7 MAKING REFERENCE

EMPIRICAL CONTEXTS, CHOICES, AND CONSTRAINTS IN THE LITERARY CREATION OF THE COMPTON EFFECT

The problem of reference haunts all studies of scientific language. How does language escape the narrow bounds of the linguistic code to say anything substantive about the natural world? How can language be anything other than an imaginative fiction, having anything to do with anything beyond the internal elaboration of the code? To anyone familiar with philosophy, sociology, linguistics, or literary theory I need hardly catalogue the way in which this issue persists, freighted with the frustration and acrimony of an irresolvable conflict over fundamental beliefs.

For just such reasons, once I became aware of the apparent intractability and acrimoniousness of the issue I tried to avoid it. I thought I could address some practical issues of writing without addressing the fundamental questions of the validity of science or belief in a natural world. However, I found I could not avoid the issue for several reasons. For one, most of the discussion over scientific language seemed driven by one position or the other—in classical rhetorical terms the discussion was epideictic, either to praise science for its truthful language or to blame it for the hubris of claiming a privileged path to knowledge. I found that every claim I made about scientific discourse was interpreted against this issue, even if there was no explicit relation and I had intended no implicit one. Rather than suffer the misunderstandings of imputed positions, it seemed wiser to address the issue full face. More substantively, I also found very early in the game that one could not contemplate any rhetorical system without taking into account the goals and objectives of the people using the system. Users of the scientific linguistic system seemed to believe their language was useful in gaining some control over the natural world, and many of their behaviors as writers and readers seemed constructed out of that belief. So at the very least I had to take the referentiality of science seriously as a communal assumption and as a question for investigation. I had to see how that belief shaped practices and to what extent the linguistic system lived up to its goals.

However, as I engaged the issue not purely as a textual matter, but within the complex matrix of social and individual practice, the issue no longer seemed to be so intractible nor rife with contradictions. My several discussions earlier and later in this book present my overall approach (see especially chapters 1, 2, and 11), but here I will examine how the developed system of scientific communication shapes the purposes, processes, and norms of statement production so as to make empirical experience a topic-, resource-, and constraint-shaping individual behavior. Specifically, the individual is placed within a communicative context that constantly encourages and demands that the individual at many junctures considers how empirical results either can advance the claim-making procedure or call for reconsideration of the claims and representations of phenomena. Through individual behavior and practice, the discourse is brought into increasingly close and precise exchange with the phenomena being examined. Through living people, the symbols of language come into contact with the world.

Language Moves People, People Move in the World

The first step out of the bind of the closed-system of language is to see that language is used by people and has an effect on them. The desire to understand and master that effect motivates the study of language and rhetoric. From the beginning of rhetoric, the ancients had no doubts that language could move people, both in their thoughts and actions. Even logic, as first developed, was not removed from human cognition. In the *Posterior Analytics,* Aristotle presents logic as a way for statement makers to move readers with greater certainty through arguments and for readers to monitor whether texts were moving them by compelling or by less certain means. Logic was a tool to help minds move in directions in accord with reason, rather than a way to enable reason to escape its human dwelling. Similarly, Plato's complaint about rhetoric was not that language moved minds, but that knowledge of rhetoric might be used to move people's minds falsely in service of unconsidered ends *(Gorgias).* That is, the speaker remains ignorant of or false to the true movement of his or her own mind, but rather speaks only to fulfill baser passions. The virtue of dialectic over rhetoric, as Plato argues in the *Phaedrus,* is that those engaged in dialectic find their words in their mutual search for truth, that the words are part of a motion upward rather than downward.

The study of rhetoric through the last twenty-five hundred years has never lost its concern for the connection between mental movement and words, sometimes the mental movement of the rhetor producing the words and more often the mental motion of the audience. Literary studies followed suit, sometimes more concerned with the overflow of the author's feelings into words that would then carry the reader, and sometimes more concerned with the emotive effects which the author could create through manipulation of form. Only in recent history has a consensus developed in the study of language and literatures, that texts could and should be considered independent of the human producers and consumers (to be discussed in chapter 11).

Much of the social sciences maintains this concern for how language moves people and how the movements of people are expressed in language. Sociology, political science, and psychology continue investigations into how groups and individuals are moved by linguistic symbols and even construct their realities out of their symbolic interactions. This concern for the effect of language on humans is shared by both cognitivists and behaviorists, although one group tends to see the human motions associated with language occurring in the mind, while the other is likely to find the motions in visible behavior, as a mother moves to a child crying and a consumer moves to a product embedded in a series of messages. Even Marxist social scientists understand language in relation to the large material forces that move individuals, whether they consider language as an epiphenomenon of superstructure, as a substantive part of the base, or in some more complex dialectic with social activity and structure.

Although many current studies of scientific discourse accept the rift between language and the natural world, they rely on this indwelling of language in humans. Through case studies they have demonstrated that scientific language is designed to move readers and derives from the various forces moving the authors. Yet they do not take the second step to see that mental motions influence behavior that occurs in the physical world. It is this second step, however, upon which the project of empirical science is founded: to create symbolic accounts that will help us understand our daily concourse with the natural world of which we are part. These symbolic accounts can help us order our relations with this natural world, either by control or by reconciliation. And these symbolic accounts are created out of close concourse with that natural world, heightened and refined through the evolving procedures of empirical investigation.

The cases examined in the previous several chapters demonstrate how the institutions and institutional practices of scientific communication have developed in constant relation to empirical experience. Empirical work cannot be separated from the communications system which gives occasion for the work and within which the work will be represented. Scientists do not simply mutely walk into a laboratory, unconscious of concerns in the literature, with no words or thoughts in mind, and do an experiment, unengaged in any symbolic processing. Writing up results and engaging in professional debates cannot be totally separated from earlier events in the laboratory. As we have seen, the institutions of language developed around haggling over experience—the best way to represent it, how the representation can be held accountable to the experience, how experience can be strategically deployed in the debate over claims. Within the institutions of scientific communication, scientists discuss experience, use representations of experience in advancing of their arguments, and constrain their statements on the basis of their own representations of experience and the representations of others.[1] Moreover, and even more essentially, these linguistic representations are created in close relation to actual manipulation of objects to create the experience represented. Within the psychological and sociological manners of the community, these experiences are attended to in the language. (These ideas will be discussed more fully in chapter 11.)

With empirical experience given such a central role in the values, norms, expectations, procedures, and evaluations of the scientific community, a major and compelling way for an individual to pursue his or her interests is to cast claims in as close a relation as possible to empirical experience both as represented in the literature and as generated in new empirical work. An individual does well for him or herself, his or her social network, and for his or her claims, by doing good science; that is, by creating representations of some stability and power when held against the accumulated and future experience of the community.

1. Further accounts of how nature and empirical experience are used as argumentative resources appear in Bruno Latour, *Science in Action*, and in the various essays in Callon, Law, and Rip, eds., *Mapping the Dynamics of Science and Technology*.

The character and quality of reference in scientific language depend on the kind of work individuals do to create that reference empirically and to adjust constantly the representation to increasing experience. The institutions of scientific communication encourage that reference-creating work; embody practices, procedures, and forms for generating such reference-laden representation; and establish an agonistic social field against which these representations are held against the experiences of others. This communal structure encourages the production of scientific results whether the individual scientists are motivated by greed, vanity, commitment to a doctrine, faith in a private experience, or love of the game.

When we step into the middle of twentieth-century physics we see the game already highly elaborated. An individual scientist has structured opportunities, resources, and constraints out of which to construct claims and arguments that will move others within the same system to come to his view of experience. The scientist behaves normatively, creatively, and self-interestedly within a complex system.

This chapter examines how one creator of significant and successful scientific claims, Arthur Holly Compton, held himself and his claims accountable to empirical experience, even though he created his texts within a community, employed the communal language and concepts, and pursued communal and private interests. The investigation here examines how those texts are embedded in situated practices, through which meanings are created and embodied in the symbols. This study will consider how the author responds to the various social and natural difficulties to creating a stable, reliable, socially persuasive claim. Compton's responses will involve less fundamental rhetorical innovation than Newton's (as examined in chapter 4). Newton's improvisations helped invent the institutions of modern scientific communication; Compton is working within an already elaborated and stabilized system. That Compton's behavior may seem familiar and predictable is just the point. The developed system of scientific communication helps scientists to behave like scientists and do good science.

The Case of the Compton Effect

The case to be examined here is of Arthur Holly Compton's announcement of what is now called the Compton effect. In the standard history of early twentieth-century physics, the Compton effect is considered the first empirical verification of the quantum theory, although verification of the quantum theory was not his purpose in designing his experiments or publishing his findings. Under current understanding, the Compton effect occurs when x-radiation is scattered by electrons. The target electron receives a quantum of energy from the incident radiation. In reaction the electron does not recoil as would be predicted by classical physics—in the direction and with the energy imparted by the absorbed energy (as would a billiard ball). Rather the electron recoils in a different direction and emits new radiation (of a lower energy than the incident radiation) in a third direction, so as to conserve momentum and energy. This discovery was announced in a May 1923 paper, "A Quantum Theory of the Scattering of X-rays by Light Elements."

The focus of the first part of this study is on the emergence of this paper out of Compton's reactions to the scientific conversation within the problem area of his work. The situation within the problem area offered Compton constraints and opportunities, out of which he made choices that shaped his contributions and reshaped the communal conversation. Compton's major discovery paper is embedded in and a response to historical forces. Yet the historical situation, the forces, and the response are all shot through with empirical experience.

This first part of the study is based on the primary record of published articles by Compton and his contemporaries and the secondary accounts of historians of this period, most notably Roger Stuewer's comprehensive history *The Compton Effect*.

To follow up the themes of constraint, opportunity, and choice in the greater detail, the second part of the study examines the emergence of a secondary paper by Compton entitled "Measurements of β-Rays Associated with Scattered X-Rays" (see appendix), written in the wake of the major discovery paper. This part of the study shows how Compton's smallest behaviors as a formulator of knowledge are shaped by his commitments as a scientist to empirical experience. The March 1925 secondary paper is chosen for examination because more extensive notes, drafts, and revisions of it are extant in Compton's notebooks than of any other of his articles. No draft material is available for the main discovery article.

Although Compton shared credit for the "Measurements of β-Rays" article with a junior author, Alfred W. Simon, Compton appears to be the actual writer and the shaping intelligence of the paper, while Simon assisted in the laboratory. All notes, draft, and revisions appear in Compton's third notebook in Compton's handwriting. Further, Simon, a graduate student when the paper was published, never pursued similar work except in collaboration with Compton (Cattell and Cattell, 897), while the paper fits closely with the topic and issues of Compton's continuing research. Finally, in the draft of the article, Compton unthinkingly refers to himself as the sole author.

For this part of the study I relied on the photocopy of Compton's notebooks at the Center for the History of Physics in New York; the original is deposited in the library of Washington University in St. Louis. The relevant materials from Compton's notebooks consist of about a dozen pages of notes on works by other authors, twenty-two pages of calculations and design sketches for a polyphase transformer, fourteen pages of analysis of photographic data, and seventeen pages of draft and revisions. Material relating to other work Compton was engaged in is interspersed, such as a draft of exam questions for a course Compton was teaching.

