INTRODUCTION

Most scientific research is, so to say, “mundane”; it is painstaking labor that requires much time, effort, study, skill, and indeed ingenuity, but it rarely involves revolutionary path-breaking, fundamental issues of Nature and her behavior or the attention of a wide audience and the concomitant glamor. The adjective “mundane” in this context should imply neither dullness nor diminution. Creativity and originality come out of this kind of research not less than from the more glorious studies. The daily basis on which much scientific work is carried out makes it neither less important nor less creative. Romantic notions of the isolated genius who exposes the great secrets of Nature by revelation, like the legendary story of Archimedes jumping out of the bath, are not confirmed by detailed studies. Despite what might sometimes be thought, mundane science can be an intriguing intellectual and practical enterprise, which, like the more glamorous stories in the history of science, involves among other things originality, surprises, and controversies.

This book is a study of a branch of mundane physics at the end of the nineteenth century. It discusses the discovery of a particular phenomenon—the generation of electricity by mechanical strain and of mechanical strain by electricity in crystals—named piezoelectricity, and the development of its research. How was piezoelectricity developed and why did it develop this way rather than in other ways? These are the basic questions of this study and their answers are sought through a thorough examination of the field’s history. To understand the particular development of the field, I have looked at the practice of physicists—in the ways they chose their subjects of research and carried them out, on their background and on the reasoning that led to hypotheses, theories, models, experiments, discoveries, and mistakes. I reconstruct the physicists’ reasons to investigate particular questions in particular ways. It is a historical rather than a rational reconstruction. The general approaches of “research programs” toward science and logical derivations are important, but the role of personal motivations, academic positions, scientific traditions, and contingent circumstances cannot be ignored.

Many historical studies have been dedicated to the discovery of novel effects and phenomena. Following the discovery of a new basic phenomenon and the emergence of a novel field, much less attention was dedicated to the early development of physical research. This book follows the history of piezoelectricity after the discovery of the effect to its consolidation into an accepted body of experimental and theoretical knowledge. This process seems to recur in the establishment of novel scientific subfields and thereby, knowledge. The mere discovery of an effect (in piezoelectricity that pressure induces electric polarity in crystals) was not enough to establish a scientific subfield. It could have been left as merely another curious experimental fact unconnected to any other. A subfield emerged only with subsequent study of the observed effect and related phenomena under various conditions, which resulted in the knowledge of their characteristics and laws. Such study is neither self-evident nor inevitable. The subsequent study depended both on the phenomenon and on a few scientists interested in issues that it raised; the interest stemming from their own theoretical experimental, or occupational concerns, and their earlier works. The emergence of a new subfield requires a basic consensus on the phenomena that it encompasses and their characteristics. As this history shows, such a consensus evolved via experimental study, theoretical arguments, and controversy. Its evolution was part of a process of consolidation that the new field underwent in the first two decades after its discovery. At the end of this process piezoelectricity encompassed an accepted body of knowledge consisting in experimental findings and a mathematical theory that accounted for them. Still, many issues were left open and were the subject of disagreement between scientists. That the subfield of piezoelectricity was compatible with the general concepts and laws of contemporary science enabled its consolidation. Discordance with the accepted truths of physics would probably have precluded a consensus on the theory. Apparently this process, which I examine here, characterizes non-revolutionary fields.

The historiography of nineteenth century physics has concentrated on the main developments in central theories that had implications in several branches of the field and continued to be important for later physics. The basic questions of thermodynamics and the kinetic theory of heat and electromagnetism have enjoyed a major attention from historians. Yet these questions occupied only a part (although significant) of physical research at the time. Many physicists were often occupied with various other questions that were only partly or indirectly connected to these issues. Many fields of research and developments of nineteenth century physical science have not been subject to an adequate historical analysis. This is true in particular of the research of the "gross matter" phenomena, like elasticity, the physics of crystals and dielectrics, and researches that had no direct impact on the study of other fields. Piezoelectricity represents both kinds of understudied fields. Therefore, its history offers an additional perspective on late-nineteenth century physics. This perspective reveals important but until now obscure developments like that of the concept and application of symmetry in physics (which involved subjects like crystallography, optics, elasticity, and heat conductivity). It turns attention to understudied fields and developments in nineteenth century physics like elasticity or pyroelectricity. The history of piezoelectricity also provides an additional view on subjects that have attracted more historical research like electromagnetism and thermodynamics by discussing their application in the research of a "gross matter" phenomenon. It shows how various approaches and theories were conceived and applied to a particular problem by working physicists. The examination of the practice of physicists provides meaning to notions like acceptance or rejection of theories. It reveals the elements of different theories that the physicists adopted or rejected and how they applied them. In other words, it shows the significance of particular views to physical research. This research examines the application of known theories like the thermodynamic potentials. The historians paid much more attention to their formulation and origins than to their application. However, their application was crucial to their influence on physical practice. Without it they would have been only slogans or issues of scientific faith.

