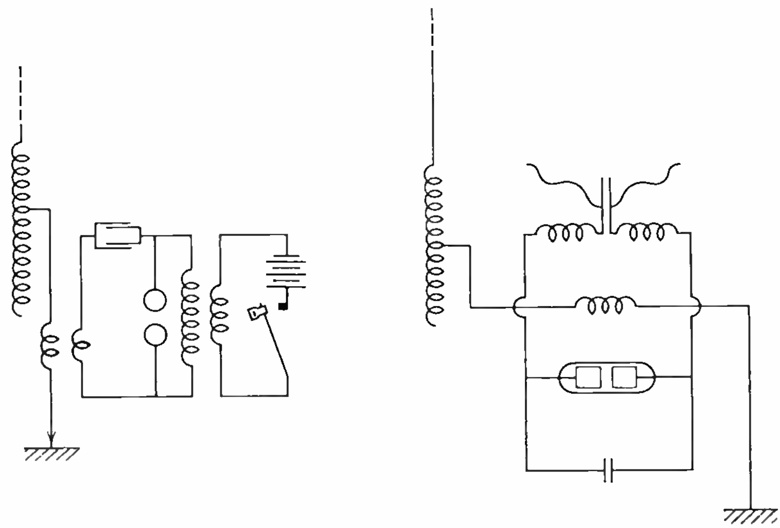


Figure 5.8 Marconi transmitter and receiver circuits with "jigger" transformer, 1898.

adopted a solution that looked very different, although functionally it amounted to the same thing. He connected his antenna to the ground, not directly or through the coherer, but through a coil of wire; and to this inductance he coupled a second coil which formed part of the coherer circuit. He created, in other words, a high frequency transformer which, like Slaby's "syntonic side wire," changed the current variations at the base of the antenna into voltage fluctuations to which the coherer could respond. Marconi christened the device a "jigger," presumably because it poured the signal from one "container" into another, and patented it in June 1898 (Patent No. 12,326) (see Fig. 5.8).

So far so good. It turned out, however, that the number of turns of wire in the two coils, and the ratio between them, were critical. Of the many high frequency transformers Marconi wound in the latter half of 1897, only a few functioned as expected; most reduced the sensitivity of the coherer rather than increased it. The intellectual breakthrough came when

Marconi realized that both the antenna and the coherer circuits, since each contained inductance and capacitance, were resonant circuits and had to be made to resonate at the same frequency, or at some harmonic of that frequency, if energy were to be transferred efficiently from one to the other. The two circuits had to be brought into syntony; if they were not, signals would not be coupled from one to the other.[72] Furthermore, for good reception, each had to be in syntony with the transmitting antenna.

This was the insight from which all else followed. Refinements came quickly: a second patent in 1898 added fixed capacitors across the secondary of the transformer to swamp the variable capacitance of the coherer and add some preset tuning; and in 1900 came a "concentric cylinder" antenna, designed to achieve sharp resonance with limited height and used by the Army in what was probably the first truck-mounted mobile station. Finally, in April 1900, Marconi was granted his master tuning patent (No. 7777, granted 26 April; U.S. Patent No. 763,772, filed 10 November 1900, issued 28 June 1904), the famous "four sevens" patent, destined to be the basis of more subsequent litigation than any other patent in radio history except Lodge's syntony patent and the later vacuum tube patents.

Marconi described the nature and purpose of this invention in language that would have been familiar to anyone who had followed Lodge's accounts of his experiments with syntony. The object was stated to be to increase the efficiency of the system described in Marconi's original patent by providing "new and simple means whereby oscillations or electric waves from a transmitting station may be localized when desired at any one selected receiving station." This was to be accomplished by providing, at the transmitter, "an oscillation-transformer of a kind suitable for the transformation of very rapidly alternating currents." This transformer would link two circuits, one of which was to be a "persistent oscillator" and the other a "good radiator," one coil of the transformer being part of the antenna circuit while the

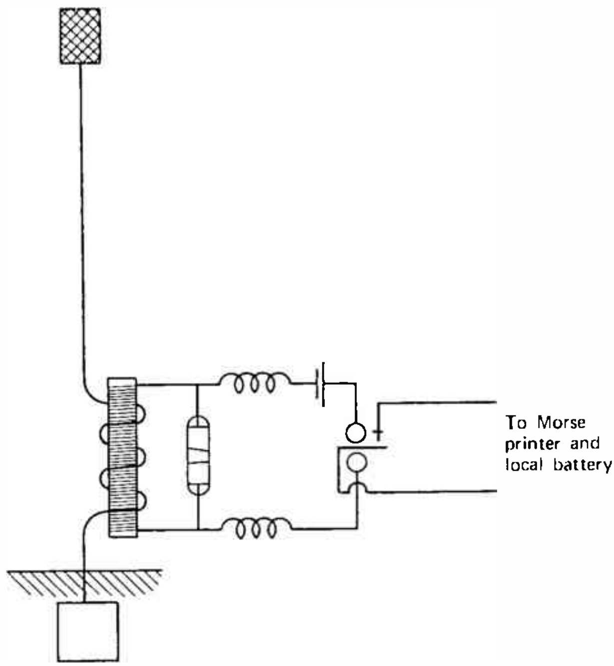


Figure 5.9 Marconi syntonic receiver circuit, 1900.

other, in combination with a capacitor, an induction coil, and a key, was part of the spark circuit. At the receiving station there was to be a similar transformer, one of its windings connected between the antenna and the earth, "constituting a good absorber," the other connected as part of the detector circuit. Means were provided to vary the inductances of the two circuits at each station so that they would "accord with each other" (see Figs. 5.9 and 5.10). By this arrangement, stated Marconi, "I am able to secure a perfect 'tuning' of the apparatus at a transmitting station and at one or more of a number of receiving stations."[73]

The essence of the patent is clearly the conception of the four tuned circuits. Two of the circuits are the antennas, and these are to be resonant at the operating frequency. The other two are

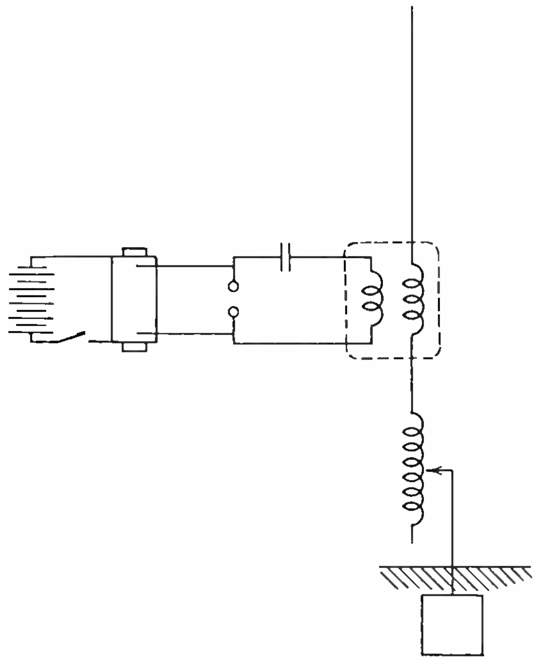


Figure 5.10 Marconi syntonic transmitter circuit, 1900.

the detector circuit in the receiver and the exciter circuit in the transmitter. All of these circuits are to be tuned to the same frequency or some harmonic of that frequency. "The capacity and self-induction of the four circuits . . . are each and all to be so independently adjusted as to make the product of the self-inductance multiplied by the capacity the same in each case or multiples of each other—that is to say the electric time periods of the four circuits are to be the same or octaves of each other."

What is the relationship between this concept and that embodied in Lodge's syntony patent of 1897? There is, first of all, a difference in language. The word "syntonic" occurs at one point only in Marconi's patent; elsewhere he writes in terms of resonance, time periods of oscillation, and tuning. This is perhaps not as trivial a matter as might appear, for the history of a technological idea can often be traced in its vocabulary. Marconi may well have been concerned to differentiate his circuits from Lodge's, and for such a purpose a semantic change helped. It is from the issuance of this patent, in any case, that the gradual disappearance of the word "syntony" can be dated; from this point on designers and operators speak more and more of tuning.

In the second place, there is the matter of the four circuits. Lodge had required in his patent that the antenna circuits be resonant; he had described a means of tuning the antennas by the addition of an inductance; and he had referred to the use of a radiofrequency transformer to couple energy into and out of the antenna. He had not, however—and this was central—specified that the other circuits, if any, in receiver and transmitter be resonant. Lodge's syntony patent describes what might be called "two-circuit tuning." It is the resonance of the antennas that determines the frequency. This is why Lodge is so insistent that the antennas be as free from ground losses as possible. Their resonance peaks had to be sharp. Marconi's patent requires resonant circuits in receiver and transmitter in addition to the antennas themselves. It is a "four circuit" tuning system, and this came to be, in fact, how it was described in popular parlance.[74]

Marconi's patent, therefore, was a significant advance over Lodge's. Lodge, however, had been the first to patent a syntonic circuit. In that sense, Lodge's patent was more fundamental. In terms of patent litigation, Marconi's position was weak unless and until he controlled the master patent that underlay his own.

This is to express the issues in legalistic terms. Lodge had been granted property rights, not indeed to the idea of syntony, but to explicitly described means of attaining syntony. And since Marconi's patent utilized those means, it could not be used without infringing on Lodge's property rights. For business purposes arguments could indeed be made to the contrary, and in fact the Marconi Company never paid any royalties to Lodge. The logic of the situation, however, eventually proved undeniable and Lodge's patent was purchased by the company in 1911, at the first indication that Lodge intended to put up a serious fight. The Lodge patent was the Achilles' heel of the Marconi Company's patent position, as the historian of that company aptly expresses it.[75] It had to be acquired by the company if it was to enforce the rights it had received in Marconi's derivative patent.

In terms of intellectual history the derivation of Marconi's concept from Lodge's is even closer. Lodge had taken the concept of syntony, which was already implicit in Hertz's experiments, and made its practical implications apparent in his classic experiment with syntonic Leyden jars. That experiment, however, had used what Lodge called closed circuits, because with such circuits resonance could be produced and demonstrated easily. Applying the concept of syntony to radio, Lodge had stipulated that the antennas of receiver and transmitter must be resonant at the same frequency, recognizing quite explicitly that in doing so he was making a design compromise—an inescapable one, as he saw it—between a persistent oscillator and a good radiator. Marconi went one step further: recognizing that an efficiently radiating (or absorbing) antenna could not be a persistent oscillator, he coupled his antennas to circuits that were persistent oscillators but not good radiators. Closed circuits, in short, were coupled to open ones, the high frequency transformer or jigger serving as the necessary link between them.

Marconi, of course, was not alone in his appreciation of the virtues of tuned circuits. As so often happens with an idea that is both intellectually "ripe" and also appropriate for the solution of an immediate problem, the years between 1897 and 1900 saw what can only be described as an efflorescence of circuits, schemes, and devices that involved resonant circuits and high frequency alternating currents. All involved the use of coupled inductances; all relied on the concept of syntony. Lodge's basic patent of 1897 we have already scrutinized sufficiently. In November 1897 that erratic genius, Nikola Tesla, also received a

United States patent (No. 539,138) for a high frequency transformer, following that up in March 1900 with a patent (No. 645,576) for a new "system of transmission of electric energy." The principal purpose of this system was to produce "currents of excessively high potential" for the transmission of power on an industrial scale—for example, "for lighting distant cities or districts from places where cheap power is available." But it had other possible uses: to illuminate the upper strata of the air; to produce changes in atmospheric conditions; to manufacture useful gases and fertilizers; and "to transmit intelligible messages to great distances." Quite explicitly, Tesla's scheme included high frequency transformers, with their primary and secondary circuits carefully "synchronized" and with the transmitting and receiving coils (both "connected to the ground and to an elevated terminal") adjusted so as to "vibrate in synchronism." Whether or not it was feasible in an engineering sense, Tesla's scheme included conceptually every element in Marconi's tuning patent except one: the use of a variable inductance to tune the antennas to resonance. Tesla may have considered that feature too obvious to mention; if so, he had little knowledge of patent lawyers. It was, in any event, included in Lodge's patent, filed (in the United States) after Tesla's but before Marconi's.

In Germany there was Ferdinand Braun, professor of physics at the University of Strasbourg, who in 1897 had begun to puzzle over why Marconi and Slaby were finding it so difficult to increase the range of transmission. The answer, Braun thought, might lie in the fact that the spark itself used up such a large fraction of the available energy, particularly when the spark gap was long and there was only small capacitance in the circuit. The solution lay in exciting the oscillations in a separate circuit which contained considerable capacitance and then transferring them to an antenna circuit which did not contain a spark gap. There were various ways of transferring the energy: inductive coupling through a high frequency transformer was one. Here were the same building blocks as in Marconi's design: the closed circuit made up of inductance, capacitance, and spark gap; and the open circuit containing the antenna. Braun himself believed that his system "brought the so-called coupled circuits into general use in wireless telegraphy" and had no hesitation in claiming credit for the idea when (with Marconi as joint recipient) he accepted the Nobel prize for physics in 1909. His German patent (No. 111,578) was dated 14 October 1898; his British one (No. 1862) 26 January 1899. Both, therefore, were later than Lodge's patent. But whereas Lodge had relied on the antenna to determine the frequency, Braun relied on the local spark or detector circuit and used loose coupling to the antenna.[76]

The man whose thinking was closest to Marconi's, however, and in some respects went beyond it, was neither Tesla nor Braun but John Stone Stone in the United States. Stone approached the problem with one clear conviction, based on his analysis of the mathematics of syntony: that true selectivity could never be obtained unless the signal radiated from the transmitter and picked up at the receiver were a pure sine wave of uniform periodicity. This was not the type of wave that a spark gap generated; what was required, therefore, was the insertion of tuned resonant circuits between spark gap and antenna that would "weed out and thereby screen" the undesired harmonics and overtones. Similarly at the receiver: tuned circuits there between antenna and detector would enable the receiver to respond to one frequency and one frequency only.

This conception of resonant circuits as filters seems to have been unique to Stone, though it has since become commonplace and was of course implicit in all approaches to the subject. His basic United States patent was applied for on 8 February 1900 and issued on 2 December 1902; both are earlier than the corresponding dates for Marconi's American patent. The principal objectives were stated to be, first, to provide "means whereby each of a plurality of transmitting and receiving stations . . . may be enabled to selectively place itself in communication with any other station to the exclusion of all the remaining stations"; and second, "to enable the vertical or elevated conductor in such a system to be made the source of simple harmonic electromagnetic waves of any desired frequency independent of its length or other geometrical constants." In his text and diagrams Stone showed a four-circuit scheme substantially the same as Marconi's arrangement (some of the diagrams, indeed, show what is in effect an eight-circuit arrangement, with three tuned circuits at transmitter and receiver in addition to the antennas); he described adjustable tuning of the closed circuits; and he stated that the antenna circuits could be made resonant at the same frequency as the closed circuits, although he did not require that this be done. Great stress was laid on the necessity for loose coupling between the primary and secondary windings of the high frequency transformer. Stone was well aware that, if the two circuits were tightly coupled together—as one might do, perhaps, in a simple-minded attempt to minimize losses—oscillation at two frequencies, not one, would be generated in the secondary circuit, and neither of these frequencies would be the same as the natural resonant frequency of either primary or secondary. Loose coupling was indispensable to produce a "single humped" frequency response; the tuned circuits were indispensable to convert the multiple frequency output of the spark gap into a single frequency sine wave. Every precaution had to be taken, said Stone, "to approximate as closely as possible to the true or absolute simple harmonic wave form, thereby reducing to a minimum the overtones which cause a departure from the true sine wave."

The original version of Stone's patent application makes it clear that he considered it beneficial, in certain circumstances, that the antenna should *not* be tuned to the operating frequency. These circumstances were when the antenna was to be used successively for different frequencies, or when it was to be used deliberately to radiate two different frequencies from separate transmitters at the same time. Even when these circumstances did not prevail, however, it was not *necessary* to tune the antenna circuit. It could be treated, not as a resonant circuit, but as nonresonant or aperiodic, and oscillations impressed on it by the tuned circuits of the transmitter. Loose coupling in the radiofrequency transformer ensured that the tuned oscillating circuits would be the dominant influence on the antenna's performance and prevented any tendency for its natural period of oscillation to influence the behavior of the transmitter.

This was Lodge's concept stood on its head. In an amendment to his original patent application, filed after Marconi's had been submitted, Stone added the recommendation that the two antenna circuits should be resonant at the same frequency as the closed circuits. This amendment, as might be imagined, became a fruitful source of later litigation, lawyers for the Marconi Company claiming that its late submittal deprived Stone of his claims to priority. The U.S. Supreme Court ruled in 1943, however—the case dragged on so long because the U.S. Government used these circuits without payment during World War I and compensation was not promptly forthcoming—that the amendment involved no substantial departure from the original application.[77] Stone had from the start mentioned that the antennas *could* be resonant; it was implicit in his theoretical analysis; the later amendment added no more than a recommendation.[78]

Our interest here is not in the legal subtleties but in what the proliferation of syntonic circuits between 1897 and 1900 means for the history of radio technology. In this context a few months' precedence by one man over another is in itself of little moment. Disputes over priority, when such small time periods are involved, can provide a reliable income for generations of lawyers but they are of little importance for an understanding of the "laws of motion" of technology. Seen in this context, our survey of the patent applications filed in this period carries two implications: first, that understanding of the theory and significance of electrical resonance had by then reached the stage where it could be reduced to feasible circuit designs; and second, that the market demand for radio equipment, present and prospective, was such that it was considered worthwhile to claim property rights to these designs. Some of the designs were for visionary purposes, such as Tesla's plan for large-scale transmission of industrial power; others, such as Marconi's, were strictly pragmatic solutions to immediate operational problems. All of the designs started from the concept of a resonant circuit containing inductance, capacitance, and a source of high frequency oscillations; all of them accepted Lodge's distinction between circuits that were efficient radiators and those that were persistent oscillators; all sought a solution by separating the two functions into distinct circuits, one open, one closed, and coupling them by a transformer of special design. The insistence on loose coupling was a refinement, characteristic of designers who were reaching out toward continuous wave propagation.[79] The requirement that the antenna itself be tuned to the operating frequency was typical of those who were thinking of fixed frequency operation and who wished to minimize problems of impedance matching. Stone was correct in describing the issue as one of convenience rather than necessity.

Among this plethora of claimants to property rights in syntony Marconi was lucky. Lodge had neither the money nor the inclination to tackle him until 1911. Stone's claims and Tesla's were ignored until much later and had no powerful corporate interests behind them. Braun's patent was strong enough in German courts to protect Telefunken against Marconi's "four sevens" patent. This is why, when the Marconi Company moved to enforce its patent rights in 1911–1912, Telefunken was willing to admit infringement of Lodge's patent—prior in both logic and time—but not of Marconi's.

Marconi's debt to Lodge is clear both from the content of his "four sevens" patent and from the nature of his first experiments with syntony. It is possible, indeed, that he received direct assistance. Silvanus Thompson, in a long letter to the editor of the London *Times* on 12 October 1906 dealing mostly with the

Marconi-Slaby dispute, stated explicitly that Marconi was given access to Lodge's unpublished investigations on syntony by means "entirely creditable to both parties." He did not elaborate on the assertion, but it was allowed to stand unchallenged despite later strongly expressed exchanges of views in the pages of the same newspaper.[80] The point is intriguing but not of great importance. Lodge's published writings provided all that was needed to start Marconi in the right direction, and his first experiment followed closely the classic "syntonic Leyden jars" experiment of Lodge, with the important modification that Marconi added a tapped antenna coil and a "jigger" to transfer the energy from the resonant circuit to the antenna.[81] What is hard to explain is not Marconi's improvement on Lodge's circuit but rather why Lodge failed to make it himself.