The Structured Situation in Which Arthur Holly Compton Worked

Arthur Holly Compton developed his claims within a situation structured at a number of levels, from the most general historical structuring of the scientific enterprise to the most immediate sequence of events occurring in the laboratory. These levels can be seen as nested within each other, each outer one providing a context for each inner one. Each outer level can, however, be seen as necessitating and depending on the inner levels for its historical realization and furtherance. All the outer contexts—of the scientific enterprise, the structuring of disciplines, the development of problem areas and emergence of specific problems, the shaping of an individual's research program, the arguments arising out of the public presentation of that program, and the designing of specific investigation—all point toward the most local context of the events happening in the laboratory, the designated empirical experience. The spot of time of this defined experience is both the greatest constraint and greatest resource for the scientist sitting down to write a specific paper. The scientific enterprise has been structured so that all the outer contexts keep pointing toward this spot of time for their resolution and fulfillment. The outer contexts are built on the representation, discussion, and accumulation of these spots of time, these spots of experience.

The largest frames for the creation of statements are the macroinstitutions of scientific community and communication, some of which were examined in previous chapters. The next context, of physics developing as a separate discipline with its own institutions and practices, has not been examined here. We will rather begin our account here with an examination of the established problem area within which Compton's work developed.

Constraints and Opportunities of the Problem Area

According to Stuewer's account, Compton's work grew out of the problem area of the nature of x-rays. In the twenty-one years between Roentgen's discovery of x-rays and the start of Compton's investigations, two competing theories developed to account for the properties of x-rays. The first, associated with Thomson and Barkla, described the x-ray as a wave-pulse phenomenon operating according to classical electromagnetic radiation theory. The second, developed slightly later and associated primarily with Bragg, held that x-rays and γ-rays were particles, neutral pairs comprised of α- and β-particles bound together electrically. Despite the publication of Einstein's light quantum hypothesis in 1905, quantum theory seemed to be ignored by those working in the x-ray problem area. Some attempts were made to provide nonquantum explanations of the photoelectric effect, which Einstein had claimed to explain.

This history of the problem area had several clear-cut effects on Compton's publications in the area. First, up until his 1922 review of the literature for the National Research Council, Compton employed arguments only from classical electrodynamics. When Compton finally turns to a quantum explanation, it is only because no other will fit the data. The consequences of this conversion to a quantum explanation for the structure of the argument in the main discovery paper will be discussed in the next section.

Second, the dispute between the adherents of the two nonquantum theories of the nature of x-rays centered around three types of empirical data resulting from x-ray scattering experiments that were anomalous in both theories: a forward-backward asymmetry in the secondary β-ray distribution, a forward-backward asymmetry in the secondary x-ray distribution, and a difference in hardness between the primary and secondary x-rays. The issue remained finding an appropriate theory or improvement on theory to fit these data. The argument of Compton's papers followed this pattern of proposing theory and evaluating data fit; moreover, these three kinds of data remained among Compton's primary data sources through the major discovery paper.

Finally, because the dispute over theories had narrowed to the issues concerning x-ray scattering, Compton tended to frame his problems in terms of explaining scattering incidents rather than identifying the nature of x-rays themselves. Although certain assumptions about x-rays are implicit throughout his work and made more explicit when he converts to quantum explanations, the problem is the scattering data, with the assumptions about x-rays only serving as part of a projected account of the scattering incidents.

The historical development of the problem area constrained Compton's work by providing the intellectual tools of classic electrodynamic theory which Compton necessarily began working with and by focusing attention on identified difficulties in the data; these difficulties provided the issues for discussion. At the same time these constraints provided the opportunities for Compton's work. They provided something to talk about and a way to talk about it—a puzzle and a method. Without the developed work in x-rays and classical electrodynamics there would be neither data difficulties to puzzle over, nor a theory against which the data would appear puzzling. There would be no occasion for a paper solving the puzzle.

Viewed as both constraint and opportunity the situation in the problem area is freighted with empirical experiences and imperatives. Classical electrodynamic theory is a generalization from the accumulated reported experience of phenomena considered relevant to the theory. Although anomalies, unreported phenomena, different selections of relevant phenomena, and alternate representations of the phenomena might exist or be possible, the theory was created to be consistent with certain classes of data and found to be continuingly consistent with ranges of new data. It was a useful generalization for the uses found for it. As a fairly robust theory, it enjoyed a substantial range of uses, generating continuing empirical contact.

Roentgen's empirical experience of unusual phenomena which he attributed to x-rays opened up the whole research area within which Compton worked. Although there were competing accounts of what these x-rays were, all the relevant researchers were able to produce these rays and observe curious phenomena in their laboratories. In particular three classes of data were regularly produced in the laboratory. When these results were first produced they provided challenges to the two popular accounts of x-rays. Thus they became interesting, were produced in a number of laboratories in the hope of understanding them better, and were the topic of professional discussion. For Compton these anomalous events became the precise research concern.

Constraining Choices of the Scientist's Research Program

Within a problem area the individual scientist's developing research program and theoretical commitments help determine the specific problems to be addressed and the kinds of answers sought. In the long range these choices amount to a line of inquiry and a process of scientific development; in the short range these choices determine how a scientific commitment is realized in specific hypotheses, lines of theoretical argument, and designed experiments, all of which may be reported on in resulting papers. In both long and short range the constraints are circumstantial as well as intellectual: where a scientist finds himself, surrounded by what ideas, and with what equipment and funding available for what projects.

Arthur Holly Compton's research program on x-ray scattering began—by Compton's own account and confirmed by Stuewer (96)—with data produced by Barkla which were not consistent with Thomson's classical electrodynamic x-ray scattering theory. In particular the data suggested that the absorption coefficient of the target material was dependent on the wavelength of the incident x-rays. Compton, deeply committed to classical electrodynamics, took on the task of reconciling the data with Thomson's theory. He first proposed alternative structures of the electron that might account for the variation in the absorption coefficient with the change of wavelength of incident radiation. Instead of considering the electron as a point charge, he proposed a large electron of a perfectly flexible shell, such that the radius would be of the order of the incident radiation allowing for diffraction as well as scattering (January 1918). When difficulties appeared with the flexible sphere, he proposed a ring electron, giving the electron magnetic properties (July 1919): this too presented difficulties. The form of his proposed solutions was clearly dictated by his perception of the problem.

Through this early period Compton was at Westinghouse Laboratories, without adequate equipment, working with crude experiments and secondary data. When he received a National Research Council fellowship to the Cavendish Laboratories to work with Bragg, he was able to devote himself to an investigation of the secondary radiation from the scattering (Stuewer, 137). From the intensity of the secondary radiation, he was able to distinguish two kinds of radiation, which he identified as scattered radiation (unchanged in wavelength) and fluorescent radiation (changed in wavelength). This change in wavelength of the fluorescent radiation would, he argued, account for the softening of intensity of the secondary radiation (May 1921). This fluorescence hypothesis became the focus of his attention, even after he left Cavendish in 1920 to take a position at Washington University to be able to pursue his own line of research unconstrained by the concerns of Bragg's laboratory. He did, however, bring back with him a Bragg spectrometer which was to prove crucial in his ensuing work.

What specific consequences for the shape of the major discovery paper, "A Quantum Theory of the Scattering of X-Rays by Light Elements," did this earlier part of his research program have? First, the commitment to classical electrodynamics causes Compton to draw crisply the issue of choosing between classical and quantum theories; his conversion to quantum approach to the problem of scattering becomes the main justificatory task. The paper opens with a review of the problems arising from the classical Thomson theory; the review is detailed and lasts four paragraphs. In the stead of classical theory, he then derives a series of equations on quantum assumptions; he follows with a report of an experiment that provides confirming data. The latter section is in fact called "Experimental Test" and is followed by a short discussion confirming the validity of the quantum hypothesis.

The character of Compton's argument stands out more sharply if we compare it to Debye's paper proposing a similar quantum theory of x-ray scattering.[2] Debye's paper appeared before Compton's, but had been received by *Physikalische Zeitschrift* after Compton's paper had been received by *Physical Review,* so that Compton received priority for the theory. That particular aspect of priority, however, is less consequential now than then, for reasons to be discussed later. What makes the comparison important here is that Debye was not associated with the x-ray problem area, but rather was already deeply involved in the quantum theory and its elaboration. Consequently, the argument of Debye's paper is to present an extension of quantum theory that explains some data anomalous to electrodynamic theory. Rather than presenting the progress and general types of difficulties run into by classical theory, Debye points to specific data anomalies. The derivation of the equations then follows not as a proposed theory to be tested, but as a direct answer to the difficulties. For Debye the quantum theory already stands, and this is only one more demonstration of its power.

Thus the individual scientist's commitments and evolving research program will shape how he will define issues, create an argument, and develop his data, yet within that framework the scientist is committed to

2. P. Debye, "Zerstreuung von Roentgenstrahlen und Quantentheorie," *Physikalische Zeitschrift* 24 (1923): 161–66.

contend with the data derived from empirical experience and uses that data to further the investigation. Compton and Debye consider the same phenomenon from very different theoretical interests and frame different kinds of argument, yet they must contend with similar data that identify the peculiar character of the phenomenon. That they develop theoretically consistent accounts would not of course be necessitated by the phenomenon or the data, but that both kinds of discourse lead to similar conclusions adds persuasive force to both accounts. If the constraints of two robust research traditions meet the constraints of data to produce similar resolutions, the shared account carries the force of that much more scientific experience.

A second constraining effect of Compton's research program on the discovery paper is to be found in the data displayed as central in the paper. His increasing concern with the softening of the secondary radiation directly leads to the prominent role in the discovery paper (and several other related papers) for wavelength shift data on the secondary radiation. Not only do the data of wavelength shift and the data's analysis provide the chief substance of the empirical presentation in these papers, but the theoretical presentations are largely devoted to deriving the equations for calculation of the shift; consequently, the discussions and conclusions are devoted to matching equations and data of wavelength shifts. The comparisons between equations and data are, as well, presented in graph and tabular forms, to provide some of the more striking features of the papers. Key moments in this focusing on the wavelength shift were the move to Bragg's laboratory, returning to St. Louis with the Bragg spectrometer, then switching the use of the spectrometer from selecting the wavelengths of the primary radiation to measuring the distribution of the wavelengths of the secondary radiation.

Finally, we can see in Compton's earlier papers a series of reformulations of the problem with implications for the form of the appropriate solution. As a problem of reconciling data cast in terms of absorption coefficients to classical electrodynamic theory, Compton's early work looked to the structure of the target electron to determine scattering properties. As Compton began to reformulate the problem around secondary radiation, the work turned to the manner in which the secondary radiation was released, leading to hypotheses about Doppler-shifted fluorescence from scattered electrons. The next step was forumulating the problem more tightly in terms of wavelength shifts in the secondary radiation, which is indeed the formulation of the problem in the major discovery paper.

Before the major discovery paper could be written, however, it was necessary for Compton to draw together all the thinking and data on the subject and reformulate the existing theory and perception of the problem. If he was to abandon classical theory and turn to quantum solutions only as a last resort, he needed a comprehensive look at the subject to convince himself that the classical possibilities were exhausted. Two months before the discovery paper was written in December 1922, Compton published a lengthy (fifty-six page) review of the literature on "Secondary Radiations produced by X-rays." Compton undertook this review for the National Research Council as part of a special Committee on X-ray Spectra. Compton's monograph was the third and last part of the report of the committee. It was this institutional situation that gave Compton the opportunity to rethink and reformulate all the material in his problem area.