Justifiably, historians have looked for the roots of the revolutions in physics at the beginning of the twentieth century. Thus, physical questions that led to the subsequent revolutionary events have gained more attention than those that did not. By focusing on the relativistic and quantum revolutions, historians have given more emphasis to breaks in the development of physics than to the continuity. Piezoelectricity displays another path from nineteenth to twentieth century physics, a path characterized by continuity rather than by rupture. This path was shared by various branches of physics that experienced only minor changes as a result of the upheaval in the fundamental laws of nature. Perhaps (but this is a speculation that lies beyond the scope of this research) the technological application of these branches was more significant to their development in the twentieth century than the revolutions in physics.

Piezoelectricity was mundane physics. Yet, it is only one example of mundane physics, not necessarily a representative one. One cannot derive general laws about the development of mundane science from it. This is far from my aim. This history of the beginnings of piezoelectricity is a "case study" in the sense that it is a detailed study of scientific work. It is not, however, a sample of general behavior like the way a quartz crystal is studied in a piezoelectric experiment. The development of piezoelectricity was unique, resulting from the specific combination of individuals, scientific questions, knowledge, and working conditions that were involved. From this unique development one can still cautiously draw historical lessons. Some features of this history are typical of similar cases or have parallels in other developments. Still, they certainly do not form a historical law. The first and foremost interest of the book is the particular history of the emergence of piezoelectricity *per se*. It is particular both because it involved one instead of another development, and because this development was shaped by a combination of causes that has not and could not have been repeated. The history of science, like any other branch of history and unlike natural science, is about particulars. Yet when dealing with local history, as in this study, historians usually aim beyond the particular examples discussed. From practices, methods, approaches, views, and devices employed in one local case, historians explore beyond its proximate. This is also my aim here.[1]

Beyond the interest in the study of mundane "gross matter" phenomena, there are good specific reasons to draw historians and philosophers of science to the early history of piezoelectricity. Though almost unknown outside the professional community, piezoelectric devices are today ubiquitous. Virtually everyone in the West

---

[1] On the tension between the local and the general in the history of science see Peter Galison, *Image and Logic: A Material Culture of Microphysics*, Chicago: The University of Chicago Press, 1997, pp. 59–63.

possesses at least one device based on piezoelectric technology. Most of us carry at least one piezoelectric device a few millimetres from the skin. I refer, of course, to the wristwatch. All quartz watches and clocks are based on piezoelectricity. The piezoelectric resonator is the basis for most electronic time keepers and regulators. Thus, most electronic devices contain such a resonator, which utilizes the two basic effects of piezoelectricity: the induction of electricity by changes of pressure and the converse induction of strain by changes in the electric field in crystals. Yet, time keeping is but one application of the phenomenon, and its scientific study continues unabated: about 1400 papers dealing with some aspect of the phenomena were published in the year 2004.[2] Transducers, sensors, actuators, pumps, motors, and "smart structures" are only some of the central devices that employ the piezoelectric effect. Electric communication, medical diagnostics, computers, industrial sensors, and microelectromechanical (MEMS) devices are a few examples for the application of the piezoelectric effect.

The scientific significance of piezoelectricity is not limited to technological applications. Discovered in 1880 and thoroughly studied in the following decade and a half, the phenomenon is an early example of complex matter physics that went beyond elasticity and optics. Piezoelectricity is a phenomenon of crystals, i.e., arranged complex matter. It does not appear in simple or randomly arranged materials; its properties are dependent on the structure of the crystal. Piezoelectricity is a reciprocal phenomenon of energy conversion from one kind (elastic) to another (electric). By relating elasticity and electricity in complex matter, it had interesting bearings on their appearance, nature, and the relationship between them in crystals and in general.

Piezoelectricity was discovered by two young physicists—Jacques and Pierre Curie. They detected the so-called direct effect: the induction of electric polarization by variation of pressure. It immediately became a subject of research by its discoverers and soon by others. The converse effect, i.e., the creation of strain by electric field, however, was discovered only a year later following a theoretical prediction. In the first 15 years of research the basic properties of the phenomenon were observed and a theory that embraced these properties was successfully formulated, elaborated, and refined. By 1895, piezoelectric research attained a firm body of both empirical and theoretical knowledge. The theory of 1895 is still the basis of current piezoelectric theory. It explains the mechanism of devices like the piezoelectric resonator. Yet, at the time, no one predicted future practical application of the phenomenon. Only 20 years later during the First World War, did the phenomenon begin to be exploited outside the laboratory, in a search for a device to detect German submarines. The sonar, the direct product of this research, was put into use only after the war had ended.

