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DISCORDANT CONSENSUS: DIALOGUES ON THE EARTH'S AGE IN AMERICAN SCIENCE, 1890–1930

A DISSERTATION APPROVED FOR THE DEPARTMENT OF HISTORY OF SCIENCE

BY

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Contents

Abstract

The history of investigation of the Earth's age during the second half of the 19th and first half of the 20th centuries has been often presented as a narrative of a disciplinary conflict. In this dissertation, I investigate the evidence which supports such accounts as well as evidence contrary to it. I conclude that this disciplinary conflict narrative underestimates the complexity of the historical situation and points to broader inadequacies in understanding of group knowledge and consensus. The larger question I ask is "what does it mean for a group of people to know something?" I argue that we need view the historical actor as composed of (dialogically interacting) multiple subjects: the individual who strives for understanding and multiple community members who strive to communicate. Furthermore, I introduce the concept of an imagined community member, the image that an interlocutor has of an anonymous member of a given community, who represents its knowledge, assumptions, and expectations.

Based on an investigation of previously unexamined archival and printed sources, I argue for a new periodization of dialogues on the Earth's age in America. Prior to the 1890s, only isolated American scientists spoke about the topic. After a period of intensified discussion during the years 1892–3, the scientific community started to give a uniform answer to the question about the Earth's age,which was the disciplinary conflict story. This uniform community response, however, corresponded only to a consensus of response, not a consensus of shared belief. The next period covering the first two decades of the next century, corresponded to the

fragmentation of the discussion of the Earth's age into multiple, non-interacting, largely disciplinary dialogues. A new consensus did not emerge again until around 1930, when a new community emerged and claimed jurisdiction over the question of geological time. In addition to challenging the disciplinary conflict narrative I also argue that the significance of the discipline of chemistry, and of two individuals, Joseph Barrell and Alfred C. Lane, to the dialogues on the Earth's age has been undervalued in current historiography.

Introduction

> "I am convinced that science goes on and progresses at the *expense* of those absorbed in her pursuit. That men's souls are burned as fuel for the enginery of scientific progress." Clarence King

The question "what does it mean for a group of people to know something?" has been under-investigated by historians. This dissertation's main argument is that the answer to the above question is non-trivial and has consequences for how we think about the role of knowledge for individuals and communities. I come to this conclusion based on an investigation of how scientists in the United States attempted to answer the question "How old is the Earth?" during the period roughly between the Civil War and World War II. Based on this investigation, I challenge the current historiography by arguing its shortcomings are the consequence of not asking the question about group knowledge. I suggest a possible way in dealing with the question of group knowledge. There is little unique to the specific case of the Earth's age research which would suggest that the solution proposed here is only limited to this particular historical event.

If one of the large questions of philosophy is how to proceed from unique, local experiences to universal knowledge, then this investigation may be considered a contribution to that project because it asks a more targeted version of the same question: How do we proceed from making claims about beliefs of single individuals to making a claim about beliefs of the entire group? This investigation assumes that there is a fundamental difference between lived experience and the account of that experience. Therefore, investigating how one goes from a group of individuals knowing to the group as a whole knowing, I explore the consequences of the difference between lived experience and its account.

This dissertation is about individuals trying to answer the question "How old is the Earth?" Science is social, of course, (and so is language) but the view of science I present is not as something that primarily exists in a society. The concern here is not with the society but with communities within the society, and with the individuals producing the knowledge and the consequences of the produced knowledge for those individuals.

In many ways, this dissertation is old-fashioned. Its subject is an intellectual question. The historical actors are mostly white males from American, east coast, establishment institutions. The sources used are traditional publications and manuscripts. Yet if my claim that there are two sites in which knowledge needs to be concurrently investigated—the individual knower and knowledge community—is correct, then there is something novel about my investigations and the conclusions have consequence for other historians relying in their investigations on what seems to be records of individual beliefs.

This dissertation presents both a historiographic and a theoretical argument. In this introduction I will first present the historiographic problem, next, the theoretical consideration aimed at resolving this problem, and finally an outline of the chapters.

Historiography

It is difficult to present a clear account describing scientific attempts to answer the question of the Earth's age during the period 1860–1940 because both historical and present-day categories used to discuss scientific knowledge fail to adequately capture the details of the historical events. The question "How old is the Earth?"

did not fall within the purview of a single scientific discipline, institution, research program, or any group of researchers. The meaning of the question was not consistent among the scientists who investigated it. The question was not even at the center of a dispute with clearly identifiable groups engaging each other. To make sense of what took place, a new way of looking at scientific texts and communities is required and a new emphasis needs to be placed on individuals and their relation to their communities.

The outline of the account that I will present is as follows. During the 1860s and 1870s, the physicist William Thomson (later Lord Kelvin) challenged the geological notion of uniformitarianism, claiming it violated the laws of thermodynamics. Uniformitarianism explained the geological history of the Earth only in terms of presently observable forces. Like Darwin's theory of evolution, which it helped to inspire, uniformitarianism required the Earth to be very old. Based on thermodynamic theories, Thomson calculated the Earth was only one hundred million years old: too short a time for most geologists. The debate about Earth's age became quite public and gained notoriety in part because the plausibility of Darwin's theory was also at stake. Though the division between short and long time scales was not entirely along disciplinary lines, by the 1890s the discussion of the Earth's age was presented as a disciplinary argument between the short time scales of the physicists on the one hand, and the long time scales of the geologists and biologists on the other hand.

During the first decade of the 20th century new scientific developments reshaped the dialogues. The discovery of radioactivity both nullified Thomson's earlier objections and also provided a new method for measuring the age of rocks. Thomson's assumptions were also challenged by a new cosmogony. However, the new radiometric methods had to compete with a number of other independent new methods that had been proposed during the first two decades of the twentieth century for measurement of the Earth's age. These methods stemmed from just about every scientific field in the university, including geology, chemistry, palaeontology, and astronomy. The proliferation of a variety of methods resulted in fragmentation of the dialogue within the scientific community.

Already during the 1910s scientists, such as Joseph Barrell and Arthur Holmes, were applying radiometric methods to questions of geological time. Those methods, however, did not gain widespread recognition in the scientific community until the 1920s and even then were still contested. Though Holmes is widely credited with establishing the radiometric methods, I contend that a largely forgotten American, Alfred C. Lane, played an equally important, though quite different, role in the success of radiometric methods.

Reflecting the preoccupations of the scientists they study, general histories of 19th century geology give only scant attention to the the question of the Earth's age.[1] Once the issue of deep geological time was settled during the first half of the nineteenth century, the details of exactly how many years old a specific mineral was or the whole planet were of much less importance. Nevertheless, the question of the Earth's age is often mentioned in passing and some books even focus exclusively on recounting the more-or-less heroic story of how we got to know that the Earth is 4.5 billion years old. Additionally, a number of works based on primary research also deal with the question of the Earth's age. The general outline presented above (with the exception of Lane's role) is in agreement with how the history of the Earth's age question is currently described by historians. It is when

[1]For example, two such histories, one from the beginning of the last century, the other from the end of it, hardly mention it, George Perkins Merrill, *The First One Hundred Years of American Geology* (New York: Hafner, 1924/1969) and Mott T. Greene, *Geology in the Nineteenth Century: Changing Views of a Changing World* (Ithaca, N.Y: Cornell University Press, 1982). A recent forty page historiographic article about 19th century Earth sciences, devotes a quarter of a page to the age of the Earth: David Roger Oldroyd, "The Earth Sciences," in David Cahan (ed.), *From Natural Philosophy to the Sciences: Writing the History of Nineteenth-Century Science* (2003): 88–128.

the outline is filled in with the details, however, that the problems arise. Historical events provide both evidence for making generalizations, as provided in the outline above, and significant cases that seem to invalidate those generalizations. The existence of those two types of evidence has long been noticed by historians who have taken one of three approaches to dealing with the incongruities of the historical record. The first approach is a simple narrative of disciplinary conflict between geologists and physicists resolved by the discovery of radioactivity (possibly with some qualifications). The second approach is a complex account in which seemingly every individual holds a different belief and no pattern is to be found. The third solution is enumeration of multiple methods for calculating Earth's age with little discussion of their interaction. All three accounts trace their origin to participant histories and are still present in current literature.

The first of those approaches is represented by Joe Burchfield's *Lord Kelvin and the Age of the Earth*, which, rightfully, stands as the standard history of the age of the Earth debates during the period of Kelvin's lifetime (c. 1850–1910). My investigation aims not to challenge the historical research of Burchfield, but to expand on it. I do not duplicate his research on the early stages of the British debates, but instead provide more details of the American dialogues and on the later stages of the debates only briskly covered by him. I do differ in evaluating the epistemic relationship of the individuals involved to their communities.[2]

Burchfield orients his study around the influence on the debates of a seemingly unlikely character, Lord Kelvin, who was not a geologist and not principally interested in geological matters. He played a significant role in the debates because of his prestige as a scientist and the prestige of physics as a science, as well as geolo-

[2]Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (New York: Science History, 1975). See also, Lawrence Badash, "Rutherford, Boltwood, and the Age of the Earth: The Origin of Radioactive Dating Techniques," *Proceedings of the American Philosophical Society* 112 (1968): 157–169.

gists' shift towards quantification. It is a story of a disciplinary conflict. However, despite all the evidence for a conflict narrative, Burchfield does not overlook evidence contrary to it. For example, he starts one paragraph: "Perhaps the most significant feature of the controversies over the age of the Earth was the degree to which the antagonists divided along disciplinary lines," and finishes the same paragraph with

> Admittedly, the disagreements over the Earth's age were not always interdisciplinary. Debates raged within the separate sciences as well as among them, and other sciences besides physics and geology were often involved. The participants themselves, however, generally saw the problem as a conflict between physics and geology, and in retrospect, that view does not seem altogether unjustified.[3]

This tension between wanting to make generalizations about disciplines, but having to qualify every such statement is present throughout the book.[4]

As Burchfield points out, part of the push for providing a disciplinary framework originates from the historical actors themselves who presented the story that way. For example, in 1893 Charles Walcott wrote: "The physicists have drawn the lines closer and closer until the geologist is told that he must bring his estimates of the age of the Earth within a limit of from ten to thirty millions of years. The geologist masses his observations and replies that more time is required"[5] even though just a few months earlier Walcott chaired a session on the age of the Earth during which it was a geologist who was arguing for yet a shorter time scale and a physicist who was opposing him.[6] It was not only the geologists who presented this

[3]Burchfield, *op. cit.*, p. 216–217.

[4]Also, when describing the situation at the end of the 19th century, Burchfield writes: "While the geologists were adjusting to Kelvin's original limits on the Earth's age, a handful of physicists and astronomers seemed intent upon reducing those limits still further. Starting usually from one or another of Kelvin's initial arguments, the 'physicists,'[36] including Kelvin himself..." note 36 reads "The term 'physicists' is perhaps not entirely appropriate since the group included astronomers, physicists and geologists with a physical bias. Because of the predominance of Kelvin and Tait, however, it was the term most frequently used during the late nineteenth century, particularly by the embattled geologists."

[5]Charles D. Walcott, "Geologic Time, as Indicated by the Sedimentary Rocks of North America," *Journal of Geology* 1 (1893), p. 639.

[6]See ch. 4 on the meeting of the Geological Society of Washington.

narrative of disciplinary conflict; the famous physicist Ernest Rutherford wrote in 1904 "The discovery of the radioactive elements [...] allows the time claimed by the geologists and biologists for the process of evolution," despite the fact that the time scales suggested by Rutherford were much longer than what "the geologists and biologists" were asking for.[7] At least from the 1890s, the story of a disciplinary conflict, despite its shortcomings, has persisted to this day in scientific textbooks and many histories.

One can avoid some of the issues arising from making generalizations by telling a complexity account: the debates were complex and involved diverse participants and arguments. In such an account each scientist presented a unique argument and was trying to achieve a slightly different goal. When Burchfield is not making generalizations, his book presents this complexity account. Stephen Brush's three volume study of the history of planetary science also is a exemplar of this approach.[8] Brush presents connections among ideas in geology, physics, and astronomy, as well as between theories of the origin of the solar system, its age, properties of the Earth and the atoms, space exploration, and others. Brush's detailed analysis is a great resource and is very insightful, but it leaves the reader overwhelmed and without a straightforward and integrated account.

The final approach tells the history of debates on the Earth's age by presenting various pieces of the puzzle independently without an attempt to connect them. This course is taken by Dalrymple who enumerates the various approaches to the problem, providing informative technical details.[9] Also, a volume edited by

[7]Ernest Rutherford, *Radio-Activity* (Cambridge: Cambridge University Press, 1904), p. 657.

[8]Stephen G. Brush, *Nebulous Earth: The Origin of the Solar System and the Core of the Earth from Laplace to Jeffreys* (New York: Cambridge University Press, 1996); Stephen G. Brush, *Transmuted Past: The Age of the Earth and the Evolution of the Elements from Lyell to Patterson* (New York: Cambridge University Press, 1996); Stephen G. Brush, *Fruitful Encounters: The Origin of the Solar System and of the Moon from Chamberlin to Apollo* (New York: Cambridge Univ. Press, 1996).

[9]G. Brent Dalrymple, *The Age of the Earth* (Stanford, Cal.: Stanford University Press, 1991), pp. 12–78.

Lewis and Knell is a source of excellent essays, each one providing vignettes on a particular individual or a specific context, but, again, by the nature of being a collection, it does not provide a unified account.[10]

Of course, the category of scientific discipline itself changes in time and the period under investigation was particularly important in formation of scientific disciplines. Unfortunately, simply historicising the concept of discipline will not resolve the confusion. The problem is that our impulse to divide individuals into groups according to their shared beliefs is misguided. The challenge provided by the unattainability of the disciplinary narrative, however, provides an opportunity to revaluate often used, but under-scrutinized categories, such as shared belief and consensus, and reexamine the epistemic relationship of the individual to the community.

No satisfactory coherent and integrated account describing the research on the Earth's age exists. The inability to produce such an account points to the insufficiency of some of the assumptions used in telling intellectual history. Those assumptions include claims that an utterance can be taken uncritically as representing the beliefs of an individual and that group consensus is equivalent to shared beliefs. My investigation of the dialogues about Earth's age challenges these assumptions. I do not simply mean that individuals have ulterior motives or are pressured into uttering what they do not believe to be case, but that there is a difference between utterance and belief even in situations when those individuals are trying to be truthful. A new theoretical approach is called for.[11]

[10] C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (London: Geological Society, 2001).

[11] I am not claiming that those are assumptions that are held by all historians, nor that they have never been challenged, but only that they too often go unchallenged and they have not been challenged by historians dealing with the question of the Earth's age.

Theoretical Considerations

To overcome the problems outlined above, I examine the process of communication. In a simple, mechanistic model of communication, a message is encoded by a sender, then it is transmitted through some medium (where noise can corrupt it), and finally it is received and decoded by the receiver. The central assumption of this way of understanding communication is that to communicate is to transmit a a message. The point is to get some information from one place to another. In this model, it is possible to achieve perfect communication—the message sent is the exact message received. There is an outside standard that can verify successful message transmission.[12]

For the purposes of this analysis of the age of the Earth debates, a different way of thinking about communication inspired by the work of Mikhail M. Bakhtin is helpful.[13] The central organizing concept of Bakhtin's thought is dialogue. Dialogue is understood differently than an everyday conversation.[14]Dialogue is an exchange of utterances; utterances are addressive rejoinders awaiting a response. Dialogue is always between two specific interlocutors. The speaker is always addressing her utterance to a specific someone whose reply the speaker anticipates. In dialogue there is never a first word uttered, each utterance is already a reply

[12]Claude E. Shannon and Warren Weaver, *The Mathematical Theory of Communication* (Urbana, Ill: University of Illinois Press, 1949).

[13]The concepts that I introduce here are at the core of Bakhtin's philosophy and can be found throughout his writing; however, the most relevant are Mikhail Mikhailovich Bakhtin, "The Problem of Speech Genres," pages 60–102 of *Speech Genres and Other Late Essays* (Austin: University of Texas Press, 1986), Mikhail Mikhailovich Bakhtin, *Toward a Philosophy of the Act* (Austin: University of Texas Press, 1993) and a text attributed to Bakhtin, V. N. Vološinov, *Marxism and the Philosophy of Language* (Cambridge, Mass.: Harvard University Press, 1973). An introduction to a systematic presentation of Bakhtin's philosophy is Michael Holquist, *Dialogism*, 2nd edition (London: Routledge, 2002). The most notable application of dialogical approach to history of science is Mara Beller, *Quantum Dialogue: The Making of a Revolution* (Chicago: Chicago University Press, 1999). See also, Daniel Aleksandrov and Anton Struchkov, "Bakhtin's Legacy and the History of Science and Culture: An Interview with Anatolii Ahutin and Vladimir Bibler," *Configurations* 1 (1993): 335–386.

[14]Dialogue has been used by a number of philosophers to mean a range of things. For a review, see Dmitri Nikulin, *On Dialogue* (Lanham, MD: Lexington Books, 2006).

to previous utterances. Dialogue exists between two interlocutors with a specified relationship between them. They do not send messages to each other but engage in the act of concurrently uttering and replying. An utterance does not send information, but rather evokes a response. The two interlocutors occupy specific points in space and time and those points are distinct.

The mind of the other is inaccessible. Unlike the perfect transmission of a message, a dialogical utterance cannot transmit the same meaning to the other interlocutor. Consequently, the only way that the first interlocutor knows if the addressee understood the utterance correctly is by judging the response to the utterance. The utterer dialogically means something if he judges the other's response to be as if the other understood. The utterer may judge that there is an indication of understanding; however, understanding is not a binary—either understood or misunderstood. Each satisfactory response is evidence only of the fact that at this particular time and place the other's reply was what the author expected. However, as the conversation continues there is always the possibility that the interlocutor may do something which will reveal that she does not understand in the way that the author does. The speaker does not even have the certainty that if the same dialogue were to take place at a different time and place it would proceed in the same way (maybe there is something in the context of this locale that made the other reply the way he did).

Bakhtin introduces the concept of addressivity, which differentiates language from dialogue analysis.[15] Whereas language can be analyzed in isolation, dialogue cannot. It is always addressed to someone in response to a previous utterance and anticipating a response. As Bakhtin states it:

> I try to act in accordance with the response I anticipate, so this anticipated response, in turn, exerts an active influence on my utterance (I parry objections that I foresee, I make all kinds of provisions, and so forth.) When

[15] That is the distinction in discourse analysis between language and language in use.

> speaking I always take into account the apperceptive background of the addressee's perception of my speech: the extent to which he is familiar with the situation, whether he has special knowledge of the given cultural area of communication, his views and convictions, his prejudices (from my viewpoint), his sympathies and antipathies—because all this will determine his active responsive understanding of my utterance.[16]

It is this concept that all utterances are addressive that forms the central lens through which I will analyze the dialogues on the Earth's age.

Bakhtin, furthermore, states that this addressee can be a specific participant-interlocutor but also can be a member of a more or less differentiated group. He or she can be an actual or an imagined interlocutor. The utterer must know something about the other to whom she is addressing the utterance, but if that is the case, what about utterances that seem to be addressed to no one specific? Bakhtin asserts:

> There can be no such thing as an abstract addressee, a man unto himself, so to speak. With such a person, we would indeed have no language in common, literally and figuratively. Even though we sometimes have pretensions to experiencing and saying things *urbi et orbi*, actually, of course, we envision this 'world at large' through the prism of the concrete social milieu surrounding us. [...] we assume our addressee a contemporary of our literature, our science, our moral and legal codes.[17]

That is, all utterances *are* addressed to someone specific. That someone may be a scientist, a geologist, a citizen, another human, a god, a future reader.

The above observation becomes even more useful when combined with the concept of an imagined community associated with Benedict Anderson, who used it to understand nations. For Anderson, the community was imagined because it depended on the imagined communion of all the members of a nation, who in reality could not possibly all know each other. It was a community "because, regardless of the actual inequality and exploitation that may prevail in each, the

[16] Mikhail Mikhailovich Bakhtin, *Speech Genres and Other Late Essays* (Austin: University of Texas Press, 1986), pp. 96.
[17] Vološinov, *op. cit.*, pp. 85–86.

nation is always conceived as a deep, horizontal comradeship."[18] In a similar way, a scientific community is one where one does not know all the scientists (current or future) yet one ideally supposes an equality among them. More recently, Charles Taylor, building on Anderson's work, has introduced a concept of the "social imaginary," again to deal with notions of people's social lives, but which can also be applicable to scientific communities. He defined the social imaginary as the "ways people imagine their social existence, how they fit together with others, how things go on between them and their fellows, the expectations that are normally met, and the deeper normative notions and images that underlie these expectations."[19] Taylor argues that the social imaginary is a more appropriate category than social theory to understand societies because people, rather than using theoretical terms, which only a small minority possess, imagine their social surroundings through images, stories, and legends, and that these resources provide a common understanding that allows for common practices. Again, this can be applied to a scientific community which, independently of philosophies of science, produces science according to the norms carried through common practices, stories, and legends, such as the scientific myth of the disciplinary conflict over the question of the Earth's age.

Drawing on Bakhtin's analysis of addressivity and Anderson and Taylor's analysis of imagined communities, we can introduce a concept of an imagined community member, that is, an addressee who is a generic imagined member of the (imagined) community. In such an analysis, the point of scientific utterance is not to transmit information, but to elicit a desired response (or a potential response) from this imagined community member. It is less important to concentrate on the meaning of the utterance in relation to the outside world, it is more important

[18]Benedict Anderson, *Imagined Communities* (London: Verso, 1983), p. 7.
[19]Charles Taylor, *Modern Social Imaginaries* (Durham: Duke University Press, 2004), p. 23.

to look at the meaning of the utterance as it exists between the utterer and his addressee, and often this addressee is the imagined community member.[20]

So each utterer has an image of a generic imagined community member who has the beliefs that the utterer imagines are shared by all the members of the community, but who has no beliefs on subjects that are not relevant to the community. That is, if one is addressing a community of geologists, one is addressing a community member whom one imagines to expect specific opinions on, say, stratigraphy, but not necessarily on one's favorite food. This is in contrast to an individual who as a real person does have a favorite food (though the speaker may not know what it is).

A parallel consideration independent of Bakhtin leads to similar conclusions about the relationship of the individual to the community. Many linguists have long realized that there exists a contradiction in the nature of language: language aims to serve as a universal code that will transmit the same meaning to all individuals, while at the same time this code needs to be subverted by individuals needing to express their unique experiences. I refer to those two goals as a desire to communicate and a desire to understand. However, this duality is contradicted by a central assumption of modern science that explicitly denies the existence of this linguistic contradiction. This assumption is that proper scientific understanding can be unequivocally communicated, and proper scientific communication can be fully understood. Both of those positions cannot be simultaneously correct. In this project, I argue that the desire to understand and the desire to communicate are, indeed, mutually exclusive goals. This leads to an inevitable tension between the nature of language and the underlying assumption of science.

The resolution of this tension is a split of the individual into multiple subjects.

[20] This is not to say that I am suggesting the world has no importance. Just the opposite: each of the interlocutors has her own lifetime of experiences of the world. Those interactions play an important consideration in what individual scientist expects the other to say about the world.

The individual who continues to strive for both understanding and communication develops multiple identities. He becomes his own self who understands, but his understanding necessarily remains personal (or not-understood if shared) and he also becomes many community members. As a community member he gives up his unique position that he occupies as an individual and assumes a shared position of the community. As a community member he no longer speaks of what he believes, but he speaks to what is expected. As a community member he can communicate, but the communicated knowledge can no longer satisfy his desire to understand, because it no longer is addressed to his unique position, but to the position of another (imagined) community member. (But we must remember as Bakhtin says, that one never fully speaks as an individual or as a community member; there are always multiple voices in any utterance.[21])

Different individuals deal differently with their multiple selves. Some (consciously or not) can assume the different roles and speak accordingly, realizing the limitations of understanding and communication. But others who do not become aware of the incompatibility of the two desires will be frustrated by their inability to communicate their understanding and by the unsatisfactory nature of communicated knowledge.

The above analysis suggests that there exist two kinds of comprehension that correspond to the two desires: individual understanding results from the desire to understand, while the desire to communicate results in community knowledge. Any analysis of scientific discourse should recognize the two aims and the two modes of comprehension. This duality can elucidate the frustration experienced by individuals and the success of science in modern society. *Individual understanding* is the way in which I comprehend the world based on my whole lifetime of experiences and memories. Since those are unique, my individual understanding will be

[21] E.g., Vološinov, *op. cit.*, p. 34.

unique. *Community knowledge* is the knowledge held by the imagined community member, or what the members of the community expect each other to expect from themselves. Community knowledge will necessarily be limited to topics of interest to the community. Because each individual forms her own conception of the community knowledge, it is a family resemblance concept.

I want to emphasize here the difference in kind between knowledge and understanding.[22] Certain operations can only be performed at certain levels. Understanding has at its disposition the cognitive processes of the individual and her memory. When a new situation is encountered, the individual can draw on her understanding to answer a new question, to face a new situation, and to attempt to understand it. Individual understanding changes as the result of the individual's cognitive activity. Changing community knowledge, on the other hand, requires community communication; it requires a series of utterances in which a number of individuals give support to a given claim. Community knowledge is a social emergent phenomenon in which the individuals create knowledge by acting as filters, silencing some claims amplifying others. As individuals we are keen observers of social and linguistic clues and with more or less difficulty we pick up which claims are obvious, common, surprising, or challenging to a given community. No single individual can create or change community knowledge. The knowledge claim must be recognized as accepted by others before it becomes community knowledge.

The other significant difference between individual understanding and community knowledge is its generative property. Since community knowledge claims are limited both in context and in their scope of application, one cannot account for new phenomena or new context based solely on the accepted community knowl-

[22]Russell Ackoff is generally credited with proposing the data, information, knowledge, understanding, wisdom hierarchy. Russell L. Ackoff, "From Data to Wisdom," *Journal of Applied System Analysis* 16 (1989): 3–9. I do not fully endorse Ackoff's hierarchy, but I agree that each of the levels has different sets of properties and that the levels are in some relationship to each other.

edge. It is only individuals who can draw on their understanding that can generate new claims, which then can lead to creation of new community knowledge.

Knowledge held by a community is much more limited. Knowledge emerges through dialogue in a community. An utterance that is accepted as knowledge is only accepted to have its meaning in the specific context in which it was established. When the context changes the community no longer can be assured that this knowledge will still hold. For example, during the 1890s a consensus emerged on how to answer questions about the Earth's age—there was community knowledge. However, during the next decade new scientific developments, which neither falsified nor provided support for the previous knowledge, changed the context. There was nothing in the knowledge that could provide the answer as to the relevance of these new developments to the already established knowledge. Individuals had to draw on their understanding to make new utterances that then could be accepted as knowledge. As it happened, it took over two decades before new consensus knowledge emerged.