As Stuewer points out, writing this report helped Compton in four particular ways; each of these four ways affected what appeared on the pages of the discovery paper. First, in reviewing the data of the large electron hypothesis, he began to have serious doubts about the attempt to reconcile electrodynamic theory with scattering data. This was part of the process of cutting himself away from strictly classical explanations. Second, in examining secondary radiation he proposed a recoil electron hypothesis for the first time—that is, in addition to the fluorescent photoelectron, a second free electron results from the interaction through recoiling after scattering radiation. This, of course, is a key element of the theory presented in the discovery paper. Third, Compton presented new data which actually appears to be old data reinterpreted and reexamined to reveal a slight shift of wavelengths of the entire spectra between primary and secondary radiation. Previously he had mistakenly focused his attention on a grosser but less coherent wavelength shift. This subtler shift, noticed here for the first time, was the main phenomenon addressed in the consequent discovery paper. Finally, in a passage that appears to be a last minute addition, Compton offered a quantum explanation of the shift. Yet in the conclusion of the report, which may have been written before this insertion, Compton criticized any quantum explanation and reaffirmed classical electrodynamic theory. We see Compton clearly vacillating between two views; he resolved the vacillation by the clear choice represented by the discovery article (Stuewer, 193–211). Nonetheless, even after the quantum theory article was published, Compton continued to follow a secondary research program exploring x-ray reflection and diffraction, which did seem to follow classical electrodynamic theory. He made one choice for one set of problems and data, and another choice for another set of problems and data. After the moment of confusion he created a bifurcation in his work, with only limited cross-reference between the two parts.

Constraints of the Laboratory

With the onset of work for any particular research project, theoretical or empirical, another process of constraint begins. Up to this point constraints helped define a problem, the starting point of the inquiry, and some formal features of the likely answer—what the field is asking the scientist to do and what the scientist would like to do. Once one gets down to the actual pen and paper work of theory construction or experimental design, however, one becomes constrained by what mathematics, logic, and prior well-established theory allow one to say, by what available equipment can do, and by what data actually turn up. In this wrestling with recalcitrant mathematics, logic, technology, and nature one finds not what one would like to say, but what one can legitimately say. If earlier constraints helped shape the form of the statement, here constraints shape one's substantive theoretical innovations and the content of one's findings.

Of this stage, unfortunately, little remains on the public record; research activities occur in relative privacy, whether tinkering with equipment in the laboratory or tinkering with equations on the back of a cocktail napkin. But imagination and mechanical creativity are not unfettered. In addition to the constraints on the focus of attention and nature of the endeavor, discussed earlier, one runs up against the limited possibilities of mechanical and intellectual manipulations and the limitations of what is out there in nature as revealed by the marks on the photographic plate or the readings on the meter. It is these resistances—called passive by Fleck because they are not under the active control of human culture—that are brought forward into the public record, in the form of data tables, methodological articles, and theoretical derivations, but the process of getting to these hard places of resistance is largely obscured in scientific texts.

The darkness which hides this stage of the emergence of scientific statements has proved intriguing to psychologists, sociologists, and philosophers of science; their inquiries have led to the observation that the process of scientific inquiry is something other than the knowledge reflected in the public record (Medawar). Latour and Woolgar, in observing the private goings on in a biochemical laboratory, have noted how real substances get reduced to symbols through mechanical and intellectual manipulations and how symbolic formulations are tried and abandoned against the criterion of what will gain the most credibility in the agonistically structured (competitive) field. Part of the process of gaining credibility requires that one's results seem not to be tied to the specifics of one's lab work. Thus the final paper gives only a thin, highly transformed, highly selective account of the biological matter investigated. Knorr and Knorr, observing another biochemical laboratory, similarly note that all that survives in the final paper from the complex wanderings of motives, plannings, errors, speculations, and tinkering with machines is a data chart. The rest of the final article seems to be created on other grounds. Both these studies imply that this process of shedding away obscures both nature and scientific activity out of the social motivations and interests of the researchers.

If one remembers, however, that the private activity of the laboratory occurs within a context created by the public record, the eliminations and reductions that occur within the laboratory and between the laboratory and the final text—the shedding away—may be seen as the mechanism by which the specialized, highly focused data of the laboratory is fit into the broader constraints of the developing science. Brute nature is, of course, not constrained by science, but only limited aspects of nature are consequential at any moment in the discourse about nature called science. Just as a fiction writer may select details according to criteria of vividness, thematic consistency, and verisimilitude, the scientific writer must seek out and select data according to such criteria as consequentiality for the problem at hand, form appropriate to the theories in question, lack of contamination by uncontrolled factors, and anticipation of what the rest of the scientific community is likely to consider as compelling proof. That is, brute nature is symbolized and those symbols refined to meet specific purposes of discourse, a discourse that must address the literature, the audience, and the scientist's own thought as well as observed nature.

This author has little evidence about the private events that led to the writing of Compton's major discovery paper, but Compton's notebooks do provide material relating to a follow-up paper, "Measurements of β-Rays Associated with Scattered X-Rays," to be discussed below, along with an extensively revised draft.

Focused Choices at the Writing Desk

By the time the scientist gets to the actual writing up of theoretical and experimental findings, much of what will appear on the page has been determined by earlier constraints and choices. Thus the writing up of results may seem to be a perfunctory necessity, a painful obligation, but not an essential part of scientific discovery; by extension the entire writing process can seem epiphenomenal, rather than essential, to science. However, the analysis here suggests that the text gets shaped over the long haul by the essential elements of science, which in turn can be well understood as parts of the process of scientific formulating. In this larger writing process, specific limited tasks of formulation are left for the overt work of draft writing and revision. In this writing-up stage, the scientist-writer must put the pieces of the argument together so as to make his purposes clear and so as to satisfy the criteria of judgment he anticipates will be imposed by his audience. Final wrestling with the applied theories, the continuity of the argument, and the data may lead to basic reformulations even at this stage. Even if no major changes occur, the author in controlling the words for the final formulation must manage the impression of the prior literature, the experimental design, the laboratory happenings, the data and its relation to the phenomenon investigated, the conclusions, and the conclusions' certainty. The scientist-writer must fine tune the language to reveal the proper levels of precision and uncertainty. Yet the writer must also project a hypothesized world in which his findings are true. That is, even while the literature, research program, problem formulation, experimental design, and data constrain the solution's formulation, all these earlier constraints are presented in the context of a formulation of the world that takes the findings for granted. Thus, for example, a scientist on the basis of a programmatic conviction bolstered by his most recent findings, in reporting those findings may dismiss work based on contrary programmatic convictions as irrelevant and insubstantial. To readers who do not share the author's clarity of vision, however, such a representation of the literature may appear worse than imprecise.[3] Such an example suggests the difficulty of managing a representation that is adequately precise for both author and audience. All this impression management must be done while attending to the stylistic conventions and preferences of the editor and audience. These conventions and preferences allow for convenient, intelligible communication which calls least attention to itself.[4]

Writing-up is not an instantaneous process; preparation of the drafts, revisions, and editorial revisions take some time. In the course of the drafts and revisions the final form of the article comes into shape. Although many of the writing choices happen in the author's head—we know only those sentences he writes down—the changes within the

3. I interpret in such a light the examples of apparent distortion in introductory sections of papers, cited by Gilbert and Mulkay in *Opening Pandora's Box*, chapter 3.

4. Such interest in the audience's convenience is the basis for the research reviewed in Ennis, "The Design and Presentation of Informational Material."

drafts and revisions reveal many of the concerns uppermost in the writer's mind at these later stages.

Rewriting in Reception

After the scientist has chosen the words that appear in the published text, the meaning of that text still must be reconstructed by the readers. The text takes on a revised meaning depending on where and how it becomes incorporated into an evolving science. Small has suggested that texts come to serve as specific concept indicators in later articles, and Cozzens has found evidence that with time references to an important article tend to become more compact and fixed in meaning ("Life History"). Messeri has likewise found that citations to seminal articles are replaced by a few key terms that come to represent the findings of those articles. This reduction and transformation of the meaning of an article depends on what happens to science after the article is published, so that the article may be seen to have a rather different set of foci and implications than intended by the original writer.

Stuewer's account (237–73) of the reception of Compton's article "A Quantum Theory of the Scattering of X-Rays by Light Elements" and a limited survey of the citation contexts of later references to that article reveal several striking features of the transformation of Compton's findings. At first the article became an object of controversy, attacked on both theoretical and empirical grounds; at the same time other scientists attempted to improve on Compton's theory. Compton in response ran further experiments and proposed his own improvements. The article gradually became accepted as fact and was cited as the basis for new work. Within a few years the article (along with several surrounding publications) came to have a limited meaning referring to empirical observation of what was coming to be called the Compton Effect. As acceptance and eponymity were granted, the discovery became retold in less specialized, less argumentative ways in order to inform wider publics about the newly accepted fact and to place the new fact in relation to other facts. Compton himself participated in this process by his speech before the American Association for the Advancement of Science in 1923, his 1925 article in *Scientific American*, his 1926 text *X-Rays and Electrons*, and his 1927 Nobel lecture.

Two particular reinterpretations are involved in the current view of the Compton Effect as an empirical verification of quantum theory. First, Compton's work is now seen as part of the research program of quantum theory, even though the article does not cite any of the prior work in quantum theory and even though Compton came to his discovery out of problems in classical electrodynamics. And although the major discovery paper offers a quantum solution, the problems of assymetry of radiation and wavelength shift which it addresses are anomalies that arise from an electrodynamic point of view. Second, the current view of Compton's work neglects his theoretical concern in developing an account of x-ray scattering consistent with electrodynamic theory in favor of an empirical result that was originally subordinate to theoretical issues. This interpretive shift began quite early. Compton's article appeared in volume 21 of *Physical Review.* Of the citations that appeared through volume 25 of that journal (a span of two years), excluding self-citations, nine appear in contexts that refer to his theory, and only one is concerned primarily with his empirical results. Of the citations in volumes 26 through 29, however, two are primarily theoretical, three are empirical, and one is mixed.[5] Given the progress of quantum theory during that period and since, and the consequent change of the importance of Compton's work, such a reinterpretation makes sense as part of the historically changing codification of the literature of a scientific field. But such reinterpretations based on current scientific belief in effect rewrite the original article.

One Paper Begets Another: "Theory" Begets "Measurements"

Almost immediately upon publication, Compton's discovery underwent a series of challenges, which Compton answered by carrying out further experiments and publishing the results, disconfirming the challenges and refining the theory. It was in this context of challenge and response, of elaboration and bolstering, that Compton pursued the work that would lead to the "Measurements" paper. The more evidence of the most varied kind he could find, the more likely he would be to gain acceptance of his original discovery claims.

At about the same time as Compton had published "A Quantum Theory of the Scattering of X-Rays by Light Elements" in May 1923, C. T. R. Wilson (and slightly later W. Boethe) identified, in cloud chamber experi-

5. I drew the citations from *A Citation Index for Physics: 1920–1929;* incidentally, Compton's "Quantum Theory" article was the most cited article in physics during the decade.

Theory citations: 22, 283; 23, 122; 23, 135; 23, 316; 24, 179; 24, 591; 25, 314; 25, 444; 25, 723; 26, 435; 28, 875.

Experiment citations: 25, 193; 26, 299; 26, 657; 29, 758.

Mixed citations: 26, 691.

ments on X-ray scattering, secondary β-ray tracks substantially shorter than photo-electron tracks. Compton immediately saw that these shorter tracks could represent the recoil electrons he hypothesized in the quantum theory article. He wrote a letter dated August 4, 1923, to that effect to *Nature,* which published the letter in the issue of September 22, 1923. Although at that time Compton continued to be mostly concerned with data revealing wavelength shifts, which data he kept gathering during the following year, he clearly understood how the cloud chamber findings filled out his work. He assimilated the cloud chamber findings into his consequent papers, often in lengthy discussions indicating how they supported his theory.