Experiments were the focus of piezoelectric research in the first years after its discovery. Physicists studied various properties of the new phenomena, like the relations between stress, direction, and the resultant electric effect in several crystals. A way to understand the effect was suggested in 1881 by the Curies, which, however,

---

[2] Science Citation Index Expanded^{TM} cites 1960 papers that mention piezoelectricity in their abstract. Random sample shows that about 30% of them only use piezoelectric instruments and do not study the phenomenon itself.

did not provide a detailed account of the phenomena. Yet, in 1889 Röntgen revealed experimental results that disagreed with that explanation. In the following year, Woldemar Voigt introduced a mathematical theory that accounted for all the experimental data and predicted further phenomena. By its applicability to all crystals under any stress, Voigt's was a general theory. Its formulation divides the early history of piezoelectricity into two successive phases, which I call a pretheoretical and a theoretical phase. The term "pretheoretical" designates a field that is not accounted for by a comprehensive theory, i.e., a theory that describes most observable behaviors in the field, while "theoretical" designates a field that is accounted for by such a theory. This is not to say that the theory had no role in the pretheoretical phase. Even during that phase, theoretical thinking and speculations had a significant role.

A theory designates quite different things in different contexts and by different authors. One is to refer to the part of science that is not empirical, i.e., which is beyond the relations observed in the laboratory. In another, more restricted sense, a theory is a set of laws or assumptions that describes a scientific field or its part in a way that accounts for central phenomena in the field, whether qualitatively or quantitatively. In this work I use the noun theory in the more restricted sense, while I employ its adjective and adverb in a more general way in reference to ideas that are not completely rooted in the experiment. Voigt's was not only a theory in the restricted sense but a rigorous elaborated mathematical one. Rigorous by the fact that its several conclusions derived from a few assumptions, elaborated as central consequences of it that had empirical significance were spelt out, and mathematical in its formulation and quantitative predictions.[3] Since it was also general, it directed both the theoretical and the empirical research in the theoretical phase, no such theoretical idea had a similar role in the pretheoretical phase. Experimentation took the lead in that phase. A combination of experiment and speculations, models, partial explanations, and theoretical derivation from other branches characterized the pretheoretical phase. It seems typical of young fields. A theoretical phase is closely related to a mathematical formulation that enables extensive predictions from a limited number of assumptions. Thus, it seems to characterize the more quantified branches of physical science since the end of the eighteenth century. A pretheoretical phase, on the other hand, does not require any rigorous theory and therefore, mathematics and quantitative rules.

Mathematization and the comprehensiveness of the theory made almost any further research, either experimental or theoretical, related to the general theory. As shown below the division into two phases appears in almost every aspect: the type of experiments performed, the reasons for their performance, the theoretical speculations suggested, the relation between theory and experiment, the scientists who contributed to the field, and, of course, the elaboration of the mathematical theory itself. The new phase also meant a renewed interest in the field as a glance at the annual number of contributions to the field (Figure 1) shows.[4] Differences between the two phases in

---

[3] In the following I use these terms. In particular I use elaborated theory to designate one that includes relations derived from its basic assumptions especially with reference to applications in different conditions.

[4] One should not over-interpret the particulars of the graph; it is mainly intended to demonstrate general tendencies in the interest in the field. The graph is based on table A.2.

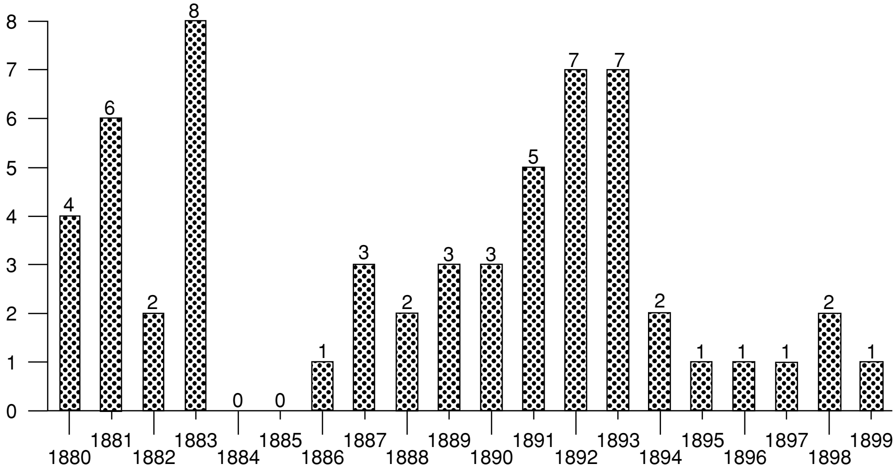

Figure 1: Annual number of publications on piezoelectricity: 1880–1899.

piezoelectricity might be more conspicuous than in other fields, but I presume that the division characterized a process of maturation of many mathematical physical branches throughout the last two centuries.