There is a difference between individual understanding and community knowledge, which could be described as private and public, however, such terms can be misleading. It is possible to have public and private separation of things of the same category. That is, there may be claims that are only expressed in private versus ones that are expressed in public. However, the fundamental difference between understanding and knowledge goes beyond that: full understanding is inexpressible. Individual understanding is the generative potential based on the individual's accumulated memories and cognitive reasoning to make new utterances. Community knowledge is the collection of the accepted usage of specific claims in specific contexts by a community. Or, without giving agency to the community, it is the imagined assumptions and expectations of a generic community member held by an actual community member. Faced with a never before seen situation, an individual drawing on her understanding can make a new utterance as an individual. She cannot make a new utterance as a community member—at least not without higher than usual risk of her utterance not being accepted as representing the community.

Based on the discussion above, we see how each utterance ought to be analyzed as being an expression of individual understanding *and* community knowledge. Some utterances will correspond more closely to expressions of individual understanding, while others will be an individual speaking as a community member. In the latter case, the individual will not necessarily speak what she believes (her individual understanding), but will speak as a representative saying what she believes the other community members are expecting her to say to represent them. The historical analysis of the dialogues on the Earth's age shows that many of the utterances included both, the individual speaking as himself and as a community member. The job of a historian, then, it is to disentangle the two (or more) voices in each utterance.

This theoretical background allows us to comprehend the dialogues on the Earth's age without the problems present in current historiography. The new account of the dialogues on the Earth's age is based on the recognition that each utterance is composed both of the individual speaking as himself and as a community member. The account that can be told about the dialogues on the question of the Earth's age is an account of the emergence, fall, and emergence again of community knowledge on the question of the Earth's age.

This analysis also suggests a classification of utterances by their audiences. If, as I argue, utterances are shaped by their addressees, then those addressees can be used to classify the utterances into three groups, utterances addressed to a specific individual who is a person, has a history, one whom the author knows (however well or not well); a community member who is not a real individual with a history and knowledge outside of the community knowledge; and a general audience who recognizes the speaker as a member of an expert community. Hence, sources such as conversations and letters (addressed to an individual), professional meetings, articles etc (addressed to a community member), and encyclopedia articles, textbooks, newspapers, and general addresses (addressed to a general audiences) need to be treated differently. What is said to one audience does not necessarily reveal what the same speaker would say to a different audience. This differentiation, as I will show, allows us to further clarify the history of the dialogues on the Earth's age.[23]

The account I present in this dissertation is of how scientists answered the question "How old is the Earth?" To tell this story we must know who is asking the question of whom. I investigate a number of possibilities: how did the individual answer it for himself, how did he answer it for the other investigator of the question of the Earth's age, how did he answer it for another scientist, and finally how did he answer it for non-scientists. By realizing that for each of those different audiences the answers were different, we can present a multi-level account, and tell how those different answers interacted with each other.

Finally, since the concept of a community is central to this account, I must address the question of what a community is in this approach. Two possibilities suggest themselves. A community may be those who share community knowledge (which leads to a seemingly circular conception of community knowledge as the knowledge shared by a community). The other option is to think of the community as the individuals who are accepted as members of a community, that is ones who are allowed to speak as community members. This second formulation highlights the dialogical nature of the community. Community is not something that

[23]Such classification also overcomes limitations of the professional/amateur dichotomy, which fails to account for the situation in American science at the turn of the 20th century.

exists absolutely—someone either is or is not a member of a community. One's membership is specified in relationship to the addressee. One is a member of a community if one is permitted to speak as a member of the community. However, an individual may be permitted to speak as a community member (representing a community) by some audiences and not by others (including the community itself). This opens a possibility for interesting situations when the members of the community do not accept an individual as one of their own, but outsiders do. The act of permitting, again, is dialogical. One is permitted to speak as a member of a community if there are no objections to him doing so or if the future replies of community members reply to him as if he were a community member. These responses can be directed by an individual or a group with a specific agenda, but they also can emerge without any premeditated motives.

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To help the reader understand my theoretical position, I situate my analysis with respect to two well-known approaches in science studies, Thomas Kuhn's and Bruno Latour's, which in parts bear resemblances to the view offered here.

There are similarities between Kuhn's view of scientific developments and the account I present. Kuhn's concept of a paradigm and my concept of community knowledge bear a degree of family resemblance.[24] We both agree that initial investigations on a given topic are relatively unconstrained and not grounded in commonly shared beliefs. The arguments are as much about establishing the proper methods and approach as about reporting specific findings. With the emergence of a paradigm, specific methods, research questions, and approaches are settled and a new user is indoctrinated by the study of textbooks. In his reports of research, he is no longer concerned with the legitimacy of the approach itself, but follows on an

[24]Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 3rd edition (Chicago and London: Chicago University Press, 1996).

already established method. I share with Kuhn the importance of the community, canonical text, education, and exemplars.

I differ from Kuhn by splitting the speaker into an individual and a community member and I take each utterance as resulting from both of those voices.[25] Just because the community seems to function as if all its members followed one paradigm, it does not mean that everyone believes all the tenets of the paradigm. This explains why I also do not see a fundamental difference between a time of normal science and revolutionary science. I agree with Kuhn that scientists are always confronted with anomalies and that those anomalies are often "shelved." However, I argue that even though the community may seem to ignore anomalies, at all times those anomalies are investigated by some individuals. The paradigm is constantly being challenged and what changes during revolutionary times is the group response to those challenges. The challenges are usually ignored, but from time to time some are picked up and produce revolutionary changes. The underlying process is constant (community members constantly evaluating whether or not to respond to a challenge), however, depending on the environment the challenges sometimes bring about revolutionary changes and other times do not.

Central to Kuhn's account of scientific change are disputes in which disagreement originates from individuals using the same vocabulary to refer to different groups of objects, that is, to classify objects into different similarity sets based on differing criteria. The debates about the Earth's age were not of that kind—it was not the case that scientists differed in their classification. Instead, there was too much data relevant to the question of the Earth's age and not all of it could be made compatible. The scientists had to decide which data were relevant and

[25] Consequently, I disagree that the individual does not need to be studied as suggested by Kuhn: "In this sense it is the community of specialists rather than its individual members that makes the effective decision. To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has vast fascination." *Idem*, p. 200.

needed to be accounted for and which data were of lesser importance and could be explained away. Different individuals often did perceive the Earth and its age to mean different things (for example, thinking of the Earth as a cooling sphere of rock or a thickness of the geological column), but even when they did so, they did not deny the validity of the other conceptions (Walcott recognized that the Earth was a cooling globe, but argued that it should be viewed as a geological column before being viewed as a cooling globe). Kuhn states that during a "communication breakdown" members of the differing groups "may [. . .] try to discover what the other would see and say when presented with a stimulus to which his own verbal response would be different."[26] For Kuhn, this process is a translation from one language to the language of the other. Before the process can take place a significant event of recognizing that the other is speaking a different language must take place. Kuhn sees a radical difference between normal and revolutionary science because for him this process of trying to imagine what the other would say only needs to take place during communication breakdown. This is in contrast to the view I advocate in which there is no strict dichotomy between communication success and communication failure. There is only the reply that the utterer must judge as being compatible with the other's understanding. But since in scientific communication the utterances (such as an academic article) are awaiting multiple responses there is always the possibility that the next reply will not be judged as compatible with the meaning accepted by others. I agree with Kuhn that within a group, the members may not agree what criteria they use to classify objects into groups as long as they divide them into the same groups. For Kuhn, those differences do not reveal themselves during normal science, but I claim that there is always some degree of miscommunication. A complex utterance such as a scientific article involves itself in many dialogues and responds to many things.

[26]Kuhn, *op. cit.*, p. 202.

Any response to it focuses only on a small part of the utterance and the many disagreements (or potential disagreements) do not go unnoticed, but are ignored.

Kuhn's view of meaning is based in a conceptual system in which individual concepts form a system that is unique at a given time and in a given community. In such a system, if the meaning of one of the concepts were changed, instantly, the whole system would become rearranged. However, the view advocated here argues that the meaning is based on the replies given in a dialogue and not on conceptual system. The effect of the changed use of a concept is not evident until the conversation occurs in which those differences demonstrate themselves as unexpected replies. Kuhn's concept of incommensurability is not very useful in my discussion. The consequences of a changed meaning only start to exist when dialogues take place. Some of those consequences may not exist until much later when the context of the dialogue changes. The two interlocutors may hold quite different meanings, but there may be no consequences of that differing meaning. It is the case that the two interlocutors hold different meanings (if they were to fully articulate their own meaning—something practically impossible since it is impossible to imagine all the uses of language), but it is also true that as long as the speed stays low, the Newtonian mass and the Einsteinian mass will be the same, to contrast with Kuhn's famous example.

The second comparison that will clarify my position involves Bruno Latour, who advocates shadowing scientists through society. Latour is interested in how scientific facts and knowledge are made and unmade. In his method, culture, logic, and mind are forbidden (or at least only permitted when everything else fails). In contrast, I present my account from the point of view of an individual and his mind. It is a different kind of a mind than the one objected to by Latour because, as Latour does, I reject rationality/irrationaity as adequate categories to describe historical events; the mind I describe is of an individual who desires to understand

and communicate. I agree with Latour that what goes on inside the other's mind is inaccessible, but I recognize that we share with all humans certain basic ways of confronting the world. In contrast to Latour, my focus is less on how scientific facts and knowledge get established by actors with agendas and more on how individuals cope with the complex world they encounter.

Latour and I start from different points and aim toward different goals. Along the way, however, we often travel similar paths. Some of the most important aspects of my approach are present in Latour, though the emphasis is often different. The importance of the addressee, the making of meaning by future replies rather than by the original author, are all points on which we agree. To get (the right) others to reply is more difficult than to get others to agree. The materiality of science and networks are things that I do not address in any detail, however, the importance of relationships among actors is central to both of our accounts. We share the notion that one often does not speak for oneself but acts as a "spokesperson." There is a similarity in the differentiation of one speaking as oneself (who holds personal desires) and as one representing a community (which holds different desires). But again, the difference is that I concentrate on the internal struggle of the individual as speaking as himself and speaking as a community member. I draw the distinction from the nature of language and not, as Latour does, from the point of view of individuals confronting the author of scientific claims.

In general, Latour's "facts" bear resemblance to my notion of community knowledge and he deals with the details of how those facts (this knowledge) are established much more thoroughly than I do. Latour himself criticizes the concept of "knowledge" as used by actors within a network as something objective and trumping subjective beliefs.[27] He rejects the use of the word "knowledge" as something

[27]Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass.: Harvard University Press, 1987), p. 182.

that is accumulated at the center of a network.[28] An interesting similarity, though one highlighting our different starting and ending points, is Latour's notion of "immutable and combinable mobiles." Latour stresses the need and import of the ability to move places and events (more precisely inscriptions which stand in their place) across distance and time in such a way that they will be unchanged, that is, usable by others and combinable with other such things (the other mobiles). The realization that some things can escape the context of their creations and that such things can travel across distance (allow one to act at a distance) is central to modern science, is similar to my description of community knowledge as a different category of comprehension, one which has the characteristics of being usable by different individuals with different understandings.[29]

The broader difference is that Latour's approach is more technical and sociological than mine, while mine is more existential and communal than his.

The big difference between both Latour's and Kuhn's work and mine is that they attempt to present a systematic approach to understanding science, whereas my ambitions are much more modest in this theoretical realm and more ambitious in the historical. My study is a historical study in which I attempt to comprehend the dialogues on the Earth's age in the context of one country in the span of forty years.

This study investigates only one historical case, and therefore, I do not have the grounds to make generalizations as to what brings about revolutionary changes. I suspect that each historical case is rather different and absolute generalizations are not likely to be found.

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[28] *Idem*, p. 220-3.
[29] *Idem*, pp. 223-8.

Dialogues on the Age of the Earth

This is not a history of the idea of "the age of the Earth;" this is a history of how members of a community answered a question "How old is the Earth?" This is not a history of ideas, but rather a history of replies. An idea is something that can exist independently of its creator, it can be stored and retrieved, it can be discovered and constructed, or even be dangerous. A reply is something that always has its author and its audience, a record of the reply may be stored and later retrieved, but the actual reply includes the intention and expectations of it author. That a reply from the past can often be comprehended (we assume), is a testament not to the nature of knowledge, but to our shared experiences with the individuals of the past. A reply is neither discovered or constructed, it is uttered.

Each utterance stands at an intersection of an individual understanding and community knowledge. Each utterance arises as an attempt by an individual to communicate her understanding. Each utterance is a combination of an individual speaking as herself and speaking as a community member. The task of a historian then is to identify those two voices in the utterance. To that end, the historian attempts to place the utterance at the intersection of two contexts, and one is the understanding of the individual. This context is formed over the lifetime of experiences and its investigation is a longitudinal study: one individual over the period of time. The second context is the community to which the utterance is addressed. This is a latitudinal investigation having in its scope the community at one particular point in time. Seeing an utterance at the intersection of those two contexts allows us to start to identify the two voices within the utterance.

This investigation is limited to the United States, however, the sources cited are not all from American authors but also include a number of Europeans who played an important role in the dialogues. I investigate the dialogues that took place in

American institutions and publications because the American side of the debates is particularly underinvestigated and this study is the first to consult various relevant archival collections. Furthermore, even though the American geologists were well versed in European literature they often wanted to challenge it (especially the 19th century explorers of the American far west). Finally, many European debates took on a different meaning in the American context. Starting in the late 1920s, however, the investigation of the Earth's age must be seen in a more global context.

The time period covered in this investigation is set by developments in investigations of the Earth's age and by the context of American science. One of the most cited and referred to articles in debates about the Earth's age, and an article that shaped the course of that dialogue for the next half century, was William Thomson's 1862 article "On the Secular Cooling of the Earth." The United States was engaged in the Civil War and as a sign of an increasing involvement of science in federal government the National Academy of Science was established in 1863.

The end of the period is again motivated by both developments in the scientific communities' comprehension of the Earth's age and of the profound change in American science brought on by World War II. The year 1931 is often used to mark the symbolic establishment of radiometric methods as the proper ways of investigating questions of geological time. Thus, my investigation ends with the first decade of the new consensus, before scientific work in America was again redirected toward the war effort.

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During the entire period covered in this study, the attitude of the historical actors toward the question of the Earth's age may be well represented by one reporter who, according to Alfred C. Lane, "was not content with a neatly typewritten interview, but wanted an estimate of the age of the Earth." The reporter wanted a number and was not much interested in how the number was obtained or what it meant.[30] To comprehend the Earth's age debates we must, unlike the reporter, look past the number and be willing to listen to what Lane and others wrote on the topic of the Earth's age. However, we must also remember that many audiences, like the reporter, only cared about the number.

Similarly, for most of the individual involved the question of the Earth's age was only a peripheral question in which they were interested only for a brief period of their career. This is not only evident in their publications but also through the lack of discussion of the Earth's age in the surviving manuscript collections of many of the individuals.

The first period, 1860–1900, is covered by the first three chapters. In chapter one, I present the discord of utterances made on the topic of the Earth's age in the second half of the 19th century. Not only did the different responses to the initial utterances create multiple meanings, but also the situation provided little guide for a new student desiring to make a reply. This was, nonetheless, the starting point for both the individuals and the community for the process of arriving at a consensus. I will provide analysis of two arguments central to understanding the debates that took place: William Thomson's argument from the cooling globe, and a geological argument from the rates of sedimentation. In chapter two I provide case studies of W J McGee and Clarence King, two scientists who in comprehending the question of the Earth's age, chose to satisfy their own understanding, that is, to be true to themselves. The way in which they approached the question, how they understood it, and how they answered it reflected their unique lives. In expressing their opinion on the Earth's age, they dealt with issues dear to them. The consequences of attempting to communicate this individual understanding

[30] Alfred Church Lane, "Measurement of Geologic Age by Atomic Disintegration," *Proceedings of the Lake Superior Mining Institute* 24 (1925), p. 114.

will be covered in the next chapter. Those first two chapters show the challenge resulting from the potential multiplicity of meanings of a given utterance faced by a community desiring a consensus.

The third chapter presents a successful attempt to form a shared answer to the question of how old the Earth is. During 1892–93 the American scientific community engaged in dialogue and accepted an address by Charles Walcott as representing its community knowledge. In the chapter, I describe the debates that took place, how the community responded to them, the address by Walcott, and the role of community knowledge in it. Finally, I provide evidence that Walcott's answer to the question "How old is the Earth?" was accepted by the community.

Chapters four and five cover the second period in which the established consensus community answer fragmented. First, I describe a series of scientific developments that made scientists reevaluate the adequacy of the answer established during the 1890s. Those developments were a new method for measuring the age of the oceans by John Joly, a new cosmogony proposed by T. C. Chamberlin and F. Moulton, and the consequences of the discovery of radioactive heat and radioactive decay. George F. Becker was one scientist who attempted to reevaluate the new evidence and provide a new consensus. However, his attempt was rejected by the community who did not accept it as representing a shared interpretation of the new scientific findings. During the 1910s, a number of independent methods of estimating Earth's age arose from multiple disciplinary approaches. In the early 1920s, the answer to the question of the Earth's age was that there were multiple ways of approaching it, originating in different disciplines. Those chapters are also a corrective to a belief which persists that the discovery of radioactivity immediately settled the dispute among the scientists. For example, even such a careful historian as Naomi Oreskes nevertheless makes the following statement: "The immediate result of these discoveries [of radioactive phenomena], besides capturing the attention and imagination of the world at large, was to settle geology's long-standing debate with Lord Kelvin over the age of the Earth."[31] Another historian, Gabriel Gohau, in his investigation of geology during the 19th century, recognizes that the change was not immediate: "Nevertheless, Kelvin's results did impress geologists who had become accustomed to longer durations. The controversy ended when John Joly published *Radioactivity in Geology* in 1909 and showed that the Earth's heat resulted not only from its initial heat, but also from heat produced by radioactivity."[32] However, I argue that even this date is too early. Historians focusing on the age of the Earth present a much more accurate picture in which the radioactive method was not generally accepted until the 1920s.[33] I hope that this work will provide even more details of how the radiometric methods emerged as the accepted methods for measurement of geologic time.

In chapter six, I discuss the emergence of a new consensus that happened on two parallel tracks. The first development, one well studied by historians, is the role of British geologist Arthur Holmes in providing technical details of a paradigm for using radioactive decay as a means of measuring geologic time. The second

[31]Naomi Oreskes, *The Rejection of Continental Drift* (New York: Oxford University Press, 1999), p. 48.

[32]Gabriel Gohau, *A History of Geology* (New Brunswick, N.J.: Rutgers University Press, 1990), p. 173.

[33]E.g, "The age of the Earth remained a subject of much confusion through the end of the [19th] century." "This confusion of numbers continued long into the twentieth century." Claude C. Jr. Albritton, *The Abyss of Time: Unraveling the Mystery of the Earth's Age* (San Francisco: Freeman, Cooper and Company, 1980), p. 199 But see "There was no consensus and geologists were wading through arguments over theories that appeared to have little foundation. Suddenly radioactivity provided one solution to this problem: heat" Patrick N. Wyse Jackson, *The Chronologers' Quest: The Search for the Age of the Earth* (New York: Cambridge University Press, 2006), p. 229. Janet Browne states that age of the Earth was the biggest obstacle that Charles Darwin thought he faced. She writes "Decades of continuing debate over the age of the Earth were resolved only with the discovery of radioactivity early in the twentieth century that, broadly speaking, allowed the Earth to be as old as evolution needed it to be." E. Janet Browne, *Charles Darwin: The Power of Place* (Princeton, N.J.: Princeton University Press, 2002), p. 315. Despite the nuances of Burchfield's account, historians seem to not notice that by the 1890s few people thought that age of the Earth was an objection to evolution or uniformitarian geology, and that the extra time afforded by evolution was not necessarily required, but also that the discovery did not settle the debates for quite some time to come.

part which has been largely omitted in histories, is the role of Alfred C. Lane, who organized the community of researchers which then carried out the program outlined by Holmes. Both of those individuals are set within the context of the National Research Council and its attempt to facilitate interdisciplinary work. I return to some of the theoretical considerations discussed this introduction in a brief conclusion.

Part I

19th Century

Chapter 1

Discord of Utterances

> "[T]he role of the *other* for whom the utterance is constructed is extremely great [...] The role of these others [...] is not of passive listeners, but of active participants in speech communication. From the very beginning, the speaker expects a response from them, an active responsive understanding. The entire utterance is constructed, as it were, in anticipation of encountering this response."
>
> M.M. Bakhtin, "The Problem of Speech Genres"

For Bakhtin meaning is an event. An event occurs at a single point in space and time. Meaning is established at the meeting of two interlocutors; meaning comes into existence when at utterance meets its rejoinder. Bakhtin finds sound metaphors useful. A sound is an event; a sound exists only in the here-and-now. A sound is different for each listener, listening from his unique place. Discord is a confused sound, an inharmonious combination of simultaneously sounded tones. I argue here that science is not disunified, but instead it is often discordant. This non-unity of science has its origin in the fact that in the sense of "meaning" needed to capture the encounters between individuals trying to communicate, no word can mean the same thing twice. Even if one utters the same word twice, the second time it already has a different meaning. The meaning depends on context, but the context is never the same. This non-unity arises from the myriad responses that each utterance may be offered, and, looking at it the other way around, from the myriad of possible responses that face each speaker as she is about to commit herself to the one response uttered.

The author addresses an utterance to a specific someone; the author expects that addressee to hold specific knowledge, to reply in a specific way. The listener, too, expects the author to hold specific knowledge, to say specific things. Any utterance always takes place between interlocutors and assumes a relationship between them. But the author does not control how his utterance will be rejoined. His effectiveness in communicating is based solely on his appropriate anticipation of the reply.

The early utterances in any dialogue are not conducive to understanding (except maybe the understanding of their authors), nor to communication. As with any other utterances they offer their listener a myriad of possible rejoinders (and through that a myriad possible meanings of the original utterance). But they also are not conducive to communication, because to communicate successfully the author must anticipate the reply by the listener and the listener must offer the anticipated reply. However, the first utterances on the topic of the Earth's age were new and the community did not know which rejoinders were anticipated. Consequently a discord of utterance followed.

In this chapter I present the discord of utterances made on the topic of the Earth's age in the second half of the nineteenth century. Not only did the different responses to the initial utterances create multiple meanings, but also the situation provided little guide for a new student desiring to make a reply. This was, nonetheless, the starting point for both the individual and the community for the process of arriving at a consensus. This process is the subject of the following chapters, but let us start from the point of view of a new student who desires to understand by attending to what has been uttered.

"On the Secular Cooling of the Earth"

Prior to the 1860s there was no sustained discussion about Earth's age. A number of individuals provided arguments as to how old is the Earth, but those were isolated instances. When during the second half of the 19th century a number of individuals started to partake in dialogues on the Earth's age, they were unconstrained in how those dialogues should proceed.[1]

The participants in the 19th century debates on the Earth's age aimed at creating a sustained dialogue in which all the interlocutors agreed on what was relevant to the dialogue, what needed a reply, what did not; what kind of rejoinder was acceptable and what kind was not. The utterances in the early discussion were not such that one could simply agree or disagree with them. They were complex utterances, encompassing many strands of scientific thought, making arguments not only about the age of the Earth and how to talk about the Earth's age, but also on a variety of other topics. The utterances did not agree or disagree along any single point. When American scientists engaged in the dialogues later in the century they were entering a dialogue already in progress. They were replying to the earlier British utterances. What were those earlier utterances? To what were the Americans replying? What was the relationship between the original utterance and its reply?

One of the most frequently referred to papers in the 19th century dialogues on the Earth's age was "On the Secular Cooling of the Earth" read in April 1862 by William Thompson to the Royal Society of Edinburgh. As such, it deserves a detailed look.[2]

[1]On the early attempts to answer the question of the Earth's age see Patrick N. Wyse Jackson, *The Chronologers' Quest: The Search for the Age of the Earth* (New York: Cambridge University Press, 2006); Claude C. Jr. Albritton, *The Abyss of Time: Unraveling the Mystery of the Earth's Age* (San Francisco: Freeman, Cooper and Company, 1980); Dennis R. Dean, "The Age of the Earth Controversy: Beginnings to Hutton," *Annals of Science* 38 (1981): 435–456.

[2]William Thomson, "On the Secular Cooling of the Earth," *Royal Society of Edinburgh Transactions* 23 (1862): 157–159. The analysis and page number given are to the reprint in William

In the very first sentence of his article, Thomson stated the motivation behind this paper:

> essential principles of Thermo-dynamics have been overlooked by those geologists who uncompromisingly oppose all paroxysmal hypothesis, and maintain not only that we have examples now before us, of the Earth, of all the different actions by which its crust has been modified in geological history, but that those actions have never, or have not on the whole, been more violent in past time than they are at present.[3]

Thomson's primary argument was against geological uniformitarianism—the theory that all forces throughout geological history have been the same ones as currently observed—because it violated principles of thermodynamics. The goal behind his calculations was to establish a specific relationship between geology and physics. Thomson's contention was that supporters of uniformitarianism were ignoring principles of thermodynamics, which if considered, had shown that the uniformitarian theory was unattainable. Uniformitarianism as proposed by Charles Lyell implied time without beginning with energy supplied by cyclical electro-chemical reactions; however, Thomson argued such theory violated second law of thermodynamics, which stated that the level of useful energy had to decrease over time. But even if the time were taken not as unbounded, even if it had a beginning, the uniformitarian theory required more time for completion of geological processes than Thomson's calculations showed was available.

The core of Thomson's argument was an assumption that the field of physics was primary to geology and if there ever existed a conflict between the two, geology should yield. This was evident through the main argument against uniformitari-

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Thomson, *Mathematical and Physical Papers Vol III: Elasticity, Heat, Electro-Magnetism* (London: C.J.Clay And Sons, 1890). Since in my project I am interested in American dialogues, I do not go into details of what prompted Thomson to engage in arguments about Earth's age, but only how his article was replied to. For excellent discussion of Thomson's interest in age of the Earth and the British public debates that surrounded it see Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (New York: Science History, 1975) and Crosbie Smith and M. Norton Wise, *Energy and Empire: William Thomson, Lord Kelvin* (Cambridge: Cambridge University Press, 1989).