Wilson's and Bothe's data, however, only offered a rough correspondence to Compton's theory, as Compton noted: "They have shown that the direction of these rays is right, and that their range is of the proper order of magnitude" ("Measurements" 307). The roughness Compton ascribes to the use of insufficiently hard and too heterogeneous x-rays. The "Measurements" article can thus be seen as Compton's attempt to tie down the connection between his theory and the cloud chamber tracks more firmly and precisely by redoing other people's experiments in a way more appropriate to his programmatic purposes. He would then obtain support for his theory from a kind of data not at all available when the theory was first framed; such data, confirming the predictive power of the theory, is rather persuasive.

In this way we can see the "Measurements" paper motivated and shaped in specific ways by Compton's theoretical program, discoveries by other scientists as reported in the literature, the desire for closer measurement of the phenomenon, and Compton's persuasive intentions. Contextual factors provide pressures and offer opportunities to gather fuller, more precise, and more focused data about the observed radiation—confirming and adding detail to the representation of nature embodied in Compton's theory. His social interest in establishing the proposed phenomenon leads Compton to search actively for passive constraints of new and more precise kinds; criticisms in the literature actively push him again to seek passive constraints that make his formulation more likely; finally, new techniques, actively created (although embodying passive constraints in what they can accomplish and in the results they produce), provide opportunities for closer looks at

6. C. T. R. Wilson, "Investigations on X-Rays and β-rays by the Cloud Method. Part I.—X-Rays," *Proceedings of the Royal Society,* 104 (1923): 1–24; W. Bothe, "Über eine neu Sekundärstrahlung der Röntgenstrahlen," *Zeitschrift für Physik* 16 (1923): 319–20, and 20 (1924): 237–55.

the purported phenomenon, adding new passive constraints to the formulation.

Specifically, this set of forces and opportunities led Compton to design experiments and write a paper reporting those experiments:

1. adopting Wilson's cloud expansion apparatus;
2. referring to and discussing his own quantum theory of scattering;
3. employing higher energy (shorter wavelength) incident radiation than Wilson and Bothe;
4. designing and employing a method of obtaining more homogeneous incident radiation than Wilson and Bothe (consequently reporting data for higher energy, more homogeneous data);
5. developing theoretical predictions about aspects of the recoil electrons measurable through the Wilson apparatus;
6. and discussing the correspondence between the theoretical predictions and the experimental data.

These effects of the rhetorical situation correspond to the major structural features of the resulting paper. Compton, indeed, alludes to these effects when he describes the paper at the end of the opening paragraph:

> The present paper describes stereoscopic photographs of these new rays which we have recently made by Wilson's cloud expansion method. In taking the pictures, sufficiently hard x-rays were used to make possible a more quantitative study of the properties of these rays. (307)

Within the stylized terms of the field, the paper describes constraints imposed by the results of more precise measurements. By showing that Compton's theory is in conformity with ever-increasing passive constraints, the article seeks to establish factlike status for Compton's claims.

Another aspect of the rhetorical context consisted of one particular challenge to Compton's quantum theory of scattering. Bohr, Kramers, and Slater claimed that at the particle level the laws of conservation applied only statistically.[7] Compton's theory required event by event application of the conservation laws; up to that point, however, Compton had established the recoil phenomenon only on an aggregate basis through measurement of radiation wavelengths. Wilson's cloud photographs provided a way of capturing and measuring single incidents and were,

7. N. Bohr, H. A. Kramers, and J. C. Slater, "The Quantum Theory of Radiation," *Philosophical Magazine*, 47 (1924): 785–802.

therefore, the ideal means of refuting Bohr, Kramers, and Slater. The full and explicit refutation of the statistical argument was to be made by Compton and Simon in a subsequent article—"Directed Quanta of Scattered X-rays," which appeared in *Physical Review* six months after the "Measurements" article—but the desire to refute the challenge remains an implicit shaping force on the earlier article. The effect can be seen in the emphasis given in both the abstract and the full paper on conclusions and evidence that the scattering occurs on an event by event basis, with each event maintaining conservation of momentum and energy. This emphasis is in fact increased in revision. Again, attack on the formulation provides pressure to seek and reveal passive constraints, consonant with the original formulation.

Laboratory Decisions, Events, and Results

The effects of the rhetorical situation are first realized in Compton's laboratory decisions before their full implications in the text are realized. The laboratory decisions, such as the use of the Wilson cloud apparatus, designing a more precise control over the incident radiation, the design of the scattering experiment, the choice of which plates to use as data, and the particular measurements taken from those plates, all have an effect on the final article, both in terms of the procedures described and the data reported. The first three decisions are design decisions based on the characteristics of the phenomenon investigated and the properties of the equipment, as both have been revealed through previous investigations. The Wilson apparatus, for example, is used only because it has earlier revealed tracks that Compton can identify with recoil electrons. Compton goes to great lengths to make design decisions that will permit observation with the desired precision; twenty-two pages of his notebooks are devoted to designs for a polyphase transformer that will provide him with stable enough voltage to provide homogeneous incident radiation of calculable energies. The experimenter can choose from among available technologies, but those technologies suffer many passive constraints. The experimenter cannot use impossible machines, nor can he make machines do what they cannot do (Notebook 3, 20–41).[8] The latter two decisions—the choice of

8. Latour and Woolgar, citing Bachelard, discuss laboratory equipment as a reification of theory. This idea is intriguing, but it must be kept in mind that no matter how fully suggested by theory, the equipment must accord with the functioning of nature to work; in this way the equipment is as much a test of theory as reification of theory.

plates and the choice of measurements to take from the plates—depend on what happens in the laboratory, on what turns up on the plates. Once the experimenter sets up the conditions of the experiment, what turns up is beyond his control. Only afterward can the experimenter reassert control through selection and manipulation.

In the final article Compton reports that he is using data from "the best 14 of a series of 30 plates taken," but the notebooks show him making calculations for 14 numbered plates running from number 15 to number 47 (Notebook 3, 49–52).[9] Assuming that plates 1 through 14 served as practice runs, that still leaves three plates unaccounted for, presumably so bad that they do not even count as plates. Although Compton gives no overt definition of what makes the selected plates "best," the sixteen deleted plates worst, and the three not plates at all, his notebooks offer two clues about his criteria of selection. First, he tends to select the higher number plates; in fact he records measurements for plates 38, 39, 40, 43, 45, 46, and 47. This suggests that Compton and Simon were still gaining the technical skill to produce plates that clearly revealed the tracks they were interested in. Second, at the bottom of the column of measurements for plate 38—which in fact was deleted from the article partway through the writing of the draft—the notation "uncertain because too crowded" appears. This notation reinforces the impression that selection was based on how clearly the plates represented and allowed distinctive counts of the data associated with the scattering phenomenon. That is, Compton and Simon were simply looking for clear and distinct tracks.

The tracks on the photographic plates are Compton and Simon's closest glimpse at the scattering phenomenon, and reproductions of some photographic plates are included in the final document to give the readers qualitative visual evidence. How those tracks are interpreted quantitatively, however, depends on a number of manipulations of measurement and calculation. The data tables in Compton's notebook, even in parts of the rough draft of the article, are filled with corrections. These corrections seem all to derive from two incorrect assumptions about the equipment which led to mistaken values for the potential of the x-ray tube and consequently for the energy of the incident radiation. The two causes for error—a warping in a frame and the effect of a condenser—are both carefully noted in the notebook and in the final article. Although on first glance all the corrections appear to be manipulation of the numer-

9. On the bottom right hand corner of page 51 there is a boxed-off set of data that is unlabelled that may represent a fifteenth plate; if so this would compensate for the apparent discrepancy caused by the later deletion of plate 38.

ical data after the fact, they really only serve to adjust the secondary numerical data to the actual event as occurring in the equipment and recorded on the plates. In addition, although Compton for the most part adheres to Knorr's observation that scientists tend not to report their wrong turnings and errors in the final report (Compton, for example, does not discuss what went wrong in the first fourteen parts nor in the later deleted ones), Compton is very careful to cover this error in both notes and text. His great care, and indeed the great detail with which he reveals this error in the article, suggests that this error is of a different order in that it comes after the laboratory event but seems to change the reality of what happened. To retain the integrity of the data, to make clear that he is constrained by the data and not fiddling with it, he must expose the error of calculation and measurement which leaves the reality of machinery and photographic plates untouched. Thus the representation of a certain class of error is necessary in the article to keep the relation between laboratory happenings and the report of those happenings as clean as possible. The purpose of exposing the error is not, as Medawar would like, to reveal the psychology of discovery.

The Writing-Up

The previous sections have examined some of the constraints and decisions that determined what the measurements article would look like, but still we do not have a text. Compton must sit down with blank paper in his notebook and create a string of words, equations, numbers, and graphics to fulfill the possibilities of the constraints. As part of that fulfillment he must represent nature at various levels of mediation: nature as perceived through the literature, as formulated in a problem and hypothesized answer, as inherent in the experimental design and the actual experimental happenings, as represented by the experimental data and the secondary calculations, as interpreted through discussions and conclusions. Thus the article, even while describing the forces that shaped it, is reconstructing views of nature at a number of levels of intellectual and physical mediation. By the convention and logic of the scientific report, however, all these representations must be weighed against the least mediated representation, the data—the photographs and numbers one carries away from the laboratory.

At this point of writing-up, the task of the scientist then becomes using language to create these various representations at a level of precision and completeness that adds no further confusion or lack of clarity at any of the levels and that allows an intelligible comparison between the data and the other more mediated representations. When we look at Compton's draft and revisions of the article "Measurements of β-rays Associated with Scattered X-rays" (Notebook 3, 59–75), we see indications of just this concern for creating an adequately full and precise representation of nature at several levels of mediation. The larger part of the many changes and corrections he makes as he writes and revises manage the representation of the x-ray-electron interaction, the theory of that interaction, the experimental design and happenings, and the kinds of interpretations and conclusions that can be drawn on the basis of the data.

The following discussion of the drafts and revisions will first present the three major tactics of revision that Compton uses—postponing, extending, and fine tuning—and then will examine epistemological, phenomenological, and social issues raised by the draft and revisions. Line numbers refer to the final version, reproduced in the appendix to this chapter.

POSTPONING

Postponing is a structural decision made in the course of writing the draft. Four times Compton starts to raise major subjects, then decides he must first reveal some preliminary information. At the end of the opening paragraph in the draft, after only mentioning the photographs, he is about to present a set of reproductions with the phrase, "A typical series of these photographs is shown in figures . . ." Before completing the sentence, however, he strikes it out in order to insert a paragraph spelling out the cloud chamber, x-ray, and photographic equipment. Then in the third paragraph (line 28) he returns to presenting the reproductions of the plates. In the second case, after qualitatively discussing the photographs, Compton begins to raise a major theoretical issue with a new paragraph beginning, "One of the most important questions is whether . . ." He backs away from his direct assault, however, by striking the incomplete sentence and beginning a different paragraph introducing quantitative theory to be matched against empirical data (39). The quantitative material then continues as the main body of the paper. Although it is unclear what important question Compton has in mind, the discussion of all the major questions follows the quantitative presentation. The third case involves the presentation of the first data table. Some time after copying the first two columns of data Compton realized the errors in the potential and energy figures discussed earlier. He apparently then went back to check his equipment and recalculate his figures. He then corrected the figures in the first two columns and copied in the correct figures for column seven, which is calculated from the first two columns. Then in the draft immediately following the table he added a paragraph explaining the error (47–59). In the final paper, however, the table is postponed until after the explanation of the error. In the last case, Compton splits his first draft of the second table, which included data on both maximum range of R-tracks and the distribution of the ranges of the full set of tracks. The latter part of the original table appears later in the article in a slightly different array as table 3. The effect is to allow complete discussion of the issue of maximum range before raising the issue of relative distributions.