Marconi may have approached the idea of syntony in an unusual way—as a question of impedance transformation—but he was not slow to recognize its more conventional virtues. A series of effectively publicized demonstrations between stations on the south coast of England showed a new capability for simultaneous transmission without interference. In the most striking of these tests two transmitters, each tuned to a different frequency, were connected to a single antenna on the Isle of Wight; and two receivers were likewise connected to one antenna at a station in Dorset. Messages were sent without any indication of interference or interaction, a remarkable advance over previous performance.[82] The effect on distance was equally striking. Now, using syntonized receivers and transmitters, Marconi succeeded in signaling across the English Channel, a feat important mainly for its symbolism, since greater distances had already been achieved in England. From this point on, resonant circuits were a standard feature of all Marconi equipment, except in very unusual situations such as the Newfoundland reception tests in 1901. Their function was not only to improve selectivity of receivers; at the big new transmitter at Poldhu, as we shall see, syntonized circuits were used for frequency multiplication. And it was the new ability to concentrate the power of a transmitter in a narrower band of frequencies that turned Marconi's thoughts to the possibility of signaling across the Atlantic.

Of course, even with syntonic circuits, equipment did not always work as it was supposed to: as, for instance, in July 1903, when Ambrose Fleming gave a public lecture on syntonic receivers at the Royal Institution, and Neville Maskelyne, who had a interest in radio and a reputation for exposing frauds, set up an untuned spark transmitter nearby and proceeded to send the word "Rats" and other disrespectful comments at crucial points in the demonstration, to the great amusement of the audience and the intense annoyance of Ambrose Fleming. And there was much subsequent huffing and puffing about "scientific hooliganism" and a crossfire of accusations that reflected no credit on either party. Fleming's receiver had, of course, been overloaded by the powerful nearby transmitter, and no syntonic circuits could protect it; but, as Maskelyne pointed out, if Fleming wanted to claim that his circuits could reject unwanted signals, he had only himself to blame if they conspicuously failed to do so.[83] Syntonic receiving circuits were a vast improvement on circuits that depended for their selectivity purely on the dimensions of the antenna, but no cascading of tuned circuits in the front end of the receivers of those days could eliminate the kind of broadband interference that an untuned or poorly tuned spark transmitter could cause. It is easy to forget that Marconi's receivers contained no means of amplifying the signal, either at radio or at audio frequencies, and therefore no means of applying regeneration to increase selectivity and gain. The amplifying vacuum tube, like the heterodyne principle and the use of crystal and mechanical filters, was an innovation of the interwar period.

* * *

Greater distance was the second of Marconi's objectives. The equipment available in 1900 was adequate for short-range operation—say within 100 miles of a shore station—but considerable redesigning and a new approach to the transmission of high power were called for if coverage of longer ranges, such as the North Atlantic shipping routes, was to be achieved. To assist him in these more ambitious plans Marconi brought in advisers of a new type. Earlier his assistants—men like George Kemp, his lifelong friend and helper—had come from the world of telegraphy. The equipment, after all, was quite similar; levels of current and voltage were not very different. Now, however, Marconi turned to the technology of large currents and high voltages, which meant to the power generating sector of the electric industry. Ambrose Fleming, professor of electrical technology at University College, London, joined him as scientific adviser in December 1900. Fleming had formerly worked for the Edison Electric Light Company of London; his special field of competence was the design of high tension alternating current systems. R. N. Vyvyan, a young engineer with experience in central power station design, came to the company at the same time. To these two men Marconi entrusted primary responsibility for design and construction of the new long-distance stations to be erected at Poldhu in Cornwall and at South Wellfleet on Cape Cod, Massachusetts. Vyvyan, in his memoirs, describes Poldhu as "the first electric wave power station ever built" and the phrasing is significant.[84] Marconi was clearly turning to power station technology to achieve greater distance; the problem was to marry the power levels of central power stations to the technology of spark transmission.[85]

The techniques used by Fleming and his associates to achieve this goal are described in readily available sources, perhaps most fully in Fleming's own *Principles of Electric Wave Telegraphy* and in Baker's *History of the Marconi Company*, and therefore require only the briefest of comments here. Fleming's essential innovations were three: the use of a low frequency alternator driven by an internal combustion engine in place of the previously standard battery-powered induction coil; power transformers to step up the relatively low voltage generated by the alternator to a higher potential; and a series of coupled tuned spark gap circuits to increase the frequency of oscillation. A derivative innovation, made necessary by the higher voltage and current levels, was the introduction of a new system of keying, the key being placed in shunt with an iron-cored choke in the alternator output circuit. In the "key-up" position the inductance of the choke cut off current to the primary of the oscillation transformer; when the key was depressed, the choke was bypassed. The result was a system of keying which worked—after a fashion: a Marconi key of the type originally used at Poldhu or Glace Bay was a ponderous affair compared to the conventional telegrapher's key and it was difficult to maintain keying speeds comparable to those normal in cable circuits.

The alternators used were low frequency devices, incapable of generating currents that could be radiated directly. They are not to be thought of as resembling, except in basic principle of operation, the high frequency alternators that Reginald Fessenden was working toward in the United States and that Alexanderson was later to perfect for General Electric. The most ingenious feature of Fleming's design was probably his use of multiple tuned circuits. The circuit diagram (see Fig. 5.11) shows three

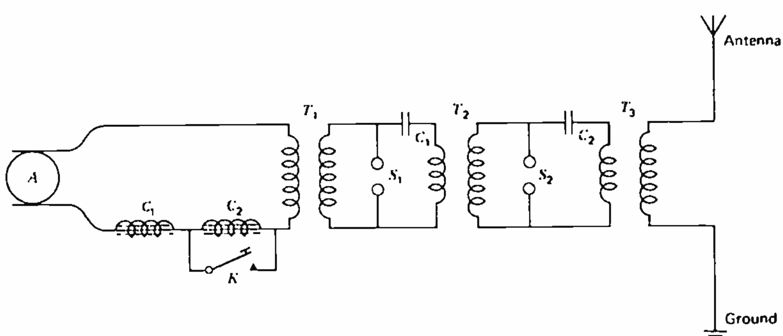


Figure 5.11 The Poldhu transmitter: Basic circuit. A: alternator. C_1, C_2: chokes. T_1: low frequency transformer (2000–20,000 volts). T_2, T_3: high frequency transformers ("jiggers"). S_1, S_2: spark gaps. C_1, C_2: capacitors. K: telegraph key.

transformers and two spark gap circuits between the alternator and the antenna. Each of the spark gap circuits, containing series capacitance and inductance in the form of the transformer windings, was designed to function as a tuned or syntonized circuit, stepping up both the voltage level and also the frequency of oscillation. The alternator originally installed at Poldhu was a 25 kilowatt device generating alternating current of 2000 volts at a frequency of 50 Hertz—close to the frequency of conventional house current today. The transformers stepped up the voltage level to 20,000 volts; the tuned spark gap circuits converted the low frequency alternations to high frequency oscillations (according to some authorities around 800 kHz in 1901) that could be fed to an antenna. The concept was a most ingenious application of the theory of syntony.

Fleming's innovations, though essential if high power levels were to be attained, marked no substantial departure from the established technology of spark telegraphy. The radiated signal was still generated by the spark discharge, and the frequency of that signal was determined by the constants of the tuned circuits. The alternators, taken over from power station practice, merely substituted for the induction coils used earlier. The use of coupled tuned circuits was probably the most radical novelty.

Losses in the oscillation transformers caused most of the early "teething troubles" and they had to be rewound by Marconi before the circuits could be made to resonate. More serious was the fact that, despite the high power used, distances of reliable reception from the Poldhu station were much less than had been hoped for. In December 1901, it is true, Marconi claimed to have received signals from Poldhu in Newfoundland, during daylight hours, using very simple equipment: a sensitive coherer connected in series with a telephone earpiece and a long-wire antenna trailing from a kite. But more systematic tests in the following year aboard the S. S. *Philadelphia*, using a tuned receiver and a carefully designed antenna, showed that 700 miles was the maximum range at which signals from Poldhu could operate a Morse recorder in daytime (though distances were, for reasons not understood at the time, much greater at night).[86] For certain purposes this was adequate. The *Philadelphia* tests showed that, with a station similar to Poldhu on the North American coast, shipping on the North Atlantic run could keep in touch with shore stations throughout the whole voyage. This was not a negligible gain, and the signing of contracts shortly thereafter for the provision of a radio news service to ships and for the handling of messages from private passengers showed that the revenue prospects were not ignored. It was much less, however, than Marconi and Fleming had hoped for, and much less than the power levels used at Poldhu and later at Glace Bay, Nova Scotia, seemed to justify.

One response was to increase power still further. The original Poldhu transmitter had a power input (that is, from the alternator) of 25 kilowatts. The Glace Bay station was built initially for 50 kilowatts; this was stepped up first to 75 kilowatts and in 1904 Glace Bay was operating at 150 kilowatts. At this point the attempt to set up a transatlantic radio service was temporarily abandoned. It was clear that the application of brute force without further modifications in equipment was ineffective. To generate power in an alternator and feed it through transformers to a spark gap was no great problem: power station technology, suitably modified to allow for keying and higher frequencies, was adequate for the task. To feed that power effectively to an antenna and radiate it over large distances, in contrast, were matters in which neither known technology nor available theory was of much assistance. Trial and error empiricism, intuition, and determination held out the only hopes for progress in these areas.

Antenna design was the first field in which it was clear that much had to be learned. For the stations at Poldhu and South Wellfleet the original plans called for a ring of 20 wooden masts, each 200 feet high, arranged in a circle 200 feet in diameter.[87] From the tops of these masts there was suspended a conical arrangment of wires gathered together at the lower point in the shape of a funnel. Whatever its electronic virtues may have been, the array was structurally unsound, despite extensive guying, and neither at Poldhu nor on Cape Cod did it survive the first major storm. For the Newfoundland tests a temporary antenna was rigged up at Poldhu in the shape of a large "fan" of wires tapering down to a single feedpoint. The permanent replacements, both at Poldhu and at Glace Bay, took the form of four wooden lattice towers 200 feet high supporting a "square cone" of 400 copper wires. This was so constructed that the whole array or any segment of it could be used as desired, and, proving mechanically secure, was used as the basis for extensive tests, with mixed results. In 1905, at Glace Bay, this antenna structure, moved to a new site, was made the central element for a much more extensive system involving the creation of an "umbrella" of 200 wires stretched out to supports in a circle of roughly half a mile diameter.

These antennas, all of them lineal descendants of Marconi's original grounded vertical, were very large, very costly, and, by Marconi's standards, disappointing in their performance. Successive extensions and the addition of new miles of copper wire typically produced some improvement in reception, but not in proportion to the funds expended nor to the increase in the size of the structure. Mechanically, they were easily damaged by wind and ice. More important, their electrical characteristics were imperfectly known. They were nondirectional; their resonant frequency, if they had one, was uncertain; their feedpoint impedance and therefore the load they presented to the transmitter was anybody's guess. In this field there was little scientific knowledge to guide Marconi; hence the necessity for an expensive, time-consuming, and often frustrating trial and error approach. What he was doing in this period was, in fact, little different from the rule of thumb followed since his time by generations of impecunious amateurs: put as much wire as possible up in the air as high as possible, and then bring it to resonance with a tuning network at the operating position. The escalation of power levels that Marconi undertook between 1901 and 1904 in his attempt to break through the barrier of distance was of little purpose without confidence that his antennas would radiate that power effectively at the transmitter and capture it effectively at the receiver. Not until 1905, however, with the discovery of the "bent" or inverted-L directional antenna, was there an important breakthrough. Marconi had been familiar with directional antennas at very high frequencies in his early experiments; now he was learning to use them at very low frequencies, with wire arrays taking the place of sheet metal reflectors. Such an array gave him a vastly cheaper and more efficient way of increasing effective radiated power in a chosen direction than did feeding additional kilowatts into the spark gap.

Along with higher power and larger antennas went a shift to longer wavelengths. In its later stages this was a deliberate policy. Earlier it seems to have been an unintended consequence of larger antenna dimensions; if these large wire arrays had any fundamental resonant frequency, it was a very low one. Furthermore, until Fleming invented his neon-tube cymometer in 1905 there was no portable test instrument available to engineers wishing to measure frequency.[88] There were laboratory procedures, at least for the higher frequencies. And there were methods for estimating the inductance and capacitance of an antenna or any other resonant circuit and calculating the fundamental resonant frequency from those parameters. The result, however, was often little more than an educated guess. Neither Marconi nor any other experimenter, in the first 10 years of practical radiotelegraphy, had any readily available means for determining where in the radiofrequency spectrum he was. Nor, in most cases, was it a question of great importance, as long as transmitter and receiver could find each other. And both were so broadly tuned that, given adequate signal strength, that was not difficult.

This persistent haze of uncertainty over frequency measurement is strikingly illustrated by Marconi's Newfoundland tests in

1901. If there were ever a situation, one might imagine, in which it would be vital for the receiving operator, straining to hear three tiny clicks through the crashes of static, to know precisely where in the vast reaches of the spectrum the transmitted signal was to be found, this would be it. Yet Marconi did not know, despite the fact that every detail of the Poldhu transmitter was known to him personally. Even later, with greater knowledge, it remained a matter of dispute. Some authorities claimed that the wavelength had been 366 meters (819.6 kHz); others, with equal confidence, placed it between 2000 and 3000 meters.[89] It may well be that the Poldhu transmitter radiated a signal at a fundamental frequency of about 100 kHz (3000 meters)—and also many other signals at harmonics of that frequency. Transmitters that used a quenched spark to generate their signal could not do otherwise, in the absence of very extensive and effective filtering in the antenna circuits. They were inherently "dirty" radiators.

In the circumstances, precise information on the frequencies used in the various tests is not to be expected. The trend toward lower frequencies is, nevertheless, unmistakable. From the 366 meter wavelength (possibly much longer) used in the Newfoundland tests of 1901, the Poldhu transmitter was moved to 1100 meters in May 1902, and by the end of that year was on 1650 meters. The Glace Bay station, and probably South Wellfleet also, was originally set up for a wavelength of 2000 meters, and by 1903 Poldhu also had shifted to that frequency. During 1904 both Glace Bay and Poldhu moved again, this time to 3660 meters. By the end of that year Poldhu, with a new T-shaped antenna, was on 4250 meters. Later both Poldhu and Glace Bay operated at very long wavelengths of between 7500 and 8000 meters (nearly five miles), while the new station at Clifden began transmitting in 1907 at 6666 meters.[90]

Behind this rapid migration to the very low frequencies lay a rationale strongly backed by Fleming and accepted apparently without question by Marconi and the other members of his staff. Radiation at short and medium wavelengths traveled in straight lines and would therefore inevitably leave the surface of the earth and escape into space. Only very long wavelengths could follow the curvature of the earth's surface and might, therefore, if radiated with sufficient power, be received at long distances. Why this should be so was not clear. Sometimes an acoustic analogy was used: the lowest notes of a foghorn—that is, the sounds of longest wavelength—traveled farthest through a dense fog. Perhaps in the same way Hertzian waves of long wavelength would travel farthest through the electrical "fog" that was the earth's atmosphere.[91] Marconi, however, was more inclined to believe that it was due to the conductivity of the earth: as the radiated waves passed over the surface of the earth their lower portion became, as it were, retarded, so that the wave-front became inclined forward and could follow the earth's curvature.[92] This might explain why distances were greater over salt water than over land, but it shed little light on the puzzling fact that, for long waves, ranges were much greater at night than during the daytime hours. This seemed to require explanation in terms of the action of sunlight and perhaps in terms of the ionized layer of the atmosphere suggested by Kennelly in the United States and Heaviside in Britain. But as late as 1916 Fleming was unwilling to commit himself on the point.[93] It was an intriguing problem and no doubt the physicists would eventually arrive at an acceptable explanation. For the radio engineer the important point was that the relationship held: very long waves could be transmitted for greater distances over the surface of the earth than shorter ones.

This belief quickly hardened into a dogma that ruled out the possibility of alternative approaches to the problem of distance. Just as Marconi, frustrated in his attempts to build a reliable transatlantic communications system, step by step increased the power of his transmitters, so at the same time he moved toward ever larger antennas and ever lower frequencies. This was the direction in which a growing body of evidence seemed to point; there were no data that suggested a contrary view; and though every increase in power and wavelength brought results that disappointed, these indications of diminishing returns were never convincing enough to turn the search in a radically different direction. Final success always seemed to depend on one more increment in power and one more increase in wavelength. Higher power, lower frequencies, and larger antennas came to be thought of, not as one possible formula for achieving distance, but as the only possible one.

This was not correct, as many later writers, including Marconi himself, have pointed out. There were other possible and available techniques for achieving the same end that would have been simpler and less costly. These techniques, however, would have implied moving in precisely the opposite direction in the radiofrequency spectrum: not down toward the very low frequencies but back up toward the short waves that Marconi (and all other experimenters of his period) had passed over. And they would have implied a mode of propagation different from that which engrossed his efforts: not the stubborn attempt to push the surface wave out ever farther across the ocean, but the reflection of the sky wave from the ionosphere. There is no radio amateur today who, with 150 watts power input and a simple wire dipole antenna on the 20 meter band, cannot communicate successfully between Europe and America at almost any time of the day or night and at almost any phase of the sunspot cycle, granted a frequency free of interference. The reason the amateur can do this is not superior equipment, and not the availability of the vacuum tube or more sophisticated antennas; it is the fact that he is operating in a different region of the spectrum, a region in which the ionosphere is a resource to be used, not an obstacle to be overcome.

To call Marconi's drive to the very low frequencies a strategic error, however, implies that he could have avoided it. Is this true? The question is not whether, if he had possessed the knowledge we have today, he would have acted differently. It is rather whether, with the knowledge available at the time, his strategy was a good one. Granted that between 1901 and 1914 Marconi was traveling down what was technologically "the wrong road," could he have chosen another one?[94]

It is not relevant, in considering this question, to point out that the scientific knowledge necessary for an understanding of ionospheric propagation was not available to Marconi. Practical use of the short waves did not necessarily have to wait on articulation of a theory of ionospheric propagation, any more than use of the long waves had depended on full understanding of the behavior of the surface wave. A series of empirical tests, if they had been undertaken with a fraction of the time and funds devoted to the very long waves, would have been enough to demonstrate their potential. Furthermore, what kind of interaction between science and technology is assumed? If Marconi had waited for science to tell him what frequencies to use, he would have waited a long time. Scientific advance in this field was not autonomous: it relied on commercial radio operation for much of its data, and it was stimulated largely by anomalies that commercial radio operation disclosed. Beyond this, to wait for science to provide him with solutions to the problems he encountered was never Marconi's style. Technology in his hands did not wait for science to catch up, far less to point the way; technology moved ahead empirically, using what scientific knowledge was already at hand, responding to whatever scraps of evidence seemed meaningful, leaving it to the scientists to systematize and rationalize what technology had discovered. There had been a phase, in the days of Faraday, Maxwell, and Hertz, when scientific knowledge was ahead of practice; that was not, however, the phase relationship that prevailed between 1900 and 1914.