[3] Thomson, *Mathematical and Physical Papers*, p. 295.

anism. This argument was demonstrated by calculations of the age of the Earth. The relationship between physics and geology, uniformitarianism and the Earth's age were topics that could have been, and often were, discussed independently. However Thomson argued that they should be discussed together. To discuss uniformitarianism required, Thomson pleaded, discussion of conservation of energy. To discuss the age of the Earth required the same. Thomson argued that thermodynamics, uniformitarianism, and the age of the Earth, were all related and were related in a specific way.[4]

This full argument included discussion of a number of other scientific topics; for example, cosmogony, heat transfer, and the internal composition of the Earth. The main goal, Thomson stated, was to establish the time of solidification of the Earth's crust (which was taken to be the beginning of geological time) based on the rate of increase of temperature with depth in Earth. He used a Laplacian cosmogony in which the Earth originated as a molten sphere of rock, which allowed him to use the Fourier equations for heat transfer. He used data on the temperature increase with depth from mine shafts as the data to be used in those equations. For his calculations to be valid, all of the heat transfer had to have taken place by conduction only (no convection was allowed) and Thomson spent significant part of the paper defending this proposition. In fact, he made an argument that the Earth was most likely entirely solid (therefore not capable of convection). In this way, he also entered into dialogue on the internal structure of the Earth as part of his argument. Simply put, the article was a complex intertwining of a legion of separate scientific investigations.

[4]During the 1860s and 70s Thomson presented two other arguments that argued for the same point by calculating the age of the Sun and age of the Earth by a different method, William Thomson, "On the Age of the Sun's Heat," *Macmillan's Magazine* (1862): 388–393 and William Thomson, "On Geological Time," *Transactions of the Geological Society of Glasgow* 3 (1871): 1–28. Also, relevant is Thomson's rejection to the Darwin's directionless, random, God-free theory of evolution, Smith and Wise, *op. cit.*, 637–41

How did this article look from a point of view of someone trying to reply to it? How was the meaning of this article established in the replies that followed? The core argument was replied to by a number of individuals who were defending uniformitarianism. The most famous of those arguments was by Thomas Henry Huxley, who accused Thomson of taking uniformitarianism as assuming unlimited time, whereas, Huxley claimed, most geologists at the time did not maintain that time was infinite, and agreed that the Earth, indeed, may be cooling down. Huxley also argued that the upper time limit provided by Thomson was not necessarily incompatible with uniformitarian geology.[5] Other individuals seconded Thomson's argument against uniformitarianism. In his criticism of Charles Darwin's *Origin of Species*, Fleeming Jenkin used Thomson's argument against the extreme time demands of the uniformitarian theory. Jenkin had serious reservations about the actual numerical results obtained by Thomson, but he thought that the overall thermodynamics-based argument against uniformitarianism was valid: "the estimates of geologists must yield before more accurate methods of computation" he stated.[6] A few decades later Clarence King repeated Thomson's calculations and argued that "the burden of proof [is] upon those who hold the vaguely vast age [of the Earth]"—uniformitarian geology was argued to be inadequate.[7]

To some scientists, the article was an application of Fourier's theory of conduction of heat to the question of the Earth's age.[8] In Fourier's theory the distribution of temperature is a function of time and depends on the initial distribution of heat. Thomson wrote

> The chief object of the present communication is to estimate from the known general increase of temperature in the Earth downwards, the date of the first

[5]Thomas Henry Huxley, "The Anniversary Address of the President," *Quarterly Journal of the Geological Society of London* 25 (1869).

[6]Fleeming Jenkin, "The Origin of Species," *North British Review* (1867), p. 295.

[7]Clarence King, "On the Age of the Earth," *American Journal of Science* 45 (1893), p. 20.

[8]Introduced in 1822 by the French mathematician and physicist Jean Baptiste Joseph Fourier (1768–1830) and already applied by Thomson in the 1850s.

> establishment of that *consistentior status*, which, according to Leibnitz's theory, is the initial date of all geological history.[9]

Thomson was going to use the known distribution of temperature at the present time to calculate the time required to achieve such a distribution from the moment a solid crust was first formed on top of the originally liquid Earth—the *consistentior status*.[10]

The current distribution of temperature was known from measurement of temperatures increases in mine shafts. Additionally, "[t]he fact that the temperature increases with the depth implies a continual loss of heat from the interior, by conduction outwards through or into the upper crust. Hence, since the upper crust does not become hotter from year to year, there must be a secular loss of heat from the whole Earth."[11] Thomson stated, however, that "we are very ignorant as to the effects of high temperatures in altering the conductivities and specific heats of rocks, and as to their latent heat of fusion."[12] Accounting for the range of possible values for that conductivity, the time of consolidation must have been somewhere between 20,000,000 and 400,000,000 years ago (98,000,000 being a very probable answer).

After the introductory paragraphs, Thomson turned to the discussion of how he calculated the estimates of the Earth's age just presented. "The mathematical theory on which these estimates are founded is very simple, being in fact merely an application of one of Fourier's elementary solutions to the problem of finding at any time the rate of variation of temperature from point to point."[13] The solution

[9]Thomson, *Mathematical and Physical Papers*, p. 297, emphasis in the original.

[10]The "Leibnitz's theory" refers to Pierre-Simon Laplace's cosmogony that postulated the Earth started as a sphere of molten rock. For most scholars the beginning of the Earth was the time when this liquid sphere solidified at the surface.

[11]Thomson, *Mathematical and Physical Papers*, p. 297.

[12]*Idem*, p. 300.

[13]*Idem*, pp. 300–301.

was represented by two questions:

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa t}}\epsilon^{\frac{x^2}{4\kappa t}}$$

$$v = v_o + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{\kappa t}}} dz \epsilon^{-z^2}$$

where κ was conductivity constant, V was half the difference of the initial temperature, v_0 was the mean of those temperatures, t was time, x was the distance of any point from the middle of the plain, and v was the temperature of the point x at time t (notice that the age of the Earth is represented in this equation by t). However, those equations specify the temperature distribution for a solid extending in infinite directions divided by an infinite plane with two different constant values on each side. Thomson argued that for any reasonable assumptions the effective temperature of the Earth's interior will only have an effect to a certain depth that is small in comparison to the Earth's radius, therefore, the curvature of the sphere can be ignored.[14] He calculated that there will be no sensible change in temperature at depths of more than 568 miles, compared with Earth's radius of 8,000 miles, for the first 1,000,000,000 years. In other words, he assumed the Earth at the beginning to be of uniform temperature and suddenly exposed to action that would keep its surface at a much lower temperature. This solution held valid for times much longer than calculated age of the Earth.

Thomson assumed the value 100,000,000 for t, calculated the functions describing the temperature and depth relationship as well as the rate of change of temperature with depth. He presented his results in a graph. The solution showed that the rate of change of temperature was rapidly diminishing with depth. Thomson used his result to argue that the terrestrial heat would not have an effect on

[14]This was a standard mathematical technique, and this simplification was not challenged by Thomson's critics.

the climate on the surface after the very brief initial period.

Already at this point, the argument presents many options in how to reply to it. For example, Peter G. Tait cited it as the answer to the question of "how long something like the present state of things has been going on the Earth's surface" and he cited Thomson as giving an answer from the internal heat of the Earth calculations. Tait summarized Thomson's argument as calculating from the heat increase with depth and applying appropriate mathematical equation the age of consolidation of the Earth's crust. Tait's conclusion, however, as to the time of consolidation of the Earth's crust is different from Thomson's. Tait stated it was ten, fifteen at most, millions years ago.[15] Clarence King, about whom much more later, obtained new data about the thermal properties of rocks about which Thomson worried, and repeated Thomson's calculation.[16] Negative arguments also could have been postulated to this reading and for example Thomson's student, John Perry, recalculated Thomson's calculation using different, but equally probable assumptions, arguing that the Earth's age could have been much more than Thomson suggested.[17]

In addition to specific mathematical calculations others have replied to the calculations by challenging the basic assumptions of the argument. Thomson's argument assumed that the Earth formed according to the nebular hypothesis that postulated that the planets were initially molten spheres of rock. T. C. Chamberlin challenged that entire hypothesis and so refuted Thomson's calculation. Chamberlin also challenged Thomson's argument simply as a mathematic argument: "There is, perhaps, no beguilement more insidious and dangerous than an elaborate and elegant mathematical process built upon unfortified premises."[18] This reply was

[15] Peter Guthrie Tait, *Lectures on Some Recent Advances in Physical Sciences* (London: Macmillan, 1876), pp.164-170.

[16] King, *Age of the Earth*.

[17] John Perry, "On the Age of the Earth," *Nature* 51 (1895): 224–227; John Perry, "The Age of the Earth," *Nature* 51 (1895): 582–585.

[18] Thomas Chrowder Chamberlin, "Lord Kelvin's Address on the Age of the Earth as an Abode

already presented by Thomas Henry Huxley and often repeated

> Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but, nevertheless, what you get depends upon what you put in; and as the grandest mill in the world will not extract wheat-flour from peascod, so pages of formulae will not get a definite result out of loose data.[19]

The most common way in which others responded to Thomson's argument was to simply take it as a calculation of the number of years since the beginning of the Earth, that is to cite the numerical answers given by Thomson without regard for how it was obtained. This was done both in specialized scientific publications, and in popular press; however, the citations were far from uniform sometimes using 98 or 100 million years, at other times 400 or 40 millions, less often presenting the actual range suggested by Thomson.

Those different replies to Thomson represent a wide range of possible responses, but they are limited to just the first part of his article. Once he gave the answer to how old the Earth is, Thomson was not finished with his argument, he still had to justify the assumptions that went into it. The rest of his paper answered potential challenges to the author's argument. The main challenge was that the Earth could not be presupposed to have been a uniformly heated solid at 7,000 degrees Fahrenheit. What if the Earth started as a liquid and started to solidify from the crust down? Thomson, however, argued that it was much more probable that the Earth originated as a solid of uniform temperature.

While a liquid mass was cooling, it would had been maintained at a thermal equilibrium through conductive currents regardless if the liquid were expanding or contracting during cooling. However, without more data on the behavior of earth's matter as it was cooling and solidifying (was it contracting or expanding), Thomson argued, it could not be known if the solidification would commence at the

Fitted for Life, I," *Science* 9 (1899), p. 890.

[19]Huxley, *op. cit.*

center or the surface. However, there was some experimental evidence to suggest that upon solidification the melted substances of the Earth do contract.

If that were the case, the nearly solidified crust would sink. It was possible that due to the viscosity of the liquid and other factors, portions of the crust could have achieved a considerable thickness before sinking. During sinking those crust fragments could have build up a honeycomb like structure that would have supported later-formed crust while leaving pockets of molten rock. Such theory was proposed by Hopkins and would account for a number of geological phenomena. However, a more probable case was that as the Earth solidified it did so in a continuous fashion from the inside out.

Thomson's arguments about the internal structure of the Earth were replied to. Osmond Fisher did not shy away from heavy use of mathematics and challenged Thomson's contention that the Earth's interior was rigid.[20] Another engagement with Thomson's argument was one of the first discussions of the age of the Earth by an American author. Clarence Dutton in his 1874 article "A Criticism upon the Contractional Hypothesis," gave a summary of Thomson's calculations of the Earth's age.[21] Along the way, he casually commented that the values used in Thomson's calculations were by no means known for certain. Dutton provided other reasonable values and he recalculated the age of the Earth to be in the range from 98 to 2,500 million years. However, Dutton was not principally interested in the Earth's age, instead he related Thomson's results about the Earth's interior to the contraction hypothesis. The contraction hypothesis explained the geological features—such as mountains—as a result of the Earth's contraction due to secular cooling (an analogy of withering apple was often used). Dutton was one of the first

[20] Osmond Fisher, *Physics of the Earth's Crust* (London: Macmillan, 1881); Osmond Fisher, "Rigidity Not to be Relied upon in Estimating the Earth's Age," *American Journal of Science* 45 (1893): 464–466.

[21] Clarence Edward Dutton, "A Criticism upon the Contractional Hypothesis," *American Journal of Science and Arts* 8 (1874): 113–123.

to criticize this popular theory and he used the results of Thomson's calculations to do so.[22]

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The main body of Thomson's paper could have been read as an application of Fourier heat transfer theory. For a spherically symmetrical object with given heat gradient the solution reduces to the solution of an infinite plane problem. The paper could have been read as contribution to cosmogonical discussion. Thomson discusses two different possibilities (a solid core with small bodies accretion or collision of two equally sized bodies). It could be read as a paper on the internal structure of the planet. Thomson did not simply combine the conclusions of those previous discussions to arrive at Earth's age; he extended them; he reached new conclusions.

Just from the analysis of this one article, we see that the meaning and possible replies were numerous and not necessarily related to each other. However, there were still other ways of getting at the Earth's age.[23]

Rates of Sedimentation and Denudation

Calculation of the Earth's age based on secular cooling of the Earth was not the only available method. Another very popular family of methods was based on the rates of sedimentation and denudation of geological formations. Those methods were based on a large number of suppositions. Which ones should be taken as justified and which ones should be reevaluated was not at all clear from the survey

[22]Dutton is remembered for proposing an alterative orogenic process: isostasy. For more on secular cooling and isostasy, see Naomi Oreskes, *The Rejection of Continental Drift* (New York: Oxford University Press, 1999), pp. 9–80.

[23]This multiplicity of meanings present in the article does not mean that all the meanings played an equal role in all places and at all times, they did not.

of literature. There were as many opinions as there were authors (and some authors offered multiple and contradictory opinions). This situation did not help in one's quest for understanding, nor did it help communication. To introduce those methods I present one of its first applications.

Two years before Thomson published his memoir on secular cooling of the Earth, a Scottish geologist John Phillips included in his book on life on the Earth a calculation of the Earth's age based on geological considerations.[24] In a section "Antiquity of the Earth" Phillips used the thickness of the geological column as a measure of geological time

> The Geological Scale of Time is founded on the series of the strata deposited in the ancient sea; if the forces tending to produce such deposits have always been productive of equal effects in equal times, the thicknesses of the strata are exact measures of the times; the thickness added in a certain historical time to the modern seabed, will bear the same proportion to the total thickness that has been added in geological time as the historical time ascertained to the geological time required. [...] Nothing can be simpler in aspect than the problem of the age of the stratified crust of the globe on the Uniformitarian hypothesis. We have only to find out the rate of accumulation of sediment in the sea—the thickness of deposits produced in a year, or century, or some long historic period—and apply this measure or rate to the ancient deposits.[25]

The basic idea was simple: if all of the strata that compose the height of the geological column formed from deposits in seas, and if we assume the rate at which those deposits were made has been constant throughout time, then if we measure the currently observable rate of deposition and the total thickness of all the strata then we can obtain the time it took to accumulate those deposits by a simple division. The condition of uniformity is crucial here. Phillips admitted that the rates differed from time to time and from place to place, but, he argued,

[24] Thomson wrote Phillips asking about the geologists' opinion on Darwin's calculation of the age of the Weald formation in the just published *On the Origin of Species.* Philips in his reply described his own research, therefore, it is possible that Thomson's suggestion of one hundred million years was influenced by Philips' calculation. Smith and Wise, *op. cit.*, pp. 561–565.

[25] John Phillips, *Life on the Earth: Its Origin and Succession* (London: Macmillan, 1860), p. 121, 124.

if we assumed that an average rate over some period of time and over the whole Earth could be taken to be relatively constant, we could still proceed with the calculation.

However,

> this proof requires that the proportion of land and water and atmosphere should be always cyclically the same, the land equally elevated, the water equally deep—that terrestrial climate should on the whole have been unchanged—atmospheric precipitations always equal in total effect—the surface of the globe always equally destructible, besides other conditions on which equality in the rate of deposition of sediments depends. Still, in spite of all these difficulties, the short measure of modern physical effects in a given time is the only standard to be applied to the immensity of past duration.[26]

There were many assumptions to be made and the result would necessarily be uncertain due to them, yet, Phillips stated, this was the only method available to measure the duration of geologic time. The rate, however, has never been constant. Some rivers have carried more deposits than others. Also, the area over which those deposits have been distributed was not entirely known. For simplicity's sake Phillips assumed that the area over which the deposits were deposited was equal to the area from which deposits were carried by a given river. A sample calculation for the river Ganges stated that the river drained 300,000 square miles and delivered to the Bay of Bengal 6,268,077,440 cubic feet of sediments that was equal to 1/111th of an inch of deposit. Since the thickness of the strata was assumed to be 72,000 feet, it would have taken more than 95 million years to deposit this thickness at this rate.

From this point on, Phillips speculated on the corrections that needed to be made to this calculation: the area of the sea over which deposits were made was less than assumed, therefore the period should have been less, but Ganges carried more deposits than most rivers so the period should be more. Shale deposits

[26] *Idem*, p. 122.

were formed at different rates than sandstone deposits. To illustrate the kind of uncertainty and corrections that needed to be made, Phillips considered coal formations in South Wales. Even though those formations were 12,000 feet thick, they were made in quiet waters: the deposits were not spread over vast areas of the sea floor, but, more like deposits in lakes, were all deposited in a much smaller delta. Additionally, according to Justus von Liebig in all plants the amount of carbon fixed was the same per area occupied by the plants: 10lbs for 244 square feet. If all of that carbon were converted to coal (which is 75 per cent carbon) it would have taken 127.5 years to deposit an inch of coal, Phillips calculated. Rather than 16 million years for the accumulation of those deposits, Phillips thought that 333,000 was more appropriate.

Another correction taken into account by Phillips was the fact that, based on the hypothesis of a cooling Earth, the temperatures were higher in the past and, consequently, so were the eroding powers of the atmosphere. Additionally in the initial stages of geologic time other processes made erosion more rapid, possibly then the rates had to be multiplied by a factor of four. This discussion might suggest that the duration for the geologic time was much shorter than initially calculated. However, just the opposite seemed to be the case. If, as assumed, the climate had cooled by 20 degrees and that cooling was solely due to the Earth itself cooling, the time required for this process seemed to be long. Phillips concluded "the period may not elude calculation, but it lies quite beyond the power of the mind to contemplate with steadiness."[27] Two years after Phillips published, Thomson would use his power of the mind to contemplate just that.

In his discussion of the antiquity of the Earth, Phillips brought together a number of suppositions based on a number of previous discussions on a variety of topics. The main feature was the centrality of the doctrine of uniformitarianism.

[27] *Idem*, pp. 137–8.

For Phillips, to speak about the antiquity of the Earth was to establish a relationship among the uniformitarian principle, the processes of erosion and deposition and the total thickness of geological strata. Each one of those aspects was a topic of its own, largely independent of each other. Phillips brought them together and attempted to specify a particular relationship among them. But his utterance was not simply using what had already been said, he contributed new claims on at least some of the topics. He discussed the increased rate of geological processes in the past. He discussed the rate of deposit formation. Even though his utterance as its main goal had a continuation of the dialogue on the Earth's age, it was also a continuation of dialogues on uniformitarianism, formation of sedimentary deposits and others.

The difficulty faced by a new student to the subject was not just picking between the two methods advanced by Thomson and Phillips. The difficulty was that there was as many variations of those methods as there were researchers using them. T. Mellard Reade used chemical denudation, rather than mechanical denudation. Reade calculated how much material was dissolved in water, rather than carried in the form of solid as Phillips and many another had done.[28] Alfred Russel Wallace argued that all the sediments were deposited near the shore, not evenly over the whole of the sea bottom.[29] James Croll opined that "This method, however, is worthless, because the rates that have been adopted are purely arbitrary."[30] Croll objected to any number of suppositions made during an application of the sedimentary method. He objected that the thickness of the strata was not mean thickness because strata get deposited very unevenly. He pointed out that

[28]T. Mellard Reade, *Chemical Denudation on Relation to Geological Time* (London: David Bogue, 1879).

[29]Alfred Russell Wallace, *Island Life: or the Phenomena and Causes of Insular Faunas and Floras, Including a Revision and Attempted Solution of the problem of Geological Climates* (London: Macmillan, 1880).

[30]James Croll, *Climate and Time in Their Geological Relations: A Theory of Secular Changes of the Earth's Climate* (New York: D. Appleton, 1875/1893), p. 360.

materials were redeposited multiple times, and that climactic changes have large effects on the rate at which materials get deposited. However, Croll believed that rates of denudation were more reliable. The reasoning in this method was the reverse of measuring sedimentation. Rather than measuring the rate at which formations were deposited, the denudation measures the rate at which geological formations are worn down, or denudated.

To add to the confusion other authors suggested yet different approaches or their combinations. Samuel Haughton combined calculations based on a cooling globe with measurement of thicknesses of sedimentary strata to make his estimate, along the way using palaeotological and climactic considerations. Haughton, complicating matters even more, addressed the issue of the Earth's time at three different occasions, reaching three radically different conclusions (100 million years for the age of the Earth; 2,298,000,000 for just a part of the Earth's history; and 153 million).[31] George Darwin, using tidal friction, calculated the time since the moon and the Earth had separated. Darwin's result (at least 57 million years ago) was often cited in support of Thomson's calculations; however, Darwin assumed that the Earth was viscous and non-rigid, exactly the opposite condition assumed by Thomson.[32]

A student desiring to make sense of this cacophony would be disappointed by looking in those various arguments for rejoinders to one another as there often were none. When authors commented on the work of others they addressed very specific issues and those comments did little to elucidate the whole discourse. The

[31] Samuel Haughton, *Manual of Geology* (London: Longman, Green, Longman, Roberts, & Green, 1865), Samuel Haughton, "A Geological Proof That the Changes in Climate on Past Times Were Not Due to Changes in Position of the Pole; With an Attempt to Assign a Minimal Limit to the Duration of Geological Time," *Nature* 18 (1878): 266–268.

[32] George H. Darwin, "On the Precession of a Viscous Spheroid, and on the Remote History of the the Earth," *Philosophical Transactions of the Royal Society of London* 170 (1879): 447–538. See also, Burchfield, *op. cit.*, pp. 112–115; David Kushner, "Sir George Darwin and a British School of Geophysics," *Osiris* 8 (1993): 196–223.

student might have looked for patterns in opinions, which methods suggested which conclusions, which group of scientists favored what judgments, but there were no patterns to be found.

The situation was further frustrated by the fact that many of the utterances that commented on the age of the Earth did so only tangentially while their primary focus was on another topic.

The Earth's Age in America

In America, scientists were familiar with the British arguments about the Earth's age; however, before 1890s they hardly ever spoke up on the topic. In the few instances in which American scientists did address the question of the Earth's age, they did so as a side matter and they did not have an intension of becoming involved the age of the Earth debates.[33]

The American dialogues mirrored the British with Simon Newcomb's calculation being based on physical considerations and James Dwight Dana's calculations of relative thicknesses of strata and Alexander Winchell's on the time of ice ages. The American dialogues also illustrate the diversity of beliefs held by the members of a scientific community.

One of the first discussions of the age of the Earth in America was the already mentioned article by Dutton.[34] This article is interesting beyond the fact that it demonstrates how easily the secular cooling calculations of the Earth's age could have been adjusted to obtain vastly different numerical answers. It is interesting because it illustrates the author's powerlessness in controlling how his utterance is used. Thomson's aim in producing his argument was to argue against unifor-

[33]For example, Reade stated that his 1876 address on geological time "excited considerable interest" in Britain and in America. T. Mellard Reade, "President's Address," *Proceedings of the Liverpool Geological Society* 3 (1874–1878), p. v.

[34]Dutton, *Criticism.*

mitarian theory; Dutton ignored that aspect of Thomson's argument, instead he used it to make a claim about mountain building processes—something in which Thomson did not have much interest. Each utterance speaks with many voices, some of which its author may not be even aware of. A subsequent interlocutor chooses which of those voices she will reply to and which ones she will ignore.

The calculation of sun's age by Simon Newcomb was another of the early American application of the use of thermodynamics to calculating age of planetary objects. Newcomb applied the contraction hypothesis in his 1878 textbook *Popular Astronomy* in which he also discussed the possibility of calculating the age of the Earth based on its rate of cooling, as Thomson had done. Unlike Thomson, thought, Newcomb concluded that the Earth's interior conditions, in particular the possibility of convective currents, as well as the addition of heat generated by Earth's contraction made the calculation impossible.[35]

In his highly influential *Manual of Geology*, James D. Dana stated that "Time is long" in support of this claim he brought on examples of calculations of the age of Niagara Falls based on its rate of recession and calculations based on the rate of growth of coral reefs. He stressed the uncertainly of the calculations based on rates of sedimentation and mentioned the limit imposed by William Thomson on the extremely long calculations by some geologists.[36] More significantly Dana calculated the relative length of geological ages. Based on the thicknesses and kinds of formations, Dana calculated that the ratios of the Paleozoic to the Mesozoic to the Cenozoic were 12:3:1. He also calculated ratios for the smaller divisions within the geological columns.[37]

The most extensive discussion of the duration of geological time was put forth

[35]Simon Newcomb, *Popular Astronomy* (New York: Harper, 1878), pp. 505–534.

[36]James Dwight Dana, *Manual of Geology,* 3rd edition (New York: Ivison, Blakeman, Taylo & Co., 1880), pp. 590–591, this section appears unchanged from the 2nd edition of 1875.

[37]Dana, *Manual of Geology*, pp. 380–381, 586.

by Alexander Winchell in his 1883 book on cosmogony. Winchell addressed the issue of the age of the Earth at two separate occasions. First when he refuted an objection made against the nebular hypothesis based on the fact that "The nebular theory does not admit as great an Age for the World as geology requires." The second occasion was a section in which he enumerated nines ways of calculating the age of the Earth. Winchell himself thought that measuring time since the end of the Glacial period was most reliable. Using the duration of the post-glacial time and the ratios of durations of geologic periods calculated by Dana he arrived at age of the Earth since encrustation to be three millions years, which, as he points out, was but a fraction of what many other geologists demanded. In his discussion Winchell explicitly deferred to the more accurate calculations based on the rigorous mathematics of the physicists and gave it as much credence as to geologists' calculations of the most recent geological, that is post-glacial, age.[38] Nevertheless, based on the calculation, he arrived at the three million years for the age of the Earth, which however was incompatible with the eighty million years that he erroneously cited that William Thomson had calculated.[39]

Robert Simpson Woodward, an American physicist and mathematician, investigated Thomson's calculations and was unsatisfied with it "although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness."[40] The problems are the unverified initial conditions and the constant of conductivity. Woodward did not have any more specific objection rather than the fact stated from the beginning that though Thomson's calculation was mathematically sound, the assumptions it made about the history of the Earth and the

[38]On the title page of his book *World-life, or Comparative Geology* featured William Thomson's quote "Geology in framing its conclusions is compelled to take into account the teachings of other sciences." Alexander Winchell, *World-life, or Comparative Geology* (Chicago: S.C. Griggs and Company, 1883).

[39]Winchell, *op. cit.*, pp. 179–181, 355–379.

[40]Robert Simpson Woodward, "Mathematical Theories of the Earth," *Proceedings of the American Association for the Advancement of Science* 38 (1890), p. 63.

data used were questionable.

In general, the little that was written on the topic of the Earth's age in America prior to the 1890s, exhibited similar discord to the British utterances on the subject.