In all four cases the postponement is to allow the presentation of additional detailed information prior to the postponed material. In the first and third cases the additional material explains the equipment that produced the postponed data; in the second and fourth cases the inserted material is data logically prior to the postponed material.

EXTENDING

Extensions, giving more information about some item already under discussion, serve to clarify or make precise the item being discussed. For example, "primary beam" is changed to "primary x-ray beam" (5); "photographs" becomes "stereoscopic photographs"(11); "the x-ray tube, enclosed in a lead box" becomes "the Coolidge x-ray tube, enclosed in a heavy lead box" (19–20); and "$\tau + \sigma$" becomes "$\mu = \tau + \sigma$"(80). In a more extensive example, "To calculate the relative number to be expected, we have arranged this expression over the range of wave-lengths used in our experiments," grows in several steps into "To calculate the relative number of tracks for different relative wave-lengths to be expected, we have arranged this expression by a rough graphical method over the range of wave-lengths used in our experiments" (138–41).

In one case the addition serves to justify a statement. The phrase "in view of the fact that the photographs were stereoscopic" adds a reason to the original phrase which now follows, "it was possible to estimate . . ." (161).

In all the above cases the addition gives detail to the originally mentioned object or event, but in at least three cases the additions redefine the object of concern more precisely. "Track" becomes "length of a given track" (135); "40 tracks" becomes "the directions of 40 tracks" (159); and "short tracks . . . and long tracks" becomes "short tracks (type R) . . .

long tracks (type P)" (41). The last example involves a change in epistemic level (to be discussed below).

FINE TUNING

Word substitutions fine tune the language through more specific, correct, or appropriate phrasing. Compton achieves greater specificity by such changes as "an" to "the" (110), "the" to "its" (103), and "those" to "the quantity S" (125). More substantive specifications are made in such changes as "acquires" becoming "moves forward with" (109).

In some cases Compton is correcting an outright error, as when he miscopies an equation from a previous article (112), or he incorrectly calls an "expression" an "equation" (147). Elsewhere he must correct an inverted ratio (85), report that there was more than one "condenser" by making the word plural (52), and relabel a "scattering quantum" as a "scattered quantum" (151). More frequently the corrections are more subtle, as when measured values are described as "summarized" rather than "Shown on the following table" (117) or when "C. T. R. Wilson's datum" is changed to "C. T. R. Wilson's result" (119). A repeated subtle error needing frequent correction is referring directly to an object instead of the appropriate quality. Compton in the draft consistently refers to *R* and *P* and *R*/*P* when discussing the number of electrons but in the final version the notation is consistently changed to N_r, N_p, and N_r/N_p (42, table 1, 75, 83, 88, 96). Related are the wavering from "apparatus" to "chamber" back to "apparatus" (15), the change from "photoelectric absorption coefficient" to "true absorption coefficient" (43), and the revision of "amplitude" to "magnitude" (185).

The last category of fine tuning revisions corrects tactical errors of exposition and thereby modifies slightly the impression of what is being discussed. Compton first begins to describe the maximum frequency "required to" and then switches to "excited by the voltage" (122); a bit later Compton cites a finding "for the number" but then changes that to a finding "that the probability" (134); and a few lines later Compton starts a sentence, "This expression assumes that the electrons all . . ." then recasts the thought changing the subject of the assumption, "This expression assumes that the exciting primary beam . . ." (137). A more clearly consequential example occurs when Compton begins to discuss "the origin of the short" tracks but then changes the focus to "the origin of the two classes of β-rays" (40). Here he changes the topic from one phenomenon to two phenomena in order to prepare for an equation for the ratio of the two later in the sentence. The original singular focus, although not a factual or technical error, was a tactical error in not providing for the continuity of the exposition; the writer must keep in mind what he will discuss in what order, and he must focus the discussion accordingly.

All three types of revision—postponing, extending, and fine tuning—indicate that the writer is moving through the imprecision and incompleteness of formulations to come to a more focused, accurate representation of what he did, saw, measured, and thought. The language of the original draft is in parts skimpy, fuzzy, misleading, and even wrong, but by struggling with the language the scientist writer can achieve a bit better fit between symbolization and experienced world.

CRITERIA OF ADEQUACY

The symbolic representation of nature is inevitably an approximation in an alien mode; absolute precision and completeness of formulation would be an endless task. Criteria are necessary for a writer to decide whether a linguistic representation is adequate. Compton's draft and revisions offer clues as to his criteria in the instances where he deletes detail or foregoes specificity. Compton seems to follow two criteria: what one can say and what one needs to say—that is, assessments of how finely one knows what one is discussing and of what level of distinction is necessary to carry the particular argument forward.

The rounding off Compton does in table 2 shows how these criteria are applied. In the original data tables in the notebook, the observed maximum ranges are all measured to the first decimal, but in the transfer of the table to the draft and the consequent revision three observed ranges are rounded off to the nearest integer, in accordance with a prior admission that the observed track lengths "could be estimated probably within 10 or 20 percent" (115–16). That is, the decimals give an appearance of greater accuracy than was probable. Two calculated values, as well, are rounded off to the nearest integer. On these calculated values no error range restrictions apply, but since the degree of statistical correspondence being demonstrated is quite broad (as large as $\pm$3mm or 33 percent of the measured value), the decimals are unnecessary for the demonstration. Compton gives no greater statistical precision than he legitimately can or needs to.

Unneeded specificity is deleted in a number of cases, trivial and substantive. In trivial cases the specification has already been achieved elsewhere in the text as in the deletion of "x-ray" in "primary x-ray beam" (18). In more substantive examples the deleted material raises extraneous theory or inappropriately narrows the discussion. The expression V_c/h is eliminated after the phrase "maximum frequency" because the expression is not used in any of the ensuing calculations (121). The phrases "but radiates uniformly in all directions" (110) and "depending on the direction" (116) are similarly deleted for raising unnecessary qualifications. Another deletion, "mean of the experimentally" from the larger phrase comparing "calculated values with the mean of the experimentally observed relative ranges" (143), emphasizes that the data fit is independent of the voltage and therefore is valid for each of the cases individually rather than only in the mean. Thus the force of an entire set of data is strengthened by the removal of an unnecessarily narrowing qualifier.

The most interesting example of deletion occurs in the description of the photographic equipment (25–27). Compton twice tries to include phrases noting that the full aperture of the lens was employed, but he twice deletes this as unnecessary. Then he twice tries to give positive judgments about the quality of the lenses and plates—"which gave excellent defin . . ." and "very satisfactory." He deleted the first completely and removed the "very" from the second so that the text is left with only the comment that the plates "were found satisfactory." This judgment is all that is needed for the exposition of the experiment. Without a scale of excellence, the more effusive judgments, moreover, do not appear legitimately knowable or supportable to Compton; only the word *satisfactory* carries a criterion of adequacy to the task at hand. Compton's obvious technological pride in the laboratory accomplishment of capturing the scattering phenomenon on photograhic plates seems to motivate all four deleted phrases, but he recognizes that such feelings are extraneous to the argument.

Control of Theory, Persona, and Audience

In addition to controlling the more obvious representations of nature, Compton is careful to control the definition of the epistemic level of the discussion, the projection of his persona, and the relationship to the audience. These factors are important to maintain under control, because if improperly treated they could not only obscure the description of nature being proposed, but undermine the purpose of the discourse. By carefully identifying the epistemic level of discussion, Compton is able to identify exactly what he is representing and at what level of mediation. By controlling persona he is able to assert his individual ownership interests, identify where his judgment enters, and limit his intellectual risks, while still keeping attention on what the data and theory suggest. By controlling the relationship to the audience, he serves the reader's convenience, helps the reader follow the argument, and submits himself to the audience's criteria of judgment, again while keeping focus of the article on the formulation and data; his most important task with respect to the audience is to maintain credibility, which is done by remaining responsible to and for the data.

EPISTEMIC LEVEL

As part of the process of adjusting language to necessary and possible levels of precision and completeness, Compton carefully assigns each statement to the appropriate epistemic level. That is, items can be represented at different levels of theoretical and empirical mediation. For example, near the beginning of the draft Compton shows uncertainty whether to discuss *rays* or *tracks*. *Rays* directly represents the purported object in nature, but *tracks* represents a manifestation of those rays as they pass through a cloud chamber to create vapor trails that are recorded on photographic plates. After a few equivocations and changes, Compton decides to discuss *rays* in the introduction and switch to *tracks* only after the photographic data are introduced. Thereafter the track terminology dominates the rest of the article. Thus Compton indicates that although rays are the object of interest, recorded tracks are all he has to observe and work with.

Even in the discussion of the purported object of nature there is recognition that the discussion is really about objects constructed in the literature. The opening sentence of the published article reads "In recently published papers, C. T. R. Wilson and W. Bothe have shown the existence of a new type of β-ray excited by hard x-rays." The word *new* is added in the draft, so its use is clearly a conscious choice. The word *new,* however, is only appropriate as meaning new in the literature, not new in nature.

Once the linguistic representation of an object is recognized as being a construction of the literature, then it is only appropriate that alternative terms should be used depending on the theoretical context invoked. Thus Compton changes "ray" to "quanta" (89) in accordance with the invocation of quantum theory a few lines earlier. Similarly, Compton begins to write "an [electron]" then corrects this to "a β-particle" (120) in accordance with an earlier switch in discussion from colliding objects to an analysis of ranges of particles. In both cases the changes are not compelled by technical accuracy, but they do help to maintain clear focus on the appropriate theoretical contexts.

AUTHORIAL PERSONA

Despite the familiar conjecture that scientists remove themselves from their writing so as to make their work appear less particular and so as to evade epistemological responsibility, Compton maintains an authorial presence in the article. The revisions in some ways enhance this presence and in other ways diminish it. The pattern is that authorial presence is decreased for the prior work, which is merged into the literature of the field, but authorial presence is increased for the current work, for which Compton and his co-worker Simon take responsibility as the thinkers, doers, and owners.

The merging of the individual into the collective of the literature for the scientist's prior work appears in a number of revisions involving self-citations. In the first paragraph of the draft, for example, Compton refers to his previous work "the quantum theory of X-ray scattering proposed by the [author]." Then Compton remembers that Simon is nominally coauthoring the article; he strikes out "the" and substitutes "one of us," to which he appends a footnote to his monograph for the National Research Council. But in the final version the entire phrase "proposed by one of us" is deleted (8–9), suggesting no credit in the text, and a citation to Debye is added to the footnote, sharing credit in the literature and emphasizing that the self-citation is part of a wider literature that is communal. Similar demotions of textual self-reference to footnotes occur at lines 101–3 and 128. In another case the self-reference is removed from the head of the sentence and given a less definitive verb; "Compton and Hubbard give for the . . ." becomes, "If the maximum range of the recoil electrons is S_m, Compton and Hubbard find . . ." (133–34). The most extreme case occurs in the last sentence, when Compton is stressing how well the current work fits with the findings of the literature. The phrase "strong confirmation of the assumptions used by one of us to explain . . ." is shortened by the deletion of the self-reference (187-88); moreover, the self-citing footnote is also eliminated, but a final phrase—the closing phrase of the article—is added: "on the basis of quantum theory" (180). Compton's earlier work is subsumed into a theory which is a fact of the literature transcending individual ownership.