It was not scientific knowledge that induced Marconi to move into the very long waves; and it was not lack of scientific knowledge that stopped him from exploring the short ones. It is worth noting that when Marconi himself looked back on these years of obsession with the very long waves, it was not the state of science that he held responsible. His rationale was more honest. He and his coworkers had not been following any scientific theory; they had been going where intuition and hunch told them to go, and their confidence that they were on the right track had been reinforced by the data they themselves generated. The difficulty was that, relying on data generated while following one particular path, they were never confronted by data that told a different story, until at length, in the 1920's, the amateurs, given the wavelengths shorter than 200 meters to play with "as one may give a toy to a child," showed what could be done with low power and high frequencies.[95] Marconi's comments to the Institute of Radio Engineers in 1927 are to the point:

> I admit that I am responsible for the adopting of long waves for long-distance communication. Everyone followed me in building stations hundreds of times more powerful than would have been necessary had short waves been used. Now I have realized my mistake. . . .[96]

"Everyone followed me . . ."; indeed they had. There is irony in the fact that Marconi, who had led the advance to the very low frequencies in the first decade of the century, was in the 1920's to pioneer in the rediscovery of the short waves. At the end of his career he was working in the area of the spectrum that he had begun with—the very high frequencies to which Righi had introduced him and which he in turn had demonstrated on Salisbury Plain many years before. It had been a long journey, one in which he had had to find his own landmarks and devise his own instruments of navigation. Marconi began in one region of the spectrum, traversed rapidly to the other extreme, and then worked his way back, finding on his return path technological riches that had been overlooked in the first flush of enthusiasm for high power and the long waves.

In accepting this formula for distance Marconi was not, of course, wholly wrong. The very low frequencies are still used today for long-distance communications, particularly when great reliability is of the utmost importance, as in some strategic uses and for radionavigation purposes. Relying as they do on the surface wave, these communications channels offer a degree of independence from ionospheric disturbances, such as solar flares, that can play havoc with higher frequencies. There are, however, relatively few channels available in the VLF segment of the spectrum, and to use them effectively requires very large antennas and very high power, as Marconi discovered. The technological strategy that Marconi followed between 1900 and 1914, in other words, was not wrong: the path he was traveling did lead to the goal he sought. There were, however, other technological options open to him—options that would certainly have presented their own problems but which nevertheless would have taken him to his objective with less investment of time, funds, and effort. He chose a high-cost option when, with the knowledge and equipment of the time, efficient lower-cost options were available. He did so partly because of haste, induced by that will-of-the-wisp hope, competition with the submarine cables. No systematic exploration of the high and medium frequencies was undertaken until the middle 1920's; yet this was a task Marconi might well have undertaken in 1901, with marine radio securely under Marconi control and no costly transatlantic ventures to drain the company's treasury. But partly also the explanation lies in Marconi's personality and style: time and again he relied on his intuition, his gift for improvisation, and his sheer will power to carry him forward when there was no familiar route to follow. He was stubborn, as both his parents had been; it was this stubbornness that carried him so far down his chosen road, and it was the same stubbornness that prevented him from seeing any other.[97]

* * *

The new Clifden transmitter, opened for service in 1907, was the culmination of the higher power–longer wavelength strategy. Designed for a power input of 300 kilowatts, it was located on the west coast of Ireland for two reasons: the Poldhu site could no longer accommodate the type of antenna planned; and Clifden, at a time when every mile counted, was nearer to Nova Scotia. Its circuits and equipment contained every improvement worked out in the previous six years of tests, along with much that was being tried for the first time. Technologically, therefore, the Clifden station represented the "state of the art" in spark radio transmission in its most advanced and perfected form. There were also implicit in its design, as we shall see, clear indications that spark technology had now gone as far as it could go. The future lay with other modes of generating radio waves, some of which were already on the horizon when Clifden was opened.

The power plant of 300 kilowatts output (equivalent to 400 horsepower) would not have been unusual for an industrial plant at the time, but it was larger than had ever been used for radio purposes and it was large enough to present fuel supply problems. Sensibly, the Marconi engineers used steam boilers fueled by locally available peat. Electric power at a potential of 20,000 volts was generated, not in alternators as earlier at Poldhu and Glace Bay, but in large turbine-driven direct current generators. Current from these generators was fed either directly to the spark gap through a series of chokes and capacitors or to banks of secondary storage batteries. This had two advantages: the station could be on the air for considerable periods of time (16 hours out of 24) without running the generators; and it gave a choice of two power levels—a working voltage of between 11,000 and 12,000 when the batteries alone were used or, when batteries and generator were used together, as high as 15,000 volts.[98]

In a circuit of this kind high frequency oscillations could not be generated, as in Fleming's earlier arrangement, by passing low frequency alternations from the power source through successive tuned spark gap circuits and thus multiplying the frequency upward. Oscillations were generated by the spark gap itself. Their frequency was determined by tuned circuits (chokes and capacitors in parallel) across the spark gap and between the spark gap and the antenna. Frequencies as high as 200 kHz could be generated in this way; Clifden's nominal operating frequency, however, was 45 kHz (6666 meters), and it was to this frequency that the transmitter output circuits and the antenna were tuned.

The antenna itself followed the directional inverted-L or "bent" design that Marconi had discovered and patented in 1905. Figure 5.12 shows the general arrangement but gives little conception of the scale of the structure. The array of parallel wires running horizontally was almost a mile in length. Underneath it there was placed a similar extensive system of ground wires. Left open at the far end, the antenna array was funneled down to a single feed point over the operating position. It showed pronounced and highly desirable directional characteristics, the major lobe of radiation being away from the open end

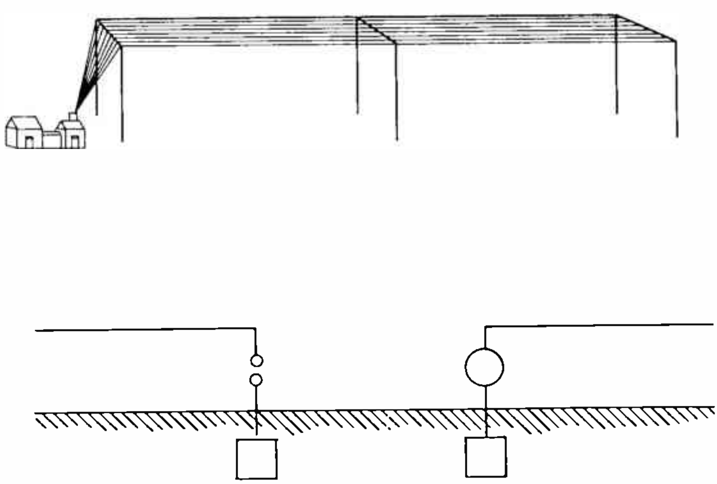


Figure 5.12 Marconi "bent" antenna, 1905.

(that is, regarded from the operating position, the antenna pointed 180 degrees away from the direction of maximum radiation). And it proved very easy to tune to resonance at any frequency desired.

There were important changes in the design of several other key components, some of them reflecting the higher power levels, others the fruits of experience at Poldhu and Glace Bay. The condensers, for example, had earlier been built with glass plates as dielectrics. Arc-overs at high voltages frequently destroyed the plates and caused shutdowns of the transmitter. At Clifden air was used as a dielectric. The condenser plates were simply hung 12 inches apart, so that arc-overs could cause no permanent damage. Greater reliability was bought at the cost of a considerable increase in scale, as a separate and capacious building was required to house the capacitor banks.[99]

One device at Clifden, however, was new in a more fundamental sense. This was the rotary spark gap or "disc discharger." Its technological ancestry is informative. Erosion of spark gaps by the discharge had always been a problem. Righi, it will be recalled, had tried to minimize the difficulty by immersing the gap in a vaseline-oil mixture. As power levels increased and sparks grew longer and more violent, these measures no longer sufficed. Particularly troublesome was the fact that, with high power, an arc tended to form across the spark gap; when this happened, no true oscillatory discharge, or only a weak one superimposed on the arc, took place, and the terminals of the gap were quickly eaten away.

This was a problem in all spark systems, not merely Marconi's, and various expedients had been devised to handle it. Nikola Tesla, with characteristic ingenuity, placed a powerful magnet so that its field was transverse to the axis of the spark gap. As soon as the arc began to form it was extinguished by the magnetic field. Elihu Thomson tried to achieve the same result with a blast of compressed air, while the Telefunken designers tackled the problem by attaching the spark balls to large metal plates for better heat dissipation. Ambrose Fleming also experimented with an air blast, but his preferred approach to the problem (British Patent No. 25,383 of 1903) was to rotate the spark balls slowly, either by clockwork or by an electric motor, so that the point at which the discharge took place moved around the circumference of the balls. Used at Poldhu, this prevented erosion at any one point and helped a little to dissipate heat and prevent arc formation. Otherwise it was ineffective. So was a similar system using rotating cast iron discs.[100]

None of these variations on the basic spark gap had been made with the intent of modifying the spark discharge itself. Reliance was still placed on rapid damping of the spark pulse—a high logarithmic decrement, as it was called—to generate the "whip-crack" effect believed to be essential for good signaling.[101] Rapid damping, of course, also generated a signal rich in spurious emissions at harmonics of the fundamental frequency. In this sense the type of spark believed to be best for long-distance signaling was incompatible with precise syntony.

The disc discharger showed that this was not necessarily the case. When Marconi invented the device in 1907 he was pursuing the same objectives as Fleming: he wanted to eliminate arcing and minimize wear on the spark gap. And the line of descent from Fleming's device is clear. Instead of Fleming's slowly rotating balls, Marconi used three rapidly rotating discs (see Fig. 5.13). The central disc, insulated from ground, revolved at high speed on one axis; the two outer or polar discs revolved somewhat more slowly at 90 degrees to the first. The edges of all three discs were very close together, and between the three spinning surfaces the discharge took place. When the discs were stationary and high voltage was applied, the discharge was an ordinary arc. When they were spinning rapidly it was more like a continuous oscillating spark. But it was different from any spark that had been used for radio before.

Marconi accomplished his original design objectives, but in the process and almost by accident he accomplished much more.

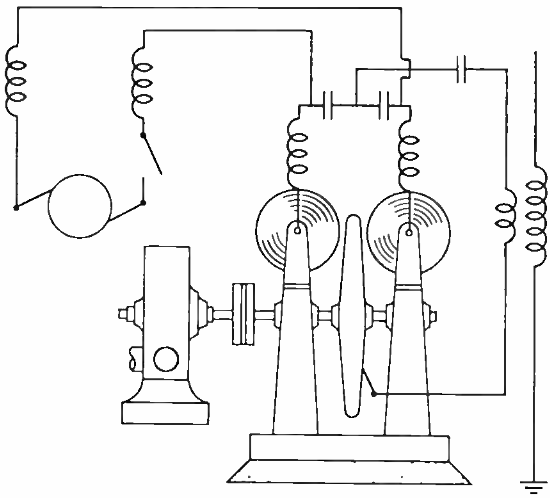


Figure 5.13 Marconi disc discharger.

In the disc discharger he had for the first time a device that would generate, if not a true continuous wave, then something very like it. Technically, the discharger generated spark pulses with a very low logarithmic decrement; each pulse decayed very slowly, and since the pulses followed each other with great rapidity, the result was something approximating closely to a continuous wave train.[102] After passing through one or more resonant circuits on its way to the antenna, it was virtually a continuous sine wave oscillation on a single frequency. No spark transmitter had ever generated that kind of emission before.

Marconi never referred to the disc discharger as an improved spark gap; he talked of it, in fact, as if the spark gap had been eliminated. In a sense it had; it depended on how one characterized the discharge across the rotating discs. A spark discharge generated high frequency oscillations: everyone who knew anything about radio knew that. An electric arc also generated high frequency oscillations. William Duddell, the English experimenter, had proved that in 1900 and Valdemar Poulsen, the Danish scientist, was at work developing the arc as a source of radio waves—not a series of pulses, as with the spark, but a continuous train of waves of constant periodicity, a carrier wave that could be keyed to transmit Morse code but that could also be modulated by audio frequencies to transmit music or the human voice. It was not the possibility of voice transmission that attracted Marconi to the disc discharger, however, but the fact that it made precise tuning possible and concentrated all available transmitter power on a single frequency. He certainly believed that he had discovered a generator of true continuous waves. Testifying in 1911 before a parliamentary committee, he flatly denied that Clifden was using spark: "I have now at Clifden a system utilizing continuous waves and employing no spark whatever."[103] And, speaking at the Royal Institution in the same year, he displayed with some pride resonance curves of the Clifden transmitter, showing an admirably sharp peak at a single wavelength. The disc discharger and its associated network of syntonic circuits had finally made possible the production of continuous waves.

In calling the disc discharger a true generator of continuous waves Marconi may have been stretching a point; but it certainly came close to it. The best evidence for this lies not in any verbal testimony but in the later history of the device itself. Ironically, it proved unusable with Marconi receivers of the day and had to be substantially modified before installation at Clifden. The explanation is simple: a true continuous wave signal at radio frequencies has no audio modulation. When keyed on and off it produces in the receiver nothing more than a series of indecipherable clicks and thumps. Modern receivers designed for continuous wave reception of Morse code use a local oscillator circuit to "beat" against the incoming signal and generate a tone at audio frequencies; but this is an application of the heterodyne principle—a concept known but not practically usable before the invention of the oscillating vacuum tube.[104] Marconi receivers in the days before the vacuum tube could detect a spark-generated signal and extract an audio tone from it because the spark pulses followed each other at an audio frequency. The radiofrequency oscillations were, so to speak, modulated by the spark pulse train.

With the disc discharger in its original form there was no modulation. It generated an unmodulated carrier, a string of virtually pure sine waves. This is why, technologically, it was an innovation of considerable significance. It is also why it was useless in practice. Before its emissions could be made compatible with the receivers of the day, audio modulation had to be reinserted. The wave train had to be broken up into pulses so that when the incoming wave was rectified in the receiver, an audible note resulted.

Marconi made the necessary modifications very simply, adding studs around the circumference of the central rotating disc —12 originally, later 24. The result was a wave train with a distinctive musical note, the pitch being determined by the number of studs and the speed at which the disc revolved. In the case of the Clifden transmitter this was a shrill whistle—an incongruous sound, perhaps, to emerge from such a massive transmitter, but one that proved highly effective in cutting through static. An unlooked-for but highly welcome byproduct was a considerable increase in keying speeds.[105]

What had been gained by the innovation? Marconi had finally generated, it would seem, something close to a continuous wave signal; having done so, he promptly had to break it up into pulses again so that his receivers could copy it. Seen in these terms, the advance seemed self-defeating. More than this was involved, however. From the earliest experiments on syntony it had been maintained that there was a necessary incompatibility between a "persistent resonator" and an efficient radiator. A circuit that resonated did so precisely because it did not radiate its energy. If it radiated efficiently, by that very fact it lost energy rapidly and its oscillations quickly died out. As long as this was true—and with conventional spark excitation it was true—designers had to choose between syntony and range. Since the spectrum was relatively empty and the new art of "wireless" had to establish itself in competition with alternative modes, the choice was usually in favor of distance. Certainly Marconi's was. He used syntonic circuits in his receivers to minimize interference and in his transmitters to generate high frequency oscillations. But the type of signal he was after was one that could be received and copied with the least difficulty at great distances. This was a rapidly quenched spark, a type of emission that could not be limited to a single frequency. With the disc discharger the need for any such design tradeoff was eliminated. Here at last was a type of spark transmitter which was both a persistent oscillator and an efficient radiator, which made it possible to feed large currents to the antenna, but which at the same time could be tuned to frequency.

It was ironic that this reconciliation should take place precisely when other, more efficient ways of generating continuous waves were becoming available. The Poulsen arc was already in use by 1911. Alexanderson's first radiofrequency alternator became available in 1915. And in 1904 Marconi's own Ambrose Fleming had invented the diode vacuum tube—a useful detector and rectifier in its own right but destined in time to play a larger role as technological ancestor of De Forest's triode, the potentials of which as a radiofrequency oscillator were discovered almost simultaneously in 1913–1914 by Meissner in Germany, Armstrong in the United States, and Franklin and Round in England. The concepts, circuits, and components that were to replace spark were already in evidence when spark attained its most refined form.

When the Clifden transmitter opened for commercial service in October 1907 it represented the ultimate in spark telegraphy. To be sure, there was an appearance of technological giantism about it, a suggestion of a brute force approach to problems that might have yielded to more subtle treatment. But, taken on its own terms, it was a magnificent accomplishment. It did the job for which it was designed; it faithfully reflected the concepts of its planners, both in its strengths and in its limitations. Syntony and spark had finally been reconciled, and for a few years they reigned supreme. Yet it was no accident that alternative modes were already on the horizon. The Clifden transmitter functioned as it was intended to; the techniques it embodied, however, had little potential for further development. There was in no sense a technological crisis, no general recognition that spark technology had reached the end of the road. The anomalies were prospective, not immediate, and only a few recognized them—perhaps most clearly Reginald Fessenden and Howard Armstrong.[106] These were men who saw, first, that the spectrum was becoming crowded; and second, that no form of spark transmission, however refined, could generate a wave stable enough to serve for the transmission of voice. Congestion in the spectrum implied higher selectivity in receivers, narrower bandwidths in transmitters, and the opening up of unused frequencies. The requirements of voice transmission implied a shift to other types of radiofrequency generators—for a brief period the arc and the alternator, then, for many years of unchallenged supremacy, the vacuum tube.

The advent of continuous wave transmission marked the opening of a new phase in the technological history of radio. Its principal result was more effective use of the spectrum, for with cleaner emissions from transmitters and greater selectivity in receivers the frontier of spectrum development could move inward, accommodating many more communications channels within the available frequencies. The more dramatic consequence, however, was probably the introduction of amplitude-modulated (AM) radio communication, making possible the transmission of voice and music and thus, in the 1920's, radio broadcasting. To some extent this latter development worked counter to the first, for AM transmissions required broader bandwidths than did code (CW) signals. The extensive frontier of spectrum use therefore moved outward into regions of the spectrum previously underutilized, the needs of voice transmission reinforcing the pressures already being felt from increased congestion on the low frequencies. All this was made possible by greater knowledge of the theory and design of syntonic circuits, and by the availability of new types of radiofrequency generators.

For Marconi himself the years after the opening of the Clifden station were occupied partly in perfecting the innovations made there (for example, the introduction of the timed disc) and partly in trials of innovations made by others. The new transatlantic transmitter at Caernarvon in Wales used the timed disc discharger until 1920; but, significantly, there were Poulsen arcs as standby transmitters and in 1920 the dischargers were replaced by Alexanderson alternators. High power and the very low frequencies, however, no longer presented a challenge to which Marconi was inclined to respond, and increasingly his attention turned to exploration of the high and very high frequencies—empty territory, of whose potential riches no one but the amateurs, those frontiersmen of radio, seemed aware.

For the company that had been the vehicle for Marconi's ideas, the years after 1910 were a period less of technological development than of commercial consolidation. Under the energetic leadership of Godfrey Isaacs—a man to whom litigation was almost a way of life—this took the form of a determined campaign to enforce what the company held to be its patent rights.[107] Basic to this strategy was possession of Marconi's "four sevens" tuning patent of 1900. In October 1911 this was reinforced, as it needed to be, by purchase of Lodge's fundamental syntony patent. Armed with these formidable weapons, which together were basic to any radio system, the company brought suit against the United Wireless Company of America, holder of the De Forest patents, and was awarded the decision in 1912, subsequently purchasing the assets of United Wireless. These included no less than 70 shore stations and 500 shipboard installations. Their acquisition transformed the Marconi organization at one stroke from a minor participant to the dominant element in North American radio.[108] A similar suit against the National Electrical Signaling Company of Pittsburgh, formed originally to develop Fessenden patents, put the American company out of business and virtually gave the Marconi Company, through its American subsidiary, total control of commercial radio in the United States. In Germany matters were not so straightforward, for the Telefunken Company had the strong support of the German Government and several large German banks. Patent litigation by a foreign plaintiff was likely to be risky and expensive. Difficult negotiations finally led to an agreement in 1912 in which Telefunken admitted infringement of the Lodge patent and joined in a consortium with the Marconi Company and the Compagnie de Télégraphie sans Fils of Brussels, licensee for Marconi patents in Europe. To this consortium was transferred control of German and later Austrian marine radio, provision being made for exchange of all patent information, present and future, and for full intercommunication between radio systems.[109] With these precedents set, the Marconi Company had little difficulty in establishing the validity of the Lodge patent in other countries.