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A few points deserve to be highlighted about the differences between the two main methods presented here. The sedimentation/denudation method took the uniformitarian principle as underlying assumption of the argument that made the calculation possible. After all, if the rate were not constant (or did not change in a predictable way), then calculations based on currently observed phenomena were pointless. In this case, then, from Thomson's point of view any demonstration that showed the Earth to be of an age greater than he calculated and attempted to use this calculation to prove that Thomson was wrong, was begging the question: uniformitarianism was assumed in the methods, therefore it could not be proved. Alternatively, we see that Thomson's calculation did not use any of the lines of evidence utilized in geological calculations. Thus the differences between the two approaches could not be evaluated based on better logic of argument, better use of data or methodology, they were two quite independent arguments. Finally, if the calculations of the Earth's age using the uniformitarian principle yielded an age the same or shorter than the age calculated by the cooling globe method, then the argument against uniformitarianism would be dismissed since it was based on the premise that the age of the Earth based on the cooling globe calculation was shorter than required by uniformitarianism, and therefore uniformitarianism was false.[41]

[41]Though it has little relevance to the arguments presented here, it must be noted, that the term uniformitarianism had many meanings and usages throughout the history of geology, often with the same author using the term to mean multiple things. See Roy Porter, "Charles Lyell

The meaning of the term 'Earth' was also vastly different in the two kinds of calculations. For Thomson the Earth was a cooling, uniform sphere of rock (which, in fact, could be approximated by an infinite plane). For Phillips and others using the geological methods, the Earth whose antiquity he was calculating was a geological column of a given thickness that needed to be built up over time. Even the vocabulary used was different. When Thomson spoke of "the age of the Earth," "age" referred to one of the variables in his heat transfer equations and the numerical answer obtained represented the amount of time required for the heat distribution to achieve the known state from the initial assumed state (the time of first solidification of the crust). Phillips spoke of the "antiquity of the Earth," many other geologists referred to it as "duration or length of geologic time;" for them, the numerical answer was the time required for the buildup of this column.

Geologists did not start routinely using the term "age of the Earth" until some years into the twentieth century. Their discussion of the Earth's age would more likely be indexed under terms such as "Geologic time, duration" or "Earth, Antiquity of." The term "age" had a specific meaning for geologists. It was used to specify a positioning along the geological column. Geological age was not the absolute age determined in years. The uses of terms like age, era, time, period were still being discussed during the middle third of 19th century and were only standardized by the end of the century.

With all those differences, the two utterances did have one thing in common. They both talked about the amount of time that had passed during which geological process formed the features of the planet currently observed. Also, most scientists did equate the formation of a solid crust with the beginning of geologic history. And despite their different conceptualizations, they both thought their

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and the Principle of the History of Geology," *British Journal for the History of Science* 9 (1976): 91–103; Martin J. S. Rudwick, *The Meaning of Fossils* (London: Macdonald, 1972); Stephen Jay Gould, "Is Uniformitarianism Necessary?," *American Journal of Science* 263 (1965).

calculations referred to the same physical object: the Earth on which they stood.

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The arguments offered on how to calculate Earth's age were complex. They brought together many different scientific topics and presented many ways in which to reply to them (either agreeing or disagreeing). The replies that were offered did not follow any single pattern. Different authors chose to reply to different parts of the arguments and they made little effort to state their positions in relation to the rejoinders of others who had already replied in a different manner. The result was a discord of utterances with nothing to guide a new student who wanted to gain knowledge by simply studying the arguments and the evidence.

Chapter 2

W J McGee, Clarence King, and Individual Understanding

> "Anguish then is the reflective apprehension of freedom by itself. In this sense it is meditation, for although it is immediate consciousness of itself, it arises from the negation of the appeals of the world. It appears at the moment that I disengage myself from the world where I have been engaged—in order to apprehend myself as a consciousness which possesses a pre-ontological comprehension of its essence and a pre-judicative sense of its possibilities." J.-P. Sartre, *Being and Nothingness*

Sartre, and other existentialists, saw as the source of anguish the recognition of one's own freedom to determine oneself, the freedom and responsibility not to take any values and knowledge from others. We do have the freedom to self-determine, but it is a freedom we do not exercise. We desire to understand and, Sartre tells us, we have the power to understand the world in ways that only we control, but, he warns, even contemplation of this power results in anguish. That is because we also have the desire to communicate, and here existentialists are silent. To communicate requires the others from whom we are trying to free ourselves. Our angst results not only from the fact that we do not trust our own will, but also from the fact that we know that by pursuing our own understanding, by becoming me-as-an-individual, we are giving up the ability to communicate with others. We become alienated. Thus the tragedy is having to choose between two options of either yielding to the desire to communicate and giving up the authenticity of being oneself, or becoming oneself, and giving up the ability to be one with the others. The extremes sharpened by philosophers appear more dulled in ordinary lives, but they did appear in the dialogues of scientists pursuing the answer to the question of the Earth's age. In this chapter, I provide cases of W J McGee and Clarence King, two scientists who in comprehending the question of the Earth's age, chose to satisfy their own understanding, to be true to themselves. The way in which they approached the question, how they understood it and how they answered, reflected their unique lives in this world. In expressing their opinion on the Earth's age, they dealt with issues dear to them. The consequences of attempting to communicate this individual understanding will be covered in the next chapter.

W J McGee

> In a quaint, old farmhouse in the lovely state of Iowa, near the city of Dubuque, in the county of the same name, there is a small library containing many old volumes some of which almost a hundred years ago came from a far-off land beyond the sea. Among these is the Good Book, sacred, not only on account of its being the inspired Word of God, but also on account of its containing the family record of births and deaths, the forms and faces of the departed seen only in dreamland.
>
> The fourth birth-date of the nine children born to James and Martha Ann Anderson McGee is this one: William John McGee, born April 17, 1853, near Farley Iowa.[1]

In this fairy-tale fashion, the sister of W J McGee (as he preferred to be called) opened a hagiography of her brother. McGee's life, just like anyone else's, had a unique trajectory. For his farm family and for the mid-west, McGee did not have much longing and he spent much of his adult life in Washington, D.C., first with the United States Geological Survey (USGS), next with the Bureau of American Ethnology (BAE) and finally with National Conservation Commission. His sense

[1]Emma R. McGee, *Life of W J McGee, Distinguished Geologist, Ethnologist, Anthropologist, Hydrologist, etc* (Farley, Ia.: Privately Printed, 1915), p. 9. McGee's wife, Anita Newcomb McGee, disputed the fact that there was any library at the house. Anita McGee to Gifford Pinchot. 5 June 1916. Box 9. General Correspondence. "Me-Pl" miscellaneous. WJM.

of self-importance was proportional to his physical figure. Often McGee felt he was the "power behind the throne" which at the USGS was probably an illusion, but at the BAE was the case. However, his administrative skills were severely lacking. Through his work in the Conservation movement he indeed seems to have had a profound influence. In conservation he found a science which benefited from his holistic view of the world, and where parts could not be analyzed in isolation, but only in relation to one another.[2]

McGee's formal education was limited to a few semesters at a rural school. The rest of his education came from his mother and his brother, and from self study that was assisted by his excellent memory. From an early age, McGee was an eccentric and not well suited for farm life. He tried a number of occupations including practicing law and blacksmithing, but he took to geology after learning surveying skills from his uncle. With those he proceeded to conduct an unsolicited geological survey of northern Iowa.

His first contact with the scientific establishment was in 1878 when a still bashful McGee attended the meeting of the American Association for the Advancement of Science (AAAS), held that year in St. Louis. There he met another first-time presenter, one J.E. Todd. Unlike McGee, Todd was educated in the East and was familiar with many of the American men of science to whom he introduced McGee. One of those was "Major" John Wesley Powell. Powell, himself a self-educated geologist from the mid-west, was always sympathetic to similar men and encouraged McGee.[3] From this meeting on, the scientific and intellectual career of McGee followed Major Powell for the rest of his life.

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[2]For biographical information see Washington Academy of Sciences, *The McGee Memorial Meeting of the Washington Academy of Sciences Held at the Carnegie Institution, Washington, D.C., December 5, 1913* (Baltimore: Williams & Wilkins Co., 1916), Nelson Horatio Darton, "Memoir of W J McGee," *Annals of the Association of American Geographers* 3 (1913): 103–110, Frederick Webb Hodge, "W J McGee," *American Anthropologist* 14 (1912): 683–687.

[3]J. E. Todd in Washington Academy of Sciences, *op. cit.*, p. 113; William Culp Darrah, *Powell of the Colorado* (Princeton, N.J.: Princeton University Press, 1951), p. 263.

Powell, who in 1881 was named the second director of the USGS, hired McGee as an assistant to the Survey and in 1883 McGee gained full employment. His first assignment was preparing a preliminary geological map of the entire United States. This task involved using available state and regional maps, each with its own scales, symbols, and nomenclature, to create one map with uniform standards.[4] The process of making different systems commensurate is the same one that McGee employed in his later calculations of the Earth's age. Between 1885 and the end of his tenure at the USGS in 1893, McGee worked on the geology of the Atlantic coastal plains and become the Geologist-in-Charge of the Potomac Division. Following Powell's resignation from the USGS, McGee followed him to the BAE where he became acting director. The years 1903–1905, McGee spent in St. Louis working with the Louisiana Purchase Exposition. His final years were spent back in Washington, where he worked for the National Conservation Commission. McGee died on September 4, 1912 alone in a room at the Cosmos Club.[5]

Interpreting McGee is not easy. Based on his achievements, one might think of him as a scientific leader. McGee at one time was the editor of *American Anthropologist*, *Bulletin of Geological Society of America*, and *National Geographic Magazine*. He was elected president of numerous scientific societies and was acting president of the AAAS. In 1893 McGee was elected to a Patron status of the

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[4]Darrah, *op. cit.*, p. 316.

[5]On treatment of McGee as an anthropologist see Curtis M. Hinsley, Jr, *The Development of a Profession: Anthropology in Washington D.C., 1846–1903* (Ph. D. diss.), University of Wisconsin-Madison, 1976; Curtis M. Hinsley, Jr, *Savages and Scientists: The Smithsonian Institution and the Development of American Anthropology 1846–1910* (Washington, D.C.: Smithsonian Institution Press, 1981); and W J McGee, *Trails to Tiburón: The 1894 and 1895 Field Diaries of W. J. McGee* (Tucson: University of Arizona Press, 2000). On the St. Louis exhibition see Robert W. Rydell, *All the World's a Fair: Vision of Empire at American International Expositions, 1876–1916* (Chicago: University of Chicago Press, 1984), pp. 160-7. On McGee and conservation see Whitney R. Cross, "WJ McGee and the Idea of Conservation," *Historian* 15 (1953): 148–62; Samuel Hays, *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890–1920* (Cambridge: Harvard University Press, 1959), pp. 102–105; John R. Ross, "Man over Nature: Origins of the Conservation Movement," *American Studies* 16 (1975): 49–62.

Geological Society of America, an honorary status bestowed on only four members in the society's history.[6] He occupied high positions within USGS, BEA and the St. Louis Exposition. Gifford Pinchot referred to him as the "scientific brains of the Conservation movement."[7] To commemorate his death, a symposium was held at the Carnegie Institution at which many of the Washington scientists spoke admiringly about McGee and were joined by others from around the nation in a publication resulting from the meeting. At the same time, many of his contemporaries were very critical of him.

There is no full biographical treatment of McGee. When historians do encounter him, he is generally given a positive treatment in histories of the conservation movement, and a much harsher one by historians of geology.[8] A survey of biographies of Major Powell provides a spectrum of the treatments McGee has received. They range from a sympathetic one by Darrah, to near total omission by Stegner, to the dismissive tone in Worster who referred to McGee as "self taught farm boy" who "assumed the air of great importance" and whose bubble needed to be pricked.[9] This spectrum of opinions was reflected by his contemporaries, some of whom, like Powell and Pinchot, held high opinions of him, while others like Samuel Langley had little positive and much negative to say about McGee. A peer evaluation reflects this spectrum

> The Director [Powell] gauges men rather on their own opinions of themselves than by that which the experienced geologists would have of them, and as McGee's strongest characteristic is unlimited confidence in his own ability, he had been advanced in a most surprising way, and although possessing undoubted natural ability, through want of control and of balance acquired by previous study and familiarity with the work of older geologists, he has

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[6] Edwin B. Eckel, *The Geological Society of America: Life History of a Learned Society* (Boulder, Col.: Geological Society of America, 1982), p. 32.

[7] Washington Academy of Sciences, *op. cit.*, p. 21.

[8] For example, Cross, *op. cit.*; Hays, *op. cit.*, pp. 102–105; George H. Daniels, *Science in American Society: A Social History* (New York: Alfred A. Knopf, 1971), pp. 298-301 Ross, *op. cit.*

[9] Darrah, *op. cit.*; Wallace Stegner, *Beyond the Hundredth Meridian* (Boston: Houghton Mifflin, 1953); Donald Worster, *A River Running West: The Life and Times of John Wesley Powell* (New York and Oxford: Oxford University Press, 2001), pp. 490, 536–538.

> made many mistakes and brought discredit on the Survey in some case where he represented it [. . .] He is a zealous worker, but hasty and impulsive. In his writings he is so fond of using unusual words peculiar to himself, that he becomes unintelligible.[10]

The cause of this disparity, I think, is the result of both historical and current unease as to what is good science and, by extension, a good scientist. It cannot be denied that McGee was arrogant and had delusions of grandeur. He was a terrible administrator, and he lacked the skills required to maneuver in the highly political Washington scientific establishment. At the same time, he achieved much success and recognition for his scientific work. It is his career in the capital city that amplified both views of McGee. Through the patronage of Powell, but also through his own effort, McGee did achieve significant power and influence in American science, though it may have not been as much as he thought. But his "air of self importance" and his administrative incompetence were also under the magnifying glass in busy Washington. His access to many journals and the Government Printing Office made it possible for McGee's scientific work to be widely distributed.[11] However, his power as the editor of *American Anthropologist* made it also possible for him to call another geologist in print a "betinseled [sic] charlatan whose potions are poison. Would that science might be well rid of such harpies."[12] Just as the Progressives in general viewed from the perspective of the early 21st century, seem to present a Janus face of things we now approve of and others we do not, McGee aimed to control natural resources under the guiding hand of an expert for soci-

[10]Quoted in Ellis L. Yochelson, *Charles Doolittle Walcott, Paleontologist* (Kent, Ohio: Kent University Press, 1998), pp. 272–3.

[11]A full bibliography of works by McGee can be found in McGee, *Trails to Tiburón*, pp. 111–125.

[12]W J McGee, "Man in the Glacial Period," *American Anthropologist* 5 (1893), p. 95. He expressed reservations in much milder language used in another review: "the world would be wiser if the book were not written" McGee to R.D. Salisbury. 20 November 1892. Box 24, Letterbooks 1891, Oct.-1893, June. WJM; W J McGee, "Man in the Glacial Period," *Science* 20 (1892): 317. The scientist with whom McGee disagreed was George F. Wright, see Ronald L. Numbers, "George Frederick Wright: From Christian Darwinist to Fundamentalist," *Isis* 79 (1988): 624–45.

ety's benefit, while at the same time promoting the superiority of the white man. Finally, much of McGee's science itself was appreciated by his contemporaries. His work in northern Iowa and in the Atlantic plains continued to be referenced for decades.[13] However, general approach often resulted in broad generalizations and philosophical discussion bothered many of his contemporaries. "His own inquiries were largely directed by his philosophic interest in principles, and the most characteristic feature of his own researches is the relatively slight basis of observation for far-reaching deduction" commented Franz Boas.[14] Such views were fast falling out of fashion, as was McGee's intellectual world overall.

As Edward Rafferty noticed about McGee colleague Lester Frank Ward: "he inhabited and worked in a lost intellectual world, the community of reformers and scientists who built the government research bureaus and scientific associations of the capital city in the late-nineteenth century Washington, D.C."[15] McGee occupied a world that does not fit into the narratives of professionalization, specialization, and the research university. Ward, McGee, and Powell had little (if any) college education (none of it in Europe). They did not work in universities, and neither taught nor trained graduate students. Their interests were not in creating ever more specialized approaches to specific problems, but rather taking their general outlook on the world and applying it to whatever problems seemed fit.

The philosophy of science shared by Ward, Powell, and McGee was a monistic positivism. Each man arrived at the USGS with their own view of science, and they all had different opinions of the details and even criticized the others' views. However, the similarities developed over years of close collaboration and long hours

[13]Jonathan M. Harbor, "W. J. McGee on Glacial Erosion Laws and the Development of Glacial Valleys," *Journal of Glaciology* 35 (1989): 419–425 positively analyzes McGee's thinking on glacial erosion form the point of view of current scientific understanding.

[14]Washington Academy of Sciences, *op. cit.*, p. 20.

[15]Edward Charles Rafferty, *Apostle of Human Progress: Lester Frank Ward and American Political Thought, 1841-1913* (Rowman & Littlefield Publishers, 2003), p. 7.

of philosophical discussion during work and afterwards at the Cosmos Club or at evening gatherings at Powell's house. None of them could be classified as practicing any single branch of science. They engaged themselves in geology, anthropology, ethnology, sociology, philosophy among other fields. Ward completed his *Dynamic Sociology*, with Powell's approval, while working as a paleontologist for the Geological Survey. Powell was the director of both the USGS and the Bureau of American Ethnology. McGee was a member of innumerable scientific associations in fields ranging from chemistry to psychic research. Unsurprisingly, they all believed that the highest goal for science was to systematize. They sought to see the world holistically and they searched for underlying patterns in the complexity of the world around them.[16]

This underlying philosophy can be seen in McGee's contemplation of the Earth's age. There was another member of the Great Basin Mess, an exclusive lunch club run by Powell, with whom McGee shared views related to geological time: Grove Karl Gilbert. Unlike Ward, Powell, and McGee, Gilbert did not enjoy philosophizing. Gilbert's science had more in common with engineering than philosophy and was based in empirical observation and Newtonian mechanics. However, when McGee presented his argument for the age of the Earth, his methodology and even vocabulary bore a striking resemblance to Gilbert's writings. For Gilbert, the organizing theme for understanding science was a rhythmic cycle and he used such cycles in his understanding of historical geology.[17] Rhythmic cycles were also at

[16]See for example, *Idem*; Gillis Harp, *Positivist Republic: Auguste Comte and the Reconstruction of American Liberalism, 1865-1920* (University Park. Penn: Pennsylvania State University Press, 1996), pp. 109–154; Michael Lacey, "The World of the Bureaus: Government and the Positivist Project in the Late Nineteenth Century," in Michael Lacey and Mary O. Furner (eds.), *The State and Social Investigation in Britain and the United States* (Cambridge: Cambridge University Press, 1993): 127–170; Worster, *op. cit.*, pp. 383–466; John Joseph Zernel, *John Wesley Powell: Science and Reform in a Positive Context* (Ph. D. diss.), Oregon State University, 1983. On the holistic view of nature in 19th century America, see Aaron Sachs, *The Humboldt Current: Nineteenth-Century Exploration and the Roots of American Environmentalism* (New York: Viking, 2006).

[17]On Gilbert and geological time see Stephen J. Pyne, *Grove Karl Gilbert: A Great Engine of*

the core of McGee's argument. Gilbert spoke of different time measures and making them commensurable: "Over and over again the attempt has been made to link together the two chronologies, to obtain for the geologic units some satisfactory expression in the units of human history"[18] and "Human history is relatively so short, and its units of centuries and years are so exceedingly brief, that the two orders of time [historical and geological] are hardly commensurate."[19] McGee also talked about the problem of incommensurability: "In this way the supposition that the climate of the Earth and the relative positions of the cosmic bodies are inter-related is again strengthened, and at the same time another conjunction between incommensurable cycles is afforded."[20] McGee used the concept of a "factor of safety," an engineering concept that Gilbert introduced in his geological writings.[21] Overall, the comparison between McGee's and Gilbert's papers on the Earth's age shows striking similarities in approach. Both men listed the different cyclical ways that are used to measure passage of time. Both claimed that the marks of astronomical cycles are the most likely to give profitable results. Yet the differences between the two are even more striking; each man's utterances reflects strictly individual understandings and concerns.[22]

By the time McGee read his comparative chronology address in 1892, he and Gilbert had worked together at the USGS for a decade. Gilbert addressed the question of the Earth's age eight years later, however, it seems probable that the main idea of using rhythmic cycles to study geological time was Gilbert's. It is an underlying theme in many other of his works, whereas McGee usually presented

Research (Austin: University of Texas Press, 1980), pp. 145–152.

[18]Grove Karl Gilbert, "History of the Niagara River," *Annual Report of the Commissioners of State Reservation at Niagara* (1889), p. 79.

[19]*Idem*, p. 76.

[20]W J McGee, "Comparative Chronology," *American Anthropologist* 5 (1892), p. 338.

[21]E.g., Grove Karl Gilbert, *Lake Bonneville* (Washington, D.C.: United States Geological Survey, 1890), p. 380.

[22]Grove Karl Gilbert, "Rhythms and Geologic Time," *Science* 11 (1900).

time as a progressive arrow. The point, however, is not who originated the concept, but that undoubtedly the two of them talked about the same notions, that both of them responded to the same utterances made during a common dialogue, and that both of them came to understand geologic time differently.

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The age of the Earth was of no great consequence to W J McGee. His work concentrated on recent geological ages. However, his philosophy of science led him to engage in the age of the Earth issue in two distinct ways. First, it led him to calculate his own estimate of the age of the Earth as part of an effort to correlate and generalize knowledge about chronologies. Second, it led him to reject calculations of the age of the Earth as a cooling globe. McGee's understanding of the age of the Earth question reflected his unique life of education, work and scientific interest. His approach to the Earth's age made sense from McGee's unique way of understanding the world, but his contemporaries, who did not share his experiences and beliefs, misunderstood him.

There were three modes of obtaining empirical knowledge, according to McGee's positivist philosophy of science. The most trustworthy was direct observation. Next in line was inference by homology, that is reasoning by comparing like things, which led to generalization. The third, least desirable, mode was inference by analogy, or comparison of unlike things that led to the multiplication of hypotheses. McGee thought there was also a fourth mode that was used not as much for obtaining as for organizing empirical knowledge. This fourth mode, which he called homogenic inference, was the culmination of the evolution of scientific reasoning. This was reasoning by identifying a genetic series or chain of causations. Darwin triumphed by achieving this level of knowledge and McGee strove for it as well.[23]

[23] W J McGee, "The Extension of Uniformitarianism to Deformation," *Bulletin of the Geological Society of America* 6 (1894), pp. 62–65.

That McGee lived what he preached is evident from the two arguments about the Earth's age he made. In 1893, he published a negative argument demonstrating that the calculations of the age of the Earth based on physical and astronomical methods were highly problematic. He stated "[the] geologic estimates concerning the age of the Earth are based on real processes and actually observed conditions."[24] While in the physical and astronomical estimates of the age, the Earth's behavior "appear[s] to be assumed analogous to that of a heated spheroid immersed in ocean, and cooling at a rate determined by relative temperatures of spheroid and water."[25] From McGee's classification of scientific reasoning, the geological method based on observation was far superior to the physical one based on inference from analogy.

Furthermore, the physical calculations were based on two assumptions: that the planet was homogeneous and that it was simple in structure. McGee argued that there was positive evidence showing both of those assumptions to be false. Terrestrial heterogeneity (the fact that the Earth has oceans and the atmosphere that trap solar energy and determine the climate) meant the Earth's temperatures were influenced much more by the energy from the sun than by the internal heat energy. The complexity of the Earth's composition, especially its chemical constitution further nullified the physical argument. Therefore, the analogy that the Earth was like a homogeneous spheroid was invalid; the two are different. McGee summarized his argument:

> The geologic estimates of the age of the Earth are based on direct observations under actual conditions so fully known, that, although certain factors are variable, all may be safely assumed to be known; while the factors involved in the non-geologic estimates—surface and sub-surface temperatures, thickness of the Earth-crust, properties and conditions of rocks, etc.—must be furnished by the geologists, so that, at the best, such estimates represent nothing more than the grist ground from a mathematical mill; and, more-

[24] W J McGee, "Note on the 'Age of the Earth'," *Science* 21 (1893), p. 310.
[25] *Idem*.

> over, it usually happens that unknown factors are introduced to give texture to the product, but which, at the same time, so far adulterate the grist as seriously to affect its value.[26]

The positive argument for the Earth's age McGee presented at the 1892 meeting of the AAAS in Rochester, N.Y. The argument appeared in an anthropological paper titled "Comparative Chronology" read to the Anthropological section of the meeting. In the paper McGee engaged a then-popular question of the antiquity of the human race by enumerating all the ways of measuring time, geological time being one of them, and attempting to correlate them, ideally in a genetic series, all with the goal of calculating the age of homo sapiens. He concluded that the various chronologies must all be used simultaneously to arrive at the time of man's origin.[27]

In this paper, after expounding astronomical, historical and biological ways of measuring time, McGee discussed geological time measures. McGee explained that geologists had estimated the relative ages of different geological periods with some confidence. If only the length of one of them could be estimated in years, or if it could be correlated with a cycle of known duration, the absolute ages for all the other periods could easily be calculated. McGee argued that the most recent periods are the ones about which geologists know the most, including the probable time since the peak of the last ice age 7,800 years ago. This was close to the 7,100 years since the middle of the last Platonic winter, a condition resulting from one of the astronomical cycles when the winter in the northern hemisphere occurs when the Earth's position on its orbit is farthest away from the sun. The coincidence, then, of the ice ages and the Platonic winter afforded McGee an opportunity to relate the natural time-units of the astronomer with the semi-arbitrary units of the geologist. Consequently, he concluded that the peak of the glacial period was

[26] *Idem.*
[27] McGee, *Comparative Chronology.*

7,100 years ago.

He performed a similar kind of correlation of another geological formation with a longer astronomical cycle that gave him duration in years of another Quaternary geological formation. As a next step, he used the ratio of lengths of geological periods to arrive at the age of the oldest one of them. Allowing for some errors, McGee concluded that the age of the Earth is in the range somewhere between 20 million and 15 *million* million years with a mean estimate of 15,000 million years.