In the previous example, however, even while self-citation is vanishing into the literature, strong reference remains to the authors as conceivers, doers, and owners of the current work. In all versions the last sentence opens with "Our results . . ."(187). Other first person usages remain through all versions to indicate the doing of the work (e.g., "photographs . . . which we have recently made" [11–12], "apparatus used in our work" [15], and "we used a mercury spark" [22]), responsibility for reporting the work (e.g., "In table 1 we have recorded the results" [47]), intellectual operations (e.g., "we have taken from his data" [78] and "the value of which we used" [81]), ownership of the data (e.g., "in our photographs [157]), the evaluation of the evidence (e.g., "In view, however, of the meager data as yet available on this point, we do not wish to emphasize this correspondence too strongly" [97–99]).

Three revisions, in addition, make the authors' role more explicit. The first two bring out the individual responsibility for the evidence. "Observed in the photographs" becomes "shown in our photographs" (115); "the experimental values" becomes "the observed lengths of the R tracks" (124). The third brings out the evaluative role; "can leave no reasonable doubt" becomes transformed to the more direct "we believe establishes" (83).

AUTHORIAL JUDGMENTS

Even where an author does not use first person to call attention to his evaluative role, he makes many evaluative judgments throughout the article through estimates of the reliability of various claims. Compton sharpens this evaluative role through revisions.

One set of judgments sharpened in revision assigns the way in which a relevant theory specifies a particular phenomenon. In the second sentence of the draft, radiation which has "been ascribed to photoelectrons" gets revised to radiation which has "been identified with photoelectrons," indicating a more specific association. A few lines later Compton flip-flops as to whether a particular interaction is "according to the predictions," "as predicted by," or "in accordance with the predictions of the quantum theory" (8); Compton winds up with the last, and weakest, assumption. As we shall see below, even the title of the article, characterizing the strength of the claim of the whole article, undergoes a similar weakening.

In the above examples the truth value of the claims was not questioned, but only the applicability to specific cases. But the larger set of revisions changes the certainty or character of a claim. "Fact" is weakened to "observation" (96); "suppose" is strengthened to "explained" (92); and a definite "are" wavers to "may be" then regroups to "are often" (68). "A satisfactory agreement" edges up to "a rather satisfactory agreement" (143–44); a "theory" is demoted to an "hypothesis" (154); and the direct identification of "are" weakens to the mediated explanation of "have tracks long enough to determine . . ." (157–58). Finally, in the last paragraph an inserted "about" (183) admits that the conclusions rest on approximate evidence.

The most direct judgments are made in the concluding section, and here we see the most adjustment of the strength of claims. In the third from the last paragraph, Compton begins to draw strong conclusions from the angles of ejection: "There can be no question but that the electrons ejected. . . ." But he then reconsiders and replaces this strong statement with a sentence about the calculation (173–74). In the next sentence he tries again: "There is undoubtedly . . ." But he also crosses this out and starts anew with a qualification: "In spite of some discrepancy at the largest angles, the R electrons ejected at small angles undoubtedly have greater energy than those . . ." In the final version, however, even this certainty is excessive, and a weaker judgment is passed to the reader who inspects the data charts: "It will be seen that the observed ranges . . . are . . . in substantial agreement with the theory" (174–77).

Again, in the next to the last paragraph, "thus constitutes a strong support of the . . ." becomes the weaker "is thus of special significance" (182). A judgment is again passed to the audience.

Despite these two weakenings the last sentence of the article is strengthened as much as it needs to be to assert the significance of the work. "Our results are thus in . . ." becomes "our results therefore afford a strong confirmation of . . ." (187). Compton thus urges no more than he has to, but does not evade responsibility for judgments. Elsewhere he calls attention to his judgments through italics in intermediate sets of conclusions (82–86 and 128–31).

AUDIENCE CONCERNS

The revisions show almost no concern with trying to urge the audience. The only persuasion seems to be that built into the article by the early constraints and early choices that shape the article. If one wishes to study persuasive intent one should look to those early decisions that position the work against previous work, that frame the problem to be addressed, and that determine the kind of evidence to be generated by the experiment; such modes of persuasion are in support of a theoretical position rather than in support of a particular set of results. The only overt attempt to urge the audience in the revisions is the addition of the word "heavy" in front of "lead box" (20) in the apparatus description to dispel criticism of contamination through inadequate shielding. All other revisions in anticipation of audience reaction have to do with the conventions and felicity of language: spelling and word form corrections, removing redundancies and excess commas, and rearranging word order and equations for easier reading. Many of these corrections occur between the completion of the revised draft and the publication of the final version. At that time certain small features are also made consistent with the journal style. *Centimeter* is spelled out, but *equation* is abbreviated; the degree symbol is substituted for the word, and the angstrom symbol is simplified by removal of the superior cycle.

Thus, although the audience is accommodated, it is not pushed. The reasons why the audience might want to believe the article are imbedded in the article's structure. A representation of the literature establishing and positioning a problem, an accurate understanding of existing knowledge, the drawing of a question sharply, the appropriateness of the research design, the fit of the results—these are what convince, but these are determined before the writing-up by the early constraints and decisions. The only thing the scientist as writer can control at the writing-up stage is the representation of these earlier constraints and choices. In the representation the scientist has some leeway, but the representations to be credible must still strike the audience as adequate accounts of actual situations. That audience has access to the same literature, has their own formulations of problems, knows what equipment is available and what the equipment can do, can inspect the author's equipment, and can replicate the author's experiment or run other experiments revealing the same phenomenon. In this light we can understand both Compton's throwing certain judgments to the reader under the assumption that the data are clear enough to speak for themselves within the theoretical context established by the article, and Compton's efforts in his revisions to make his descriptions as accurate and precise as needed for the argument. His credibility and persuasiveness depend finally on how close a fit his readers find between what he says and what is.

In order to maintain credibility Compton takes great care not to misrepresent his data. Not only is the first person maintained in contexts indicating his responsibility, the author takes explicit responsibility for miscalculations and errors, both through the section added prior to table 1 describing the sources of error and through another estimate of error (115–16). This latter discussion of error is difficult for Compton to formulate; he must make several revisions before he can make a reasonable and not misleading formulation of the probable errors. Finally, since the experimental error affecting the data was not discovered until Compton was part way through the draft, a number of corrections had to be made of figures in the text and in the first table.

Text as Object

Through all the constraints and choices we see the gradual emergence of a text—a literary object, separate from, although the consequence of, all that went before. Particularly as the text takes shape in drafting and revision, we can see it take on the quality of an object, open to all the limitations and manipulations of language. But still the text is a linguistic object that takes on the overriding task of the representation of nature.

The act of revision itself treats language as an object. Certain revisions in particular call attention to the text as linguistic construction: the sharpening of the recognition of the obscuring effect of reproduction on photographs (33); the retrospective addition of a phrase because certain terms are needed in an equation on the next line (41); deletions in recognition of later repetitions (90 and 116).

Large organizational shifts call attention both to the manipulable quality of a text and to the gradual construction or emergence of the textual object. The splitting of table 2 indicates that Compton is developing an organizational sense of the article that he did not have as he started the draft. Similarly, he did not begin with the subtitles that mark the major divisions of the revised article in mind. The first subtitle in the draft, *"Number of Tracks,"* is clearly an afterthought, squeezed in between lines. But when he reaches the second set of data, Compton realizes that the organization does have major divisions, so he rather emphatically begins the next section with the title *"Ranges of the R Tracks"* on a separate line and centered. By the time he reaches the third of the ultimate divisions, he seems to have gotten used to the organizational structure, and he presents the title *"Angles of Ejection of R Tracks"* in a more subdued position, on the same line as the new paragraph. This is the position the subtitles take in the printed version.

If the subtitling indicates Compton's increasing awareness of the role of blocks of text, his titling of the whole article indicates his judgment of what the whole text does. The original title in the draft is "Measurements of β-rays Excited by Hard X-rays," but before publication the title was softened to "Measurements of β-rays Associated with Scattered X-rays." The changed title recognizes that the text is not so much concerned with the mechanisms of excitation as with the association of the rays through measurement and photographs of individual incidents. The text is limited to just an aspect of the phenomenon and just an aspect of Compton's thoughts and convictions about the phenomenon. A text is a limited object.

THE ABSTRACT

The article's abstract serves as one further step in turning the article into an object, for the abstract considers the article as a whole and then makes a representation of it. In this regard the point at which Compton decides to write the abstract is a good indicator of when he gains a grasp of the whole text. The draft of the abstract appears about two-thirds of the way through the draft of the main text, at a spot corresponding to line 142 of the published version. The earlier part of the abstract draft, in addition, contains the kinds of numerical errors that Compton was not aware of until he reached table 1 (59). These facts indicate that Compton probably began the abstract when he was part way into the article; he apparently turned to a blank page where he thought the main draft would end. He did not have a grasp of the whole when he began the article and had to wait until he saw what he had written before he wrote the abstract; nonetheless, he felt he needed to write the abstract before completing the article, in order to articulate his sense of the whole and to keep the later parts logically and structurally consistent.

Even in the abstract itself he seems to need to recapitulate the entire argument before summarizing the conclusion. He reduces the summary of the data to a one-sentence statement recounting the main topics: "Measurements were made of the maximum range, the relative number of different ranges, the relative number ejected at different angles, and the relative ranges of the R tracks ejected at different angles." This sentence does not find its way into the published abstract, but rather seems more for Compton's own benefit.

Furthermore, the draft of the abstract is not complete on the notebook pages allotted it, suggesting that Compton returned to the main article before finishing the abstract and did not leave enough blank space for the completion of the abstract. The abstract draft breaks off in midsentence at the bottom of a page; the next page continues with the main text in midsentence. If the abstract did get written in stages coordinated with the writing of the main text, that correlation would further emphasize the interaction between the gradual creation of the text and the growing perception and command of the text as an object.

The specific content of the abstract and its revisions further reveal Compton's perception of what kind of object the text is. The substantial discussions in the main text of the background literature and the experimental apparatus become only sketchy mentions via secondary phrases in the first few sentences of the abstract. The sentences are more concerned with the data and findings; the grammatical subjects are reserved for "photographs," "kinds of tracks," and "ratio." Moreover, the problem addressed in the paper, "a more quantitative study of the properties of these rays" (14), does not receive explicit mention in the abstract.

The first eight of the nine sentences of the abstract are devoted to reporting the findings in some statistical detail. The organization of sentences 3 through 8 follows exactly the structure of the body of the paper reporting the data and findings, with two sentences devoted to each of the topics announced in the subtitles of the paper. The conclusions are reported in the last sentence of the abstract; however, that sentence is very long, about eighty words, and manages to incorporate almost all the substance of the final two paragraphs of the full paper. The one-sentence summary in fact incorporates verbatim many of the key phrases of the full version.

The abstract, therefore, focuses on the outcome of the experiment rather than on the background, formulation of the problem, or the experimental design. Nor does the abstract try to recapture a coherent argument, which would require more emphasis on theory and context. The emphasis is entirely on what can be formulated about the out-there physical phenomenon as a result of the experiment.

The revisions of the abstract draft emphasize this focus. Specifying phrases are added about the observed phenomenon, and excess theory and reference to calculations are eliminated. Finally, the original terse summary of conclusions is greatly expanded to incorporate almost all the substance of the full conclusions, as previously noted.

Conclusions

This examination of the emergence of two of Compton's texts reveals that many forces, constraints, and choices shape the final textual object. A. H. Compton, as all authors do, chooses the words that go on the page and thereby creates a statement—a text, a linguistic object—that did not exist before. But Compton's choices are severely constrained by contextual forces, directed by procedures of scientific argumentation and motivated by his personal commitment to record his claims and data as accurately as he is able. Some of the contextual constraints are active (in Fleck's terminology) in that they reflect the structure of the scientific community, the thought style and expressive habits of the period, the social position and interests of the investigator within the scientific community, the research program and theory commitments of the scientist, and the nature of the challenges to prior formulations of theory.