These careful consolidations were typical of an industry that felt itself to be in possession and control of a mature technology. They did not, of course, survive the 1914–1918 war, which led to new consolidations on a more nationalistic basis, typified by the formation of the Radio Corporation of America in 1919. But it is doubtful whether, in any event, they would have survived the new technological revolution that followed perfection of the vacuum tube, the discovery of the oscillating triode, the invention of the regenerative and later the superheterodyne receiver circuits, and finally the explosive growth of public broadcasting in the 1920's. These developments were to make of radio by the end of the 1920's an industry fundamentally different in structure and function from the industry of 1900–1914. In the face of this "perennial gale" of competition from new technology, old firms would die and new ones would be born. New types of equipment would be created, based on new concepts, and old ones would become obsolete. By 1930 a spark transmitter would be little more than a museum piece, and the word "syntony" would have almost disappeared from the language.

Notes

1. A. A. C. Swinton to W. H. Preece, as reproduced in Degna Marconi, *My Father, Marconi* (New York: McGraw-Hill, 1962), p. 40.
2. Reproduced in Degna Marconi, *My Father, Marconi*, illustration following p. 182, and in W. J. Baker, *A History of the Marconi Company* (New York: St. Martin's Press, 1971), illustration following p. 88.
3. W. H. Preece, "Signaling through Space without Wires," abstract of a Friday Evening Discourse delivered before the Royal Institution, 4 June 1897, in *The Electrician*, 11 June 1897, pp. 216-218.
4. Marconi did, however, use a version of the Righi dipole as a wavemeter. See fn. 20 below.
5. The coherer principle was, however, discovered almost simultaneously by Professor Calzecchi Onesti of Termo, Italy, and it may be that Marconi's knowledge of the device came from that source rather than from Branly.
6. For Giuseppe Marconi's personality and its influence, see Degna Marconi, *My Father, Marconi*, pp. 3–35.
7. Charles Susskind, *Popov and the Beginnings of Radiotelegraphy* (San Francisco: San Francisco Press, 1962), pp. 7–8, fn. 25.
8. Popov's researches, which have been analyzed with considerable acuity by Charles Susskind, were briefly described in the *Journal of the Russian Physical and Chemical Society*, Vol. 27 (1895), pp. 259–260, and at greater length in the same journal, Vol. 28 (1896), pp. 1–14. See Oliver Lodge, *Signalling Without Wires* (New York: Van Nostrand, 1902), pp. 60–62; J. A. Fleming, *The Principles of Electric Wave Telegraphy* (London: Longmans, Green, 1908); pp. 362–363 and 425; Ellison Hawks, *Pioneers of Wireless* (London: Methuen, 1927), pp. 202–204; and Susskind, *Popov*, pp. 10–11. A recent Russian work on the subject is I. V. Brenev, *Izobretenie radio A. S. Popovym* [The invention of radio and A. S. Popov] (Moscow: Sovetskoe Radio, 1965).
9. See pp. 150–51 above.
10. Fleming, *Principles*, p. 345.
11. Luigi Solari, *Marconi nell' Intimità e nel Lavoro* (Milan: A. Mondadori, 1940), p. 13.
12. Compare Orrin E. Dunlap, *Marconi: The Man and His Wireless* (New York: Arno Press and The New York Times, 1971), pp. 28–39, quoting from an

interview with Marconi in *McClure's Magazine*, March 1897: "I discovered that the wave which went to my receiver through the air was affecting another receiver which I had set up on the other side of the hill. In other words, the waves were going through or over the hill. It is my belief that they went through, but I do not wish to state it as a fact . . . . I find that while Hertz waves have but a very limited penetrative power, another kind of wave can be excited with the same amount of energy, which waves, I am forced to believe, will penetrate anything and everything." It is worth noting that in the Preliminary Specification of the 1896 patent application Marconi specifies use of the grounded antenna "when transmitting through the earth or water," but the ungrounded dipole "when transmitting through the air." It is possible that, at this stage, he believed that radiation from a grounded antenna traveled physically through the ground; if so, it would be natural for him to think of the waves as different from Hertzian waves. Compare Silvanus P. Thompson, letter to the editor of the London *Times*, 12 October 1906: "When . . . Signor Marconi came to this country, he gave out that he had a new and wonderful discovery. He did not use Hertz waves; Hertz waves would not work his receiver, and his transmitter would not, he declared, work with Hertz waves!"

13. Niels H. de V. Heathcote, *Nobel Prize Winners in Physics, 1901–1950* (New York: Schuman, 1953), pp. 75–76.

14. Compare Fleming, *Principles*, p. 603, where this is referred to as "Marconi's Law," and R. N. Vyvyan, *Wireless over Thirty Years* (London: Routledge, 1933), p. 18. A. Frederick Collins, in his *Wireless Telegraphy, Its History, Theory and Practice* (New York: McGraw, 1905), p. 236, ascribes the general law to Marconi but attributes its mathematical formulation to a Professor Ascoli.

15. Captain (later Admiral Sir Henry Bradwardine) Jackson carried out the first experiments in radio signaling in the Royal Navy on board H. M. S. *Defiance* in 1896, using an induction coil, Hertzian oscillator, and coherer. He succeeded in maintaining communications between the *Defiance* and the gunboat H. M. S. *Scourge* over a distance of 3 1/2 miles—about the same range as Marconi had achieved at that date. Later he collaborated with Marconi in a series of tests for the Royal Navy. He became First Sea Lord in 1916 and was chairman of the Radio Research Board of Britain's Department of Scientific and Industrial Research in 1920. See Orrin Dunlap, Jr., *Radio's 100 Men of Science* (New York: Harper, 1944), p. 72; Baker, *History of the Marconi Company*, pp. 29 and 164; and Susskind, *Popov*, p. 6.

16. The patent is reprinted as Appendix E, pp. 316–340, in J. J. Fahie, *A History of Wireless Telegraphy* (New York: Arno Press reprint of 1901 Edition, 1971).

17. British law required that, in the Provisional Specification, the nature of the invention should be clearly foreshadowed or described in outline. In

the Complete Specification it had to be described in such detail that any person skilled in the art could carry it out without further instructions. The claims had to define precisely the new and useful invention and nothing else. A single invalid claim made the whole patent void.

18. The text and diagrams of the United States version of the patent (No. 586,193, filed 7 December 1896 and granted 13 July 1897) are substantially the same as in the British version, but the individual claims are stated in much greater detail and with greater care to insure that all possible combinations are covered. This results in a total of 56 claims in the U.S. patent.

19. As the managing director of the Marconi Company succinctly expressed it in 1907: "Marconi's patent is the first patent for wireless telegraphy; before that patent there was no wireless telegraphy." *Select Committee* (1907), testimony of Mr. Cuthbert Hall.

20. Also shown, as Figure 12 in the British application, is an interesting variant of Righi's metal-film detector. This is described as a rectangular strip of tinfoil glued to a glass plate and cut through at the midpoint by a sharp knife. The text makes it clear that this dipole was intended to serve as a method for determining the proper dimensions of the "plates" in the receiving and transmitting antennas. In other words, it was an early form of wavemeter or cymometer.

21. *The Electrician*, 17 September 1897, pp. 686–687.

22. From the text of the Complete Specification in Fahie, *History*, p. 323.

23. It is not entirely frivolous to point out, however, that the financing of his experiments had been part of the domestic economy of the Marconi family in Bologna. Family financing and family support were to continue vital to Marconi for many years after his move to England.

24. *Select Committee* (1907), testimony of Sir William Preece.

25. On the importance of studying technological failures, particularly if we wish to form a correct judgment of the costs of invention, see Nathan Rosenberg, "Science, Invention and Economic Growth," *Economic Journal*, Vol. 84, No. 333 (March 1974), pp. 90–108.

26. For Preece's career and methods, see Fahie, *History*, pp. 135–160. A detailed description of his system, characterized as "the first practical wireless system, that is to say one actually used for commercial purposes," may be found in Hawks, *Pioneers of Wireless*, pp. 159–167.

27. Oliver Lodge calculated in 1898, for example, that to send a message from London to New York by inductive telegraphy would require a sending circuit encircling the whole of England and a receiving circuit as big as the state of New York. See Lodge in *Journal of Institution of Electrical Engineers*, Vol. 27 (1898), p. 80 and ff., cited in H. Poincaré and Frederick K. Vreeland, *Maxwell's Theory and Wireless Telegraphy* (New York: McGraw, 1904), p. 127.

28. Cf. Elliot N. Sivowitch, "A Technological Survey of Broadcasting's Pre-History, 1876–1920," *Journal of Broadcasting*, Vol. 15, No. 1 (Winter 1970–1971), p. 6. Technically knowledgeable readers will recognize the statement in the text as a drastic simplification. Radiation field strength (in volts) is inversely proportional to distance in the case of free space propagation. In the case of transmission between stations near the surface of the earth, the same relationship holds when the "numerical distance" (as determined by ground conditions, frequency, and the actual distance expressed in wavelengths) is moderate; but when the numerical distance is great (of the order of 10 or greater) radiation field strength is inversely proportional to the square of the distance. At very high frequencies we have to consider both a direct wave and a ground-reflected wave, and the field strength at the receiving antenna will be the vector sum of the two, since they will be to some extent out of phase. In these circumstances, when the distance is great enough, radiation field strength is inversely proportional to the square of the distance. See F. E. Terman and J. M. Pettit, *Electronic Measurements* (New York: McGraw-Hill, 1952), pp. 417–432.
29. *Select Committee* (1907), testimony of Sir William Preece.
30. Baker, *History of the Marconi Company*, p.30. For the rationale behind state ownership of the telegraph network in the United Kingdom, see Jeffrey L. Kieve, *The Electric Telegraph: A Social and Economic History* (Newton Abbot: David and Charles, 1973), especially p. 230: "In the United Kingdom, unlike on the continent of Europe, there was never a suggestion that government control of the telegraph was a political-strategic necessity; the state monopoly was set up as a business proposition."
31. This emerges very clearly in Preece's testimony before the Select Committee of 1907: "[Marconi] came to me at a very fortunate time for myself, for I was just then smarting under the disappointment of having made a failure in communicating with the East Goodwin Lightship. In fact the system that we had developed was not able to communicate with moving ships and Mr. Marconi came to me with a plan by which he used Hertzian waves applied to a coherer . . . and I saw at once that this was something that would enable us to communicate with moving ships and at my recommendation every possible facility was given to Mr. Marconi." Later, misunderstanding a question about certain "exceptional advantages" the Marconi Company was alleged to have enjoyed, Preece replied: "The exceptional advantage is this, that it is able to maintain communication with moving ships, and moving ships with each other." The particular advantage of radio, in other words, was seen not as distance but as the ability to communicate with stations in motion.
32. Baker, *History of the Marconi Company*, p. 29.
33. For the Bristol Channel tests, see Fahie, *History*, pp. 218–219; Degna Marconi, *My Father, Marconi*, pp. 46–47; and Baker, *History of the Marconi*

Company, pp. 32–33. There is a map of the test area in Preece, "Signaling through Space without Wires."

34. Baker, *History of the Marconi Company*, p. 32.

35. As quoted in a letter to the Editor of the London *Times*, 17 October 1906, from John D. Oppe, Acting Manager of Marconi's Wireless Telegraph Company Limited. Slaby's comments, which appeared in *The Century Magazine*, April 1898, are reproduced *in extenso* in Vyvyan, *Wireless*, pp. 16–17.

36. See Baker, *History of the Marconi Company*, pp. 32–33. Slaby's equipment was no slavish imitation of Marconi's. In particular, his method of coupling the coherer to the antenna represented an early and significant improvement. Slaby was quick to admit, however, that until he inspected Marconi's apparatus he had never been able to signal farther than 100 meters. The "demonstration effect" of Marconi's tests was clearly of critical importance in setting Slaby off on his own semi-independent line of development.

37. S. G. Sturmey, *The Economic Development of Radio* (London: Gerald Duckworth & Co., Ltd., 1958), pp. 18–19.

38. For Marconi's family background, see Degna Marconi, *My Father, Marconi*, pp. 5–9, and B. J. Jacot and D. M. B. Collier, *Marconi—Master of Space. An Authorized Biography of the Marchese Marconi* (London: Hutchinson, 1936?), pp. 18–19.

39. Degna Marconi, *My Father, Marconi*, pp. 6–8. Compare Solari, *Marconi*, p. 7.

40. Sturmey, for example, quotes a provincial newspaper, the *Eastern Daily News* of Norwich, as reporting on 16 June 1897 that Marconi was "tired of kicking his heels on the inhospitable doorstep of the British Treasury" (Sturmey, *Radio*, p. 19 and p. 31, fn. 6).

41. S. G. Sturmey, *The Economic Development of Radio*, pp. 18–19, and Baker, *History of the Marconi Company*, pp. 33–35. Sturmey's account appears to be based in part on official Post Office records not used by other historians.

42. *Select Committee* (1907), testimony of J. Gavey, former Engineer in Chief of the Post Office. Partly at the insistence of Marconi's father, the name of the company was changed in 1900 to Marconi's Wireless Telegraph Company Ltd. Except where confusion with other Marconi enterprises might result, I refer to it throughout as the Marconi Company. The letter in which Marconi informed Preece of the sale of his patent rights to the company is reproduced inside the front and rear covers of W. P. Jolly, *Marconi* (London: Constable, 1972). In it Marconi cites as reasons justifying his action the facts that extensive experiments were still required to construct his apparatus "in a more practical form"; that the business was growing too extensive for him to handle alone, "as all the governments in

Europe want experiments carried out"; that the expense of patents was becoming too great for his private resources; and that the life of patents was very uncertain, especially in view of "the vigorous opposition made to me by Lodge in England, Tesla in America, and others in Europe."

43. Sturmey, *Radio*, p. 19. For the Italian rights, see below fn. 48.

44. *Select Committee* (1907), Report.

45. Licensed private amateur radio operators and certain experimental stations are exceptions to this generalization.

46. The Marconi Company shore stations were purchased by the British Post Office in September 1909 and operated as a public facility thereafter.

47. See Frances Donaldson, *The Marconi Scandal* (London: Rupert Hart-Davis, 1962), *passim*. For the "Imperial chain" negotiations, as seen from the Marconi Company's point of view, see Baker, *History of the Marconi Company*, Chapters 17, 25, and 26.

48. For Preece's initial reaction, see the correspondence cited in Jolly, *Marconi*, pp. 47–48, and *Select Committee* (1907), testimony of Henniker Heaton, M.P. Marconi was in Italy at the time the company was formed, demonstrating his apparatus at Spezia at the request of the Italian Government. This was apparently part of the special arrangements Marconi made to avoid being drafted for compulsory military service at this critical stage in his career. He was attached to the Italian Embassy in London as a naval cadet, his salary being donated to charity; the Italian rights were explicitly excluded from the sale of his patent to the Marconi Company; and he agreed to demonstrate his equipment for the benefit of the Italian Navy. The tests were relatively successful; in May 1898 the Italian Minister of Marine announced that the Navy was to adopt the Marconi system. There is no better testimony to the practical impact of the British Post Office tests than this. Marconi had offered his system to the Italian Government before ever leaving for England. His offer had been rejected. Yet it was precisely the same system that, two years later, the Italian Navy proposed to adopt.

49. *Select Committee* (1907), testimony of J. Gavey, former Engineer in Chief of the Post Office.

50. Sir Ambrose Fleming, "Guglielmo Marconi and the Development of Radio-Communication," *Journal of the Royal Society of Arts*, Vol. 86, No. 4436 (26 November 1937), pp. 42–63, at p. 58. The other relevant personal assets noted by Fleming were Marconi's "remarkable power of intuitive invention," his enormous perseverance and power of continuous work, and his "great power of persuading others to believe in the ultimate commercial success of his work as he did himself."

51. It is very difficult to ascertain from Baker's semiofficial *History of the Marconi Company* the company's earnings record in the first 15 years of its existence, or when the first dividends were paid. It seems evident, how-

ever, that no profits were distributed to shareholders from 1897 to 1910, when Godfrey Isaacs took over as managing director.

52. W. R. Maclaurin, *Invention and Innovation in the Radio Industry* (New York: Macmillan, 1949), p. 37; L. S. Howeth, *History of Communications-Electronics in the United States Navy* (Washington: Government Printing Office, 1963), p. 22, fn. 29. The terms of the contract were revised in July 1903 to provide for payment of £20,000 in cash plus £1600 in consideration of royalties due to that date, and a rental of £5000 per annum for the next 11 years. For the background to the revision of the contract, see Howeth, *loc. cit.*

53. The evidence cited by Jolly suggests strongly that the British Government considered Marconi equipment overpriced. Of the 32 sets purchased in 1900, one was promptly sent to the Ediswan Company to be copied. The Admiralty later admitted that it had had 50 copies made on which no royalties were paid. Consideration was also given to the use of Braun equipment, and in 1900 Oliver Lodge and Silvanus Thompson were commissioned by the Post Office to report on the validity of the Marconi patents and the possibility of getting around them by the use of similar but legally different apparatus. Captain Henry Jackson, who was throughout this period an invaluable ally of the Marconi Company, advised against the use of Braun equipment and against any attempt to contest the Marconi patents in court. See Jolly, *Marconi*, pp. 68–70, 85–87, and 91–92. By 1907 an Admiralty spokesman testified that the Navy was no longer dependent on the Marconi Company for further development in radiotelegraphy. See *Select Committee* (1907), Report.

54. Note, however, that this was the course followed by the United Fruit Company in the United States, as well as some smaller mail and packet lines in Europe.

55. Baker, *History of the Marconi Company*, pp. 59–60 and 85–88.

56. *Select Committee* (1907), Report, paragraph 22. The successful conclusion of these negotiations seems to have hinged upon private arrangements made by the Marconi Company with the Secretary of Lloyd's, Colonel Hozier, who had his own financial stake in wireless. Cuthbert Hall, managing director of the company, wrote on June 28, 1901: "Hozier would take about £3000 for his syndicate and come on the Board; and make an arrangement with us with Lloyd's. This arrangement will of course have to be precedent to the purchase of his syndicate. I told him that we did not value his patents at all and that the only reason for taking them over would be with a view to making an arrangement with Lloyd's. He has to prepare a statement of what Lloyd's stations he would advise his Board and on what terms" (Jolly, *Marconi*, p. 94). The contract with Lloyd's was signed on 26 September 1901. Hozier joined the board of the company shortly thereafter. Jolly reports that the company had at once to raise

£3000 of extra capital to pay Hozier and also to transfer to him 500 shares obtained from Marconi at £3 each.

57. Opposition to these provisions was led by Britain and Italy, whose governments had entered into agreements with the Marconi Company based on exclusive use of Marconi equipment. For details of the negotiations and of the final agreement, see Howeth, *History*, pp. 117–124.

58. In 1905, however, the Marconi Wireless Telegraph Company of America brought suit against the American De Forest Company in order to restrict its activities. Judgement was in favor of the plaintiff.

59. Baker, *History of the Marconi Company*, p. 88.

60. Compare the testimony of Cuthbert Hall, managing director of the Marconi Company, before the Select Committee of 1907: "We have got all the first-class liners and we have got them notwithstanding the fact that their governments have put a good deal of pressure on them not to adopt our system. The German government has pressured the German shipping companies, the French government has pressured the French shipping companies, and the same thing has happened in other countries."