At the end of the paper, McGee discussed dating the origin of mankind. It is at this point that the reason for the previous discussion comes into focus. Anthropology lacked its own chronology, but it could be correlated with other chronologies like the geological and the astronomical. For example, from anthropological evidence it was known that an astronomical period of 1,461 years had been known in Egypt five millennia ago. McGee believed that for primitive peoples such as ancient Egyptians to have known such a long cycle, they must have observed it multiple times, leading him to conclude that the origin of homo sapiens must have been at least 10,000 years ago, maybe even 30,000. However, this fact had to be reconciled with the chronology from cosmic cycles and with the geological record. If Europe and North America were covered by glaciers until 7000 years ago and homo sapiens existed 20,000 years ago, this meant that modern man must have originally existed in Asia or even Africa, a difficult proposition to accept for a white, progressive scientist. However, if the glacial period was pushed back farther in time, the age of the Earth became outrageously long. The evidence from all the chronologies had to be considered concurrently to arrive at an answer; that is, one could not push back the date of ice ages without considering the age of the Earth, and one could not calculate duration of geological ages without considering astronomical and anthropological data.

The final age of the Earth was of no great consequence to McGee. "These general estimates are indefinite, and the minima, mean, and maxima are alike unworthy of final acceptance [. . .] as far as the science of geology is concerned, the maximum estimate is quite as probable as the minimum, while the mean is much more probable than either."[28] In his "Comparative Chronology" paper McGee committed an "arithmetic error" and reported the final numerical estimates of the Earth three times as large. The fact that he only nonchalantly corrected this error in a footnote of his second paper on the topic, further highlights the fact that he did not attach great significance to the numerical results. In fact, McGee believed that by offering such a wide range of possible values he would appease all of his contemporaries whose results fit within that range.

In his age of the Earth papers, McGee stayed true to his philosophy, he argued for unified science and he aimed to unify the various chronologies with causal relationships. McGee's argument for age of the Earth shows how his individual life experience, his education, and his intellectual companionship are reflected. McGee combined a unique mix of features resulting from his life experiences, his broad self education, the philosophical thought of Powell and Ward, the rhythmic view of time of Gilbert. The age of the Earth argument represents this unique individual understanding. The argument also presents his individuality by the way it differentiates itself from the views on which McGee drew. Even if the arguments of McGee, Ward, Powell, and Gilbert had many similarities, McGee not only arranged the combination of those influences in a unique way, but even the arguments themselves were very distinct.

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McGee's last publication appeared posthumously. It was a short article describing the symptomatic development of his cancer in which the author listed a

[28]McGee, *Age of the Earth*, p. 310.

series of incidents that had taken place over the last seventeen years of his life. At first, the sufferer had not perceived the connection between the isolated ailments until finally in April 1911, after a surgery, the true common cause of the discrete symptoms was discovered. McGee concluded his report "Any significance this record may have lies merely in bringing out the associations between a series of obscure and puzzling symptoms developed in the course of several years, which finally seem to have found explanation in the cancerous growth revealed will toward the end of the series."[29] One can fancy, McGee's satisfaction from being able to infer the connection of the initially unrelated phenomena, to offer that most evolved of scientific inferences one from a genetic series. The mere fact of death being footnoted.[30]

Clarence King

> I imagine that in comparing our impression of him, the thought which come uppermost in the minds of all of us, is that Clarence King resembled no one else whom we have ever known.[31]

So remembered Clarence King, his friend John Hay.

On January 6, 1842, his mother birthed him and christened him Clarence Rivers King. His father was in China, on business. He was engaged in trade and the family was well off; however, he died when Clarence was six. His mother took young Clarence to various part of New England for different stages of his schooling commencing his appreciation of arts and literature. His mother remarried, Clarence

[29]W J McGee, "Symptomatic Development of Cancer ," *Science* 36 (1912), p. 350.

[30]McGee's estranged wife, Anita Newcomb McGee after his death pleaded to Gifford Pinchot "I sincerely hope that you will not repeat statements made in an article he wrote about the history of his illness, as I have been greatly distressed by its contents. [...] Doubtless you know him well enough to understand that his mental processes obliged him to find a cause and logical sequence for events, and in his later years his previously excellent memory played him many strange tricks." McGee to Pinchot, 5 June 1916. Box 9 General Correspondence. "Me-Pl" miscellaneous. WJM.

[31]John Hay, "Clarence King," in James D Hague (ed.), *Clarence King Memoirs: The Helmet of Mambrino* (1904), p. 131.

found a job in the business world. But he was not happy with it.[32]

In 1860 King entered the recently opened Yale's Sheffield Scientific School, which the student of Yale proper looked down upon (probably with good reason, as even a high school diploma was not necessary for acceptance—well for King, as he did not have such a diploma). Two years later he completed a course in chemistry, taking along the way classes in other sciences including some physics and even a semester in geology taught by James Dwight Dana.

In the fall of 1862, King listened to Louis Agassiz lecturing on the glacial hypothesis. Agassiz disagreed with the new theory proposed by Darwin and the older theory proposed by Lyell and instead he viewed Earth's history as filled with catastrophic changes of which glaciation was the most recent example. This lecture had a lasting effect on King and its conclusions were reinforced by his next adventure. In the summer of 1863 (after spending the spring in New York organizing a new art movement) King was off to California to join a geologic survey being conducted there by Josiah Dwight Whitney. From Whitney and his assistant William Brewer, King obtained his geological training and from the exploration of the Sierra Nevada he obtained the conviction that the uniformitarian geology taught to him by Dana was insufficient to understand the observed geological formations. The remnants of monumental volcanic eruptions which King observed on his very first expedition under Whitney was evidence for him of a discontinuity of geological processes.

Under the authority of the U.S. Congress, between 1867 and 1872 the twenty-six year old King led his own survey along the 40th parallel assisted by James D. Hague and Samuel Franklin Emmons, both of whom had more academic educa-

[32]The biographical sketch is after Thurman Wilkins, *Clarence King: A Biography*, 2nd revised and enlarged edition (Albuquerque: University of New Mexico Press, 1988). For an account of King's life which centers on his personal life, see Martha A. Sandweiss, *Passing Strange: A Gilded Age Tale of Love and Deception across the Color Line* (New York: Penguin Press, 2009).

tion in geology than did King and from whom he also learned.[33] His exploration of American West still more reinforced the notions that the West was unlike the East, and the textbook geology which described the British Isles, European continent and the Appalachians was not adequate to describe this new land. The Uinta Mountains, the peaks of which once towered higher than the Himalayas do today, had been reduced to half their height. The glaciers had chiseled out huge canyons in the past. And, disproving the general belief that there were no glaciers left in the United States, King stood on top of one flowing down Mt. Shasta—three miles long, a "shattered chaos of blue rock."[34] The publications resulting from the 40th parallel survey established King as one of the top American scientists, fact testified to by his 1876 election into the National Academy of Science as its youngest member.[35] However, King was never interested in the community of scientists. He hardly ever attended the meetings of the societies to which membership he was elected and never contributed to their proceedings. King took his final official scientific post in 1879 as the first director of the newly established United States Geological Survey; a post which he held for only two years. The 1880s and 90s were a time when King attempted to amass a fortune based on his knowledge of geology. Yet, he had not abounded the pursuit of science, but he was no longer satisfied with what it offered him towards the understanding of the world in which he lived.

Clarence King died alone on December 24, 1901 in Arizona.

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By 1877 King was one of the most famous American scientists and was invited to give an address at the commencement at the Sheffield School. The address was the

[33]Formally known as the U.S. Geological Exploration of the Fortieth Parallel.

[34]Cited in Wilkins, *op. cit.*, p. 143.

[35]King's two books were Clarence King, *Mountaineering in the Sierra Nevada* (Boston: Osgood & Co., 1872) and Clarence King, *Systematic Geology* (Washington: Government Printing Office, 1878).

first public occasion on which King argued for a modified catastrophism, a view for which the later age of the Earth calculation would serve as evidence. The address was delivered on June 26 and later published under the title "Catastrophism and Evolution."[36]

In his talk, King stated that the supporters of catastrophism and uniformitarianism did not disagree about the empirical facts to be explained, "No geologist will hesitate a moment to admit that the question between the schools is not one of geological result, for both read the results alike."[37] The difference was that uniformitarianism stated that all geological action must be explained by phenomena of today whereas catastrophism "assert[ed] that the present furnishes absolutely no key."[38] For King the objections raised to catastrophism could be easily dismissed. Just because today we do not observe catastrophic events, therefore they never occurred, to King seemed as an unsatisfactory argument. Second objection, that catastrophes require great amount of energy, also seemed fallacious. If both schools agree that the same amount of work had to be done, and they disagree only about the time in which the work was done, they must agree that the same amount of energy was required to perform the work. King resorted to fundamental laws of physics.

King further belittled the uniformitarian view by listing among its supporters Aristotle and Pythagoras and "army of scientific fashion followers who would gladly die rather than be caught wearing an obsolete mode or believing in any penultimate thing."[39] Catastrophism, on the other hand, was supported by the "great *savant* as Cuvier, and still counts among its soldiers a few of the cast-iron intellects of to-day."[40] King, however, was not ready to defend the "sweeping catas-

[36]Clarence King, "Catastrophism and Evolution," *American Naturalist* 11 (1877): 449–470.
[37]*Idem*, p. 453.
[38]*Idem*.
[39]*Idem*.
[40]*Idem*, p. 454.

trophism" which postulated great if not total destruction of life. He admitted that for most of geological history uniformitarian processes took place and he did not believe in global catastrophes. Instead, King suggested a modified catastrophism in which local catastrophes did not totally extinguish life, but rapidly changed the environment to such a degree as to force life to quickly adapt to the new conditions or die.

The evidence in support of this view of catastrophism was plenty, much of it obtained during the six years of the Fortieth Parallel survey directed by King. First, during the early Paleozoic period the geologic record in the Eastern United States showed very uniformitarian processes of a gradually sinking land mass. King, was happy to yield this as evidence for uniformitarianism. However, in the Western United States, the region explored by King, the situation was quite different. There was a sharp transition from land deposits to deep ocean deposits. The rate of subsidence was immensely different at the two locations, precisely the condition forbidden by uniformitarian geology. King found a number of sudden transitions in the geological record of the West: sudden transition from deposits of limestone to deposits of land-detritus in the western Carboniferous; folding of 40,000 foot mountains at the end of Cretaceous age; volcanism during the Tertiary when lava poured out for hundreds of miles in a "deluges of molten stone."[41] The geological changes were not the only kinds of catastrophes which also included climatic changes. Many of the climatic changes occurred contemporaneously with the geologic catastrophes, others occurred independent of them. The Quaternary glacial ages were one such example. The melting of the glaciers resulted in catastrophic floods and rivers which carved out the canyons. To King, it seems nonsense to think that the Cordilleras canons, which were created during the Quaternary, could have been the result of the extremely small rivers flowing through them right

[41] *Idem*, p. 460.

now; the canyons are "overwhelming evidence" of a "water catastrophe."[42]

King reconstructed the argument by supporters of uniformitarianism

> they start with a gratuitous assumptions (vast time), fortify it by an analogy of unknown relevancy (the present rate), and serenely appeal to the absence of evidence against them as proof in their favor. The courage of opinion had rarely exceeded this specimen of logic. If such a piece of reasoning were uttered from a pulpit against evolution, biology would at once take to her favorite sport of knuckle-rapping the clergy in the manner we are all of us accustomed to witness.[43]

King was not defending all religious attacks on evolution, but was criticizing the lack of reflexivity on the part of scientists. He was worried that by engaging in public debates with poor arguments and problematic logic, as he thought many geologists and biologists were doing, they threatened the credibility of those scientific disciplines.

King proposes that climatic changes, earthquakes and volcanoes could have produced the catastrophes about which he writes. Again, he saw no reason why only current magnitudes of those events should be used in accounting for geological conditions.

In the second part of the paper, King analyzed biological evolution. He argued that the current theory of evolution was highly uniformitarian. Of the two parts of evolution, heredity and adaptation, he thought that adaptation and, more specifically, the effect of the inorganic environment on life had not been studied sufficiently, while natural selection had been in fashion. Again, King admitted that natural selection worked as advertised during periods of uniformitarian change, however, when those periods were interrupted by catastrophes, natural selection no longer worked because the whole system had been thrown out of balance: the climate had changed, the environment had changed, and some species had been exterminated. What happened next, according to King, was that species were

[42] *Idem*, p. 461.
[43] *Idem*, p. 462.

forced to behave very plastically, to undergo significant changes to adapt to the new environment or die. Whereas during normal course of evolution the strong survived, during catastrophic changed it was the plastic ones that did.

King in the geological record observed that biological evolutionary changes always occurred after a period of geological catastrophe. As an illustration, he took the most celebrated illustration in support of evolution: the series of horse fossils collected by O.C. Marsh and praised the previous year by Thomas Huxley. The fossils undoubtedly demonstrate descent, but they provide no clues to the cause of the evolutionary change.

> Huxley and Marsh assert that the bones prove descent. My own work proves that each new modification succeeded a catastrophe. And the almost universality of such coincidences is to my mind warrant for the anticipation that not very far in the future it may be seen that the evolution of environment has been the major cause of the evolution of life; that a mere Malthusian struggle was not the author and finisher of evolution, but that He who brought to bear that mysterious energy we call life upon primeval matter bestowed at the same time a power of development by change, arranging that the interaction of energy and matter which make up environment should, from time to time, burst in upon the current of life and sweep it onward and upward to ever higher and better manifestations. Moments of great catastrophe, thus translated into the language of life, become moments of creation, when out of plastic organism something newer and nobler is called into being.[44]

Geology could offer evidence in an effort to understand evolution, it could provide a mechanism for evolution which, at least to King, was superior to natural selection, if for no other reason, it allowed God to directly guide development of species toward His own end. King saw his theory as a compromise theory between the extreme of uniformitarianism and global catastrophism, between the Malthusian development and outright creation(s) by God.

In this article King only stated that the uniformitarian view requires "gratuitous assumption" the amount of vast, or even unlimited time. Yet, he did not present any evidence that such time is unwarranted. It is uncertain of whether

[44] *Idem*, p. 470.

King was aware of William Thompson's arguments for the limitation of the Earth and sun's ages; he does not bring them up in this argument. What his argument did imply, if not directly stated, was that however long the uniformitarian theory required for the duration of time, the duration of time according to King's catastrophist theory had to be shorter. King's theory also made inaccessible a method of determining time by denudation or sedimentation both of which as a fundamental assumption took a uniform rate of the geological process, which was exactly the assumption which King rejected. In summary, as far as age of the Earth was concerned, King needed time scale shorter than most geologists did, and, if he were to establish it, he could not use the method used by most geologists. His 1893 age of the Earth calculation did exactly that.

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Clarence King was interested in conducting physical experiments to obtain missing information for his speculations of dynamical geology. According to Wilkins, it was on George F. Becker's recommendation that King hired Carl Barus to be the physicist for the USGS. Barus stated that he received a letter from Ogden N. Rood, who had recommended Barus to King, in which Rood informed Barus that King was establishing a physical geological laboratory. In any case, in 1880 Barus started to work for Becker who was stationed in Virginia City, Nevada. King at the time was preoccupied in Mexico. After King had quit the directorship of the Survey he wanted Barus to perform physical investigations which would provide data on subterranean fusion and which would be necessary for King's theory of catastrophic geology. John Wesley Powell, the new director of the USGS, was not very enthusiastic about the physical work and agreed to provide only the salary for Barus. King had to equip the physical laboratory out of his own pocket, which he reluctantly did. Barus himself felt a bit uneasy about being paid a government salary while working for King, who was no longer officially associated with the USGS.[45]

In was in 1881 that Barus and King established the research plan the execution of which would preoccupy Barus for the next twelve years, and which only was interrupted by the shakeup of the USGS initiated by the Congress. The research project was ambitious: among other things Barus was going to investigate the rock fusion, latent and specific heats, melting points, relationship of volume to temperature, elasticity, plasticity and other physical properties or rock at extreme temperatures and pressures. King needed all of this to provide support for his catastrophic geology and to prove a mechanisms for the sudden movements of the Earth's crust.

Barus complained about King being a poor correspondent and it seems that during the first years of the 1880s King was not very engaged in scientific activities. During that time, Barus was developing instruments and techniques which were necessary to carry out the desired experiments. It was only around 1890 that Barus developed the required experimental setup and was able to start investigating physical properties of rocks. By that time King had expressed a desire to get back into scientific research. In the early 1890's he was considering a chair of geology at Columbia College, but did not take up either. In any case, all this time he remained in contact with Barus and they often met at Henry Adams's house, where King lodged when in Washington.

By 1891 King believed there was enough physical data that in December he wrote a short piece outlining his theory of dynamic geology; however, he encountered major problems in all the theories of geological upheaval, including his own.[46]

[45]On Barus and King's collaboration see Carl Barus, *One of the 999 about to Be Forgotten: Memoirs of Carl Barus 1865–1935* (BoD GmbH, Norderstedt, 2005), pp. 115–120, 250–275 and Wilkins, *op. cit.*, pp. 289, 293–295.

[46]On the events during the 1890s, see Wilkins, *op. cit.*, pp. 369–378 and Barus, *One of 999*, pp. 168–170, 251–254, 273–275.

At the same time Barus published two short papers on thermal properties of diabase which he concluded by stating "[t]he immediate bearing of all of this on Mr. Clarence King's geological hypothesis is now ripe for enunciation."[47]

As King was contemplating his geological theory, a significant faction in the U.S. Congress was becoming unsatisfied with the USGS under the directorship of Major Powell. During the spring and summer of 1892 debating congressmen attacked USGS for being engaged in research that did not have any practical applications. The Survey's budget went through several versions, all of which severely reduced appropriations. Barus and his physical research were let go and the geophysical laboratory and its equipments were scraped. However, most of the equipment that belonged to King personally was saved. Following the closure of the lab, Alexander Graham Bell agreed to pay for rent of a new lab in which Barus worked without pay in addition to his new job at the Weather Bureau.[48]

King who preferred to hold off publication until he had his theories finally worked out seeing the uncertainty of the situation at the Survey and published a paper on the age of the Earth in 1893. He started his article by stating that Lord Kelvin's attempt to estimate geological time was the most promising of such attempts, however, when Kelvin did his calculations in 1862 accurate data on the physical properties of rocks were lacking. Those data were now available for the first time. King reported as "unshaken" the results as to the rigidity of the Earth citing in support Kelvin, G.H. Darwin and Simon Newcomb. From that claim, King concluded that the rocks in the upper quarter of the Earth's radius could not be fused (if they were, they would have been displaced by tidal forces and Earth would

[47]Carl Barus, "The Contraction of Molten Rock," *American Journal of Science* 42 (1891): 498–499 and Carl Barus, "The Relation of Melting Point to Pressure in Case of Igneous Rock Fusion," *American Journal of Science* 43 (1892), p. 57.

[48]For more about the crisis at the USGS, see Mary C. Rabbitt, *Minerals, Lands, and Geology for the Common Defense and the General Welfare: Volume 2, 1879–1904* (Washington, DC: US Gov Printing Office, 1980), pp. 196–217.

not have been tidally rigid). King explained Barus' involvement in the investigation of obtaining the desired physical properties of diabase. King justified using diabase to represent all of the materials composing the Earth, because diabase had similar density to Earth's mean density.

In the actual calculation, King stated that pressure and temperature were the two quantities which were responsible for other properties of materials. He used Laplace's equations for deriving the pressure at different depths of the Earth. King extrapolated the melting temperatures of diabase from Barus's data to melting temperatures at higher pressures. Combining those two calculations, King drew a line of melting temperatures for diabase at different depths, from the surface of the Earth to its center. Next, King calculated the temperature distribution within the Earth according to initial condition used by William Thompson (3900°F and 100 million years) showing that they would result in most of the Earth interior being molten thus excluded by King's initial assumption of solid interior. There were a number of sets of initial conditions which resulted in allowable temperature distributions. One of them, for example, was an Earth 600 million years old with surface temperature of 3900°F. However, that distribution resulted in a temperature gradient near the surface of the Earth which was much higher than the observed value. The conditions which satisfied King's requirements were Earth of no more than 24 million years with initial surface temperature of 1950°F.

King acknowledged some of the objections which could be raised against his calculation. The first one was that King extrapolated physical properties of diabase at thousands of atmospheres from experiments made under one atmosphere of pressure. He also admitted that the internal composition of the Earth may be different from diabase; the Earth could have an iron core, for example. King was not too disturbed by those objections and was reassured that his own estimate of the age of the Earth was corroborated by other calculations.

The rest of the paper was devoted to analysis other ways of estimating the age of the Earth. No measure, King argued, could be derived from the tidal-retardation. Croll's calculations of Earth's age based on climatic consideration were rejected because they relied on "questionable physical geography and left so many physical doubts."[49] There were similar reservations about Axel Blytt's calculations. The final consideration was restricting the age of the Earth by calculating the age of the sun. Here King found support in calculations of Newcomb, Kelvin and Helmholtz all pointing to the age of the sun about 10 or 20 million years old. King did not consider any time estimates based on denudation or sedimentation.

The paper did not make a positive argument for King's geology, but it made a negative one against the accepted uniformitarian theories. King concluding stated "Yet the concordance of results between the ages of the sun and Earth, certainly strengthens the physical case and throws the burden of proof upon those who hold to the vaguely vast age derived from sedimentary geology."[50]

Both King and Barus believed that in 1892 finally the long years of preparatory work were starting to pay off and geological results were about to start forthcoming. The age of the Earth paper was by no means the last word King wanted to utter on the topic. King believed that if only his financial situation stabilized and he could devote three years to geological study, he would be able to write his magnum opus. The opposite happened. In late 1893, after a series of financial failures King suffered a mental breakdown, and, with the advice of his friends placed himself in an asylum. His recovery proceeded faster than expected; however, his financial situation did not improve greatly and he never again devoted serious time to scientific inquiry. King was indebted to his friends, he only used that money for business investment, for purchases of works of arts, and for support of his secret

[49] Clarence King, "On the Age of the Earth," *American Journal of Science* 45 (1893), p. 18.
[50] *Idem*, p. 20.

family. However, he turned down offers from both Hay and Adams who wanted to sponsor his scientific research. King wanted to be a patron himself; he did not want to be the one patronized. Barus, does mention though, an unsuccessful attempt by King to solicit financial support for geophysical work from J.P. Morgan.[51] In King's memoir, Emmons wrote that King's theory of upheaval and subsidence was in "an advanced stage of completion," however no manuscripts survived.

King's utterance on the question of the Earth's age, his last scientific publication, has its origins in the very beginning of his career, in the 1862 Agassiz lecture and the Whitney Survey. Historians studying King agree that his years in California (and in the West in general) were formative for his beliefs. Additionally, Herron argues that King's science cannot be separated from his religion and esthetic. Ann E. Lundberg argues that King's science has to be understood as his attempt to deal with the American identity in the face of forces of socio-cultural change. Aaron Sachs sees in King Humboldtian notions of unity in diversity and chain of connections. All those authors, as well as Thurman Wilkins, King's biographer, agree that King's understanding of science was inseparable from his life experiences and from his understanding of the world in its totality. For King science, art, religion, and society were not separate spheres to be investigated in isolation, but were all to be understood together. This was a tall order, one which King struggled all his life, never fully finding a way to comprehend it all, after trying a number of variations. His life was full of tensions arising from his desire to understand the world in its totality.[52]

Earlier historians, even the ones sympathetic to King, were rather dismissive

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[51] Barus, *One of 999*, p. 254.

[52] Keith R. Burich, "'Something Newer and Nobler is Called into Being?' Clarence King, Catastrophism, and California," *California History* 73 (1993): 234–249; John Paul Herron, *"The Reality of Living": Science, Gender, and Nature in American Culture, 1865–1965* (Ph. D. diss.), University of New Mexico, 2001, ch. 2, 3; Ann E. Lundberg, "'The Ruin of a Bygone Geological Empire': Clarence King and the Place of the Primitive in the Evolution of American Identity," *ATQ* 18 (2004): 179–203; Sachs, *op. cit.*, ch. 6, 7.

of what seemed an outdated notion of catastrophism. Since Stephen Jay Gould's notion of punctuated equilibria has gained popularity, some historians focusing on King's geological theories, have been evaluating them more positively.[53] However, a much better judgment is one like the one proposed by John Herron who writes "King's catastrophism, then, was not outdated, but more accurately, reflected his understanding of science and natural development."[54] King's catastrophism needs not be compared with previous or future scientific believes, but needs to be seen as part of his attempt to understand the world.

King's paper on the age of the Earth was a major, but not primary, aspect of his system of geology which King held for most of his career. The calculations in the paper were results of over a decade long program of physical geology initiated by King with the explicit purpose of providing data for King's system of catastrophic geology, a system which owed its origin to King's lifetime of experiences. The actual publication of the age of the Earth paper was prompted, at least in part, by the uncertainty of further research cased by political dissatisfaction with the work of the U.S. Geological Survey.

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There is nothing unusual in showing that individuals arrive at their beliefs based on their lifetime experiences nor that their beliefs about the Earth's age were connected to their other beliefs. A similar study could be performed on any other individuals and the same findings would be expected. In fact, any decent biographical investigation shows that much. However, even though when investigating any individual we take care to put his or her beliefs in the context

[53] "And yet, while this stating the issue in general writings, all paleontologists knew that the practical world of fossil collecting rarely imposed such a dilemma. [...] But how could traditional expectation of gradual transition and the practical knowledge of stability and geologically abrupt appearance as the recorded history of most species?" Stephen Jay Gould, "Opus 200," *Natural History* (1991).

[54] Herron, *op. cit.*, p. 50.

of their experiences, we do not do so when discussing a group of individuals. This is understandable. Such a task would require not only forbidding amounts of labor, the results of it would be difficult to present. However, if we assume that McGee and King are representative (at least of late nineteenth century American scientists) only in the aspect that they arrived at their beliefs through a unique set of circumstances and that their beliefs on any given topic were situated in a larger (more or less) systemized understanding of the world, are we justified in making generalizations about beliefs of a group of people composed of just such individuals? Do we assume that an individual whom we pick for detailed investigation has different views from the community of which he or she is a member, a community which we too often treat as if it were composed of clones who shared the same beliefs regardless of their own individuality?

In an analysis of utterances of a single individual we can draw the connections among all the meanings, and we can search for reasons for replies which were made as I illustrated in this chapter and as is evident from biographical studies such as, for example, Crosbie and Smith's study of William Thomson or Pyne's study of Grove Karl Gilbert.[55] However, any study of the reaction of the whole community encounters serious problems. Analysis of each individual is not only impractical, but also overwhelming. As historians, we search for generalizations and search for consensus among the individuals. We want to group them into communities who shared beliefs. However, in nineteenth century debates on the Earth's age it is difficult to find ways to generalize from individuals to groups. Historical studies which aim for a chronological treatment encounter the problem that there was no consensus among the individuals. Other scholars group the utterances on the Earth's age by the various methods of calculating Earth's age. While in

[55]Crosbie Smith and M. Norton Wise, *Energy and Empire: William Thomson, Lord Kelvin* (Cambridge: Cambridge University Press, 1989); Pyne, *op. cit.*.

those studies the material can be organized in a simpler manner, the complexity of historical interaction and development is lost. Chronological presentation of all the dialogues is overwhelming, without providing generalizations, which, however are difficult to locate. Separating the different methods into noninteracting groups, on the other hand leaves historians wanting more. Treating each utterance as polyphonic and engaged in multiple, but specified, ongoing dialogues, allows us to reduce the complexity to a manageable level without sacrificing the depth of analysis. Analyzing dialogues and what I will call community knowledge will allow us to find the thing shared among the individuals, the common reply, however, first we must identify what was the thing to which that reply was directed.