Within this context Compton has some freedom in choosing what claims to advance, in formulating or reformulating those claims, and in designing experiments or other means of advancing those claims. It is at this point that Compton seems to have the most leeway to frame his work strategically, positioning it against other claims and challenges. It is at this stage of basic positioning, I believe, that we should look for the locus of persuasive strategy rather than at the actual writing-up stage with its narrower manipulation of language. At this stage Compton decided what the real issues in the problem area were and how he could address them in the way most persuasive to his colleagues.

These strategic choices, nonetheless, were subject to constraints, but the constraints were passive. Compton could not violate the bulk of previously gathered data (although he could actively reinterpret or offer alternate explanations for the data.) He could not make equipment do what it could not do, and he could not control what data ultimately got recorded on the photographic plates. Moreover, given the canons of scientific argumentation which Compton observed, the center of the persuasive strategy was the active search for passive constraints. Compton bolstered his original discovery claim by developing a new source of data; he answered challenges by finding specific refuting data; and he advanced his own career by revealing more about the phenomenon and developing techniques for looking more intimately into nature.

Once the experiment has run its course, Compton could only choose to publish or not publish the results. Having chosen publication Compton is committed to presenting his theory and results as clearly, accurately, and precisely as the material and language allow. This precision, accuracy, and clarity in part serve the persuasive intention by identifying the tightness of fit among his claim, experimental procedures, and observed nature; in part they protect him from criticism of fuzziness or fraudulence (note particularly his careful revelations about the necessary recalculations to preserve the integrity of the data).

But the revisions are so careful on even such apparently inconsequential matters as his estimate of the quality of the photographic technique or the choice of "the" over "an" that they reveal a deeply internalized commitment to the best possible representation of the material within his theoretical, experimental, and linguistic scope.

Since there is no guarantee of an essential link between the objects of nature and the words and equations scientists formulate to describe those objects and their behavior, the nonfiction created by Compton, or any other scientist, cannot be taken as absolute, a transparent and congruent presentation of nature as it is. Compton, however, has worked to create orderly, significant experimental events that will produce results speaking to the issues before him and his colleagues. These issues are social, symbolic creations; scientific questions would not exist without scientists to find motives and ways to vex each other and nature with peculiarly human concerns of understanding and control. The representations of results so as to speak to those issues are equally human constructions, which we again see Compton working at. Compton's text-constructing work creates strong bonds between the rhetorical tasks before him in the scientific forum and the empirical tasks he sets for himself in the laboratory. In revision work, Compton keeps those bonds as strong and untangled as possible by being as precisely explicit and detailed as the argument warrants about what those empirical experiences were and what abstractions he draws from them.

Compton creates a crispness of argument not only by detailed revisions of the representation of the experiment and the results, but also by his careful control of epistemic level, authorial voice, and authorial judgments. His persuasive ends can only be met if he maintains the confidence of his readership that his representation on all levels adheres to the current standards of scientific practice.

Although by the time Compton completes the text he treats it as a manipulable object, the text contains references constructed and maintained by Compton's active commitments throughout the constructive process. And although the reference is not absolute in the sense of a one-to-one correspondence with objects having self-evidently natural and unchangeable designations and divisions, the reference is more than a literary fiction. The scientist's hands, eyes, ears, and laboratory apparatus stand between the physical events and the symbolic representation. Compton is neither a fiction writer nor a mute mechanic. The experiments are worked out both in the library and the laboratory, and the writing occurs both over the lab bench and over the desk.

Compton's behavior as revealed here should not be surprising to anyone familiar with the practices of modern science. All the evidence here indicates he is acting just as a good scientist might be supposed to; in so doing has managed in this case to create a statement of some endurance and force within the canon of communally accepted scientific claims.

Of course, not all claims have such good fortune. Many are fleeting, many fail, many are of little force or interest; the majority of Compton's articles suffered such fates. This hardly means that these articles were less well written or that the authors were not acting as quite so good scientists. Which articles get identified as right and significant depends on many factors—including changing interests and future empirical experiences of the community, as well as luck. Moreover, the details of what it means to act as a good scientist change through time and from locale to locale, as each research community evolves around its own problems and emerging work.

Moreover, it would be wrong to hold up this single case as absolutely indicative of the scientific procedures even within Compton's particular time and community. Compton's work may be idiosyncratic in his explicit concern for language, for within twentieth-century physics his drafts and revisions seem unusually detailed. Further, the stakes involved in Compton's claims and reputation at this juncture were high, encouraging heightened care.

Yet within these cautions, Compton's strong drive to hold himself and his claims accountable to his and his colleagues' experiences suggests the mechanism which keeps reference alive and makes language capable of interacting with the physical world. In this one concrete case, we see in detail the kinds of material relations between word and world around which we have seen the larger institutions of communication developing. Further case studies of how people move in the world to create words and how words then move people to interact with the world will increase our understanding of the varieties, characters, and qualities of reference in language. Only if we imagine that people never lift their heads out of books can we accuse their words of being only bookish.

Appendix

MEASUREMENTS OF β-RAYS ASSOCIATED WITH SCATTERED X-RAYS

By Arthur H. Compton and Alfred W. Simon

Abstract

Stereoscopic photographs of beta-ray tracks excited by strongly filtered x-rays in moist air have been taken by the Wilson cloud expansion method. In accord with earlier observations by Wilson and Bothe, two distinct types of tracks are found, a longer and a shorter type, which we call P and R tracks, respectively. Using x-rays varying in effective wave-length from about 0.7 to 0.13 A, the ratio of the observed number of R to that of P tracks varies with decreasing wave-length from 0.10 to 72, while the ratio of the x-ray energy dissipated by scattering to that absorbed (photo-electrically) varies from 0.27 to 32. This correspondence indicates that about 1 R track is produced for every quantum of scattered x-radiation, assuming one P track is produced by each quantum of absorbed x-radiation. The *ranges* of the observed R tracks increase roughly as the 4th power of the frequency, the maximum length for 0.13 A being 2.4 cm at atmospheric pressure. About half of the tracks, however, had less than 0.2 of the maximum range. As to *angular distribution*, of 40 R tracks produced by very hard x-rays (111 kv), 13 were ejected at between 0 and 30° with the incident beam, 16 at between 30° and 60°, 11 at between 60° and 90° and none at a greater angle than 90°. The R electrons ejected at small angles were on the average of much greater range than those ejected at larger angles. These results agree closely in every detail with the theoretical predictions made by Compton and Hubbard, and the fact that in comparing observed and calculated values, no arbitrary constant is assumed, makes this evidence particularly strong that the assumptions of the theory are correct, and that whenever a quantum of x-radiation is scattered, an R electron is ejected which possesses a momentum which is the vector difference between that of the incident and that of the scattered x-ray quantum.

In recently published papers, C. T. R. Wilson[1] and W. Bothe[2] have shown the existence of a new type of β-ray excited by hard x-rays. The range of these new rays is much shorter than that of those which have been identified with photo-electrons. Moreover, they are found to move in the direction of the primary x-ray beam, whereas the photo-electrons move nearly at right angles to this beam.[3] Wilson, and later Bothe,[4] have both ascribed these new β-rays to electrons which recoil from scattered x-ray quanta in accordance with the predictions of the quantum theory

[1] C. T. R. Wilson, Proc. Roy. Soc. A **104,** 1 (1923)
[2] W. Bothe, Zeits. f. Phys. **16,** 319 (1923)
[3] See, e.g., F. W. Bubb, Phys. Rev. **23,** 137 (1924)
[4] W. Bothe, Zeits. f. Phys. **20,** 237 (1923)

of x-ray scattering.[5] In support of this view, they have shown that the direction of these rays is right, and that their range is of the proper order of magnitude. The present paper describes stereoscopic photographs of these new rays which we have recently made by Wilson's cloud expansion method. In taking the pictures, sufficiently hard x-rays were used to make possible a more quantitative study of the properties of these rays.

The cloud expansion apparatus used in our work was patterned closely after Wilson's well-known instrument except that all parts other than the glass cloud chamber itself were made of brass. The timing was done by a single pendulum, which carried a slit past the primary beam and actuated the various levers through electric contacts. The Coolidge x-ray tube, enclosed in a heavy lead box, was excited by a transformer and kenotron rectifiers capable of supplying 280 peak kilovolts. For illumination we used a mercury spark, similar to that of Wilson, through which discharged a 0.1 microfarad condenser charged by a separate transformer and kenotron to about 40 kv. The photographs were made by an "Ontoscope" stereoscopic camera, equipped with Zeiss Tessar $f/4.5$ lenses of 5.5 cm. focal length. Eastman "Speedway" plates (45×107 mm) were found satisfactory.

A typical series of the photographs[6] obtained are reproduced in Plate I, (a) to (f), which show the progressive change in appearance of the tracks as the potential across the x-ray tube is increased from about 21 to about 111 kv.

Especially in view of the fact that the original photographs are stereoscopic, the negatives of course show much more detail than do the reproductions. These suffice to show, however, the two types of tracks, the growth of the short tracks with potential, and the fact that while the long tracks are most numerous for the soft x-rays, the short tracks are most in evidence when hard rays are used. These results are in complete accord with Wilson's observations.

Number of tracks. It has been shown[7] that if the above interpretation of the origin of the two classes of β rays is correct, the ratio of the number of short tracks (type R) to that of long tracks (type P) should be

$$N_R/N_P=\sigma/\tau \qquad (1)$$

where σ is the scattering coefficient, and τ the true absorption coefficient of the x-rays in air; for σ is proportional to the number of scattered

[5] A. H. Compton, Bulletin Nat. Res. Council, No. 20, p. 19 (1922); and P. Debye, Phys. Zeits. (Apr. 15, 1923)

[6] These photographs were shown at the Toronto meeting of the British Association in August 1924.

[7] A. H. Compton and J. C. Hubbard, Phys. Rev. **23**, 448 (1924)

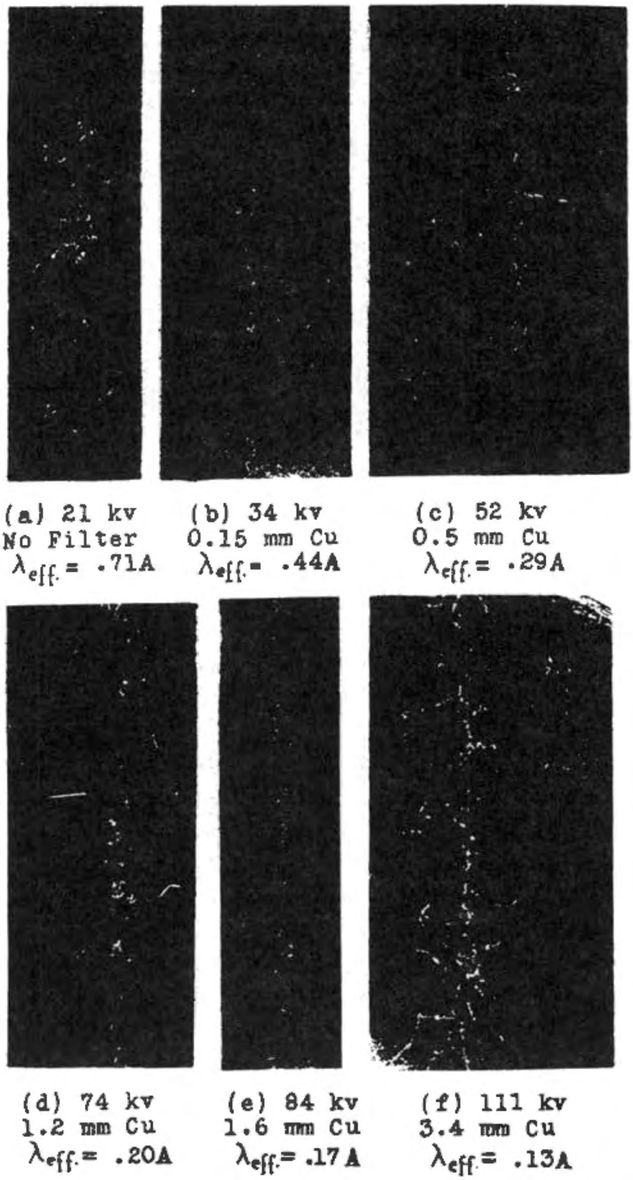

Plate I. The x-rays pass from top to bottom. In addition to the copper filter, they traverse glass walls 4 mm thick. For the short waves the shorter (R) tracks increase rapidly in length and number. Thus while in (a) nearly all are P tracks, in (f) nearly all are R tracks.

quanta, and τ to the number of quanta spent in exciting photo-electrons, per centimeter path of the x-rays through the air.