61. An earlier cable had been opened for traffic in August 1858, but failed by October of the same year.

62. Sturmey, *Radio*, pp. 75–76.

63. An article published in *Century Magazine* in 1902, and based on interviews with Marconi after the Newfoundland tests, suggested however, a very different conclusion. "Mr. Marconi . . . believes that his system may become a formidable competitor against the ocean cables. To do so on land is not easy, as the lines there cost only one hundred dollars a mile, whereas the cables cost one thousand dollars a mile, and require expensive steamers to repair and maintain them. A transatlantic cable represents an initial outlay of at least three million dollars, besides the cost of maintenance. A Marconi station can be built for sixty thousand dollars. Three of these, bringing the two worlds into contact, will cost only one hundred and eighty thousand dollars, while their maintenance should be insignificant." P. T. McGrath, "Marconi and His Transatlantic Signal," *The Century Magazine* (new series), Vol. 41, (1902), pp. 769-782, at p. 781.

64. Baker (*History of the Marconi Company*, pp. 62–63) reports uneasiness on the part of the directors about probable costs and (seemingly a matter of more acute concern) the possibility of interference between the new superpower stations and existing low-powered equipment. The latter uncertainties were partially dissipated by a series of tests using syntonic tuning.

65. *Select Committee* (1907), testimony of J. Gavey.

66. Interview with Pupin as quoted in "Marconi Signals Across the Altantic," *Electrical World and Engineer*, 21 December 1901, pp. 1023–1025, at p. 1025: "One great drawback of the system is that you cannot work more

than one set of instruments at any one time between two continents on account of mutual interference."

67. For details of the occasion, see Howeth, *History*, pp. 38–39. Howeth includes a report, the truth of which he does not guarantee, that interference was deliberately caused by the American Wireless team, which in the course of the race transmitted long dashes at high power in a code of their own devising and, as the yachts crossed the finish line, put a weight on the key and left it there, thus generating what one participant called "the longest dash ever sent by wireless."
68. Howeth, *History*, p. 32.
69. There is some uncertainty as to whether the Marconi receivers used in these tests contained tuned circuits or not. Lt. John Blish, one of the official observers, stated that a coupling transformer was used and included a diagram of the circuit containing such a device in a subsequent report to the U. S. Naval Institute. Marconi sources indicate, however, that the coherer was connected directly between the antenna and ground. See Howeth, *History*, p. 30, fn. 15.
70. This would be true of any dipole, nor merely the grounded vertical, if the coherer were placed midway between the two arms. It would not be true, however, of a horizontal long-wire antenna half a wavelength long or some multiple of a half-wave. Paradoxically, a horizontal antenna of this type is often popularly called a Marconi antenna.
71. See Fleming, *Principles*, pp. 473–480, and Baker, *History of the Marconi Company*, pp. 32–33.
72. For Marconi's own description of the steps leading up to this discovery, see Guglielmo Marconi, "On the Progress of Electric Space Telegraphy," Royal Institution of Great Britain, *Proceedings*. Vol. 17 (1902), pp. 195–210, at p. 199. Baker's exposition (*History of the Marconi Company*, pp. 53–57) is very good.
73. All direct quotations are from the United States version of the patent.
74. This analysis parallels that of the majority opinion in *Marconi Wireless Telegraph Company of America v. United States*, U.S. Supreme Court, No. 369, October Term, 1942, decided June 21, 1943.
75. Baker, *History of the Marconi Company*, p. 134.
76. For Braun's work, see Heathcote, *Nobel Prize Winners*, pp. 81–86; W. H. Eccles, *Wireless* (London: Butterworth, 1933), pp. 77–78; and Fleming, *Principles*, pp. 489–495.
77. On 20 November 1919 the American Marconi Company had assigned to the newly formed Radio Corporation of America all of its assets, including the Lodge and Marconi tuning patents, but reserved to itself the claims at suit against the United States and committed itself to prosecute them. Suit against the United States Government was initially instituted on 29 July 1916.

78. For information on Stone, a much neglected figure in the history of radio technology, see George Clark, *Life of John Stone Stone* (San Diego: Frye and Smith, 1946); Louis Duncan, "The Stone Wireless Telegraph System," *Electrical World and Engineer*, Vol. 42, No. 17 (24 October 1903), pp. 675–676; and U.S. Supreme Court, *Marconi Co. v. United States* (1943). Much of Stone's testing was conducted in Cambridge, Massachusetts, near the site where the Massachusetts Institute of Technology dormitories now stand. One of his tests of the ability of his receivers to reject interference was to see whether signals could be copied through the electrical noise generated by the streetcars passing on nearby Massachusetts Avenue. As Stone pointed out, a streetcar's pickup from the overhead power line made a very effective vertical radiator.

79. Reginald Fessenden should be included in this company. Compare his testimony in the suit brought by the Marconi Wireless Telegraph Company of America against the National Electric Signaling Company [quoted in Helen M. Fessenden, *Fessenden, Builder of Tomorrows* (New York: Coward-McCann, 1940), p. 83]: "my primary idea was that the Lodge system (and later, Marconi's modification of it) was based on the wrong principle and that a successful apparatus must be built which radically differed from it in the three essential points of generating prolonged wave trains, including continuous reception, of tuning circuits together in true resonance, and of employing a current-operated constantly receptive and directly and proportionally acting receiver." Fessenden's search for a generator of true continuous waves stemmed from his interest in the transmission of voice.

80. In a similar vein Alvin Harlow, in his *Old Wires and New Waves* (New York: Appleton-Century, 1936), pp. 441–442, states that Lodge helped Marconi in 1897–1898 and granted him permission to use the idea of syntony in his system. Harlow, however, did not write from personal knowledge as Thompson apparently did.

81. The experiment is briefly described in Fleming, "Guglielmo Marconi"; in Eccles, *Wireless*, pp. 81–82; and in Baker, *History of the Marconi Company*, pp. 56–57.

82. The tests were described by Fleming in a letter to the London *Times*, 4 October 1900. It is interesting to note that, in this case, the antennas cannot have been tuned to resonance, since two different frequencies were being transmitted and received.

83. For this episode see *Electrical World and Engineer*, Vol. 43, No. 1, pp. 15–16, and Vol. 42, No. 5, p. 166. Maskelyne was transmitting from the roof of the Egyptian Theatre in Piccadilly, owned by his father, J. N. Maskelyne, a well-known illusionist who had used a form of radiotelegraphy in a "mind reading" act. Neville Maskelyne had earlier been associated with the Secretary of Lloyd's in a project to set up an automatic radio beacon in the Thames estuary. He was later to be involved in attempts to secure

financial backing for exploitation of De Forest and Poulsen patents in Britain. See Jolly, *Marconi*, pp. 142–143. Fleming claimed to be receiving signals from the Poldhu transmitter, but there was some question about whether any transmissions had actually been made from Poldhu at the time Fleming claimed to be receiving them in London. A somewhat similar episode occurred in the summer of 1902, when Czar Nicholas visited the Marconi radio station aboard the Italian cruiser *Carlo Alberto*, then visiting Kronstadt. Marconi on that occasion showed the Czar copies of messages said to have been received from Poldhu, including a congratulatory message addressed to the Czar himself. It subsequently appeared that this message had been sent from a transmitter especially installed for the purpose in another part of the ship. See Degna Marconi, *My Father, Marconi*, pp. 131–132.

84. Vyvyan, *Wireless*, p. 27.

85. Vyvyan, who was in close touch with Marconi's thinking, states that the decision to use higher power followed from the belief that antennas could not be raised any higher. This would be consistent with Marconi's theory (see above, p. 197) that range varied with the square of antenna height (Vyvyan, *Wireless*, p. 25).

86. Baker, *History of the Marconi Company*, p. 73; Vyvyan, *Wireless*, pp. 32–33. It is interesting to note that the reason why the letter "S" (three dots in Morse code) was used for the Newfoundland tests was that the Poldhu transmitter could not stand up to prolonged "key down" periods; there is no reason to believe, as is sometimes suggested, that the sequence of three dots would stand out from the background static more clearly than, say, three dashes. By the following year the keying system had been improved and normal messages could be sent. There has always been, of course, considerable skepticism as to whether Marconi did in fact receive the Poldhu signals. Belief that he did so is not made easier by our knowledge of the primitive receiving system used, and by the facts that the time and content of the transmission had been prearranged; that, of the three people present, only two (Marconi and his assistant, Kemp) heard the signals, the third being somewhat deaf; and that the transmission times and frequencies were, as was later learned, the worst possible in view of propagation conditions on the North Atlantic path. There is no need for us to enter this controversy here. Baker (*History of the Marconi Company*, p. 71) judiciously points out that, in 1901, anyone who believed that the Poldhu signals had crossed the Atlantic "did so as an act of faith based on the integrity of one man." The same is true today. It would be interesting, however, if those who accept Marconi's account uncritically would undertake to repeat the experiment, using the same location, duplicating his receiver and antenna, and attempting to receive signals of approximately the same radiated power on the same frequencies.

87. The practical limit to the height of antenna masts at this time was thought to be 200 feet. See Fleming, *Principles*, p. 450.

88. Baker, *History of the Marconi Company*, p. 114; for the design and use of the cymometer, see Fleming, *Principles*, pp. 404–417.

89. Baker, *History of the Marconi Company*, p. 68. The 366 meter estimate was made by H. M. Dowsett, an engineer employed by the Marconi Company at the time but not involved in the transatlantic tests. Fleming, in his description of the tests, provides many other technical details but nowhere estimates the frequency used (*Principles*, pp. 449–450); neither does Marconi in his "Progress of Electric Space Telegraphy," pp. 195–210.

90. Baker, *History of the Marconi Company*, pp. 64, 76, 78, and 116; Vyvyan, *Wireless*, pp. 37 and 44. Readers who prefer to think in terms of kilohertz may divide the figures given into 300,000. The same trend toward lower frequencies is evident in the frequencies or "tunes" for short-range ship radios, from 100 meters in 1902 to 300, 450, and 600 meters in 1907. The International Convention of 1907, Articles 2 and 3, specified 300 and 600 meters for general public traffic and reserved wavelengths between 600 and 1600 meters for government use.

91. Douglas Coe, *Marconi: Pioneer of Radio* (New York: J. Messner, 1943), p. 237.

92. Guglielmo Marconi, "Radiotelegraphy," *Annual Report of the Board of Directors of The Smithsonian Institution, 1911* (Washington: Government Printing Office, 1912), pp. 117–131, at pp. 128–129.

93. J. A. Fleming, "Radiotelegraphy: A Retrospect of Twenty Years," *The Electrician*, 15 September 1916, pp. 831–836, at p. 834.

94. The phrase is that used by Baker in his *History of the Marconi Company*, p. 97; but compare Edwin H. Armstrong, "Wrong Roads and Missed Chances: Some Ancient Radio History," *Midwest Engineer*, Vol. 3 (March 1951), pp. 3–5, 21–25.

95. Fleming, "Guglielmo Marconi," pp. 42–63, at p. 60. See also Clinton B. DeSoto, *Two Hundred Meters and Down: The Story of Amateur Radio* (West Hartford, Conn.: American Radio Relay League, 1936).

96. Quoted, Coe, *Marconi*, p. 237.

97. This is to express the problem in terms of individual personality, which makes sense in this instance. Marconi was not operating under any severe organizational constraints; the closely held family ownership of the corporation minimized normal pressures for immediate earnings. At another level of analysis we are clearly dealing with a case of "technological momentum," a concept whose possibilities have been explored in another context by Thomas P. Hughes. See Hughes, "Technological Momentum in History: Hydrogenation in Germany, 1893–1933," *Past and Present*, No. 441 (August 1969), pp. 106–132; and Eugene S. Ferguson, "Toward a Discipline of the History of Technology," *Technology and Culture*, Vol. 15, No. 1 (January 1974), pp. 13–30.

98. Baker, *History of the Marconi Company*, p. 117.

99. Vyvyan, *Wireless*, pp. 44–45.

100. Fleming, *Principles*, pp. 505–508, and "Radiotelegraphy," p. 834.

101. That is, by most experimenters and operators. Lodge despised the "whip-crack" effect because it was incompatible with precise syntony. Reginald Fessenden and John Stone Stone thought along the same lines, as did Braun. These were men who gave higher importance to syntony than to distance. Each regarded true continuous wave oscillations (CW) as preferable to spark. The problem was to find a means of generating them.

102. The logarithmic decrement at Clifden was between 0.015 and 0.03, which meant that each pulse went through about 30 or 40 oscillations before their amplitude became insignificant. See Marconi, "Radiotelegraphy," p. 120.

103. Quoted in Donaldson, *The Marconi Scandal*, p. 160.

104. It was possible in theory to use an alternator or even an arc as a local oscillator in the receiver, but in practice application of the heterodyne principle had to await the oscillating triode.

105. Fleming, "Radiotelegraphy," p. 834.

106. The concept of technological anomalies is discussed and illustrated in Edward W. Constant, III, "A Model for Technological Change Applied to the Turbojet Revolution," *Technology and Culture*, Vol. 14, No. 4 (October 1973), pp. 553–572.

107. For an interpretation of Isaac's personality, see Donaldson, *The Marconi Scandal*, pp. 137–156, 243–248, and 255.

108. For details of the takeover of United Wireless, which among other things set the stage for the Marconi scandal in England, see Donaldson, *The Marconi Scandal*, pp. 49–51.

109. The negotiations and the provisions of the final agreement are described more fully in Baker, *History of the Marconi Company*, pp. 130–135; but see also Sturmey, *Radio*, pp. 20–21.

SIX

EPILOGUE

The story we have been telling is a narrative of particular people, places, and events. But it is more than that. It is also a case study in the interaction between three fields of social action: science, technology, and the economy. It is appropriate, therefore, to ask as an epilogue or postscript what this story can tell us about how, in this instance, these three systems were related.

Responding to this question calls for courage, but also for caution. We have been dealing with a single case and no great weight of generalization can be rested on it. This case, however, did not exist in a historical vacuum. Hertz, Lodge, and Marconi were, each in his own way, actors in historical processes that stretched far back in time and that still shape our lives today. And the idea of syntony, given its full richness of meaning, has in one guise or another been a fundamental component of Western intellectual history. The events we are analyzing, then, must be interpreted as episodes in larger historical processes. These are the processes that have shaped the development of modern science, that have since the eighteenth century generated successive spurts of technological innovation almost cyclonic in their impact, and that have raised levels of real income per capita, for the minority of the world's population immediately affected, to unprecedented levels. Among these processes there has been constant interaction. Scientific change, technological change, and economic change have influenced one another in many complex and subtle ways, the outlines of which historical scholarship is now beginning to perceive. Our single case study takes a cross-section across these interrelated processes of change. If it cannot in itself throw much light on long-run trends, it may nevertheless highlight some important relationships and suggest some general hypotheses.

We began our narrative by describing how Heinrich Hertz and almost simultaneously Oliver Lodge discovered ways of submitting to empirical test Clerk Maxwell's model of the electromagnetic field. This was an episode of more than ordinary importance in the history of science, and from one point of view it would seem that from this all else followed. The later work of Lodge, Marconi, and others on radiocommunications represented the unfolding of the consequences of this scientific advance in the spheres of technology and economics. Essentially, then, from this perspective, pure science was the prime mover; technological invention and economic innovation were phases in the process by which the implications of the new knowledge created by science became evident.

From a different point of view, however, the central theme of our story has been the discovery of a new resource—the electromagnetic spectrum—and the invention of techniques by which it could be exploited for human use. Analogies with geographic or geological discovery suggest that knowledge of the mere existence of this resource was a necessary but by no means sufficient condition for its development. The new knowledge generated by science was an input, but only one, into the process of technological change that made practical use of the electromagnetic spectrum at radio frequencies a reality.

And lastly, it might be suggested that, for the history of modern society, the focus of our story must surely be the creation and development of a new industry, an industry that has revolutionized the business of communications and, according to some analysts, has also in the process revolutionized our habits of thought and perception, our politics, literature and art, our wars, our crimes, and our patterns of family life. This is the industry of electronics. Built, to be sure, upon new scientific and technological knowledge, the genesis of this new industry stands out as a creative act in its own right; its existence is one of the massive facts of history that differentiates our age from all previous ones.

Clearly nothing is to be gained by disputation over which of these perspectives is "correct," or which of the several themes of our story is or should be dominant. All can be valid and informative. This is why we selected as the thread on which to string our narrative no single one of these themes but rather an abstract idea—the idea of syntony—that was central to all three: to the scientific breakthrough itself, to the technologies devised to exploit that breakthrough, and to the industry that grew up to use those technologies. The point at which serious analysis begins is surely the recognition that creative behavior—the doing of new things, the learning to think and act in new ways—was involved in all three of the spheres of human action we have specified: science, technology, and economics.

What kind of explanatory model can we construct that will encompass the particulars of our case but also have more general usefulness? We can proceed by successive approximations.

What we are looking for is a frame of reference that will identify the significant types of interaction between the three sectors in such a way as to help us to understand how, historically, changes in any one have affected the other two.

The simplest model is one that identifies science as the source of new knowledge and then defines technological and economic change as stages in the process by which new knowledge is translated into forms suitable for the satisfaction of human wants. Many scholars appear to accept this model as an appropriate paradigm for the explanation of technological change, at least since the middle of the nineteenth century. In its simpler versions it conforms to what is popularly believed to be the case in the contemporary world. It is in this sense that the "science-based" technology of today is often contrasted with the more traditional and craft-based technologies of previous eras; and the rates of economic development achieved by industrialized nations in the nineteenth and twentieth centuries are frequently explained in terms of the larger outputs of new knowledge generated by science, and the more efficient institutional arrangements that emerged to make full and prompt use of that knowledge.

This model implicitly assumes that no problem can arise in deciding what is and what is not "science." For studies of the contemporary world this may be only a trivial matter; for historians it is more serious. In particular, the model runs into difficulties when applied to historical periods earlier than the eighteenth century, or to societies in which the social role of scientist is not as clearly differentiated as in our own. One escape route from this problem is a semantic one: to define as science any activity that results in the production of new knowledge. This reduces the explanatory scheme to a tautology.

There are other difficulties. The situation postulated by the model is one in which outputs generated by science ("discoveries") become inputs into technology and are there converted into useful devices or processes ("inventions"). The model assumes, in the first place, that the only source of technologically relevant new knowledge lies in that set of activities socially recognized as "science." It also assumes that outputs of new knowledge from science are necessary to the production of new inventions by technology, and further that they in some sense determine their timing and content. And in the same way it assumes that innovations in the economic sphere arise only from technological invention, and that their timing and content are determined by technology. The emphasis, in short, is entirely on supply-side variables, with scant attention paid to demand. The assumptions required to make such a view plausible are large indeed. More important—for implausible assumptions can still yield useful conclusions—this simple model leaves too many questions unasked and unanswered.