And what about the scientific communities themselves? If the utterances made by their members such as McGee and King had their meaning in those persons' individual understanding, understanding which could not fully be known by the community at large, what meaning was transmitted to the community? How did a community function if all of its members had different understandings? Did such a community ever achieve a consensus, and if it did what did this consensus look like? I attempt to answer those questions in the following chapters.

Chapter 3

The Desire to Communicate: The 1890s and a Common Reply

> "In the actual life of speech, every concrete act of understanding is active: it assimilates the word to be understood into its own conceptual system filled with specific objects and emotional expressions, and is indissolubly merged with the response, with a motivated agreement or disagreement. To some extent, primacy belongs to the response, as the activating principle: it creates the ground for understanding, or prepares the ground of an active and engaged understanding. Understanding comes to fruition only in the response. Understanding and response are dialectically merged and mutually condition each other; one is impossible without the other." M.M. Bakhtin, *Dialogic Imagination*

The previous two chapters showed that each utterance considered was complex and grounded in individual understanding. How another individuals have replied to such an utterance which they could not understand in the way its author did? What was there to guide them? Each scientist replied differently, but how was a community to deal with such a cacophony of discordant utterances? The community used shared *community knowledge* to evaluate each utterance. This chapter describes an episode in which new community knowledge emerged.

Already prior to 1890s there were some patterns in the discordant utterances on the Earth's age. Each author formulated their utterances to meet some expectations from his addressee. Those anticipations of the expectations of the other interlocutor (when she is not a specific individual but a generic community member) constitute the community knowledge. An author first and foremost must know those expectations, because it is only in relations to them that his own utterance will be comprehended. Therefore, when speaking to the community, a community member does not necessarily express his own beliefs, but might only be situating his utterance in relation to those expectations. W J McGee was misunderstood because he did not speak to those expectations; Clarence King received replies because he did. Finally, in 1893 an popular address by Charles Walcott became a new standard text on the Earth's age; it canonized community knowledge.

Before going on to the details of the arguments presented, we must first learn more about the community in which those dialogues took place. After, all community knowledge exists in a specific community.

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The course of scientific dialogue depends on the distribution of individuals participating in it and on provocations to commence discussion on a given topic. A brief review of the development of American science at the end of the nineteenth century will provide the context for the following discussion. The mid-19th century in American geology was a time of exploration. The first geological surveys of the eastern states began prior to the Civil War and surveys of southern and western states continued in the decades following the war. It was also during this time that federally sponsored surveys of the western territories started. Initially independent surveys often overlapped those areas of their explorations until in 1879 the surveys were combined into a new federal organization, the United States Geological Survey (USGS). USGS continued its mission of exploration and by the end of the century most of the territories of the lower continental United States had been mapped and geologists spent more time organizing and theorizing about the data they had gathered during the preceding decades.[1]

[1]The literature on nineteenth century geology and exploration in American is vast. Some

By the 1890s Washington had become a major academic center. The USGS under its second director "Major" John Wesley Powell had become a centralized institution in with all the activity concentrated in Washington making the country's political capital also its intellectual capital. The importance of Washington was further intensified by the fact that Powell managed to extend of the Survey's jurisdiction over all of the United States, including areas previously investigated by state surveys. However, geology was not the only science represented in the District of Columbia. The variety of other federal scientific institutions such as the Naval Observatory, the Smithsonian Institution and others brought in a critical mass of individuals who met at work, each other's houses or in the Cosmos Club, a scientific social club, and talked about science with their colleagues and families.[2]

In Washington during the 1880s and 1890s scientific dialogue proceeded at the Cosmos Club. The Club which was founded in 1878 on Major Powell's initiative had as its goal to bind the elite of Washington science with social ties. Its membership, though limited to 200, was broad. It was not restricted to professional scientists and was meant to overcome the centripetal forces of specialization. As

examples are William H. Goetzmann, *Exploration and Empire: The Explorer and the Scientist in the Winning of the American West* (New York: Knopf, 1966); Thomas Manning, *Government in Science: The U.S. Geological Survey 1867–1894* (Lexington: University of Kentucky Press, 1967); Mary C. Rabbitt, *Minerals, Lands, and Geology for the Common Defense and the General Welfare: Volume 2, 1879–1904* (Washington, DC: US Gov Printing Office, 1980); William H. Goetzmann, *New Lands, New Men: America and the Second Great Age of Discovery* (New York: Viking Penguin, 1986); Stephen P. Turner, "The Survey in 19th-century American Geology: The Evolution of a Form of Patronage," *Minerva* 25 (1987); Edward C. Carter II (ed.), *Surveying the record: North American scientific exploration to 1930* (Philadelphia: American Philosophical Society, 1999) Peter Bayers, "Exploration and Adventure in the 19th Century American West: Introduction," *ATQ* 18 (2004): 125–129,; Aaron Sachs, *The Humboldt Current: Nineteenth-Century Exploration and the Roots of American Environmentalism* (New York: Viking, 2006).

[2]In 1893 the local Joint Council of scientific societies accounted for over 1500 members. By 1900, 15% of government employees, nearly 4000 individuals, were "professional, technical, and scientific" Of those three hundred were "scientific experts and investigators." Michael Lacey, "The World of the Bureaus: Government and the Positivist Project in the Late Nineteenth Century," in Michael Lacey and Mary O. Furner (eds.), *The State and Social Investigation in Britain and the United States* (Cambridge: Cambridge University Press, 1993), p. 129; anonymous , "A Scientific Clearing House," *Journal of the American Geographical Society of New York* 25 (1893): 194–198.

J. Kirkpatrick Flack notices, the Cosmos Club served the same function to the Washington community as Powell's career did to his science, that is to impose harmony and synthesis. In addition to bringing together men of science (women were not allowed), the Cosmos Club through its Auditorium also provided a meeting space for the half a dozen specialized societies which existed in Washington. It is therefore not an exaggeration to say that it was the center for Washington science in the last two decades of the twentieth century.[3]

Exactly what was said as the genteel scientists gathered at the club we do not know. What we do know that they talked science, in the Club house and in the Auditorium where day after day the same group of people gathered to listen to presentations of the various specialized societies. Even though many of the members of the Club were also members of the scientific societies, part of the agreement for the use of the auditorium was that any of the members of the Club could attend the meetings.

During this time period Washington science achieved its maximum prominence. It was a peculiar kind of science: at the same time seeking professionalization and embracing amateurs, seeking independence while embracing its ties to the government, exhibiting specialization and seeking to overcome it. In Washington no easy lines could be drawn between professional and amateur scientists. There were men who earned their wage by doing science, but often did something else than what they were paid for. Even as they organized new societies aimed at promoting

[3]For the description of scientific life of Washington, see James Kirkpatrick Flack, *Desideratum in Washington: The Intellectual Community in the Capital City, 1870–1900* (Cambridge, MA.: Schenkman Publishing Co., 1975). See also Lacey, *op. cit.*; Donald Worster, *A River Running West: The Life and Times of John Wesley Powell* (New York and Oxford: Oxford University Press, 2001), ch. 10 & 11, esp. pp. 437–440. For additional information on the Cosmos Club, see Wilcomb Washburn, *The Cosmos Club of Washington: A Centennial History, 1878–1978* (Washington, D.C.: Cosmos Club, 1978); George Crossette, *Founders of the Cosmos Club of Washington 1878 a Collection of Biographical Sketches and Liknesses of the Sixty Founders* (Washington, D.C.: Cosmos Club, 1966). For Charles Walcott's heavy involvement in the Cosmos Club and other Washington societies, see Ellis L. Yochelson, *Charles Doolittle Walcott, Paleontologist* (Kent, Ohio: Kent University Press, 1998), pp. 225–230.

their professional status they did not exclude lay individuals, they embraced them to join the societies and encouraged them to contribute. The differentiation was not between professionals and amateurs, but between honest men and charlatans. But as they extended their hand to the amateurs they also saw themselves as different, as the elite. The societies which were open to anyone "at all interested" were structured to retain the power in the hands of this elite.

Another aspect of the tension between professional and amateur was the difference between specialized and general knowledge. While many of the scientists thought it important to present popular lectures that would appeal widely, they also wanted venues for presentation of their specialized research, and as that research became ever more advanced, it also became relevant (and understood) by only a subset of the larger community. The specialized societies, Anthropological, Chemical, Geological and others, gave an outlet for expression of those topics. However, people like Powell were bothered by this splintering of societies and in 1888 a Joint Commission of the Scientific Societies of Washington was formed which later transformed into the Washington Academy of Sciences.

It is in light of this intellectual culture that we must read the dialogues on the Earth's age which took place in 1892–93. The dialogues were among individuals who saw themselves as intellectuals, as men of science, prior to seeing themselves as professional, disciplinary scientists. Those were men who spent time together at the Cosmos Club, attending each other's meetings in the Club's auditorium.

The Washington scientists also established themselves as the leaders of American science in general. This was particularly true of geology.[4] But was also true of other disciplines. For example, in 1887 the balance of power in chemistry had shifted from the New York to Washington as every Washington member of New

[4]Many of the country's geologists, at one point or another, were employed by the USGS which "outsourced" some of its work to local scientists and amateurs.

York Chemical Society resigned from that institution and joined the Chemical Society of Washington which grew in national prominence. Also the Anthropological Society of Washington which started with an advertisement in a local paper, after less than two decades of existence saw its publication transformed into the premier national journal.

This trend of turning from broader to local societies originated from the self sufficiency reached by the Washington community and from its sense of power. To the members of the Cosmos Club as they looked out its windows on the White House, the local seemed the national. Many outside of Washington had a different view of science and many, especially geologists, resented the power of the USGS scientists.[5]

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Prior to the 1890's American scientists only occasionally spoke about the age of the Earth; however, they were familiar with the British debates on the topic. When asked "How old is the Earth?" American scientists answered by either enumerating the multiple opinions which had been proposed previously or admitted that because there were so many differing opinions the answer was not yet known. As described in chapter one, the vague responses were due to the many different dialogues in which each utterance contemporarily participated. American scientists prior to 1893 had not encountered a combination of means and motives conducive to starting a dialogue in which a consensus could emerge. However, during the early 1890s several new articles provided the motivation for evaluation of the knowledge on the Earth's age and the Cosmos Club provided the means for the dialogue out of which a new consensus answer emerged. The arguments presented during those

[5]This discussion is not meant to suggest that the science centered in Washington was the only, or even the most popular or important kind of science. See Daniel Goldstein, "Outposts of Science: The Knowledge Trade and the Expansion of Scientific Community in Post-Civil War America," *Isis* 99 (2008): 519–546.

dialogues were not novel and the conclusion differed little from the previous ones; however, after 1893 scientists had a new consensus reply to the question " How old is the Earth?"

I suggest that analyzing the utterances as polyphonic, that is having multiple coeval meanings and participating in multiple dialogues concurrently, helps us explain the varied receptions of the individual pronouncements on the topic of the Earth's age as well as helping us resolve seeming contradictions contained within those pronouncements, something I claim cannot be done if we think of scientific utterances as representing the scientist's beliefs or ideas rather than thinking of them as replies. This analysis leads me to conclude that consensus exists when a community of scientists utters the same replies when confronted with the same question, but that consensus does not mean that the individuals within the community share the same beliefs.

As I demonstrated in chapter one, there were voluminous differences among the calculations of the Earth's age presented by different individuals; however, there were also common themes in those discordant answers. All of the authors were familiar with the British debate. Alexander Winchell explicitly cited most of the individuals taking part in it and most of the authors cited William Thomson and his calculations. The geologists such as Winchell and Dana mentioned the fact that Thomson's calculations had restricted the ages proposed by other geologists (even though both of them proposed ages which were well within Thomson's upper limit). Finally, both of them stressed the uncertainly of any numerical calculations, even as they offered their guesses. Dana did so explicitly "The use of those numbers is simply to prove the position that Time is long,—very long [. . .] In calculations of elapsed time, from the thicknesses of formations, there is always great uncertainly."[6] Winchell also explicitly mentioned the uncertainly

[6]James Dwight Dana, *Manual of Geology*, 3rd edition (New York: Ivison, Blakeman, Taylo

of calculations, but the fact that in over twenty pages of discussion he presented over a dozen of different calculations based on different methods, many of them by recognized scientific authorities, showed that his own calculation was but one of many. Finally, the topic was of low importance as is demonstrated both by the stress on uncertainly and by the fact that it was rarely discussed, especially when compared to the number of publications in Britain.

A forced generalization could be made that prior to 1893 to answer the question "How old is the Earth?" was to enumerate various previous answers given. A more accurate generalization would be to state that the various authors were aware of that the "problem may be attacked from either the geological or physical side."[7] The arguments from the physical side tended to be for shorter time scales while the geological side traditionally suggested longer time scales. There were multiple ways of calculating Earth's age and there was a lot of uncertainty in all the calculations. Means to navigate between the various estimates had to be employed. Geologists additionally knew that the age of the Earth, no matter how calculated, was long enough for all the geological processes to take place.

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Whereas in the previous chapter I discussed how each individual attempts to understand for his own sake, now I discuss how those individuals tried to communicate with each other. In addition to understanding themselves, they wanted to share that understanding, they wanted to be understood. Yet as Clarence King shared with his friend James Gardiner: "I am convinced that science goes on and progresses at the *expense* of those absorbed in her pursuit. That men's souls are burned as fuel for the enginery of scientific progress. And that in this busy ma-

& Co., 1880), p. 591.

[7]Archibald Geikie, *Geological Sketches at Home and Abroad* (New York: Macmillan, 1882), p. 54.

terialistic age the greatest danger is that of total absorption in our profession."[8] However, King understood that what he was saying was unorthodox, describing his 1877 address as "nothing less than an ignited bomb-shell thrown into the camp of the biologists."[9] Because King was aware that he was disagreeing with community knowledge, he was able to present his utterance in a way that made the community respond to it. McGee, on the other hand, did not manage as much.

W J McGee's 1892 paper on the age of the Earth is interesting because he did not successfully reply to the previous knowledge on the Earth's age and consequently its novel analysis had little impact on how scientists answered question on this topic. McGee's argument appeared in an anthropological paper titled "Comparative Chronology" read to the Anthropological section of the meeting.[10] In the paper McGee engaged the question of the antiquity of the human race by enumerating all the ways of measuring time, geological time being one of them, and attempting to correlate them, ideally by causal relationships, all with the goal of calculating the age of homo sapiens. He concluded that the various chronologies must all be used simultaneously to arrive at the time of man's origin.

The scientific community's reaction to McGee's paper did not alter the way in which they answered the question "How old is the Earth?" By the time when McGee spoke, American scientists came to expect a knowledgeable utterance on the Earth's age to address the common themes of previous dialogues on the Earth's age. An author was expected to speak to a multitude of previous estimates, especially to how the calculations based on physical methods, such as cooling globe computations, related to calculations based on geological methods, especially the

[8]Quoted in Thurman Wilkins, *Clarence King: A Biography*, 2nd revised and enlarged edition (Albuquerque: University of New Mexico Press, 1988), p. 187.

[9]Quoted in Wilkins, *op. cit.*, p. 221.

[10]This paper was an address presented at the 1892 meeting of the American Association for the Advancement of Science in Rochester, N.Y. The manuscript for the talk deliver is in Box 24, Letterbooks 1891, Oct.-1893, June. WJM. A modified version of the talk was published as W J McGee, "Comparative Chronology," *American Anthropologist* 5 (1892): 327–344.

denudation method. The narrative for discussion of age of the Earth at its core had the competition of long time scales of geologists versus short time scales of physicists. Scientists were not necessarily expected to provide any given answer or interpretation of the previous calculation; however, they were expected to place their work in relation to those specific interpretations of the previous work. To successfully engage in dialogue one needed to address these expectations and reply to the community knowledge.

In 1892 there was no one answer to the question of how old is the Earth, but there were the answers which did not fit the community expectation. The scientists may have not known what the right thing to say was, but they did know when someone did not speak with a voice representing them. W J McGee's discussion of the age of the Earth did not successfully address the issues that needed to be addressed as community knowledge.

Besides McGee's own method, the manuscript for McGee's "Comparative Chronology" talk did not include any mention of the other methods of calculating Earth's age, either physical nor geological; it did not address the issue of the conflict between the disciplines; it did not enumerate the various approaches. Yet the scientific community expected one to address those issues if one were to speak authoritatively about Earth's age. That his audience expected to hear those things is evident from a reporter who describing McGee's original talk noted:

> It is easy to see from these figures, when compared with the time-ratios for the geological ages as given, e. g., by Dana, how stupendous a time is demanded by Professor McGee's view, and how extreme is the difference between the geological requirements on the one hand and the duration allowed by the physicists and astronomers on the other.[11]

The reporter presented McGee's talk as participating in the physicists versus geologists narrative, even though McGee did not mention the physicists in his talk. In a lively discussion following McGee's presentation those shortcomings were un-

[11]D. S. Martin, "The American Association at Rochester," *Science* 20 (1892): 146–7.

doubtedly pointed out as the published version of the paper included an extensive footnote discussing both physical and geological estimates of the Earth's age and McGee referred to the conflict with the physicists in the significantly enlarged final section of the paper.

Warren Upham criticized McGee's estimate by linking them to what he considered an incorrect theory of glaciation by James Croll.[12] McGee aimed to clarify his position in a letter to Upham

> [A]ll my estimates of geologic time are based not upon that a priori or speculative method so well exemplified in several of your recent writings, but upon the Baconian method of proceeding from carefully observed facts and generalization to inferences. I emphatically deny the opinion expressed by you that my time estimates are based upon or even strongly tinctured by Croll's theory. I again deny this in most emphatic terms![13]

Again, McGee was evaluating the arguments in light of his philosophy of science, but no one seemed to appreciate his strict adherence to the scientific method.

McGee's approach to the Earth's age did not reply in the expected way to the community expectations and consequently it was dismissed. Even the expanded version of McGee's paper did not address the geology vs. physics debate in the expected terms. He did not use rates of denudation to calculate the age of the Earth, and his final estimate was taken to be exuberantly too large. In addition, even though W J McGee was an established scientist, he was known to hold some idiosyncratic beliefs. Consequently each member of the scientific community could safely assume that McGee's argument would be dismissed by the other community members and dismiss it himself—McGee did not require an urgent reply. However, it is not the case that McGee's extreme result was not cited at all. It was cited by some of the more thorough authors who in reviewing research on the Earth's age included his calculations as the longest extreme, which could therefore

[12]Warren Upham, "Estimates of Geologic Time," *American Journal of Science* 45 (1893): 209–20.

[13]W J McGee to Warren Upham, March 24 1893. Box 24, Letterbooks 1891, Oct.-1893, June. WJM.

be dismissed, just as Alexander Winchell's estimate of three million years which provided the shortest extreme also could be dismissed. McGee and Winchell were not directly replied to, but were used in replies to others; authors did not engage with their arguments and did not analyze them, but merely enumerated them for the sake of completeness. One could still speak with authority on the Earth's age after 1892 without mentioning McGee.[14]

As I argued previously, the utterances on the Earth's age replied to multiple distinct sets of knowledge and could be read as taking part simultaneously in multiple dialogues and reaching multiple unrelated conclusions. Though I have concentrated my discussion of "Comparative Chronology" as an (unsuccessful) reply to knowledge on the Earth's age, McGee's main goal in this paper was anthropological investigation of the antiquity of the human race. McGee's position as an anthropologist was more established than as a geologist. As a paper about the age of Homo sapiens the paper was more successful and it did receive a direct reply. Warren Upham in an 1893 article engages the issue of duration of glacial time and the time of the origin of Homo sapiens.[15]

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Clarence King's article on the age of the Earth elicited a loud response. King's article demonstrates that to participate in a dialogue, one does not have to agree with the community knowledge—King definitely did not—but one must comprehend the community knowledge and formulate one's utterances in reply to it.

[14]Most of the citations to McGee were within the first few years after his paper appeared. When McGee's argument was no longer current it was no longer mentioned. Examples include: Charles D. Walcott, "Geologic Time, as Indicated by the Sedimentary Rocks of North America," *Journal of Geology* 1 (1893): 639–676; Josiah Thomas Scovell, *Practical Lessons in Science* (Chicago: Werner Co, 1894); Henry Shaler Williams, *Geological Biology: An Introduction to the Geological History of Organisms* (New York: H. Holt and Co, 1895).

[15]Warren Upham, "Geologic Time Ratios, and Estimates of the Earth's Age and of Man's Antiquity," *Bibliotheca Sacra* 50 (1893): 131–149. Abridged version appeared as Upham, *Estimates of Geologic Time*.

The community of American geologists felt a strong need to reply to King who was a respected scientist and who was challenging the most fundamental tenet of geology—uniformitarianism. The goal of the replying community was not to establish a numerical value for the age of the Earth. The goal was to establish whether the pre-1892 responses to questions about the Earth's age remained adequate after King's pronouncement.

It is through dialogue that a consensus can be established. Whereas individuals, without consulting with each other, decided that McGee's article on the Earth's age was dismissible, this was not the case with King's paper. Individuals did not know how other community members were going to respond to King. They did not know if their colleagues would respond to King by admitting that uniformitarianism was inadequate, or how they would rebuke King's arguments. Such dialogue could take place through the printed media, but it would be more efficient if carried out face to face. Not having to commit to a finalized comment such as a printed article also gave the scientist more freedom to explore opinions and possibilities. King's article appeared at a time when the leaders of the American scientific community were in a position to engage in face to face dialogue. On April 12, 1893 at the Cosmos Club auditorium, the newly established Geological Society of Washington held a special meeting to discuss King's paper. It had the highest attendance of all the GSW meetings that year (58 people). What has been said can be gathered from the minutes of the meetings, reports in two local papers and and extract's from McGee's paper published in *Science*. The geologist Charles Walcott presided over the meeting and a number of scientists including Simon Newcomb, Clarence Dutton (via read letter) and McGee, who had previously published numerical estimates of the age of the Earth, expressed their opinions. Though previously they disagreed as to the methodological approaches and numerical answers to the question of the Earth's age, those scientists now set aside their individual understandings and stressed that the numerical answer was unknown and depended on one's starting point and the assumptions made. King's paper was important to the geologists as an argument against uniformitarianism which was nearly universally supported (even though they took it to mean different things). Even though what each of the individuals—McGee, Newcomb, and Dutton—said during the symposium originated from the same understanding as those scientist's previous publications, in this new context their expressions made their supposed same beliefs sound quite different: they emphasized the assumptions and the uncertainty which in their previous utterances were overshadowed by their numerical answers. Now when the goal of the dialogue changed (answering King's challenge) so did the things the individual scientists said about Earth's age. In the end, they agreed on the fact that King's argument was in error. It did not matter as much why he was in error, but it mattered that they all agreed that he was. The discussion continued during the next meeting on April 26th when physicists Robert S. Woodward disagreed with King's paper.[16]

During the symposium, the participating scientists had a chance to hear how others were going to react, to form an opinion, or if they already had an opinion to attempt to persuade others to it. That is they had a chance to develop a common reply. After the meeting, the participants came away knowing that they would not be alone in rejecting King's argument and that their earlier community knowledge about the Earth's age as well as their reliance on uniformitarianism were still valid.

The meeting of the Geological Society of Washington discussing King's paper was the best attended of all its meetings that year, yet drew only a small group of local, but important, participants from the Washington area. McGee published

[16]Minutes of meetings for the year 1893. Geological Society of Washington. http://www.gswweb.org/minutes/1893.pdf; "The Age of the Earth" *The Evening Star* (Washington, D.C.), April 13, 1893; "Old mother Earth" *The Evening News*(Washington, D.C.) April 13, 1893. W J McGee, "Note on the 'Age of the Earth'," *Science* 21 (1893): 309–310.

a short version of the paper he presented at the meeting in *Science*, but it was Charles Walcott who decided to reexamine the issue and present his finding to the wider community.[17]

After the April meetings, Walcott spent several months working on the problem of the Earth's age and presented his result at the August 1893 meeting of the AAAS which met this time in Madison, Wisconsin. His paper successfully engaged in the dialogues on the Earth's age; however, the discussion of the age of the Earth was limited only to the introduction and conclusion of the paper. The main body was a geological investigation of historical geography and the processes of denudation and sedimentation. As with the previous papers, I suggest that this dichotomy is evidence of polyphony and that this text should be analyzed as a polyphonic addressive reply. Such analysis explains contradictions which are present in the text if it is viewed as an expression of Walcott's beliefs.[18]

In the introduction of his paper, Walcott restated the community knowledge about the Earth's age; that is, he presented the answer that a geologist could give when asked about the Earth's age. Walcott wrote:

> The physicists have drawn the lines closer and closer until the geologist is told that he must bring his estimates of the age of the Earth within a limit of from ten to thirty millions of years. The geologist masses his observations and replies that more time is required.[19]

[17]Some historians suggest that Walcott's decision to speak on the topic was aimed to present the Survey scientists as speaking with a unified voice and to neutralize McGee's exuberant claims as to the Earth's age which could have been used by the already suspicious members of Congress in further attacking the work of the USGS. I am agnostic about this claim; however, if it is true then we need to read McGee's article as participating in yet another dialogue, engaging the political usefulness of the Survey (something which, of course, all the other utterances participated in as well, although most of them did not meet with a reply). See Rabbitt, *op. cit.*, pp. 213–216.

[18]The following summary is based on the text of the address published as Walcott, *Geologic Time*. Ellis L. Yochelson, "'Geologic Time' as Calculated by C. D. Walcott," *Earth Sciences History* 8 (1989): 150–158 presents a technical discussion of the argument. For a summary of the calculations see G. Brent Dalrymple, *The Age of the Earth* (Stanford, Cal.: Stanford University Press, 1991), pp. 59–69.

[19]Walcott, *Geologic Time*, p. 639. This is similar to Geikie's "The geologists found himself in the plight of Lear when his bodyguard of one hundred knights was cut down. 'What need you five-and-twenty, ten or five?' demands the inexorable physicist, as he remorselessly strikes slice after slice from his allowance of geological time." Archibald Geikie, "Address of the President,

There was a dispute between the disciplines and the thing at stake, the only important thing, was that the duration of geological time be long enough. The rest of the introduction further stressed the uncertainly of the results with statements like "The geologist recognizes that geological time cannot be reduced to actual time in decades or centuries; there are too many particularly recognized or altogether unknown factors."[20] Walcott also praised the principle of uniformitarianism which "render[s] possible a computation of the age of the Earth on the principle that geologic processes were the same in the past as at present."[21] Next, Walcott gave a thorough review of the literature on the Earth's age, enumerating the different approaches and answers obtained so far. This introduction replied to the community expectation. It placed Walcott's work within the narrative of physicists versus geologists, it enumerated the previous answers and stressed their uncertainty, while reaffirming support for uniformitarianism.