The turn from a study dominated by experimental research to one directed by comprehensive mathematical theory might be a characteristic of the growth of new non-revolutionary physical fields in physics after the middle of the nineteenth century. The history of pyroelectricity, a related phenomenon discovered at the middle of the eighteenth century, suggests that earlier, experiments and sporadic hypotheses could dominate a field for a long period. Pyroelectricity gained a mathematical theory only in the account given by Voigt to piezoelectricity.

The early history of piezoelectricity until the consolidation of the field circa 1895 raises intriguing historical questions, which I examine in this study. An interesting question about almost any new field concerns its emergence. How and why the phenomenon was discovered and how its knowledge developed from that point? Although scientific discovery has been extensively discussed by historians and philosophers of science, the discovery of piezoelectricity seems to defy common classification. It was not an accidental discovery; the Curies had looked for the phenomenon. Yet, it was neither an empirical confirmation of an established theory, nor a result of any ‘crisis state;’ it followed neither the use of a new instrument nor experimental method.[5] Nevertheless, contingency, theoretical derivations and speculations, and experimentation all played their part in the discovery. The story of this discovery demonstrates the tension between logical and physical necessity on the one hand and contingency on

---

[5] On categories of discovery see for example, Thomas S. Kuhn, “The Function of Measurement in Modern Physical Science,” in *The Essential Tension*, Chicago: The university of Chicago Press, 1977, pp. 178–224, on p. 204. Kuhn also mentions discovery of “quantitative specification of what is qualitatively already known,” which is clearly irrelevant to the qualitative discovery of piezoelectricity.

the other, a tension that characterizes much of the history of science. This tension continued to be manifested in the subsequent research of the phenomenon, especially in the pretheoretical phase when contingent causes and traditions independent of theory shaped much of its experimental study.

Piezoelectricity involved several scientific subdisciplines: elasticity, electromagnetism, physics of crystals, and crystallography. Various approaches and ideas common either to the physics of the time (e.g., thermodynamics), or to a particular subdiscipline (e.g., considerations of symmetry current in crystallography and the physics of crystals) shaped the research in the field. This influence is apparent from the discovery of the phenomenon, which was based on knowledge, attitudes, experimental procedures, and theories from these fields. It turns out, therefore, that the discovery of piezoelectricity was a product of familiarity with more than one subdiscipline. Acquaintance with a few subdisciplines might also have been a key for other discoveries. Various theories and subdisciplines continued to be relevant for the study of the new phenomena. Even after the field had obtained a systematic theory of its own, it was not isolated but had interesting interactions with various connected theories (e.g., electromagnetism) and approaches (e.g., thermodynamics). This history examines the relations and interactions between the various approaches and theories relevant to the study of piezoelectricity. These include the central approaches of contemporary physics. Thereby, it throws light on the central issues of contemporary physics and suggests a picture of physics at the end of the nineteenth century. In particular, it displays the use and application of the new thermodynamic concepts and formulations.

Among the relations discussed here, the relationship between the mechanistic-molecular approach on the one hand and the thermodynamic-phenomenological on the other is especially interesting. According to the molecular approach, phenomena should be explained in terms of molecules, atoms, and their interactions. On the other hand, according to Woldemar Voigt, who developed the phenomenological theory of piezoelectricity, in such a theory, "a small number of principles, i.e., rules derived from experience and ascribed hypothetical general validity, support an edifice of mathematical conclusions that yields the laws of the phenomena in the field concerned."[6] Phenomenological theories aimed at describing the phenomena and their relations as found empirically by using a minimal number of laws. They did not aim at explaining the relations between the phenomena on the basis of another effect or process (hence regarded as more basic) but only at describing them. Molecular theories or continuum models like the vortex atom, in contrast, aimed precisely at such an explanation by invoking various hypotheses that could not be derived from empirical knowledge. The phenomenological approach accepted only hypotheses grounded on empirical results, either of the particular phenomena examined or of a larger field.[7]

---

[6] Woldemar Voigt, "Phänomenologische und atomistische Betrachtungsweise," E. Warburg, ed., *Die Kultur der Gegenwart*, dritter Teil, dritte Abteilung erster band - *Physik* (Berlin, 1915), 714–731, on 716.