The source of the difficulty is that, in this model, the inputs to each sector are incompletely specified and it is assumed that the level of activity in each sector is completely supply determined. We are asked, in effect, to believe that the only transaction taking place between science and technology is the one-way transfer of new knowledge from science, and that the rate of technological invention is completely determined by the level of that one input. Similarly for the transactions between technology and the economy: the implicit assumption is that the level and content of economic innovation is completely determined by changes in the supply of new information from technology. The artificiality of these assumptions calls for little emphasis. It is no derogation of the importance of technology to hold that the level of activity in an economic system and its rate of growth are influenced by other variables, such as population growth, capital formation, and organizational change, as well as by new technology. Indeed, we would have to consign most of modern macroeconomic theory to oblivion if we were to argue otherwise. There are exogenous forces impinging on an economic system other than the rate of technological change; and there are endogenous processes within the economic system itself which influence its level of activity and therefore its demand for inputs from other sectors, including its effective demand for new technology. In the same way, within the technological system there are internal processes that affect its level of activity and its effective demand for inputs, including (but not limited to) inputs of new knowledge from science. Technology has its own immanent logic and its own "laws of motion," though we know less about them than we do about the structure and dynamics of the economy.[1]

These a priori considerations receive ample support from our case study. If there ever was an industry that could with confidence be referred to as "science based," it must surely be radiocommunications. Yet the content of the technological system that emerged from the work of Hertz, Lodge, and Marconi was by no means uniquely determined by the nature of the scientific advances made by Faraday and Maxwell.[2] This was true as regards the particulars: the development of radiofrequency detectors, to take only one example, from Hertz's ring detector to Fleming's diode valve, was a strictly technological process, highly empirical in nature, very much a matter of trial and error, and far more dependent on technological refinements (for example, in the production of purer metals and higher vacua) than on new breakthroughs in science. The same can be said of the development of antennas.[3] But, transcending the question of particular devices, we can make a similar generalization about the system as a whole.[4] The technological system of "telegraphy without wires" that Marconi took to England in 1896 certainly *used* the new scientific knowledge generated by Maxwell, Hertz, and Righi; but it used much else besides. Specifically, it used much of the established technology of wired telegraphy, much of the accumulated lore of generations of practical work with static electricity and electric currents, and a substantial measure of inspired amateurish guesswork. The new knowledge furnished by science was essential, and it was catalytic; but the particular technological system in which that knowledge came to be embodied was a highly creative blend of new and old. Consider the components that were the typical symbols of "wireless": the Leyden jars, induction coils, Morse keys, buzzers, and batteries. These were not new. They were part of the familiar knowledge that was, so to speak, the capital stock of late nineteenth-century technology. The process of technological change in this case was, in short, no mere passive reception of new knowledge from science, but a creative integration of this new knowledge with what had once been new but was now part of the familiar "state of the art"—the known devices and proven ways of doing things.

It would make no sense, therefore, at least in this case, to say that inputs of new knowledge from science uniquely determined the content of the particular technological system in which they were used. Can we perhaps say that they determined the timing? Not without careful qualification. If we take 1865, the year in which Maxwell's *Dynamical Theory of the Electromagnetic Field* was published, as the date at which the scientific ideas on which radiotelegraphy was based were first propounded, it was more than 20 years before Hertz translated these ideas into laboratory equipment with which measurements could be made and hypotheses confirmed. Only nine years elapsed, however, between the publication of the work of Hertz and Lodge and the issuance of Marconi's patent. At what date, then, shall we say that the new knowledge produced by science became "available" as an input to technology? An answer depends on where we think pure science stops and technology begins. Both Hertz and Lodge were engaged in testing Maxwell's model; the breakthrough came, for both, when they hit upon a particular technique—a technique for measuring standing waves. The brilliance of the experimentation involved is not in question. What is interesting to consider is the extent to which their achievements can reasonably be called technological—a highly refined and sophisticated technology, it is true, but nevertheless technology. It was Hertz's ring detector— a technological device

—that impressed Lodge. It was Lodge's use of long wires—a miniaturization of telegraph technology—that made his measurements possible. And it was the fact that both men had shown how electromagnetic radiation could be *detected*—a technological advance—that set afire the imagination of men like Marconi.

There is no need to belabor the point. Most of the three-decade time interval between Maxwell's *Theory* and Marconi's patent was occupied by the process of devising apparatus by which the phenomena implied by Maxwell's equations could be generated and detected. Whether we choose to stress the technological character of the creative work that finally made this possible, or alternatively to emphasize that the knowledge implicit in Maxwell's theory was not available *as usable knowledge* until 1888, is largely a matter of taste. But precisely because of this indeterminacy, to say that the timing of the scientific discovery determined the timing of the technological advance seems a more confident statement than the evidence warrants.

It is only within broad limits, then, and with extensive qualifications that the scientific discovery in this instance can be said to have determined the content and timing of the technological advance. Obviously other variables have to be considered. In the same way, the technological advance did not uniquely determine the nature and timing of the economic innovation. Oliver Lodge provides the clearest evidence on this point. Here was a man who by 1894 had assembled and demonstrated a complete functioning system of radiocommunication. Yet he saw no immediate use for it and was quite prepared to set it to one side as an interesting curiosity, despite the fact that Crookes in 1892 had clearly envisaged communication by Hertzian waves, and that Alexander Muirhead, his future partner, had explicitly called the economic potential of the system to his attention. Why bother to communicate with difficulty without wires when it was so much easier to communicate with them? Lodge, however, is not the only example; if he were, we could dismiss it as a quirk of individual personality. Popov knew as much about Hertz's work as Lodge did and was well acquainted with Lodge's experiments, yet the only immediate utility he saw in the new technology was a meteorological one.

But what of Marconi? Surely he understood the economic significance of Hertzian waves? Surely in his case we can say that science, translated into usable technology, dictated the form and timing of economic innovation? Quite the contrary. To express the matter in these terms is to misinterpret Marconi's genius and the importance of his role in history. What differentiated Marconi from his contemporary rivals was not his scientific knowledge nor, initially, the distinctive excellence of his technology. It was his sense of the market, of where a demand for this new technology existed or could be created. A creative genius in electronic engineering Marconi may have been; but he was also a commercial entrepreneur. And his entrepreneurship was a vital element in the creation of a radiocommunications industry precisely because the nature of the technology itself did not unambiguously indicate the economic uses to which it could be put. Marconi saw where the technology could find a point of entry into the economic system; more than that, he made others see it as he did. The showmanship that set the teeth of scientists on edge, the flair for public relations that made his name and "wireless" almost synonymous—these were not accidents or unnecessary characteristics of an idiosyncratic personality. On the contrary, they were functional. And the fact that they were functional is strong evidence for the assertion that the new technology did not in any necessary or automatic way find its economic place. A place had to be found for it.

It is worth reemphasizing, too, how narrow this place was. What was the new technology good *for*? Until World War I the answer could without serious error be summed up in two words: ships and lighthouses. The idea of radio is today so closely associated with public broadcasting that it comes as a recurrent shock to realize that this particular use of the new technology was in no way part of the thinking of the inventors who developed the technology nor of the entrepreneurs who initially built an industry around it. Rather, it was grafted onto a technology already created. As for long-distance point-to-point communications, we have already seen that for the emergent technology of radio this was a marginal and highly speculative use. Until the rediscovery of the short waves in the 1920's the established technology of wired telegraphy, overland and submarine, had little to fear from radio.

The central point is indeed incontrovertible. Marine communications, civil and military, provided virtually the sole point of entry for the new technology. Even for these uses its appropriateness had to be determined in the teeth of considerable skepticism. And its diffusion, even for these limited purposes, called for commercial entrepreneurship of a high order. The new technology did not translate itself into an economic innovation, any more than the scientific breakthrough had translated itself into a new technology.

Let us turn now to the question of timing. It is clear that commercial development followed hard on the heels of technological feasibility. The Marconi Company was incorporated in July 1897. This was a scant three years after Lodge's demonstrations of signaling at Oxford and within weeks of the date of Marconi's first patent. If, more realistically, we date commercial development from 1901, when the Marconi Company won its first large contracts, we are still within a decade of the Oxford demonstrations and within five years of the Marconi patent. The rate of commercial development is indeed remarkable. The market was limited, but it was exploited vigorously and successfully.

To say this, however, is not to say that the demand side of the relationship was irrelevant. Regarded in the broadest perspective, the appearance of radio was without question a response to the rising demand for long-distance communications in the closing decades of the nineteenth century. The innovation was, in that sense, demand induced. More particularly, the rapidity of commercial development strongly suggests that the new technology appeared on the scene at a favorable economic and technological juncture. Without overestimating William Preece's role, we must recognize that by the mid-1890's the futility of further work with inductive telegraphy was becoming apparent. On the other hand, the specific communications needs that Preece, Lloyd's, the Masters of Trinity House, and the British Admiralty so clearly recognized could not be met by any seaward extension of the now mature technology of wired telegraphy. There was, in other words, an unsatisfied demand for the particular kind of communications service which the new technology could provide, and by the middle of the 1890's the existence of this demand was coming to be explicitly recognized—not, to be sure, by the man in the street, but by a number of strategically placed individuals and government departments with responsibilities in the field of communications. The focus of our story has been on Britain. But in Germany the Slaby, Arco, and Braun patents were exploited almost as rapidly and with much heavier bureaucratic support. In the United States commercial development was more speculative and erratic, but men like De Forest, Fessenden, Stone, and Shoemaker were keenly aware of the latent demand for their systems, and the U. S. Navy Department was not slow to realize what the new technology implied for strategic deployment and tactical maneuvering. There was, in short, by the mid-1890's, a felt need for a form of telegraphic communication that could transcend the limitations of wired or visual systems. The pertinacity of experimentation with inductive telegraphy and the alacrity with which agencies like the British Post Office and the several naval staffs reacted to "wireless," once Marconi demonstrated what it could do, both demonstrate the fact.

But the economic conjuncture was favorable in other respects also. The year 1896 has been one of the pivot points of our story because it marked Marconi's arrival in London and the start of the phase of commercial development. But the years that bracket 1896 are well known to economic historians for reasons of a quite different kind: they marked the close of what has come to be called—somewhat inappropriately—the "Great Depression" of the late nineteenth century, a 20-year period of sagging price levels, falling interest rates, declining profits in most industry, depressed trade, and bearish business expectations.[5] This period was not, however, one of mass unemployment nor even of falling wages. Real wages in Britain and the United States followed a rising trend throughout the period. There is no evidence of falling per capita incomes nor of a decline in the standard of living of the working and middle classes.[6]

Because of these seemingly paradoxical characteristics, the two decades that ended in 1896 have proved of intense interest to economic historians. There is no need for us to explore the ramifications of the analysis here; we may safely leave it to others to debate whether the mid-1890's marked a climacteric in the British (and American) economies and, if so, what the reasons for the apparent retardation in the rate of economic growth may have been.[7] For our purposes it is enough to note that 1896 was a year in which the trend of prices began to turn up, and in which business confidence began to revive.[8] That Marconi arrived in London in that particular year was coincidental, but there is an undeniable symbolic appropriateness in the timing. More than symbolism is involved, however. The radio industry was launched at a time when the rate of return on capital was low and when, with the change in business expectations, risk capital was more readily available to finance new ventures than it had been for some time in the past. To Marconi personally this made a difference. The family capital that provided the initial financing for the Marconi Company and that underwrote his later experiments came from an industry that, unlike some others, had done well in the previous decades. Distilling was not a bad trade to be in when business was depressed; and, as long as there was no mass unemployment, rising real wages meant rising sales for a quality whiskey. What was true of Marconi individually was true also, in a larger context, of the new industry he helped create. It appeared on the scene and was introduced to investors in the early phases of a period of reviving business confidence, when the long downward sag of prices seemed at last to have ended, when interest rates were low and hopes were sanguine. If there was, in terms of long-term trends, an economic climacteric in the mid-1890's, there was also, from the point of view of the entrepreneur and investor, a new flush of confidence, a willingness to venture, an openness to new things. It is no coincidence that, when one lists the technologies that have done most to shape twentieth-century culture—electronics, the automobile, aviation, reinforced concrete construction, to name only a few—one finds that they were first launched as industries between 1896 and the outbreak of the First World War. The statistics may tell a story of retardation, but the industries that were to sustain later development were being established precisely in the period when the potentials of the older technologies—the technologies of iron, coal, and steam—were approaching exhaustion.

To complete the picture we must also remind ourselves that radio technology appeared on the scene in a period of rising nationalism. Adoption and diffusion were accelerated by government sponsorship and government contracts, awarded not because the new technology offered a marginal reduction in costs over the next best alternative but because it could do things of military importance that no other technology could do. Price competition was of small importance in this market; what counted were the completely new capabilities. Navy departments in Britain, Germany, Italy, and the United States were for this reason among the first to appreciate the potentials of radiotelegraphy and, despite the opposition of many line officers, to insist on its use for strategic and tactical control. They were also, outside Britain, the first to emphasize the risk to national security inherent in dependence on a single company, and that a British one. Official support for rival systems and opposition to the Marconi "monopoly" followed; by differentiating and fragmenting the market, this may well have stimulated experimentation with variations on the basic technique and thereby accelerated the pace of technological advance. Nationalism also led, in Britain, to ambitious schemes for an imperial telecommunications system, and in Germany, with greater success, to the construction before World War I of high-powered stations to maintain communications with the United States and the African colonies should the submarine cables be cut in the event of war. The demand for the new technology of radio stemmed not only from the services it could perform for maritime commerce but also from its significance for naval supremacy and imperial defense.

What are the implications for our explanatory model? We have now found good reason to doubt several of the assumptions and implications of our original "first approximation." First, to identify science as the only source of new knowledge requires us either to so broaden the meaning of the term "science" as to make the statement tautological, or else to define "new knowledge" so narrowly as to make it coterminous with that particular kind of systematized abstract knowledge that is the output of science as conventionally defined. We have also seen that no sharp boundary exists between science and technology; there is a gray area where scientific technology and technological science overlap, and it is in this gray area that some of the most productive interactions between the two fields take place.

Second, we have seen that neither the content nor the timing of technological advances are uniquely determined by prior changes in the supply of new knowledge by science. Technology is more than a mere passive recipient of new information. It is also the custodian of an accumulated stock, to which current inputs from science are marginal increments. The transfers of new information that take place between science and technology are determined as much by the demand function of technology as by the supply function of science.

Third, we have found reason to believe that the interactions between technological and economic change, far from being simple and one way, are complex and reciprocal. What we call an economic innovation is in the typical case a creative synthesis of old and new technologies: a melding of new increments with elements from an inherited stock. It is understandable that the new increments often seem more dramatic and historically significant, since it is they which lend a distinctive character to the innovation. But not all possible new increments to technology are in fact invented; and not all of those that get invented are adopted. As with the interactions between science and technology, so it is with the interactions between technology and the economy: the demand side of the relationship cannot be ignored. Economic factors exert a selective, "screening" effect on technological change, determining what kinds of new technology are demanded and when. The demand for new technology varies over time in response to changes in the growth rate of the technology-using industries. Relative factor prices and in some cases the structural imbalances created by other innovations bias the choice of new technology in particular directions. And, where defense or war is involved, new technology may be introduced, absorbed, and diffused at a rate that purely economic considerations would never warrant. New technology may indeed have a cyclonic effect on economic systems and on whole societies; but it is not a wholly exogenous force, blind and unpredictable in its impact. What technology is introduced, and when it is introduced, depends partly on the nature and timing of the demand.[9]

Clearly our model needs refinement. As a second approximation, therefore, let us extend it by taking explicit account of the fact that the transactions between science, technology, and the economy are of several different kinds and that they are not one-directional. Technology, for example, not only receives inputs from science but also transfers part of its output to science. Similarly it sells part of its output to the economy and absorbs resources from it. Furthermore, as we shall see, the "markets" (using the term in its most general sense) in which inputs and outputs are exchanged between the three systems are of different kinds and follow different "rules of the game." The outputs of science are not property in the sense in which the outputs of technology are, and they are not transferred by the same processes. What are the relevant inputs and outputs? What determines the volume of transactions? How does each system achieve a viable "balance of payments"? How does it learn which inputs to select and which outputs to produce? How does each system respond to changes in the demand for its outputs or in the supply of its inputs? With what lags in adjustment? What factors influence the productivity of each system, the efficiency with which it transforms inputs into outputs? How does each system "track": that is, how does it set out on and persist in a particular path of change over time?

Two markets or fields of interaction are involved: that between science and technology, and that between technology and the economy. We assume, that is to say, that no exchanges take place directly between science and the economic system. Technology serves as the intermediary. Now, it is evident from our discussion so far that in each of these markets there exist not only the forward transfers emphasized in our "first approximation" but also at least one significant feedback loop. For example, in the market between technology and the economy there occur both the forward transfer of new devices, products, and processes and also two important reverse flows. The first of these feedback loops carries information concerning demand functions and cost functions; or, to be less cryptic, information concerning which elements in the output flows of technology "make sense" in terms of the calculus of profit and loss by which the economic system lives. (With proper adjustments in terminology, analogous statements could be made for a socialist economy, since the rules for optimal allocation of scarce resources among alternative uses are formally the same for each.) By virtue of this reverse flow of information, in other words, a process of screening and filtering takes place that separates out those new devices and processes that stand a reasonable chance of proving profitable from those that are dismissed as "crackpot notions" or, more mercifully, as ideas whose time has not yet come. Reinforcing this feedback of information is a second reverse flow: a discriminatory allocation of economic resources, flowing easily and in ample volume to those inventions whose economic profitability is seen, but reluctantly if at all to the rest. This market, in short, besides transferring property rights, serves two important additional functions. For the economy, it discriminates between technological "noise" and technological information; for the technological system it furnishes signals indicating which lines of activity are rewarded and should be expanded and which are regarded as uneconomic and should be contracted. The market is, in other words, a filtering or switching system.

With appropriate modifications many of the same generalizations can be made about the market in which science and the technological system meet and interact. The forward transfers we have already described: the outputs of new knowledge which it is the specialized function of science to generate. And among these outputs, as among the outputs of technology, a filtering and screening process takes place, as technology discriminates between those which it can use and absorb into its stock of accumulated useful knowledge and those which it cannot. The nature of the property rights that are transferred is, however, very different, and so are the institutional arrangements that govern behavior in this market.

Most of the informational output of science—the new knowledge generated — is channeled back into science itself. It is produced not in response to technological or economic demand but to the internally generated demands of science. Once produced, it is reinvested within science. This is a form of internal feedback, of regeneration in the electronic sense, which goes far toward explaining why scientific knowledge, over considerable periods of time, tends to grow exponentially. Scientists discover new knowledge, in other words, largely in response to problems posed by scientists, and the new knowledge so generated is consumed largely by scientists themselves.[10]

The consequences for the organization and development of science and for its relations with other sectors of society are profound. To the extent that there is an "export" of new knowledge from science to technology, it is usually a byproduct of the scientific process itself. There are, of course, exceptions to this generalization, particularly cases where an urgent military demand is reflected directly back through the intervening technological system and becomes part of the system of imperatives to which scientists respond. These cases are, however, exceptions, and scientists themselves are the first to insist upon it. One way of expressing this situation is to say that, in most cases, outputs of new knowledge *from* science are costless duplicates of outputs generated and reinvested *within* science itself. They are costless because they would be produced in any case irrespective of their use as inputs by technology. They are duplicates because exports of new knowledge to technology, and indirectly to the economic system, do not decrease the size of the internal flows of new knowledge within science.

Outputs of new knowledge from science, furthermore, fall into the category of public goods. They are not and probably cannot be institutionally defined as private property, and they cannot be traded or priced. A new scientific discovery becomes available for use by technology when it is published and becomes public property. A new technological discovery, in contrast, becomes available for economic use when it is patented and becomes private property.

These considerations help to explain why the course of scientific research—the way in which science "tracks"—is largely independent of technological or economic need. The signals to which science is organized to respond are internal signals, generated within science itself, not signals transmitted from the outside. Thus science is largely insulated from the end-use demands that, through the price system, affect technology so significantly. One of the functions of technology is, in fact, to serve as a protective buffer, intervening between pure science and the urgent day-to-day demands of industry; and there are other buffers, notably the applied sciences and the institutional association of pure science with the educational system.

At the human level these differences in market structure are reflected in striking differences in modes of behavior. As Hirschleifer has suggested, we can describe the seller of outputs in the market for new technology as typically a "pusher," in the same sense as that in which we use the word with reference to the traffic in addictive drugs.[11] He is prepared to incur short-run costs—indeed even to give his product away free—in the hope of building up a long-run market. In contrast, pushing of new knowledge in the market where science and technology meet is highly unusual. Science is not autarkic; it does provide outputs to technology, and it is critically dependent on the economic system for the income by which it lives. But it is relatively autonomous in the sense that it is not so organized as to respond to signals from the outside. Thus the typical mode of transfer in the market between science and technology is one in which information is pulled out of science rather than pushed by it. Modern societies have in fact developed specialized professions —the applied sciences—to perform precisely this function.