The main body of Walcott's paper had little in common with the introduction; it comprised a discussion of the geography during the Paleozoic area around an area Walcott referred to as Cordilleran Sea, work which fitted with the mainstream of geological practice. Fred B. Weeks abstracted the article as follows:

> Gives the estimates of different authors of the duration of geologic time. Describes the continental growth during the various geologic ages, the geographic conditions limiting the extent of Paleozoic sediments in the Cordilleras, the source and character of the material, and the conditions under which they were accumulated, with a discussion of the various processes of depositions and the estimates of the length of Paleozoic time in this region. Gives the ratios of geologic periods adopted by other writers and presents the author's summary of the duration of each geologic period.[22]

This abstract correctly represents that the majority of the article was an impeccable treatment of a standard geological research topic by an expert; only the

Geology Section," *Report of the 62nd meeting of the British Association for the Advancement of Science* (1892), p. 19.

[20]Walcott, *Geologic Time*, p. 639.

[21]*Idem*, p. 640.

[22]Fred Boughton Weeks, *North American Geological Formations Names Bibliography, Synonymy and Distribution* (1902), p. 633.

introduction and conclusion related to issues of geologic time, however "age of the Earth" is not mentioned. The paper was cataloged under the heading of physical geography in the *Bibliotheca Geographica*.[23] Such a classification was reinforced by the fact that the only illustration in the paper is a map of North America showing the hypothetical locations of the Paleozoic seas.

Walcott's discussion in the main body of the paper was very detailed and rigorous. Walcott gave careful consideration to the land which was eroding, and the area of the sea over which those sediments were deposited. He gave a careful review of the literature on the rates of deposition, discussing both mechanical and chemical ones, and he discussed in detail the process of sedimentation. All throughout, he demonstrated great familiarity with the literature as well as with the geographic region which he was describing. In the article, Walcott used figures with five significant digits, giving an impression of great precision. Just as when addressing the issue of the Earth's age, Walcott provided an utterance which satisfied expectations. The argument he presented fit within the accepted way to discuss issues of denudation and sedimentation, in fact, it was one of the most detailed studies on the topic.

However, in this article Walcott was contradicting himself and not saying what he knew to be the case. For example, Walcott knew that it was not a physicist who had "drawn the lines closer," but Clarence King, a geologist; and one of the few physicists who stated his opinion of the topic, Robert S. Woodward, rejected King's argument. Furthermore, if "geologic time cannot be reduced to actual time" why did he spend the rest of the paper performing exactly such a calculation? For the majority of the paper, Walcott used very precise numbers, yet as he arrived at the numerical measure of the Earth's age he stated without much fanfare or explanation

[23] Gesellschaft für Erdkunde zu Berlin (ed.), *Bibliotheca Geographica. Band III, Jahrgang 1894* (Berlin: Kühl, 1897), p. 338.

"In reviewing the proceeding estimates we must consider that throughout, I have increased the various factors above those usually accepted."[24] In the concluding part of his paper Walcott stitched together the discussion of rates of sedimentation and age of the Earth. He calculated that the Earth was 55.14 million years old or, if errors were admitted, somewhere between 30 and 60 million years. In the last sentence of his article he made his result even more vague by declaring that it was in the "tens of millions."

When seen as expression of Walcott's beliefs, Walcott's article seems to contain contradictions; however, when analyzed as a polyphonic addressive reply Walcott's article makes sense: it replied to two separate sets of community knowledge each one requiring different expressions of knowledge. The author at one point wrote that the age of the Earth cannot be calculated in years, then he calculated it to be 55.14 million years, finally he concluded that the Earth is tens of millions years old. The different statements, seemingly contradictory, result from Walcott engaging with different dialogues. In the introduction and conclusion, when he spoke of not being able to calculate precisely the Earth's age, he addressed the community knowledge on the Earth's age and stressed the same uncertainly expressed by the participants of the Geological Society of Washington symposium. When in the main body of the paper he spoke of precise calculations he was replying to research on denudation rates. That research had its own, distinct community knowledge which Walcott successfully engaged and which, among other things, required him to be very precise. His remark "increas[ing] the various factors above those usually accepted" and dropping the precision we can see as a transition from expectation

[24]Walcott, *Geologic Time*, p. 673. Walcott was not alone in performing such maneuvers, for example Haughton used four significant digits throughout his calculations, despite the fact that at one point he arbitrarily adjusts the numbers by a factor of 10. Samuel Haughton, "A Geological Proof That the Changes in Climate on Past Times Were Not Due to Changes in Position of the Pole; With an Attempt to Assign a Minimal Limit to the Duration of Geological Time," *Nature* 18 (1878): 266–268

of one voice to the other. Analyzing Walcott's paper as a polyphonic addressive reply solves the seeming contradictions contained within it. However, because he spoke with many voices and represented many points of view, we cannot take what Walcott's stated as representing what he believed.

Walcott's audience wanted to accept his calculation that the Earth's age was long enough for the uniformitarian processes to form the geological features observed. They wanted to have an authority to which they could point when asked about King's challenge. The geologists wanted the issue closed and as long as no one challenged Walcott's calculations they would stand as the canonical text on the Earth's age—a position which the article served into the next century. Nobody objected to the contradictions contained within the paper, as the audience received the two separate voices within the paper in the terms of the two different discourses in which the paper participated: one on the Earth's age, the other on historical geography and denudation. His audience would not remember the details of the proof, but they would remember that the proof was convincing and that what they already knew was still valid.

The dialogues on the Earth's age were complex. The scientific audiences had clear expectations of what needed to be addressed, even if they did not have clear expectations as to what was the correct answer. McGee in his paper did not respond to those expectations and he was dismissed. King and Walcott replied satisfactorily even though they reached different conclusions. Their papers were highly influential. The paper also shows that the dialogues on the Earth's age continued to be polyphonic and the audiences discriminated the various voices. Because each utterance was a polyphonic reply it was formulated to meet the various expectations of the dialogues in which it participated, what the author expressed cannot be taken as representative of what he believed. As the case of Walcott illustrates, even within a single article Walcott expressed multiple opinions and stated facts which he knew were not true, in order to place his work within the standard narrative of the dialogues on the Earth's age. To comprehend those utterances we need to look at them as replies to audience expectations rather than expressions of beliefs.

In the last section of this chapter I will show that Walcott's paper became the standard text on the topic of the Earth's age and I will further argue that after the dialogues of 1892–3 a discordant consensus emerged.

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When we focus on knowledge claims in scientific utterances as expressions of beliefs of the individual community members, we may find it difficult to make any generalizations about that knowledge. For example, just about every article on the Earth's age argued for a different conclusion. Instead, I suggest that the commonality of the community was to be found in the fact that the utterances replied to the same concerns (even if everyone responded uniquely). This is the reason for treating the utterances as replies.

After the dialogues of 1892–3, the community expected a more specific reply to the question of the Earth's age, one based on Walcott's paper. However, we must still be cautious when speaking about consensus. Even as the authors came to provide a common answer to the question of the Earth's age, they did not share the same beliefs. When they cited Walcott's paper they took it to have many meanings. What emerged was a common reply representing a discordant consensus, but not a shared belief.

The arguments by King and Walcott were not based on new methods of investigation, nor were their conclusions entirely novel. However, they did change the way in which American scientists replied to the question "How old is the Earth?" Prior to 1892 the problem of the Earth's age was rarely discussed and individual scientists by themselves interpreted the isolated utterances on the subject. There were some common features which scientists usually replied to; however, in their reply they had considerable latitude. The number of papers which appeared during 1892–3 made the scientific community much more aware of the topic, and even the scientists who previously had no knowledge of age of the Earth were able to discern common features of the various arguments made. Even though it was not the first to introduce the disciplinary conflict narrative, Walcott's paper was very well known. It played a central role in forming the common reply for a number of reasons. It was the last of the papers presented during this period and as such had a chance to respond and situate itself with respect to the previous ones. Walcott's reply to community expectations of both of the main voices present in it was exemplary. Also, his paper which was first read at the vice-presidential address as the AAAS, and was reprinted in at least three different publications.[25] The fact that in 1894 Walcott was named the director of the USGS, also increased the authority of his work.

Ellis L. Yochelson, Walcott's biographer, laments the fact that Walcott's paper was not highly cited despite its seeming importance.[26] It is true that there are few explicit citations of the paper. However, the importance of the paper and the community familiarity with it is evident from the number of implied citations to it, that is references to Walcott's research without giving the complete bibliographic citation. After 1893 American geologists treated the last sentence of Walcott's address as part of their community knowledge and used it in replies to answer the question "How old is the Earth?": "Geologic time is of great, but not infinite

[25]The additional publications were Charles D. Walcott, "Geologic Time, as Indicated by the Sedimentary Rocks of North America," *American Geologist* 12 (1893): 343–368; Charles D. Walcott, "Geologic Time, as Indicated by the Sedimentary Rocks of North America," *Annual Report of the Smithsonian Institution* (1893): 301–334.

[26]Yochelson, *Geologic Time*, Yochelson claims that it was the most widely distributed 19th century American scientific paper.

duration. I believe it can be measured by tens of millions but not by single millions or hundreds [of] millions of years." This is illustrated, for example, by an abstract of Walcott's article in *American Naturalist* which summarized the bulk of the paper by stating: "A careful consideration of all the factors of denudation and deposition led him to consider that it would have required 17,500,000 years for the deposition of the calcium and the mechanical sediments of the Paleozoic time." There follows a direct quote of the entire conclusion of the paper.[27]

Many other utterances gave the common answer to the question of the Earth's age and represented references to Walcott without, however, giving an explicit citation. Though some of those references were in popular works which generally do not give citations to their sources, other references in academic publications indicate that the author assumed his audience was familiar with the referenced work and exact citation was not required.[28]

In the common reply which emerged during the 1890's the narrative of physicists versus geologists became much more prominent. Whereas previously William Thomson was cited to represent the physicists and Geikie or Dana represented the geologists, after 1893 King was often added to the list of physicists and Walcott was often cited to represent the geologists. For example, Warren Upham in his 1893 paper cited Geikie and Dana as the geological authorities, but in the introduction to his 1901 paper he added Walcott to this list when he stated that the Earth's age "as estimated by Dana, Walcott, and others is about one hundred or two hundred million years."[29] Lester Frank Ward in his 1903 *Pure Sociology*,

[27] anonymous , "General Notes," *American Naturalist* 28 (1894), pp. 793–794. Another example is Agnes Crane, "The Submergence of Western Europe Prior to Neolithic Period," *Science* 2 (1895), p. 4 who writes: "Mr. C. D. Walcott, who has recently passed this subject in review, came to the safe conclusion that" followed by the quote of the last sentence.

[28] Examples of the non-specialist sources are Appletons, *Annual Cyclopaedia and Register of Important Events of the Year 1893* (New York: D. Appleton, 1894), p. 28; anonymous , "The Geological Age of the World," *Scientific American* 70 (1894), p. 180; Samuel Christian Schmucker, *The Meaning of Evolution* (New York: Macmillan, 1913), pp. 149–150.

[29] Warren Upham, "Preglacial Erosion in the Course of the Niagara Gorge, and Its Relations

used the age of the Earth to emphasize the brevity of time that the human race existed compared to the world.[30] Walcott was the only scientist whom Ward cited. However, he referred to Walcott's 55 million years as conceding to the physicists and Ward himself used 72 million years (he does not make clear the source of this figure).

Perhaps the most telling indication that Walcott's paper has become part of the community knowledge and that the individual members of the community gave a common reply are references like this one from the 5th edition of Dana's *Manual of Geology*:

> While neither the geological nor the physical modes of calculation can yield any certain results in the present state of our knowledge, it may be considered probable that geological time from the beginning of the Cambrian is measured by tens of millions, rather than by millions, or by hundreds of millions, of years.[31]

which is the conclusion of Walcott's paper in nearly his own words.[32]

It is not only in print that Walcott was singled out as the canonical text on the Earth's age. Fifteen years later, George F. Becker wrote to Bertram Borden Boltwood, who was using new methods based on radioactive decay to calculate ages of rocks and had reached vastly different conclusions than previously calculated. Becker was somewhat skeptical of the new high numbers and pointed Boltwood to Walcott's much smaller results (in print he called Walcott's paper "more minute and comprehensive than any other with which I have met."[33]) Another scientist

to the Estimates of Postglacial Time," *American Geologist* 28 (1901), p. 244.

[30]Lester Frank Ward, *Pure Sociology: A Treatise on the Origin and Spontaneous Development of Society* (New York: Macmillan, 1903), p. 38–39 n. 2.

[31]James Dwight Dana, *Revised Text-book of Geology*, 5th edition (New York: American Book Compnay, 1897), p. 445. This was actually a posthumously published textbook based on the 3rd edition.

[32]Other implied citations include Charles Stuart Gager, *Fundamentals of Botany* (Philadelphia: P. Blackinson's sons, 1916), p. 620; anonymous , *op. cit.*, pp. 793–4; Crane, *op. cit.*, p. 4; Scovell, *op. cit.*, p. 373

[33]G.F. Becker to B.B. Boltwood. 30 November 1907. Box 18. General Correspondence 1901–1907. GFB; George Ferdinand Becker, "Relations of Radioactivity to Cosmogony and Geology," *Bulletin of the Geological Society of America* 19 (1908), p. 128. At a different occasion Becker

interested in the Earth's age William Diller Matthew. In a 1918 letter to Henry Fairfield Osborn he wrote "I believe [this paper by Joseph Barrell] will be a classic—as much as Walcott's essay of twenty-five years ago." Still, a generation later, Walcott's paper could be referred to without an explicit citation.[34]

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Community knowledge is a set of utterances that can be made when addressing a member of the community which will not lead to a reply that challenges them. It is what one community member expects his interlocutor to expect him to know. The set of utterances which form community knowledge are not shared assumptions of the community members. A shared assumption presumably has the same meaning for all the community members, but that is not the case for community knowledge. Each of the utterances has different meanings for each of the community members. That is why any elaboration on or use of the utterances in any context other than the specified one is not a part of the community knowledge. The individual members need not even believe the utterances to be true at all, but they will expect their audience of other community members to accept those utterances.

The discussion of Walcott's paper already illustrated how we cannot assume a statement represents the beliefs of an individual who uttered it without proper examination. However, the point seems important enough that few more examples seem in order. The fact that individuals do not always represent their own

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cited Walcott's paper as providing 45 million years since the appearance of first fossils, again giving yet another reading of it George Ferdinand Becker, "The Age of the Earth," *The Cosmopolitan; a Monthly Illustrated Magazine* 16 (1894): 512.

[34] W.D. Matthew to H. F. Osborn. 1 July 1918. H.F. Osborn Administrative Correspondence folder 4, general correspondence box 70, folder 70, WDM. In 1936, another paleontologist W. K. Gregory, cited Walcott's results as the only one from the previous generation of estimates of the Earth's age, William K. Gregory, "On the Meaning and Limits of Irreversibility of Evolution," *The American Naturalist* 70 (1936), p. 520.

individual understanding in their utterances, but report community knowledge is perhaps best illustrated by Grove Karl Gilbert who in a review stated that

> [J. D. Whitney's] hypothesis that the intensity of solar radiation is gradually lessening, by reasons of the dissipation of solar energy, and that the paleontologic record in arctic and temperate regions is in close sympathy with this lessening, will be admitted by most students.[35]

Gilbert did not include himself among the "most students," a fact which he clarified in reply to a criticism "In saying that the hypothesis of a diminution of solar radiation through the dissipation of solar energy would be admitted by 'most students' I did not intend to include myself, for I am really a dissenter."[36] In the original review, Gilbert also demonstrated his familiarity with the subject of the age of the Earth debates by stating that the lowest allowed age of the Earth is ten million years, calculated by Tait (again a position which he personally did not share, but uses as part of his argument).[37]

Community knowledge can best be found in instruction for students. An article by Henry S. Williams on the geological time-scale printed in the "Studies for Students" section of the *Journal of Geology* illustrates very well community knowledge of geochronology as well as the difference between accepted community knowledge and individual understanding proposed for discussion (which could result it becoming community knowledge).[38] Williams started his article by providing a brief history of the development of the geological time scale and introduced the subdivision of the system of stratified rocks as well as the geological revolutions which separate them. Next he stated

[35] Grove Karl Gilbert, "Review of Whitney's *Climatic Changes.* Part II," *Science* 1 (1883), p. 169.

[36] Grove Karl Gilbert, "Sun's Radiation and Geologic Climate," *Science* 1 (1883): 458 The criticism to which Gilbert was replying was C. B. Warring, "Sun's Radiation and Geological Climate," *Science* 1 (1883): 395.

[37] Grove Karl Gilbert, "Review of Whitney's *Climatic Changes.* Part III," *Science* 1 (1883), p. 194.

[38] Henry Shaler Williams, "Studies for Students: The Elements of the Geological Time," *Journal of Geology* 1 (1893): 283–295.

> While the conditions of deposition for a particular region remained relatively constant and uniform, the strata were accumulated in successive beds one upon another, and then the thickness of the deposits of the same kind, with proportionate thickness for deposits of different kinds, constitutes a scale of definite time value; a foot of deposit representing a period of time, and the relative time-separation of two faunas would be represented by the thickness of the strata between them.[39]

Here Williams laid out the assumption of uniformity in the rate of geological change. He qualified it by writing "relatively constant" and "with proportionate thickness for deposits of different kinds." The quotation illustrates another point: length of time is expressed in feet of deposits. Next Williams stated that based on their relative ages geological periods are established. He gave the values arrived at by Dana and Ward and then suggested his own corrections. All this suggested the usefulness of the rate of deposition as the measure of time, but also its uncertainty and variations

> However, the conditions of deposition, the fineness of coarseness of the clastic fragments, the abundance or rarity of supply of materials and other variable conditions must be taken into consideration in the accurate reduction of thickness of strata into length of time. Errors, also, whose value is almost impossible of estimation, arise from the intervals between strata, particularly those where unconformity exists. [...] It is doubtful if it is possible with our present knowledge to reach an estimate in years or centuries, of the actual length of geological time, which is within 100 or perhaps 200 per cent. of the truth. [...] The actual length of time in years, however, is of less importance to the geologists that the relative length of time for each of the ages [...] Relative thickness is certainly one of the elements in the determination of the time values of the geological formation.[40]

There was no questioning of the order of the formations and periods, its primary importance was implied.

It was only at the very end of the article, after the standard community knowledge had been given, that Williams proceeded to express his own individual understanding. In the article's conclusion, Williams introduced the concept of

[39] *Idem*, p. 291.
[40] *Idem*, pp. 292–293.

"geochrone" and "geochronology." That this section was Williams' understanding and not shared community knowledge was clear "I realize that such a proposition furnishes many point for disputes."[41] In addition to the explicit warning about the disputable nature of the claims, the use of the first person singular alerts the reader that this was not a view of the community. The resulting time ratios were introduced. "Such a standard time-scale of geochronology, on the basis of the Eocene period for a time-unit or geochrone would read as follows"[42] followed by the time ratios. Again the reader was warned about the unfinalized nature of the claim by the use of the subjunctive. The statements of the ratios of Dana and Ward were presented in the indicative mood.

Williams aimed to convey community knowledge to students. Concluding his article he went a step further and showed the student a way in which that community knowledge was used in a scientific dialogue which took that knowledge as shared assumptions of his interlocutor and presented his own understanding of the matter. Williams believed in his idea of the geochrone but he knew that his belief was not shared by the community.

That each of the authors considered in this chapter had a clear audience in mind should be evident from the proceeding discussion, one final, if not too subtle, example will reinforce the point. Warren Upham originally published his article on the Earth's age in a theological journal *Biblioteca Sacra* published by his friend, theologian and geologist, George F. Wright (the one whom McGee called a charlatan). The article was then reprinted in Dana's *Journal of American Science*. The changes made to the article reflect the changes in the community knowledge to which the article was addressed. In the introduction of the original article Upham argued that the story of *Genesis 1* and the story obtained from the geological

[41] *Idem*, p. 295.
[42] *Idem*.

record are in complete accord (several isolated mentions of the Creator are left alone). Also a section on cosmogony was omitted. What remained was an argument in support of the geologists knowledge on the age of the Earth and and an argument showing why McGee was wrong.[43]

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After 1893 there was a consensus, both from experts on the Earth's age as well as from writers of popular articles and textbooks that when asked about the Earth's age the correct reply was to describe the dispute between physicists and geologists, a dispute in which Walcott was one of the significant figures. Yet those authors did not necessarily share any ideas about the age of the Earth or about the meaning of Walcott's article. This shared reply to the question "How old is the Earth?" did not mean that the scientific community shared beliefs. Even though there was consensus that Walcott was the authority to cite, different authors understood him differently, and used him for different purposes. Two considerations support this claim.

First, common citations do not necessarily indicate that the citing authors understood the cited source in the same way. While all the above authors agreed that Walcott needed to be cited, they represented his position quite differently. Walcott was cited as supporting any of the three ages he presented in his paper (55.14, 30 to 60, or tens of millions of years), as well as some he did not (100 million or more). Some authors assigned the number of years (whichever one they chose) to the time from the beginning of Cambrian, while others took it to be the age of the Earth. Most authors took Walcott's argument to be against the short time scale of King and Kelvin, but Ward thought that Walcott's position was a compromise to accommodate King's position. Such diversity was in line with the

[43]Upham, *Geologic Time Ratios*; Upham, *Estimates of Geologic Time*.

citation patterns in earlier articles on the Earth's age which often also showed a multitude of opinions as to what the sources they cited actually had argued.

Second, the individuals who propagated the narrative of physicists supporting short time scales and geologists long ones, knew, just as Walcott did, that this was not the case. Winchell arrived at the shortest estimate based on geological methods, and soon John Perry, a physicist and a student of Lord Kelvin, would argue for ages in the 1000s of millions years.[44] King, a geologist, argued for a short time and Woodward, a physicist, disagreed with him. The reason why scientists answered the age of the Earth question by narrating the disciplinary conflict was that they were echoing the consensus answer which emerged from the dialogues which did not necessarily correspond to what they believed, but which was the expected answer. Each utterance replied to a specific previous utterance and was addressed at a specific person; it aimed to meet its audience's expectations (in content and in form) as much as it corresponded to the speaker's beliefs. Just as during the meeting of the Geological Society of Washington, when the context of the debate shifted, so did the content of the opinions expressed on the Earth's age,

The conclusion which can be drawn is that the scientific community did not share a single idea of what Walcott argued. What they shared was the knowledge that when discussing the age of the Earth one needed to mention the geologists vs. physicists conflict. Just as Lord Kelvin was the representative for the physicists' point of view, Walcott was the representative for geologists' point of view. There was consensus, not a consensus of shared belief, but a consensus of a shared reply. To the question "how old is the Earth" scientists answered: physicists and geologists are in conflict, Kelvin says it is short while Walcott says it is long(er).

[44]John Perry, "On the Age of the Earth," *Nature* 51 (1895): 224–227; John Perry, "The Age of the Earth," *Nature* 51 (1895): 582–585; Brian C. Shipley, "'Had Lord Kelvin a right?': John Perry, Natural Selection and the Age of the Earth, 1894–1895," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 91–105.

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Based on an analysis of dialogue a notion of discordant consensus becomes attainable. It is *discordant* because it is based on unique individual understandings and because with each new utterance, with each new reply, the new utterance risks exposing the dissimilarity of individual understandings. But it is still a *consensus* because for a moment, in a specific place and time, there does exist a common action—the common reply.

For every group there are some utterances which elicit a common reply, and I think it is those common replies that we refer to as shared knowledge. In this view, consensus is an event, not an object. Consensus is a phenomenon which emerges—not from its correspondence to the world; nor from constructed, intersubjective agreement; but from temporarily common responses. But those temporary agreements, once they emerge, can achieve (relative) permanence when they are written down, especially in things like textbooks.

Analyzing Walcott's paper as an addressive reply solves the seeming contradictions contained within it, but it also points to a larger issue. Much of what scientists have said on the Earth's age was in presidential addresses and other utterances aimed at wider audiences. Those utterances did not necessarily represent what the authors knew to be the case; however, historians have taken them at face value and assume they do represent what the authors believed.

Part II

20th Century

American Science at the Turn of the 20th Century

Historians of science are attracted to revolutions even when they deny their existence.[45] One example of this is our drive to discuss debates about the age of the Earth in disciplinary terms and to speak of a revolutionary change that took place in our view of the Earth's age: we know the simple story is not exactly the case, but something rings true about it. In 1899, the consensus (of a discordant kind) was that the Earth's cooling was the source of its energy, the Earth was about 100 million years old, and that this was the time frame in which all geological and biological changes took place. By 1930, the Earth was over a billion years old, heated by radioactive decay, and geological and biological evolution took place in this extended time frame. There was a lot of new temporal territory to be filled by geologists. Such a radical expansion of time in which to fit the geological column should have left one breathless. Yet, in the remainder of my dissertation, I show that this change happened without a violent revolution, without any crisis to match the size of the change, and without much of a public debate. It happened in the background by small shifts in priorities.

This was a "velvet revolution" because the age of the Earth and calculations of absolute measures of geologic time were not the primary focus of any discipline and only a few (if any) individual scientists. (However, the question still remained of much interest to many). The second reason is that the situation in 1899 was not

[45] For example, see the famous first line "There was no such a thing as the Scientific Revolution and this is a book [titled *The Scientific Revolution*] about it." Steven Shapin, *The Scientific Revolution* (Chicago: University of Chicago Press, 1996), p. 1.

free of anomalies. As I described in Part I, there was no way to account for all the accepted facts and not produce some contradictions or have some facts left over to be explained away. The consensus achieved was an accepted way of dealing with those contradictions. Throughout this period, there remained contradictions and unaccounted facts; however, the facts that were unaccounted changed.

Finally, the revolutionary shift without a revolution can be seen in light of the understanding of the community as described in Part I; that is, as the difference between responding as a community member versus responding as an individual. The revolutionary change was that, in 1899, researchers, when responding as community members, believed that they were expected to speak as if the community knowledge was the Earth was about 100 million years old and measured by denudation and sedimentation methods, whereas after 1930 they spoke as if the community knowledge was the billion year time scales and radiometric methods of determining them. I will schematically illustrate how this change could take place without a large public controversy.