[7] In adopting the term phenomenological theories from the physicists of the epoch I do not apply the term as it is sometimes used especially by philosophers. Thus a few clarifications might be helpful. First, the phenomena accepted by this approach were the results of experiments rather than the readings of indicators in the laboratory, e.g. they described a relation between pressure and charge, voltage or

The phenomenological approach did not require direct reduction to magnitudes and entities observable in the laboratory. In the later part of the nineteenth century, the laws of elasticity and the basic laws of electrostatics (i.e., Coulomb's law in its various forms and the relationships between electric charge, voltage, force, and moment) were conceived as expressing true verified relations. Therefore, physicists employed these laws in elaborating phenomenological theories for particular branches like piezoelectricity. Similarly, they used entities like polarization that are not directly measured.

Piezoelectricity was first explained by a molecular model, but in 1890 it was superseded by Voigt's phenomenological theory, which was, like virtually all of its kind, expressed with continuous differential equations. Subsequently, it guided most research in the field, but physicists continued to propose molecular models and preferred it to molecular suggestions. This development from an explanatory to a descriptive theory seems to contradict the logical order from a description of phenomena to their explanation.[8] Moreover, the transition from molecular to continuum theory runs against the current of the time toward corpuscular theories in physics. The two most famous examples are the kinetic theory of gases and statistical mechanics for the science of heat, and the advent of the "ions" followed by the electron in electromagnetism. Contemporary developments in spectral analysis, the theory of anomalous dispersion, electrolysis and discharge are less known examples of the same current.[9] The success of corpuscular theories was only partial, however. Even the famous achievement of reducing heat to motion encountered difficulties, most famously in attempted explanations of the second law of thermodynamics.

Nonetheless, contemporaries regarded the theories of discrete matter as successful. At the end of the century most physicists believed in a molecular-atomistic structure of matter, even though they interpreted it in different ways. The critical response to the opposition well demonstrates the view of the majority. In 1895, the German Scientific Society invited Georg Helm to present his criticism of the mechanical-atomistic view at its annual meeting. "The meeting was an unmitigated disaster" for the opposition. Eminent scientists attacked the speaker. Most physicists in the audience conceived the assumption of atoms or molecules as indispensable. Shortly after, Wilhelm Ostwald,

---

polarization rather than between a mass and a declination of a needle; these indicators were translated into experimental results by theories that were by then already well accepted. (In that the use of 'phenomenological laws' here agrees with that of Nancy Cartwright. However, unlike Cartwright, here the term phenomenological is not restricted to laws but is employed also for theories: Nancy Cartwright, *How the laws of physics lie*, Oxford: Oxford University press 1983, 1–3). Second, in my use of the term "Phenomenological theories" I do not confine the theory to empirical regularities. Phenomenological theories can be based on various assumptions and principles that are not derived from the empirical data of the **specific** theory, like the principle of energy conservation and considerations of symmetry. Both were assumed in piezoelectric phenomenological theory. Terms like "macroscopic" or even "continuum" theory would not do, since they do not exclude assumptions about hidden entities or mechanisms (notice that phenomenological theory does not exclude the use of analytical concepts like energy or entropy).

[8] Yet historically the later theory described phenomena unaccounted for by the earlier theory.

[9] John L. Heilbron, *A History of the Problem of Atomic Structure from the Discovery of the Electron to the Beginning of Quantum Mechanics*, PhD. Dissertation, University of California, Berkeley, 1964, pp. 16–24, Olivier Darrigol, *Electrodynamics from Ampère to Einstein*, Oxford: Oxford University Press, 2000, pp. 265–294.

the leading German antagonist to atomistic-mechanical explanation, regrettably observed: "Repeatedly one hears and reads that no other understanding of the physical world is possible except that based on the "mechanics of atoms"; matter and motion seem the final concepts to which the manifold of natural phenomena must be reduced." Even if Ostwald exaggerated the commitment of physicists to atomistic theories, and neglected the will of most to admit additional concepts like force and tension to the two mentioned, he was correct in pointing out the general preference of atomistic-molecular theory.[10] Still, some physicists preferred to base their accounts on overall principles, occasionally on those of thermodynamics, while others did not regard mechanical concepts as final. A few physicists, especially in Britain, favored reduction of atoms and molecules to singularities in a continuous medium.[11] Another small group considered an electromagnetic reduction of physics; yet, that did not deny the centrality of corpuscles.[12] Nevertheless, as the response to Helm's address displays, the growing explanatory power of corpuscular theories was generally recognized. Despite the general tendency toward molecular theories, piezoelectricity was accounted for by a continuum theory. Even after virtually all had accepted the atomistic assumption toward the end of the first decade of the twentieth century, no satisfactory molecular theory was suggested for piezoelectricity. Voigt's phenomenological theory still prevailed.