The professional ethics of pure science and the processes by which it maintains its status-ranking strongly reinforce its insulation, in normal circumstances, from technological and economic demands. Not so in the world of technology. We have seen in the case of Oliver Lodge how confusion between the different modes of approved behavior in the two markets could cause personal distress and indecisiveness. As a producer of new scientific knowledge, Lodge felt that he had done all that was expected of him, and all that he should properly do, when his discoveries had been published and made freely available to all. As a producer of new technology, however, he could not afford to publish until he had first patented his discovery and thereby secured the property rights that entitled him to exclude others from unauthorized use. Conditioned to accept the mores appropriate to pure science, Lodge found himself acting in the market for technology in which different rules and values prevailed. The long delay in enforcing his patent rights undoubtedly stemmed from the resulting ambivalence.

In the market where science and technology meet and interact there are also important reverse flows. These are of two main types: reverse flows of information, and reverse flows of technological devices. Reverse flows of information have been of great importance historically. Until the nineteenth century, indeed, a major function of science was to codify, rationalize, and explain what technology discovered, rather than to provide new knowledge for technology to use. An encyclopedia of the arts and trades was a work of science because it systematically classified and organized the information generated by technological practice, much as a botanist might classify plant varieties. Technology, in short, was the teacher, science the observer, the learner, the student.[12] As the nineteenth century progressed, however, and science became a self-consciously specialized and professionalized activity, with a rigorous methodology of proof that called for systematic experiments and observation under controlled conditions, the sources of information on which it relied became increasingly self-generated. More and more, scientists attended only to evidence created by scientists. Applied science developed as a way of translating the outputs of pure science forward into technology, not of reporting the information garnered by technology back into science.

Reverse flows of information from technology into science never completely disappeared, however. What might be called the general reverse flow—the routine feedback of everyday information on shop practice, craft knowledge, and technological lore in the broadest sense—became attenuated and less highly valued, although in some fields of science, such as chemical engineering and the agricultural sciences, it retained much of its former importance. More significantly, science could never completely insulate itself from a particular kind of informational feedback. This was the reporting of anomalies: bits of empirical information that conflicted with accepted scientific theories or that, even if they did not conflict, called for extension, elaboration, or refinement. Thomas Kuhn has stressed the important role that anomalies play in precipitating major changes in scientific paradigms; he has also reminded us of the facility with which normal science can ignore anomalies generated by the work of scientists themselves or, up to a point, "take care" of them by ad hoc extensions of accepted theory.[13] Even more easily could scientists ignore or misinterpret anomalies reported from outside science, the reports "from the field" (that is, from technological experience) that did not confirm scientific expectations. In doing so science was correctly following its own rules of conduct: data from uncontrolled, ill-reported, and often nonrepeatable experiments were not the kind of signals to which science was organized to react. Information flowing backward across the interface between science and technology was therefore heavily discounted. Beyond a certain point, however, it could not be ignored and, once past this threshold, it could precipitate important changes in the direction of scientific inquiry.

We have seen a number of examples of this process in our case study, and it is worth noting that they arise much more in connection with the work of Marconi, who was not an academically trained scientist, than in that of Lodge, who was. Marconi's entrepreneurial drive—his determination to make a commercial success of radio—carried him into areas where the science of his day could offer no guidance. If he had known more science, if he had been more thoroughly conditioned in the systematic cautiousness characteristic of normal science, if he had thought of his reputation as depending primarily on the respect of scientists, he might well have accomplished less. This was particularly true in his work on antennas and on long-distance propagation. Marconi's antennas, up to 1914, typically had no scientific rationale behind them: he used them because, for reasons imperfectly understood, they gave the results he wanted, particularly greater distance. The endless variations made in their dimensions and configuration speak eloquently to the pragmatic empiricism that inspired them. Lodge, in contrast, had a theory of antennas, albeit an incomplete one, derived from his work on syntonic Leyden jars, and his practical designs were deduced from that theory. He knew a priori what should work and what should not; the range of variation of his antenna designs was, in consequence, very limited. Now, antenna theory and design is a field still awaiting its historian, but even a superficial knowledge of later antenna configurations suggests that Marconi's models had multiple progeny while Lodge's capacity areas proved relatively sterile. It was Marconi, not Lodge, who achieved with his antennas the kind of anomalous unexpected results that called for a rethinking of antenna theory. Left to themselves, without these inputs of information from technological and commercial experience, scientists would have had little incentive to move beyond the theory of the Hertzian linear dipole, which was simple to work with mathematically and perfectly adequate for all ordinary laboratory experiments. Problems arising in the commercial use of radio received initially ad hoc technological solutions; only later was the rationale for these solutions systematically elaborated by scientists.

Long-distance propagation provides an even more explicit example. This was a case where the results achieved by Marconi's technology were clearly anomalous, in terms of received scientific belief. As Lodge expressed it: "To a public ignorant of the work of Clerk Maxwell and Hertz . . .[short-distance wireless telegraphy] came as a great surprise and seemed very novel and mysterious. To physicists it did not seem so: it was a natural application of what was known. But when Senatore Marconi found experimentally that the waves would actually curve around the earth and reach the American continent, physicists were surprised. It was an important discovery."[14] Important, that is, for science, and precisely because it was anomalous. Inputs of information from technology in this instance quite unambiguously challenged accepted scientific assumptions, compelled a rethinking of theory, and elicited from science a response in terms of research on solar radiation, geomagnetism, and the earth's ionosphere that is still in progress today.

The second component of the reverse flows from technology into science consists of the instruments, tools, and materials on which scientists rely to do their work. Here the reference is not to the everyday hardware and materials that can be bought on the market and that are properly classified as outputs of the economic system, but rather to the special-purpose devices that are critical to the success of experiments. Measuring instruments are a particularly interesting subclass of these: Faraday's galvanometer and Michelson's interferometer are only two examples of many.[15] But, besides measuring devices, the technological system provides a myriad of other processes, materials, and bits of hardware that help make the scientific enterprise possible: Ruhmkorff coils, Leyden jars, Dewar flasks, klystron tubes, mercury vacuum pumps, cyclotrons, and linear accelerators—the list could easily be extended. These devices are not initially normal outputs of the economic system but rather special-purpose outputs of technology itself, produced, designed, and often invented specifically to serve as inputs into science.[16] And more than just hardware is involved: the invention of the transistor would have been impossible if there had not become available materials (germanium and silicon) of a purity never before achieved. The attainment of this degree of purity was an achievement of technology, not of science. The history of astronomical, chemical, and physical research is full of similar examples. Creative technology, time and again, has been the key to scientific discovery, measurement, and proof.

Considerations of this kind suggest the addition of yet a third component to our list of reverse flows from technology to science: skilled manpower, or what economists now call human capital. The flow of manpower from technology into science has never been large. The medieval separation of the man of learning from the craftsman has its modern counterpart in the distinction between the scientist and the "mere" engineer or technician; and the "web of osmotic ties" which, in Rostow's phrase, linked scientists, inventors, and innovators in eighteenth-century Britain comes more and more to seem in retrospect a highly unusual product of historical circumstances.[17] Professionalization always generates barriers to entry, and these are reinforced by the increasing length of formal education in highly structured curricula required for professional competence. It is difficult to imagine Michael Faraday following the same path to eminence today. In the contemporary world the flow of manpower seems clearly to be out of pure science into applied science and technology rather than the reverse. We find scientists becoming successful inventors, even successful businessmen, but few following the reverse route. Nevertheless, there is one kind of reverse flow of human talent that we should not ignore. These are the instrument makers, the laboratory assistants, and the scientific technicians—those unsung heroes who have, one suspects, played a vital but relatively unrecognized role in the history of scientific discovery. These are not the stars in the scientific firmament. They do not win the Nobel prizes, though without them many prizes might not have been won. No great scientific laboratory has been without its cadre of skilled technicians who, by their personal craftsmanship, have brought the skills, special knowledge, and inherited lore of technology to bear on the manufacture of apparatus and the conduct of experiments.

Let us summarize our "second approximation" model so far. The major addition has been the specification of reverse flows or feedback loops in each of the areas of interaction. Between science and technology, in addition to the forward flow of systematized knowledge, we have called attention to reverse flows of information, of technical devices, and to a limited extent of manpower. Between technology and the economy there is a forward flow of new inventions, and a reverse flow of resources and of information on demand and costs. From the economic point of view these areas of interaction are markets where exchanges take place, though not necessarily in terms of prices or property rights. But in terms of information theory they are fields where searching, choosing, and steering processes occur. Just as the economy searches among the outputs of the technological system to discriminate between what it can and cannot use, reinforcing its verdict by transferring command over resources to those it favors, so technology searches among the new knowledge generated by science to find what can be integrated into its existing stock of knowledge to generate new technological outputs.

Despite the formal resemblance between the two markets, however, there are important differences in their structure and functioning. The technological system is so organized as to react promptly to the signals it receives from the economy. It tracks in response to market demands. Science, in contrast, tracks largely in response to internally generated signals; although not totally insulated from indications of technological "need" (and therefore, at one remove, of economic need), it is to a much higher degree self-steering. To influence the direction of scientific advance, information from the technological system has to be recognized as constituting an anomaly posing a problem for science; or alternatively, it has to be represented as a technological need of overriding importance backed by the authority of the state.

These differences are reflected in different modes of economic support for the two systems. The technological system, in a capitalist economy, derives its income primarily from sales of its output and only secondarily from grants and subsidies. It participates as a seller of outputs and buyer of inputs in the price system, a fact that has much to do with its rapid response to economic requirements, at least insofar as the price system reflects these. Science, in contrast, derives its economic support primarily from grants and subsidies and hardly at all from sales of output. It is, in Kenneth Boulding's phrase, part of the "grants economy," living off subsidies from individuals, foundations, corporations, or the government, either directly or indirectly through its close association with the educational system.[18] This is because its outputs are not property in the legal sense. They are public goods. They do not command a price, though they may be very costly to produce.

This mode of supporting science has a long history. The desire of scientists to insulate their decision-making processes from the pressures of the marketplace has reflected a conviction that science achieves higher levels of productivity when given such protection. To say that science does not in the short run respond to social need is, therefore, not a criticism of science but a description of the institutional arrangements that have been developed in the conviction that they offer the best assurance of continued high productivity in science. The thrust of these arrangements, in capitalist countries, has been to limit what Harold Innis called the penetrative power of the price system; in centrally directed economies they serve the analogous function of limiting the penetrative power of the planning system.[19] Close affiliation with academic institutions has provided added protection. And, most important, technology and the applied sciences have served as buffers, cushioning the impact of social demands by providing responses from the inherited stock of usable information.

Technology, it is clear, can be steered toward the production of particular outputs needed in the economy by means of the price system, which acts both as an efficient disseminator of information and as an allocator of resources. Whether science can, in an analogous way, be steered toward the production of particular items of new knowledge or the solution of particular problems posed by technology is very much an open question. The market between science and technology neither transmits signals to which science promptly responds nor does it allocate the resources on which science lives. The availability of funds by grant or subsidy to particular fields of pure science can certainly attract human resources and physical facilities to those fields. But it is not clear that such a differential allocation of economic support increases in any predictable way the probability of a particular scientific "solution" to the problem posed. If it did, we would have a cure for cancer by now. Increased allocation of resources to a field of scientific research may produce only ever more refined versions of partial solutions already discovered: perfected iron lungs to treat polio rather than a Salk vaccine. Science insists on being self-steering, not because scientists are insensitive to social and economic problems but because scientific discoveries cannot be produced to order, nor is their nature predictable in advance. Economic support for pure science is in this sense a continuing act of faith, resting on a record of past performance and a generalized assurance of future productivity, but not on contracts for the delivery of specific future outputs.

This is why, if in the development of a particular sector of the economy problems are encountered that cannot be solved within the framework of existing practice, or only at costs considered excessively high, recourse is had not to pure science but first to technology and then to the applied sciences: to technology because it is the custodian of society's stock of useful knowledge; to the applied sciences because it is their specialized function to scan the current and past outputs of pure science and convert them into forms that technology can use. In both fields creative skills of a high order may be required. Technological solutions, even where no new inputs of knowledge from science are involved, require new combinations of items already known. The applied sciences, precisely because they mediate between fields of knowledge organized on different principles and oriented to different objectives, are called upon to make connections between items of information previously disjunct and separate. These are creative functions: their performance leaves knowledge organized in new and different configurations.

This is completely consistent with recognition that pure science is that sector of society which specializes in the discovery of new knowledge and its systematic organization into formal logical structures called theories. But the scientific way of knowing is not the only way of knowing, scientific knowledge is not the only systematically organized body of knowledge, and scientific creativity is not the only form of creativity. Technology is also an organized body of knowledge and there are technological ways of problem-solving which are not the ways of the scientist. It may indeed be true that the technological style of thinking and acting—with its heavy reliance on intuition, on design, on solutions that feel right, look right, and work right—has more in common with art than with science. Be that as it may, human societies had technology and knew technological change long before they knew science. Modern technology may well be science-based in a sense in which older technologies were not; this reflects essentially the fact that the level of scientific output is higher and the forward transfers of new knowledge from science to technology are more efficiently organized and more completely institutionalized than in the past. It does not mean that technology, once the teacher of science, is now a mere processor of scientific byproducts.

These suggestions receive ample support from our case study, although it must be remembered that in it we are dealing with a period in which the applied sciences were far less organized than they are today, and in which corporate research and development was unusual, particularly in Great Britain. Nevertheless, it is clear that, while science played an essential role in making radiotelegraphy possible, it contributed little to the development of the technology thereafter, up to the close of the period with which we have been concerned. A standard manual of the first decade of the twentieth century, such as Fleming's authoritative *Principles of Electric Wave Telegraphy* or Poincaré and Vreeland's *Maxwell's Theory and Wireless Telegraphy*, is replete with detail on the design of apparatus and circuits but has nothing to say of scientific contributions once the basic phenomena of radiation and resonance are described. Our description of the evolution of Marconi's equipment, and of that of Lodge and Muirhead, bears witness to technological advances but not to scientific ones. The true continuous wave generators, such as the oscillating arc and the high frequency alternator, with whose emergence our story ends, called for brilliant engineering work but not for new knowledge from science. Even Fleming's diode valve, the strategic invention that ushered in the second phase in the history of radiocommunications, required no new scientific knowledge for its discovery. The so-called "Edison effect" in electric light bulbs had been observed many years before; Fleming's insight was to see how this known phenomenon could be used to make a detector less sensitive to "atmospherics" than the coherer or crystal rectifier. The same generalization can be made with reference to De Forest's triode vacuum tube, a device of major technological importance whose principles of operation the inventor himself did not understand and which certainly called for no new inputs of information from science.[20] Improvements in the technology of vacuum pumps and of glass-to-metal seals were of far greater significance in the development of the vacuum tube than any discoveries in pure science. The story is, in short, one of technological creativity, a matter of ingenious recombinations of items of knowledge already present in technology's inventory; after the initial scientific breakthrough—itself, as we have seen, largely due to advances in the technology of laboratory experimentation—no further inputs of new knowledge from pure science were forthcoming during the period with which our case study has been concerned.

If our "second approximation" model brought into sharp focus no considerations other than these, it would still represent a worthwhile improvement over the first. In fact, however, the implications go deeper. The model assumes the existence of three specialized systems of social action—science, technology, and the economy—and points to interactions between them which we have described as two-way flows of inputs and outputs. Because these flows provide mutual support, the levels of activity in the three systems, and the rates of change of those levels, are to a degree interdependent. They are, as it were, linked together as trading partners. But because the flows also serve as searching and steering processes, because they transmit signals as well as sustenance, the *directions* of change in the three systems are also interdependent. Because the flows between them serve as communications channels as well as trading channels, the three systems do not track independently over historic time. The direction technology takes is not independent of the particular characteristics of the new knowledge supplied by science, for changes in these characteristics can radically modify the probable cost of inventions of particular types, and the likelihood of success. On the other hand, as Schmookler has shown, the nature of the inventions that are in fact made, as well as the number occurring in specific fields, are clearly influenced by estimates of their probable economic value. Thus both demand and the changing state of knowledge guide the path of technological change, the one by signaling probable value, the other by indicating probable feasibility and cost.[21]

To assert interdependence without specifying the form such interdependence takes and how it can be measured is to indulge in the kind of generalization which can hardly be refuted and which is therefore not very interesting. Clearly the interdependence is not of the simple mechanical form assumed in our first model, in which one-way forward transfers from science into technology and thence into the economy tied the level of economic innovation directly to the rate of technological invention and that to the rate of scientific discovery. The linkages are more complex than that: the markets in which the transfers take place differ in important structural ways; and there are stocks of knowledge in each system available for current use. In particular, the stock of knowledge held by technology serves as an inventory on which the economy can draw even in the absence of new inputs from science. The level of transactions, in other words, between technology and the economy is not a simple function of the level of transactions between science and technology.

If we are to go beyond conclusions of this type, more is involved than merely analysis of the endogenous factors which determine the level of activity in each system. In the case of the economic system, for example, we already have available a highly sophisticated body of theory developed precisely for that purpose. This body of theory, however, with few exceptions takes technological change as an exogenous influence, not determined by any of the internal processes of the system but impinging on it "from the outside" and not calling for explanation in economic terms.[22] Likewise historians and sociologists of science have thrown considerable light on the internal processes by which science advances, and in most cases have shown greater sensitivity to the relationships between the system they study and the society in which it lives and functions than have economists. But few would claim that an adequate theory of these relationships, or even a framework of concepts within which such a theory might be formulated, has yet appeared. As for technology, we are only now beginning to appreciate (although Marx told us long ago) that there is in every society such a thing as a technological system, just as there is in every society an economic system of some sort, that this system has a structure which can be analyzed, and that its relationships to the wider society can be systematically studied, to the mutual benefit both of the student of technology and of the student of social change in general.[23]

If ever a unified theory of the relations between science, technology, and the economy does emerge, it will surely include a more complete specification of the flows between the three systems than our "second approximation" has emphasized. And it will also include analysis of the ways in which these flows have been influenced by other social systems that have been referred to here only indirectly, such as government, religion, and the arts. The construction of such a theory is a large assignment; too large to be appended as a postscript to an essay on radio technology. There is, however, no reason to believe that it is impossible; indeed, some of the key processes and the variables that influence them are already apparent.[24]

Can we go any further at this point? In one respect we can. Our "second approximation" has been a formal scheme, a matter of flows and interactions, of markets organized in different ways. For a historian this may be a helpful guide to systematic analysis; but it may also seem somewhat bloodless and impersonal. History is richer and more human than that. A "third approximation" model should go beyond such a formal system of transactions and markets and recognize that we are dealing with patterns of human life, with the hopes, fears, frustrations, and disappointments of human beings. Any scheme of analysis we use should be competent to show not only what the events described meant for social processes in the large but also what they meant for the individuals participating in them.

Our model so far has been described in terms of specialized systems of social action and the markets in which they interact. Each of these specialized systems, however, is also a kind of subculture. That is to say, it provides a framework of shared values, of accepted and sanctioned ways of thinking, perceiving, and acting in terms of which people organize their lives. The markets in which science, technology, and the economy meet are areas of interaction between subcultures: between ways of life that have their own differentiated values, their own sanctioned modes of behavior, their own systems of allocating rewards and punishments, and above all their own languages.

Seen in these terms, the central interest in our case study has been in how ideas are transferred from one subculture to another. If one were to conduct a similar study today, one would be concerned primarily with the institutions that have emerged to mediate between science, technology, and the economy: with the formal organizations, corporate, governmental, and educational, that now specialize in the transfer of information among the three systems. Our case study has dealt with an earlier period, one in which these functions were less institutionalized than they are now. For this reason the problems involved in cultural contact and the transfer of ideas between subcultures stand out more clearly. What have now become specialized areas of functional responsibility within formal bureaucracies were, in the period of our story, problems and opportunities for individuals, who had to figure out for themselves what roles to play and how to play them.