In Table I we have recorded the results of the examination of the best 14 of a series of 30 plates taken at different potentials. The potentials given in column 1 of this table are based on measurements with a sphere gap. The potential measurements required corrections due to a slight warping of the frame holding the spheres, and to the lowering of the line voltage when the condenser was charged for the illuminating spark. The latter error was eliminated in the later photographs, at 34, 21, and 74 kv, and the former error was corrected by a subsequent measurement of the sphere gap distances, checked by a measurement of the lengths of the P tracks obtained at the lowest potential. The probable errors of potential measurements are thus unfortunately large, amounting to perhaps 10 percent in every case except that of 74 kv, which is probably accurate to within 5 per cent.

TABLE I
Number of tracks of types R and P.

Potential	Effective wave-length	Total tracks N	R tracks N_R	P tracks N_P	N_R/N_P	σ/τ
21kv	.71A	58	5	49	0.10	0.27
34	.44	24	10	11	0.9	1.2
52	.29	46	33	12	2.7	3.8
74	.20	84	74	8	9	10
84	.17	73	68	4	17	17
111	.13	79	72	1	72	32

The effective wave-lengths as given in column 2 are the centers of gravity of the spectral energy distribution curves after taking into account the effect of the filters employed. Because of the strong filtering, the band of wave-lengths present in each case is narrow, and the effective wave-length is known nearly as closely as the applied potential.

All the tracks originating in the path of the primary beam are recorded in column 3. Of these, the nature of some was uncertain. At the lower voltages it was difficult to distinguish the R tracks from the "sphere" tracks which Wilson has shown are often produced near the origin of a β-ray track by the fluorescent K rays from the oxygen or nitrogen atoms from which the ray is ejected. At the highest voltage the length of some of the R tracks is so great as to make it difficult to distinguish them from the P tracks. The numbers of R and P tracks shown in columns 4 and 5 are those of the tracks whose nature could be recognized with considerable certainty, the uncertain ones not being counted. This procedure probably makes the values of N_R/N_P in column 6 somewhat too small for the lower potentials and somewhat too great for the higher potentials.

The values of σ and τ given in column 7 are calculated from Hewlett's measurements[8] of the absorption of x-rays in oxygen and nitrogen. We have taken from his data the value of τ for 1 A to be 1.93 for air, and to vary as λ^3. The difference between the observed value of $\mu=\tau+\sigma$ and this value of τ gives the value of σ which we used.

The surprisingly close agreement between the observed values of N_P/N_R and the values of σ/τ we believe establishes the fact that the *R tracks are associated with the scattering of x-rays*. In view of the evidence that each truly absorbed quantum liberates a photo-electron or P track,[9] the equality of these ratios indicates that *for each quantum of scattered x-rays about one R track is produced.*

The fact that for the greater wave-lengths the ratio N_R/N_P seems to be smaller than σ/τ may mean that not all of the scattered quanta have R tracks associated with them. This would be in accord with the interpretation which has been given of the spectrum of scattered x-rays. The modified line has been explained by assuming the existence of a recoil electron, and the unmodified line as occurring when the scattering of a quantum results in no recoil electron. On this view the fact that the unmodified line is relatively stronger for the greater wave-lengths goes hand in hand with the observation that N_R/N_P is less than σ/τ for the greater wave-lengths. In view, however, of the meager data as yet available on this point, we do not wish to emphasize this correspondence too strongly.

Ranges of the R tracks. The range of the recoil electrons has been calculated on the basis of two alternative assumptions.[10] First, assuming that the electron recoils from a quantum scattered at a definite angle, its energy is found to be

$$E=h\nu\frac{2\alpha\cos^2\theta}{(1+\alpha)^2-\alpha^2\cos^2\theta}, \tag{2}$$

where $\alpha=h\nu/mc^2$, and θ is the angle between the primary x-ray beam and the direction of the electron's motion. This energy is a maximum when $\theta=0$, and is then,

$$E_m=h\nu\frac{2\alpha}{1+2\alpha}. \tag{3}$$

[8] C. W. Hewlett, Phys. Rev. 17, 284 (1921)

[9] See, e. g., A. H. Compton, Bull. Nat. Res. Council No. 20, p. 29, 1922

[10] See Compton and Hubbard, loc. cit.[7]

The second assumption is that the R electron moves forward with the momentum of the incident x-ray quantum. In this case the energy acquired is

$$E' = h\nu \cdot \tfrac{1}{2}\frac{\alpha}{1+2\alpha}\left(1-\tfrac{1}{4}\alpha^2+\cdot\cdot\cdot\right). \tag{4}$$

Eq. (3) was found to agree considerably better than Eq. (4) with Wilson's experimental results.

The lengths of the tracks shown on our photographs could be estimated probably within 10 or 20 per cent. These measured values, reduced to a final pressure of 1 atmosphere, are summarized in Table II. In column 2 are recorded the lengths of the longest tracks observed at each potential. S_m is the range calculated from Eq. 3, using C. T. R. Wilson's result[1] that the range of a β-particle in air is $V^2/44$ mm, where V is the potential in kilovolts required to give the particle its initial velocity, and the frequency ν employed is the maximum frequency excited by the voltage applied to the x-ray tube. S' is similarly calculated from Eq. (4).

TABLE II

Maximum lengths of R tracks.

Potential	Observed	Calc. (S_m)	Calc. (S')
21kv	0mm	0.06mm	0.004mm
34	0	0.3	0.02
52	2.5	1.8	0.1
74	6	6	0.4
88	9	12	0.7
111	24	25	1.5

It is evident that the observed lengths of the R tracks are not in accord with the quantity S' calculated from Eq. (4). They are, however, in very satisfactory agreement with the values of S_m given by Eq. (3). This result agrees with the conclusion drawn from Wilson's data,[11] but is now based upon more precise measurements. It follows that ***the momentum acquired by an R particle*** is not merely that of the incident quantum, but ***is the vector difference between the momentum of the incident and that of the scattered quanta.***[12]

This conclusion is supported by a study of the relative number of tracks having different ranges. If the maximum range of the recoil electrons is S_m, Compton and Hubbard find[7] that the probability that the length of a given track will be S is proportional to

$$\left(2\sqrt{S/S_m}+\sqrt{S_m/S}-2\right). \tag{5}$$

[11] Compton and Hubbard, loc. cit.,[7] p. 449.

[12] That this is true for the β-rays excited by γ-rays has been shown in a similar manner by D. Skobeltzyn, Zeits. f. Phys. **28**, 278 (1924).

This expression assumes that the exciting primary beam has a definite wave-length. To calculate the relative number of tracks for different relative lengths to be expected, we have averaged this expression by a rough graphical method over the range of wave-lengths used in our experiments. These calculated values are given in the last column of Table III, for the relative ranges designated in column 1. A comparison of these

TABLE III
Relative lengths of R tracks.

Range of S/S_M	Per cent of R tracks within this range 52kv	74kv	88kv	111kv	Mean	Calc.
0- .2	44	66	60	54	56	53
.2- .4	34	20	26	32	28	22
.4- .6	19	8	4	8	10	14
.6- .8	0	3	5	3	3	8
.8-1.0	3	3	5	3	3	3

calculated values with the observed relative ranges shows a rather satisfactory agreement throughout. It will be noted further that the probabilities of tracks of different relative ranges is found to be about the same for x-rays excited at different potentials. This is in accord with the theoretical expression (5) for the probability, which is independent of the wave-length of the x-rays employed.

Angles of ejection of R tracks. On the view that the initial momentum of an R electron is the vector difference between the momenta of the incident and the scattered quantum, it is clear that these electrons should start at some angle between 0 and 90° with the primary beam. The probability that a given track will start between the angles θ_1 and θ_2 is on this hypothesis,[13]

$$\int_{\theta_1}^{\theta_2} P_\theta d\theta = 3ab\int_{\theta_1}^{\theta_2} \frac{a^2 \tan^4\theta + b^2}{(a \tan^2\theta + b)^4}\,\frac{\sin\theta}{\cos^3\theta}d\theta\,, \qquad (6)$$

where $a = (1 + h\nu/mc^2)^2$, and $b = (1 + 2h\nu/mc^2)$.

In our photographs only those taken at 111 kilovolts have tracks long enough to determine the initial direction with sufficient accuracy to make a reliable test of this expression. In all, the directions of 40 tracks were estimated, with the results tabulated in the second column of Table IV. In view of the fact that the photographs were stereoscopic, it was possible to estimate the angles in a vertical plane roughly, though not closer perhaps than within 10 or 15°. The values in the third column are calculated from Eq. (6). It is especially to be noted that, in accord with the

[13] See Compton and Hubbard, loc. cit.,[7] Eq. (14).

theory, no R tracks are found which start at an angle greater than 90° with the primary x-ray beam. In view of the small number of tracks observed and the approximate character of the angular estimates, the agreement between the two sets of values is as close as could be expected.

A more searching test of the assumption that the R tracks are electrons which have recoiled from scattered quanta is a study of the relative ranges of the tracks starting at different angles. (See columns 4 and 5 of Table IV.) The calculated ranges in column 5 are based on Eq. (2) for

TABLE IV

Number and range of R tracks at different angles, for 111 kv x-rays.

Angle of emission	Per cent of total number (obs.)	(calc.)	Average range (obs.)	(calc.)
0°-30°	34	28	9 mm	11 mm
30°-60°	39	50	4	4
60°-90°	27	22	0.9	0.3

the energy at different angles. In this calculation the effective wave-length, as estimated in connection with Table I, is employed. It will be seen that the observed ranges of the tracks ejected at small angles are much greater than that of those ejected at large angles, in substantial agreement with the theory.

It is worth calling particular attention to the fact that in comparing the theoretical and experimental values in these tables, no arbitrary constants have been employed. The complete accord between the predictions of the theory and the observed number, range, and angles of emission of the R tracks is thus of especial significance.

The evidence is thus very strong that there is about one R track or recoil electron associated with each quantum of scattered radiation, and that this electron possesses, both in direction and magnitude, the vector difference of momentum between the incident and the scattered x-ray quantum. Our results therefore afford a strong confirmation of the assumptions used to explain the change in wave-length of x-rays due to scattering, on the basis of the quantum theory.

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