This book studies the reasons for the peculiar development of piezoelectric theory. Why did the phenomenological become the dominant theory of the field? Why was none of the molecular models proposed after the introduction of Voigt's theory accepted? On the other hand, why did physicists continue to propose molecular explanations? Many protagonists did not view these different approaches as contradictory. Furthermore, most physicists did not follow a rigorous philosophical system. Their ideas on science rarely follow a logical derivation from basic principles; they are better characterized as *Weltanschauung* or world-view (i.e., a collections of positions not necessarily systematic) than as philosophy. These views of science were shaped primarily by scientific education and experience of the scientists rather than by elaborated philosophies of science.[13] The relinquishing of the molecular models of piezoelectricity did not originate in a rejection of molecularism, nor were the later attempts at a molecular theory derived from a realist rejection of continuum

---

[10] Heilbron, *Problem of Atomic Structure*, 16–24, 41–43, quotations on p. 42. Ostwald himself also mentioned interactions between atoms. Later evidence convinced Ostwald by 1909 that "we have experimental proof for the discrete or grainy nature of matter" (*ibid.*, p. 44).

[11] William Thomson and Joseph Larmor are two representatives of this approach.

[12] The influence of and commitment to the "electromagnetic view of nature" was more limited than that occasionally attributed to it by the secondary literature. Hardly a handful of physicists was committed to the view. See Shaul Katzir: "On 'the Electromagnetic World-View': a comment on an article by Suman Seth," *HSPS* 36 (2005) 189–92. On that view see Russel McCormmach, "H. A. Lorentz and the electromagnetic view of nature," *Isis* 61 (1970), 459–97.

[13] For example, "Einstein could learn from [the textbook of] Drude [a student of Voigt] the principle of the economy of thought and the critical attitude toward mechanism. Drude's phenomenology excluded any picture of ether process." Darrigol, *Electrodynamics*, p. 373. On physicists loose employment of philosophical doctrines see for example Mara Beller, *Quantum Dialogue: The Making of a Revolution*, Chicago: The University of Chicago Press, 1999, pp. 3–5.

theory. A close look at the particular developments in the field is required to answer the questions posed above. Attempts to explain the appearance of piezoelectricity by molecular models reveals tension between basic hypotheses about the building blocks of nature and more complex phenomena. This is an early manifestation of the problem of reducing physics to the emerging atomic and later subatomic physics.

Experiments played various and changing roles in the early history of piezoelectricity. Its history provides a good opportunity to observe these functions of experiments, their varying uses and designs, and their changing relation with theory and assumptions in a pretheoretical and a theoretical phase of research. Although this is not a study of experimental culture, a few experiments that were important to the subsequent history of the subject are closely examined. In the pretheoretical phase most experiments were qualitative. Following the introduction of a quantitative theory, exact measurements became central. German physicists distinguished between these two laboratory activities, which they called "measuring physics" and "experimental physics", respectively. "Measurements" were carried out to obtain precise quantitative data, while "experiments" did not necessarily involve quantitative information. Qualitative or approximate quantitative results were usually sufficient for the latter kind of laboratory activities, which were still the majority. Exactitude was needed in "measuring physics," which aimed not only at accurate results but also at exact values that were required for the determination of constants of nature. Since precise quantitative values were often based on mathematical theory, exact measurements were usually carried out by theoretical or mathematical physicists in Germany, while "experimental physics" was dominated by experimental physicists.[14] Both kinds of laboratory activities played significant, though somewhat different, roles in the history of piezoelectricity. The choice between these methods reflected both the successive stages of study and the personal tendency of the experimentalist.

During the nineteenth century, exact numerical values and precise results became increasingly important in physics. By the second half of the century, exact quantitative measurement using precision instruments had become a distinctive and essential practice of physics. Later, they gained even more importance.[15] This makes exact measurements and their development an important subject for historical inquiry. One issue discussed here is the role of the exact measurements of piezoelectricity.[16] One aim of "measuring physics" was the determination of constants, and such constants were often considered as means to higher ends. Another almost obvious goal was the confirmation of mathematical theory that could not be tested qualitatively. Yet, as I show below, these were not the only roles of quantitative experiments. Moreover, historical

---

[14] Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein*, Volume 2, Chicago: The university of Chicago Press, 1986, p. 120.