The trend toward institutionalization of these relationships should not, of course, be exaggerated. There is plenty of evidence to indicate that today, behind the screen of formal organization or completely outside it, particular individuals still play vital and highly personal roles in transferring ideas among the three systems. Nor should we overemphasize the extent to which the subcultures are differentiated. There is a substantial body of shared values and patterns of behavior today, no matter whether an individual is a research scientist, a field engineer, or an executive in the public relations department of a large corporation. This was probably even more true in the period of our story, when specialization of function had gone less far and bureaucratic organizations were less in evidence in all three areas.

Nevertheless, just as there is differentiation of function among the three subcultures, so there are differences in approved patterns of thought and action. Effective transfer of information between any two of them depends on the presence of individuals or institutions capable of functioning in both, of being to a degree accepted by both, of talking in both languages and of efficiently translating one into the other. The two-way flows of information stressed in our "second approximation" do not occur spontaneously or without human intervention. The efficiency with which they take place, indeed whether they take place at all, depends on the functioning of these intermediating individuals and institutions. They are the agents of transfer, the translators who make it possible for science, technology, and the economy to "speak" to one another.

Clearly this translating function can be performed in a variety of ways. The technical magazine; the public lecture; the professional association; institutions like the Royal Society and the famous Lunar Society of Birmingham; government bureaus like the British Post Office or the U.S. Department of Agriculture; technical schools, universities, and institutes of technology; even personal friendships, family connections, and the contingencies of everyday life; these are only a few of the ways in which, in modern societies, transfers of information between the three subcultures take place. Some of them have played an important role in our case study. More conspicuous, however, have been particular individuals—Hertz, Lodge, and Marconi—who, each in his own way, served as a translator, taking information generated in one subculture and transforming it into information relevant to another.

In phrasing the issue in these terms, we intend to point to the existence of a particular social role that, certainly in the history of radio technology and probably in other areas also, was vital to the creation of new technology and to its economic adoption and diffusion. This social role evades identification in everyday parlance precisely because it does not fit into conventional categories of occupational specialization. We are pointing to scientists like Lodge who were more than scientists, and to inventors like Marconi who were more than inventors. In the case of Marconi, indeed, the difficulty is acute. Was Marconi a scientist? Hardly. Was he a businessman? Not of any conventional type. Was he an inventor? Yes, if you define the word carefully. Such semantic difficulties are informative: they tell us that we are dealing with a social role that society does not recognize as a job or profession. And yet there can be little doubt that, in the history of technology, performance of this role has been of more than incidental importance.

Individuals who function effectively in such a role cannot in the nature of the case be specialists. They are translators. They must be fluent in more than one "language," at home in more than one world, adept at playing by more than one set of rules. Indeed, it is probably the case that such individuals do not possess the skills or qualities of personality that would enable them to function comfortably and to full effectiveness within the confines of a single system. The person who is most at home in pure science, for example, is probably not the sort of person likely to be at home in the area where science and technology interact. To function effectively in this area such individuals must be able to tolerate a degree of ambivalence, to recognize and respond to two different sets of signals, to live with continuing compromises between the abstract logical rigor of science and the pragmatic problem-solving imperatives of technology. They must also, unless their scientific triumphs are already behind them, be willing to accept the fact that, in acting as translators between two systems, they are unlikely to win the highest prizes in either.

Such ambiguities and ambivalences are particularly likely in the gray area between science and technology. The reasons are implicit in our earlier analysis: property rights in this area are ill-defined and information passing from technology to science tends to be systematically discounted. Extreme sensitivity to status differences is one symptom of the stresses that can result. But translation between technology and the economy is also not without its problems, particularly because it is at this stage that considerations of prices, costs, and profits first become salient. The annals of technology are full of stories of virtuosos of technology who proved incapable of coping with the stresses of the marketplace. Reginald Fessenden is the clearest case in the history of radio technology, but Howard Armstrong is a second and more tragic example. Individuals who combine within themselves the necessary aptitudes—Edison, Marconi, Elmer Sperry, Edwin Land—are historically unusual. More common are those who, by partnership or incorporation become part of a team in which technological and commercial skills are synthesized. Boulton and Watt are the classic example; Lodge and Muirhead, at a lower level of effectiveness, provide another.

Partly because of the demands they impose on individuals, partly because of economies of scale in information processing, these translator roles have in the contemporary world become highly institutionalized: in the applied sciences, and in corporate or government research and development laboratories. We have become sensitive to the degree to which our new technology is "science-based"; to the degree to which a business firm, if it wishes to retain its share of the market, must systematically exploit the potentials of new technology; to the unpredictable ways in which advances in pure science influence technology and, through technology, the structure and performance of the economy. We have, in short, become self-consciously aware of some of the transactions that take place between science, technology, and the economy; and we have tried, with mixed success, to exert deliberate policy control over them.

Our story has dealt with an earlier phase, when the issues themselves were only dimly perceived and when institutions to cope with them had barely begun to evolve. As a result, the problems stand out more starkly in human terms. Hertz, Lodge, and Marconi were translators, middlemen between the world of pure science—the world of Maxwell's equations—and the world of commerce, where rival communications systems vied for traffic. Hertz was, so far as the historical record indicates, completely disinterested in any kind of technological or commercial applications. The problem he undertook to solve was a problem posed by science, not by technology or economic need. The signals to which he responded and the rewards he considered worth striving for were set by the internal structure of science, not by the demands of the outside world. Yet it was Hertz who took the first indispensable step in translating Maxwell's theory of the electromagnetic field into a system of laboratory technology by means of which radiation could be emitted, detected, and measured. Lodge, starting from very much the same position in science but with a lifelong interest in "practical applications," went further; by 1894 Lodge had produced and demonstrated, in embryonic form, a system of radiotelegraphy by which signals could be, and in fact were, exchanged. The transfer from pure science to usable technology had been made. Lodge, however, did not at that time take the next step. The reasons are partly clear—his developing interest in a purely scientific problem, the measurement of aether drift—and partly conjectural: a dominant orientation to the values and folkways of science rather than to the commercialism of the marketplace. It was left to Marconi to complete the process. Marconi had minimal scientific training; yet, through his association with Righi, he had access to scientific knowledge, literature, and equipment. By the same token, he had no personal experience of the world of business; but, through his mother's family, he had access to business advice and business capital. Above all, as we have already stressed, although Marconi originally contributed little except refinements in detail to the new technology, he saw more clearly than most where it could be made to fit into the economic system of his day. Just as Hertz had translated a set of mathematical equations into experimental apparatus by means of which measurements could be made and hypotheses tested; just as Lodge had translated experimental apparatus into what was at least potentially a feasible technology of radiocommunications; so

Marconi took laboratory technology and translated it into an economically viable business, the root from which sprang the electronics industry of today. At each stage in the process of translation, information generated in one system was converted into a form that "made sense" in terms of another; and at each stage new information was blended with what was already known to create something essentially new.

No single case study can ever prove a generalization, nor do an accumulation of case studies necessarily add up to a theory. What is possible, however, is that analysis of a single episode in the interactions between science, technology, and the economy can suggest relationships of wider applicability. A framework of ideas that does justice to the particulars of this case but at the same time lends itself to generalization must recognize the presence of creativity at each of the levels of action we have been concerned with. Modern science is without question that segment of society which specializes in the generation of systematically organized new knowledge, and there is no doubt that the new knowledge generated by science is becoming more and more central to the creation of new technology. But technology is also a body of systematically organized knowledge, and in the ways in which it combines and recombines components of its inventory, and adds to them new increments from science, it is capable of a high degree of creativity. And similarly the ways in which new technologies are screened, modified for economic use, and integrated into a functioning economic system require and exhibit creativity of high order. If we wish to understand these creative processes of change, and particularly if we wish to exercise a measure of control over them, our attention must focus on the ways in which knowledge is transferred. Vital to these transfers are the individuals and institutions that perform what I have called the translator function, decoding information generated in one system and transforming it into information usable in another. Historically, these individuals and institutions have served as the carriers of technological change. In the future they may also serve as the agents for responsible control over it.

NOTES

1. And this despite the fact that, in the Western world, there has developed extreme sensitivity to the social and ecological impact of new technology, while in the Soviet Union, according to one recent analyst, "Absolutely no research has been done into such a key question in Marxist-Leninist theory as the constantly postulated 'laws' of technology—the inner logic and necessity of technological progress." See Reinhard Rürup, "Historians and Modern Technology," *Technology and Culture*, Vol. 15, No. 2 (April 1974), pp. 161–193, at p. 181 For an attempt to grapple with the problem, see Robert Heilbroner, "Do Machines Make History?", *Technology and Culture*, Vol. 8, No. 3 (July 1967), pp. 333–345.
2. The role of demand in influencing the rate and direction of inventive activity, and indeed of scientific research also, has been brought into sharp focus by the brilliant empirical work of Jacob Schmookler. Demand, in Schmookler's model, induces the inventions that satisfy it. See his *Invention and Economic Growth* (Cambridge, Mass.: Harvard University Press, 1966), and *Patents, Invention and Economic Change* (Cambridge, Mass.: Harvard University Press, 1972). As Nathan Rosenberg has pointed out, however, "The role of demand side forces is of limited explanatory value unless one is capable of defining and identifying them *independently* of the evidence that the demand was satisfied" ("Science, Invention and Economic Growth," *Economic Journal*, Vol. 84, No. 1 (March, 1974) pp. 90–108). Rosenberg's position is that, if we wish to explain the historical sequence of inventions, we must pay close attention to the particular characteristics of the stock of scientific knowledge at particular times. Demand forces may determine the payoff to successful invention; but supply side forces determine the probability of success and the prospective cost of succeeding. This seems fully compatible with the findings of our case study.
3. On the importance of such progressive refinements made within the technological system itself, see Schmookler, *Invention and Economic Growth*, esp. Chapter 4, and Nathan Rosenberg, *Technology and American Economic Growth* (New York: Harper & Row, 1972), *passim*.
4. The scientific knowledge generated by Maxwell and Hertz could, of course, have been used initially for quite different purposes: for example, for radiotherapy, much as the earlier "electrical machines" had been used for medical purposes, fake or genuine. Why the new scientific knowledge was used first for communications is a historical problem that calls for explana-

tion; that this should be first and for long the most important use was not somehow latent in the knowledge itself. If Western man had not felt a compulsion to "shout a long way" (Lodge's phrase), the use made of the new knowledge would have been quite different.

5. See S. B. Saul, *The Myth of the Great Depression 1873–1896* (New York: St. Martin's Press, 1969). There is, of course, room for disagreement about timing. C. H. Feinstein dates the cyclical turning point in total output (gross domestic product) at 1892–1893. As regards the trend of prices, his implied price deflator for gross domestic product (1913 = 100) reaches a peak of 109.2 in 1873 and declines irregularly to a low of 86 in 1896. See C. H. Feinstein, *National Income, Expenditure and Output of the United Kingdom 1855–1965* (Cambridge: Cambridge University Press, 1972), p. 16 and p. T132, Table 61.

6. According to Feinstein's estimates, in only six of the years between 1870 and 1896 did the percentage of the working population in the United Kingdom unemployed rise above five. Gross domestic product at constant factor cost (1913 prices) was £34 per capita in 1876 and fell below that figure (to £33) in only one year before 1896, when it stood at £41 per capita. See Feinstein, *National Income*, p. T125, Table 57, and p. T42, Table 17.

7. Serious analysis of this problem dates from the pioneering article by E. H. Phelps Brown and S. J. Handfield-Jones, "The Climacteric of the 1890's: A Study in the Expanding Economy," *Oxford Economic Papers*, Vol. 4, No. 3 (October 1952), pp. 266–307. A number of generalizations, once widely accepted, regarding the rate and timing of economic growth in this period have fared badly when subjected to critical scrutiny in the light of better statistics. Joseph Schumpeter set the date for the end of his "Second Kondratieff" or long wave at 1897, suggesting that "symbolically as it were," that year could be taken to mark the end of an era. The electrical industry (including radio) was in his view the major "carrier innovation" for the Third Kondratieff, the long swing in economic growth that was supposed to have begun in 1898, just as railroads had been for the Second. See Joseph A. Schumpeter, *Business Cycles: A Theoretical, Historical, and Statistical Analysis of the Capitalist Process*, 2 vols. (New York: McGraw-Hill, 1939), Vol. I, pp. 304 and 397. Feinstein's estimates of the rate of growth of domestic product at constant factor cost (per cent per year compounded) yield a peak rate of 2.4 for the period 1866–1873, a sharp decline to 1.9 for the years 1873–1882, and a partial recovery to 2.0 for 1882–1890 and 2.1 for 1890–1900. Retardation of economic growth seems to have been most marked, in the United Kingdom, in the first decade of the twentieth century. See Feinstein, *National Income*, p. 19, Table 1.7; compare Donald N. McCloskey, Ed., *Essays on a Mature Economy: Britain after 1840* (Princeton: Princeton University Press, 1971), and his "Did Victorian Britain Fail?", *Economic History Review* (2nd series), Vol. 23 (December 1970), pp. 446–459. Retardation of economic growth in the United States in the same period seems to have been of roughly the same order of magnitude. See J.

G. Williamson, "Late Nineteenth Century American Retardation: A Neoclassical Analysis," *Journal of Economic History*, Vol. 33, No. 3 (September 1973), pp. 581–607.

8. Compare Willard L. Thorp, *Business Annals* (New York: National Bureau of Economic Research, 1926), p. 172.
9. Compare Schmookler, *Invention and Economic Growth* and *Patents, Invention and Economic Change, passim*.
10. This is not to say, however, that levels of support for science are unaffected by hopes, on the part of scientists and others, that the new knowledge supplied by science will prove to have practically useful results. Once again, Schmookler provides a useful corrective to simplistic interpretations; see his "Catastrophe and Utilitarianism in the Development of Basic Science," *Patents, Invention and Economic Change*, pp. 47–59. Recognition of this fact is entirely consistent with the view, expressed below, that the course of scientific research is relatively independent of the price system. In the one case we are talking about the flow of inputs into science; in the other, the characteristics of the outputs that result. If there were any close relationship between the two, the history of science would be a simpler enterprise than it is.
11. J. Hirschleifer, "Where Are We in the Theory of Information?", *American Economic Review, Papers and Proceedings*, Vol. 43, No. 2 (May 1973), pp. 31–49.
12. Thus David Felix points out that "Early nineteenth-century scientific theorizing was still impelled by autonomous advances in technological practice, rather like the social sciences today adjust their theories with a lag to ongoing policy innovations." See David Felix, "Technological Dualism in Late Industrializers: On Theory, History and Policy," *Journal of Economic History*, Vol. 34, No. 1 (March 1974), pp. 194–238, at pp. 229–230. Compare Peter Mathias, "British Industrialization: Unique or Not?" in *L'Industrialisation en Europe au XIX' Siècle*, Colloques Internationaux du Centre National de la Recherche Scientifique (Lyons, 1970), and David Landes, *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe from 1750 to the Present* (Cambridge: Cambridge University Press, 1969), p. 104.
13. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press; 1962).
15. Oliver Lodge, *Talks About Radio* (New York: Doran, 1925), pp. 60–61.
15. L. Pearce Williams, *Michael Faraday: A Biography* (New York: Basic Books, 1965), esp. pp. 168–183; Loyd Swenson, *The Ethereal Aether* (Austin: University of Texas Press, 1972). Derek de Solla Price is one of those who have argued strongly for more study of scientific instrumentation and "the special craft of experimental science," which he refers to as "a technology which has been more crucial to the advancement of science and perhaps a domain of strong interaction much more important than the apparently

weak and infrequent 'application' of science to make new technology." And he attributes to this technology an important role in bringing about the scientific revolution of the seventeenth century. See Price, "On the Historiographic Revolution in the History of Technology: Commentary," *Technology and Culture*, Vol. 15, No. 1 (January 1974), pp. 42–48, at p. 47. See also Samuel Lilley, "The Development of Scientific Instruments in the Seventeenth Century," Chapter 6 in *The History of Science; Origins and Results of the Scientific Revolution: A Symposium* (Glencoe, Ill.: The Free Press, 1951); Lilley, *Men, Machines, and History* (New York: International Publishers, 1966); Herbert J. Cooper, *Scientific Instruments* (Brooklyn: Chemical Publishing Co., 1946); and Silvio Bedini, *Early Scientific Instruments and Their Makers* (Washington, D.C.: Smithsonian Institution, 1964).

16. And often of direct use to inventors also. Thus Rostow argues that "the experimental method, built into the scientific revolution, directly increased the supply of inventions, through the two-way linkage of scientists and toolmakers. The scientists needed pumps and telescopes, the microscope, the thermometer, the barometer, and accurate clocks. Inventors and others could also use them." See W. W. Rostow, "The Beginnings of Modern Growth in Europe: An Essay in Synthesis," *Journal of Economic History*, Vol. 33, No. 3 (September 1973), pp. 547–579.

17. Rostow, "Modern Growth," p. 561; A. E. Musson and Eric Robinson, *Science and Technology in the Industrial Revolution* (Manchester: University Press, 1969); A. E. Musson, Ed., *Science, Technology, and Economic Growth in the Eighteenth Century* (London: Methuen, 1972).

18. Kenneth E. Boulding, *The Economy of Love and Fear: A Preface to Grants Economics* (Belmont, Calif.: Wadsworth, 1973). See also Kenneth E. Boulding and Martin Pfaff, Eds., *Redistribution to the Rich and the Poor: The Grants Economics of Income Distribution* (Belmont, Calif.: Wadsworth, 1972).

19. Harold A. Innis, "The Penetrative Power of the Price System", in *Essays in Canadian Economic History* (Toronto: University of Toronto Press, 1956), pp. 252–272.

20. J. A. Fleming, *Memories of a Scientific Life* (London and Edinburgh: Marshall, Morgan and Scott, 1934), pp. 140–143; Sir Oliver Lodge, *Past Years: An Autobiography* (London: Hodder and Stoughton, 1931), pp. 333–334; Georgette Carneal, *De Forest, A Conqueror of Space* (New York: Liveright, 1930), pp. 185–196; Robert A. Chipman, "De Forest and the Triode Detector," *Scientific American*, Vol. 212 (March 1965), pp. 92–100; George Shiers, "The First Electron Tube," *Scientific American*, Vol. 220 (March 1969), pp. 104–112.

21. Compare Schmookler, *Invention and Economic Growth*, pp. 165–188, and Rosenberg, "Science, Invention and Economic Growth."

22. Thus Schmookler in 1966 could refer to technological change as the *terra incognita* of modern economics—a factor that had to be introduced into the traditional analysis ad hoc, "like a war or an earthquake." See his *Invention*

and Economic Growth, pp. 1–17, and, for more extended comments, "Technological Change and Economic Theory," *American Economic Review*, Vol. 55 (March 1965), pp. 333–341.

23. Anthropologists have always been aware of these relationships, but there has been disappointingly little "spill-over" of their findings and intuitions to the other social sciences or to history, partly because of their dominant interest in preliterate and preindustrial cultures. Lewis Mumford is, of course, the towering exception.

24. If elaborated to include other social subsystems, our model would eventually resemble a Leontief-type input-output matrix, the rows and columns of which would represent the outputs that each sector provides to the other sectors, and the inputs it receives from them. For a pioneering effort in this direction, see Joseph S. Berliner, *Economy, Society and Welfare: A Study in Social Economics* (New York: Praeger, 1972). Berliner correctly stresses that the size of the matrix depends on the interests and hypotheses of the research worker.

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