[15] Thomas S. Kuhn, "Mathematical versus Experimental Traditions in the Development of Physical Science," in *The Essential Tension,* pp. 31–65.

[16] On the roles of experiment in science see for example Allan Franklin, *The Neglect of Experiment*, Cambridge: Cambridge University Press, 1986; *id.*, *Experiment, Right or Wrong*, Cambridge: Cambridge University Press, 1990; *id.,* 'The Roles of Experiment,' *Physics in Perspective* 1 (1999), 35–53. D. C, Gooding, T. Pinch, and S. Schaffer (eds), *The Uses of experiment: studies in the natural sciences*, Cambridge: Cambridge University Press, 1986 and M. Heidelberger and F. Steinle (eds), *Experimental Essays - Versuche zum Experiment*, Baden-Baden: Nomos, 1998.

understanding of exact measurement at the end of the nineteenth century requires more than a recognition of their roles. The practice of such measurements and their relations to other experiments, measurements, and theories should also be studied.

At least since the period discussed here the ability of experiment to test theories was doubted. Experiments, the argument went, are theory laden, and thus cannot be used to test particular theoretical claims.[17] Indeed experiments involve theory, but as the discussion below shows, theory is only one component of their complexity. Seemingly paradoxical, the recognition of this complexity leads to the conclusion that experiments can test some theoretical claims. In the following (also see Chapter 5) I show that experiments as well as their interpretation depended on material apparatus, various levels of theory and experimental analysis, contingent circumstances, and at least in one case on previous experimental results. Earlier, empirical results determined the evolution of an experiment and its conclusion.

"Measuring physics" was a German concept, and indeed Germans were most prominent in the quest for exact numerical results at the time. Nevertheless, even in a field dominated by Germans, like piezoelectricity, precise measurements and determinations were also carried out by French scientists. The French and German methods, however, were very different, suggesting the existence of disparate traditions in their approach to precise experiments and their physical and mathematical analysis. Their alternative methods of determining piezoelectric constants demonstrate the differences between two experimental traditions. National differences can also be seen in other realms, for example, in the type and nature of explanations suggested by physicists from different countries. The history of piezoelectricity supplies intriguing and partly contradictory evidence for the perplexing character of "national styles" at the end of the nineteenth century. Even the case of the exact measurements shows that nationality was not the decisive factor in determining scientific approach. Affiliation with a particular school or tradition was more important. German experimentalists shared the attitude of their colleagues across the Rhine rather than of their compatriot theoreticians. Still, German experimentalists also adopted techniques and attitudes of German measuring physics, in particular its use of mathematical analysis to reduce experimental error. This step displays a combination of different experimental traditions, and point out an important stage in the mathematization of experimental physics.

Despite all these, and other interesting historical questions associated with the early history of piezoelectricity, it lacks a historical discussion. For more than 120 years since its discovery, the field has received, at best, only cursory expositions in texts on physics and in discussions on certain famous scientists who contributed to it (e.g., Pierre Curie, Pierre Duhem, Röntgen, and William Thomson).[18] The developments in the study of piezoelectricity were not examined. As a field of static electricity it was not treated by scholars of late-19th century electrodynamics and is not mentioned

---

[17] Pierre Duhem, *The Aim and Structure of Physical Theory*, tran. Philip P. Wiener, Princeton: Princeton University Press, 1954.

[18] Among these most detailed are the two biographies of Pierre Curie: Loïc Barbo, *Pierre Curie 1859–1906: Le rêve scientifique*, Paris: Belin, 1999 and Anna Hurwic, *Pierre Curie*, Paris: Flammarion, 1995.

in general histories of the subject like those of Whittaker and Darrigol.[19] Even the basic historical plot concerning the contributors to the field, their central contributions and the dates thereof have not been set down. The elementary aim of this work is to fill this gap and relate the story of the development of the field with its changes and turning points, in its historical context. Although science has a very wide and rich history, many studies are concentrated on a small number of (indeed important) developments. By merely relating the history of a field that has not been studied, this book suggests novel historical evidence relevant to known issues in the history and philosophy of science. Yet, as stated above, my goal here is in a sense more concrete and wider. I wish to explore beyond the local history of piezoelectricity to practices and characteristics of late-nineteenth century physics.

A historian needs documents to reconstruct the history of piezoelectricity on the issues raised above. In this case, the principal and almost only source are original scientific publications. Unfortunately, I could not find relevant archival documents such as correspondence, notebooks or drafts. This is probably connected to the peripheral character of the subject,[20] to the period and to personal habits of work. Notwithstanding the possible gains from archival sources, published contributions to physics from the end of the nineteenth century contain highly valuable historical information beyond the scientific content of theories and experiments. Physics papers at that period varied in length and style: from short two to four pages notes in the *Comptes rendus* of the French Academy of Science to papers stretching anything between five and 60 pages in the leading scientific journals,[21] to a couple of hundreds of pages in proceedings of scientific societies. Research papers were long enough to include "extra-scientific" details, like stated motives for the work and its short history. Moreover, they were mostly written in the first person, and allowed a tone personal enough to mention such issues. "Extra scientific" issues usually appear in the introduction or conclusions of the papers, or in footnotes. The latter are an important source for the historian. Reasons to do particular research or take a particular approach are not always spelt out, but are often implied in the scientific publications. Except for Röntgen's 1889 experiment, the central reasons for carrying out the major steps in the history discussed can be inferred from published sources. These sources also include secondary documents like recollections and obituaries by colleagues and students that fill in a few more details. However, most of the information comes from published scientific papers. Of course, such public sources that reports on most events in retrospect (e.g., descriptions of experiment are usually, but not always, written after they were done) do not supply all the relevant information. Still, they reveal quite a lot including motivations of the participants, changes in their research, and even failures and mistakes.

---

[19] Edmund Whittaker, *A History of the Theories of Aether and Electricity*, New York: Humanities Press, 1973, Vol. 1 (originally published in 1953), Olivier Darrigol, *Electrodynamics from Ampère to Einstein*, Oxford: Oxford University Press, 2000.

[20] Nothing even slightly resembling the effort of "Archive for the History of Quantum Physics" has been undertaken for piezoelectricity.

[21] These are journals like the German *Annalen der Physik und Chemie*, the British *Philosophical Magazine* and the French *Journal de physique.*

THE STRUCTURE OF THE BOOK

The structure of this book follows the division of the early history of piezoelectricity into two phases. The first part (Chapters 1–2) relates the discovery and the following developments in the field to the introduction of Voigt's theory in 1890, developments that led to its formulation. These chapters discuss theoretical and experimental developments together, as they were closely interrelated during the pretheoretical phase. The second part (Chapters 3–5) deals with the history of the field in the theoretical phase. This part is thematically divided into three chronologically parallel chapters that discuss explanatory models of the phenomena, the elaboration of the general theory, and experimentation. In this story the reader is presented with a complex and unfamiliar history of a chapter of physics that is not widely known. To obtain an overview, the reader might like to consult Appendix 4, which offers a few tables of events set out by categories.

The first chapter traces the origin of the discovery of piezoelectricity and its background in the study of pyroelectricity, which had been known since the eighteenth century. In Appendix 1, I show that other earlier observations that were later linked to piezoelectricity had neither significant historical nor scientific links to the later discovery. After suggesting what led to the discovery, the chapter continues to discuss the research and findings in the field up until 1883. These include the discovery of the converse effect. At that time the first wave of research ended, central properties of the phenomena were discovered and its basic interpretation suggested. Chapter 2 traces the development of mathematical description of the phenomena from 1887, when Czermak suggested a quantitative account of piezoelectricity in quartz. This attempt, however, was not only partial but also inaccurate. The road to the more elaborate and valid mathematical theory of Voigt passed through Röntgen's qualitative experiment of 1889. Both are discussed in this chapter, which explains the reasons for the adoption of the phenomenological theory and rejection of the molecular explanation.

Attempts to explain piezoelectricity continued after the introduction of the general theory. Chapter 3 discusses such theories including some suggested before 1890. It answers what led to their construction, what were their assumptions, how well they accounted for the observations and why none of them was adopted. Chapter 4 traces the elaboration of the mathematical theory during the early 1890s. It focuses on the thermodynamic formulation of the theory and its significance. Relations to other theories like Maxwell's electromagnetism are also discussed. The last chapter examines the experimental work in the 1890s. The discussion of exact measurements, which dominated that research, is the focus of this chapter. It ends with an examination of later connected measurements of J. Curie and Röntgen that shed more light on methods of exactitude under question. The conclusions discussed the meaning and implications of the pretheoretical and theoretical phases and the shift from the one to the other, after they were separately examined in the previous chapters. The relations between the research done in different countries—the issue of national style—receive an explicit treatment in the conclusions' second section. Lastly, the tension between the molecular and the phenomenological theories is discussed again in light of the findings of the previous three chapters.