Assume that at one point an individual believed there was way a and way b to deal with the age of the Earth (e.g., using radioactive decay measurements and using salt content of the oceans). The individual believed that b was slightly preferable, but still problematic and a, though less preferable, had some merits as well. Or maybe the individual held that both are of equal merit even if both cannot be concurrently true. Also, assume that at this time the community knowledge was b. The individual could answer questions of the Earth's age with b and concur with community knowledge. If then the situation changed that community knowledge became a, and the individual decided to concur with community knowledge and answer questions about Earth's age with a, she could do so without a revolutionary personal change, especially if the question of the Earth's age was not initially of high importance. However, analyzed at a community level, if most individuals started answering *a* over *b*, a radical shift had occurred. This is exactly what happened. How and why is the subject of the next three chapters.

In brief, there were very few individuals who were personally deeply invested in the question of the Earth's age. They argued for their own understanding to become community knowledge. The rest of the community members had little interest in what the Earth's age was and when answering questions on the Earth's age were only mirroring what they believed was the view of the community. This community view during the 1920s was still without a clear preference for a time scale or method of determining the time scale. However, at the same time, a new group of researchers, who would later become accepted as authorities on the question of geologic time, was already well underway assembling a research program

Contrary to science textbook history, it was not the case that the discovery of radioactivity immediately settled age of the Earth questions. Radioactivity was just one of multiple new developments relevant to the Earth's age research. The first decades of the twentieth century were not a period of increasing consensus, but of splitting opinions. Though by mid-1920s the research program that dominated the rest of the 20th century had its foundations established, to the contemporaries it was not at all clear that the issue of the Earth's age, or how to go about answering questions about it, was established.

In this Part, I also argue that the opening decades of the 20th century, the discussion of the Earth's age fragmented from one dialogue among the majority of scientists, to separate disciplinary discussions. This fragmentation will make more sense when seen as happening against the backdrop of the changes taking place in American Science during the period under consideration.

An important year was 1893. Political and economic turmoil was at least partially responsible for Clarence King's rush to publish partial results before funding ran out and work conducted by Carl Barus stopped. Challenges to Powell's leadership resulted in his resignation (and with it, McGee's) and a change of direction for the USGS. Walcott took over as the director, giving even more authority to his paper.

King and Powell had been leaders of the pioneering surveys in the middle third of the century—surveys that filled in the blank spots on America's map. By the 1890s the continental United States was no longer terra incognita to scientists. The work was no longer exploration, but explanation. Through the 1880s and the 1890s, USGS was the biggest scientific organization in the world, employing over 200 scientists on full or part-time bases with an annual appropriation reaching nearly million dollars.[46] However, just as Washington science was peaking, its time was over. Two years prior to the funding of the Cosmos Club, in nearby Baltimore, the Johns Hopkins University was funded. The effort of the various societies in Washington to unify themselves during the 1890s was against the trend of establishment of ever-more important disciplinary associations in the rest of the country. This was was also the time when the University of Chicago opened its doors. Whereas throughout the second half of the 19th century it was the federal government that supported science, with the turn of the century an increasingly larger share of the scientific activity was conducted outside government agencies. The universities, along with private institutes and industrial research labs, were the new centers. The science that was performed there was no longer in service of the nation, but in pursuit of "pure science" and profit.[47]

[46]$879,000 in 1890, Stephen P. Turner, "The Survey in 19th-century American Geology: The Evolution of a Form of Patronage," *Minerva* 25 (1987), p. 323.

[47]The following brief description is mostly based on Roger L. Geiger, *To Advance Knowledge: The Growth of American Universities 1900–1940* (New York: Oxford University Press, 1986). For more on the changes in organization of American science during this period see also, Laurence Veysey, *The Emergence of American University* (Chicago: University of Chicago Press, 1965); Stanley M. Guralnick, "The American Scientists in Higher Education," in Nathan Reingold (ed.), *The Sciences in the American Context: New Perspectives* (Washington, D.C.: Smithsonian Institution Press, 1979): 99–141; Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Cambridge University Press, 1978); Julie A. Reuben, *The Making of the Modern University: Intellectual Transformation and the*

The 1890s experienced an "academic boom," as Laurence Veysey called it.[48] University orientation shifted to a research university centered around natural sciences and graduate education. During the 19th century, even the few universities that had Ph.D. programs were not the primary sites of education for professional scientists. However, that changed dramatically during the first two decades of the 20th century. A symbolic event of this transformation was the formation of the Association of American Universities, an organization of *research* universities, in 1900.

One of the many changes brought on by the new university ideals was the rise of the academic departments. The University of Chicago, under the direction of William R. Harper is perhaps the best example of this change. During the 1890s, for administrative reasons as more than one faculty were hired to teach the same subjects, the new universities were structured into disciplinary departments that became isolated from each other and from other parts of the university in general. The standardization of the meaning of the term 'department' by the Association of American Universities in 1908 symbolized the growing importance of this administrative unit. A number of those departments started publishing disciplinary journals just as the federal institutions and scholarly associations once had. One example was *Journal of Geology* published by the University of Chicago Press. There was a relationship between departments and disciplines as the scientific disciplines looked to universities for their legitimization (by forming disciplinary departments) and used their place as university departments as their guaranteers

Marginalization of Morality (Chicago: University of Chicago Press, 1996); John Lankford, *American Astronomy: Community, Careers, and Power, 1859–1940* (Chicago: University of Chicago Press, 1997); Sally Gregory Kohlstedt, Michael M. Sokal and Bruce V. Lewenstein, *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science* (New Brunswick, N.J. and London: Rutgers University Press, 1999). A historiographic overview is David Cahan, "Institutions and Communities," in David Cahan (ed.), *From Natural Philosophy to the Sciences: Writing the History of Nineteenth-Century Science* (2003): 291–328.

[48] Veysey, *op. cit.*, pp. 264–8.

of jurisdiction over their topic areas. (Unlike the professional groups, scientists did not seek that jurisdiction through public licenses.) Trends toward specialization were driving science toward fragmentation and new subspecialized associations and journals emerged outside of universities. Without support from universities, however, they could not achieve permanent institutionalization. The academic departments increasingly began to control the agenda of scientific disciplines.[49]

Geiger argues that the academic disciplines emerged as a result of a more general move toward professionalization and increasingly specialized knowledge. Associations and other social institutions created community structure and universities provided a place in which to practice the disciplines. "The process of discipline formation is thus one of the indispensable components in the shaping of the research universities."[50] The changes led to homogenization of American universities, which by 1910, looked much more like each other than they did in 1890 (though there were still large differences among them), not only in their administrative structure, but also by the introduction of disciplinary undergraduate majors.[51]

However, the university, too, had its competition. It had to compete with the government and private institutions. Even by 1920 it was still not clear that the research university was the proper seat for scientific research. When Andrew Carnegie and John D. Rockefeller looked to endow academic institutions, they

[49]Robert Kohler, *From Medical Chemistry to Biochemistry: The Making of Biomedical Discipline* (1982), Geiger, *op. cit.*, pp. 20–39, Kevles, *Physicists*. Hugh Hawkins, *Banding Together: The Rise of National Associations in American Higher Education, 1887-1950* (Baltimore: Johns Hopkins University, 1992) argues that the standardization of the university administrative structure was part of an effort to increase the prestige of American universities abroad and to ensure distinction between research universities and other institutions of higher learning. In 1901, Columbia University's Frederick Keppel advocated that the department be the fundamental unit of university.

[50]Geiger, *op. cit.*, p. 20.

[51]The academic disciplines understood as "groups of professors with exchangeable credentials collected in strong associations" was a uniquely American phenomenon prior to mid-[20th] century."Andrew Abbott, *Chaos of Disciplines* (Chicago: Chicago University Press, 2001), p. 123.

created the independent Carnegie Institution of Washington and the Rockefeller Institute of Medical Research in addition to having endowed universities.[52]

This change in organization was in large part driven by the increase in size of the institutions. For example, in 1899 there were 167,999 students enrolled at research universities. By 1929 this number rose over five-fold to 924,275 while the student-to-faculty ratio actually decreased. Over the same period, library holdings of some of the top fifteen research universities increased by as much as ten-fold and all of them increased at least three-fold, while the number of Ph.Ds awarded by those institutions increased from 208 to 1420.[53] The membership of the American Association for the Advancement of Science increased from 1,721 in 1899 to 18,462 in 1929.[54] The number of entries in the 1906 edition of *American Men of Science* was about 4,000. The 1921 edition included about 9,500. According to the editor, the main reason for the larger number was "increase in the number of scientific men."[55] In addition to changes in the organization of science, this increase of the number of scientists and publications in connection with the ever-more specialized and divided scientific community, diluted the literature relevant to the Earth's age. The chances of an article relevant to the discussion of the Earth's age being read by any one scientist presumably decreased.

This period also saw the rise of disciplinary professional associations. The Geological Society of America, American Physical Society, Astronomical and Astrophysical Society of America, and American Society of Zoologists were all established between 1888 and 1899. Many of those new disciplinary societies and departments within years of their establishment commenced publication of disciplinary journals that became the primary fora for the presentation of research by

[52] Geiger, *op. cit.*, p. xi.
[53] *Idem*, pp. 270–277.
[54] Kohlstedt, Sokal and Lewenstein, *op. cit.*, p. 174–175.
[55] James McKeen Cattell and Dean R. Brimhall, *American Men of Science: A Biographical Directory* (Garrison, N.Y.: The Science Press, 1921), p. viii.

the societies' members. As this specialization and professionalization heightened, scientists preferred to give papers in front of their disciplinary societies, rather than general ones such as the AAAS, though many of the disciplinary associations met concurrently with the AAAS.[56]

Not all scientists were happy about this splintering of the AAAS and American science in general. A few, such as McGee, unsuccessfully pleaded against the fragmentation.[57] However, the majority either did not object or proposed to impose additional organizations to counteract the specialization trend. As Andrew Abbott points out, interdisciplinarity arises together with disciplinarity.[58] Thus, American scientists formed organizations like the Committee of One Hundred on Scientific Research in 1913 and its competitor the National Research Council in 1916, which explicitly intended to create interdisciplinary links among scientists of various fields.[59]

From the point of view of the dialogical analysis, the academic discipline composed of university departments and their journals provided a new space for dialogue with an audience defined by intellectual interests much more than geographic location.[60]

The dialogues on the Earth's age paralleled the development in the change of organization of American science. Federal institutions took a back seat to the research university and privately funded institutes as centers of research, and the

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[56]Michael M. Sokal, "Promoting Science in a New Century," in *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science* (New Brunswick, N.J. and London: Rutgers University Press, 1999): 50–102; Geiger, *op. cit.*, pp. 20–39.

[57]W J McGee, "Fifty Years of American Science," *Atlantic Monthly* (1898): 308–320; Sokal, *op. cit.*, p. 51.

[58]Abbott, *op. cit.*, pp. 131–136.

[59]Sokal, *op. cit.*, p. 66–71; Glenn E. Bugos, "Managing Cooperative Research and Borderland Science in the National Research Council, 1922–1942," *Historical Studies in the Physical and Biological Sciences* 20 (1989): 1–32.

[60]See also, Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines* (Stanford, Cal.: Stanford University Press, 1997), pp. 50–51 who argues that disciplines provide stability to scientific social practice overcoming the disunity of science.

synthesizing approach of Powell was supplanted by splintering diversification of university departments. At the same time as those organizational changes were taking place, new research results challenged the recently established consensus on the Earth's age. George Becker tried to reestablish a consensus on the Earth's age by revaluating the new evidence, but unlike previous decade's effort, this one failed. Following this failure, during the 1910s a number of separate approaches based in different disciplines developed independently of each other. A new consensus was achieved around 1930 as a result work sponsored by NRC; however, this consensus emerged independently, or maybe even despite, NRC's effort.

Chapter 4

Challenging the Scope of Consensus

I continue my revision of the narrative presented by the historical actors under investigation (and still appearing in some histories) that the discovery of radioactive heat resolved the Earth's age problem.[1] The science textbook narrative is that after 1860, the main argument of "the physicists" was that the Earth was cooling and that there was not enough heat to account for the long-time scales demanded by "the geologists." After 1903, this argument could be countered by a new source of energy, heat from radioactivity, that could keep the Earth hot indefinitely. The standard explanation given by historians of the seeming delay by geologists in accepting radioactivity as the source of heat and the longer time estimates that resulted from early measures of radioactive decay, is that the geologists had their own ways of estimating the age of the Earth that pointed toward one hundred million years.[2] This is partially true, but the situation during the first two decades of the 20th century makes more sense if seen as a time of con-

[1]Though both Oreskes and Brush in their excellent studies present much more nuanced views, they still make the following statements: "The immediate result of these discoveries, besides capturing the attention and imagination of the world at large, was to settle geology's long-standing debate with Lord Kelvin over the age of the Earth." Naomi Oreskes, *The Rejection of Continental Drift* (New York: Oxford University Press, 1999), p. 48. "The debate between Kelvin and the geologists on the age of the Earth was, of course, eventually settled by the discovery of radioactivity [...] By 1905 Rutherford and his colleagues were proposing a time scale of billions of years" Stephen G. Brush, *Fruitful Encounters: The Origin of the Solar System and of the Moon from Chamberlin to Apollo* (New York: Cambridge Univ. Press, 1996), p. 4.

[2]Stephen G. Brush, "The Age of the Earth in the Twentieth Century," *Earth Science History* 8 (1989): 170–182.

fusion about the age of the Earth in the scientific community. Prior to 1900, the geologists shared an answer to the question "How old is the Earth?" They did not manage to convincingly claim that the age of the Earth lay in their jurisdiction, but at least geology had a shared claim with the physicists (who at least in the U.S. were not vocal). After the turn of the century, the geologists lost both any kind of consensus answer and a claim that the Earth's age question was a question to be answered by geology.

There were four developments that challenged the consensus answer to the questions of the Earth's age: T.C. Chamberlin's new cosmogony, a new method for measuring the age of the oceans, discovery of heating effects of radioactive elements, and the use of radioactive decay series as time measuring device. In this chapter, I discuss each in turn, again stressing how each came about as part of a specific dialogue that was not primarily about geologic time or theEarth's age.

The consensus as described in Part I depended on the shared expectation of a reply. However, because such shared expectations are based on previous experiences, there is never any guarantee that the next time a question appears the reply that was once adequate will be again. That is the consensus about conversation in light of specific background: when that background changes, the status of the consensus is threatened. Any new development is in a way anomalous because the previous consensus has not been developed with it in the background. It may be the case that the members of a community will respond uniformly to the new development; however, that is not guaranteed. The new developments described below need to be seen as presenting that kind of challenge to the consensus. It was not the case that they inherently supported or challenged the status quo, but they simply had no place in the consensus answer because they were not around when this answer emerged in the early 1890s. Before a new consensus answer could emerge, scientists would have to reengage in a dialogue to situate the new knowledge in relation to the consensus answer. One such attempt to reestablish a consensus failed. There was no new consensus until the 1930s. But first let us review the challenges to the consensus answer.

Chamberlin-Moulton Cosmogeny

Thomas Chrowder Chamberlin's (1843–1928) interest in the question of the Earth's age was due to his individual understanding of the world and was secondary to issues of the origin of the Earth, an interest itself secondary to his investigations of the cause of ice ages. Chamberlin's development, presentation, and reception of his beliefs by others are in line with the discussion of individual understanding and community knowledge described in Part I.[3] In 1899, Chamberlin published a criticism of Lord Kelvin's estimate of the age of the Earth. Chamberlin suggested that Kelvin was wrong in assuming that the Earth started as a molten sphere of rock. Chamberlin, expanding on his calculations of the Earth's historical climate, calculated that an initially hot Earth, rotating with high velocity, as assumed by Laplacian nebular hypothesis, would not be able to retain any atmosphere. By 1905, in collaboration with Forest Ray Moulton, Chamberlin devised a new cosmogony which proposed that the Earth originated by aggregation of small solid bodies. Such an Earth would have never been entirely molten and therefore the starting premise of Kelvin's calculation was removed. The Chamberlin-Moulton theory, know as planetesimal hypothesis, was relatively widely accepted, at least in the United States.

T.C. Chamberlin's geological interests were in the very recent geological period and his investigations focused on the theories of the ice ages.[4] It is those theories

[3]For biographical information, see Rollin Thomas Chamberlin, "Biographical Memoir of Thomas Chrowder Chamberlin 1843–1928," *Memoirs of the National Academy of Sciences* 15 (1932): 307–407; Susan F. Schultz, *Thomas C. Chamberlin: An Intellectual Biography of a Geologist and Educator* (Ph. D. diss.), University Wisconsin-Madison, 1976.

[4]For example, see Susan F. Schultz, "The Debate over Multiple Glaciation in the United

that provided a connection between the most recent geological events with the oldest ones (and skipped over the vast majority of the geological column in between). Chamberlin started to work on his planetesimal hypothesis in 1892, the same year he took a job as a chairman of University of Chicago's geology department. He was led to it by investigating theories of the glaciation periods, the geology of which was his specialty. Initially he followed theories that the decrease in carbon dioxide was the cause of the Ice Ages. That is, the initial hot Earth had an atmosphere high in carbon dioxide. Chamberlin proceeded to investigate the early atmosphere and was influenced by the Irish scientist G. Johnstone Stoney who argued that the gases with smaller molecular weights and/or at higher temperatures were more likely to escape from an atmosphere. In his first published remarks on cosmogony in 1897, Chamberlin attacked the nebular hypothesis and especially the assumption that the Earth originated as a hot molten sphere by arguing that if that were the case and if the atmosphere were as hot as the molten rock beneath it, then, based on the kinetic theory of gases, just about every gas would had attained escape velocity and left the planet beneath it with hardly any atmosphere (even more so if the planet was rotating rapidly as suggested by George Darwin). Chamberlin saw formation of a cool Earth as a solution to the problem of an early atmosphere. He speculated that the evidence usually presented in support of the nebular hypothesis (the shape of nebulae, Saturn's rings) were not really evidence for it at all, and it was more likely that the gas ejected from a rotating nebula would solidify into solid particles before condensing as a planet. If this were the case, the Earth's age must be considerably older than previously calculated based on the assumption of a molten globe.[5]

States: T. C. Chamberlin and G. F. Wright, 1889–1894," *Earth Sciences History* 2 (1983): 122–129 which discuses Chamberlin and George F. Wright—the same disagreement discussed in previous chapter in which McGee was involved. See also, James R. Fleming, "T. C. Chamberlin, Climate Change, and Cosmogony," *Studies in History and Philosophy of Modern Physics* 31B (2000): 293–308.

[5]Thomas Chrowder Chamberlin, "A Group of Hypotheses Bearing on Climatic Change,"

Chamberlin was provoked to present his views on the Earth's age publicly by an 1899 reprint of an 1897 address in which Kelvin praised Clarence King's estimates and revised his own calculations for the Earth's age to be "more than 20 and less than 40 million years ago, and probably much nearer 20 than 40."[6] Chamberlin replied with a sharp critique.[7] He started by acknowledging that "contributions of Lord Kelvin, based on physical data, have been most powerful influences in hastening and guiding the reaction against the extravagant time-postulates of some of the earlier geologists. [...] Geology owes immeasurable obligation to this eminent physicist for the deep interest he has taken in its problems and for the profound impulse which his masterly computations and his trenchant criticisms have given to broader and sounder modes of inquiry."[8] After this initial recognition, the praises ended and Chamberlin proceeded to the standard critique of Kelvin's calculations: "it must be recognized that any one line of reasoning, however logically and rigorously followed, is quite sure to lead astray if it starts from limited and uncertain premises. [...] There is, perhaps, no beguilement more insidious and dangerous than an elaborate and elegant mathematical process built upon unfortified premises."[9] Chamberlin in particular called out the presumed accuracy of Kelvin's calculations who wrote about "half an hour after the solidification" or "a depth of 'several centimeters.' "

Chamberlin noticed that Kelvin was not clear on the crucial details of the origin of the Earth, such as the rate at which the meteorites that originally formed the planet fell. For the Earth to have started as a molten sphere, that rate would

Journal of Geology 5 (1897): 653–683.

[6]William (Lord Kelvin) Thomson, "The Age of the Earth as an Abode Fitted for Life, I," *Science* 9 (1899): 665–674, quote on p. 671; William (Lord Kelvin) Thomson, "The Age of the Earth as an Abode Fitted for Life, II," *Science* 9 (1899): 704–711.

[7]Thomas Chrowder Chamberlin, "Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life, I," *Science* 9 (1899): 889–901; Thomas Chrowder Chamberlin, "Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life, II," *Science* 10 (1899): 11–18.

[8]Chamberlin, *Lord Kelvin on age of the earth I*, p. 890.

[9]*Idem.*

have to have been high, which, Chamberlin argued, was not necessarily the case. He taunted "has Lord Kelvin, or any other of our great teachers in physics or in astronomy, followed out to a final conclusion, by the rigorous processes of mathematics, the method and rate of aggregation of a multitude of meteorites into a planet, so as to be able to authoritatively instruct us as to the rapidity at which the ingathering would take place?"[10] pointing to the authority of mathematics that Kelvin so valued. Chamberlin was not arguing that the slower rates of meteorite infall would necessarily make a difference in calculating the Earth's age; however, he insisted that high rates could not be simply assumed as the starting conditions.

Chamberlin's challenge to Lord Kelvin shocked some British observers, but it was much less of a surprise on this side of the Atlantic, coming as it did from a geologist who rose to prominence in the USGS during its early years when all early explorers prided themselves in challenging the authors, both European and American alike, of books from which they learned their craft.

Chamberlin understood which audience he was addressing and how this audience evaluated scientific claims. He wrote "Can the uninstructed layman or the young geologist safely restore confidence in these or any other chronological conclusions as determinate?" He questioned whether Kelvin's utterance "can [...]be allowed to pass without challenge?"[11] Chamberlin was aware that seasoned scientists were familiar with the earlier debates on the Earth's age, but that the uninitiated needed to see a challenge to Kelvin's statements so as to make it clear that Kelvin's utterance was not representative of the scientific community.

Chamberlin reevaluated the arguments presented for estimation of geologic ages. He proposed as one possible answer to the limited supply of energy that the initial homogenous distribution of metal and rock has been slowly rearranging

[10] *Idem*, 893-4.
[11] *Idem*, p. 891.

so that the heavy rock has been moving toward the center. If this were the case, not all of the potential energy of the meteors would have been lost a long time ago, but it would have been continually released. That is, one cannot assume that the Earth has been cooling. Chamberlin further suggested that slow convection could have taken place, which led to Earth's shrinking, releasing even more heat energy. Similarly he proceeded to reevaluate arguments for the Earth's age based on theories of tides and moon formation. Again he found them not very conclusive. He also had an answer to the third way of limiting Earth's age by arguing for the limited energy supply of the sun. Chamberlin asserted that "no careful chemist would affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. [. . .] Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy."[12] However, even without resorting to an unknown source of energy, Chamberlin presented a list of problems not accounted for by those, like Kelvin, who calculated only short time periods for the adequate energy supplied by the sun. Chamberlin, for example, noticed that the physical conditions occurring in the sun had not been properly accounted for and Kelvin and others treated the gas in the sun, which they admitted must have had existed in unearthly conditions, as though it were a regular gas. Finally, he objected that even if the calculations for the total amount of energy produced by the sun were correct, those calculations told us little about the time during which this energy had been dissipated.

Chamberlin directly investigated cosmogenic questions following the observation of the Nova Persei of 1901, which many scientists attributed to a collision of two stars and thought that such collisions were not terribly uncommon. Chamberlin pointed out that a full collision was not necessary and a close approach, a

[12]Chamberlin, *Lord Kelvin on age of the earth II*, p. 12.

much more frequent event, was enough to destroy one of the bodies and give rise to a nebula that would be the origin of a planetary system. By 1902 Chamberlin realized he needed more help developing his theory and wrote to Charles Walcott. He knew Walcott from the USGS and Walcott now was on the board of Carnegie Institution of Washington.

The first public presentation of the planetesimal hypothesis, as Chamberlin-Moulton's theory was called, was at a talk given at the Washington Academy of Science on 29 March 1904. Chamberlin's paper was commented on by Simon Newcomb, George Becker, and Grove K. Gilbert. Newcomb gave it qualified support, but Becker objected to it, as did Gilbert. Robert Simon Woodward (the physicist who objected to King's theory at the 1893 meeting) was impressed with Chamberlin's theory. Chamberlin was not impressed with those comments and believed that the commentators missed much of his argument, but that "Many pleasant things were said privately."[13] The theory was published in Carnegie Institute of Washington yearbook for 1904.[14]

The first full elaboration of the theory was in the second volume of Chamberlin and Rollin Salisbury's geology textbook. The Chamberlin-Moulton theory, as it was referred, was the most important among several theories that challenged the validity of the Nebular hypothesis. When presenting the theory in full for the first time in his textbook, Chamberlin (with Salisbury as his co-author) realized that textbooks increasingly had as their role introducing the student to the community knowledge, rather than introducing new theories of the authors as was often the case during the 19th century. Chamberlin did introduce the new theory in a textbook; however, the authors presented in detail the alterative theories with

[13]Chamberlin to Moulton, 28 April 1904, cited in Stephen G. Brush, "A Geologist Among Astronomers: The Rise and Fall of the Chamberlin-Moulton Cosmogony, Part I," *Journal for the History of Astronomy* 9 (1978), p. 25.

[14]Thomas Chrowder Chamberlin, "Fundamental Problems of Geology," *Year Book Carnegie Institution of Washington* 3 (1905): 195–228.

which the planetesimal theory was competing (though, they made a strong case for planetesimal theory).[15] That the presentation of competing theories was a faithful effort, not just a straw man, is testified by H.L. Fairchild's review of a later edition of the book in which he wrote that in treatment of the origins of the Earth "the authors have been fair in the treatment of old views and modest in presenting" their own ideas.[16] Despite its popularity, the Chamberlin-Moulton theory was never fully embraced by geological or astronomical communities. However, the negative arguments presented against nebular hypothesis were convincing to both communities and the hypothesis had an impact on the discussion of the Earth's age, for example, in the writing of Arthur Holmes, the British promoter of radiometric methods for measurement of the Earth's age, who thought the theory was the best of the available alternatives to the Nebular hypothesis.[17]

The Earth's age always had been peripheral in Chamberlin's considerations of geologic theories. Rollin T. Chamberlin, remembering his father, wrote, that after 1899 "Lord Kelvin's time restrictions never bothered geologists."[18] That paper certainly was a discussion of the Earth's age, but primarily it was a response directed at the nebular hypothesis. The age of the Earth was implicated by its association with that hypothesis in Kelvin's calculation. Through the rest of his career, Chamberlin adjusted what he thought was the age of the Earth to the current general opinion. During the first decade of the 20th century, Chamberlin referred to a time scale of a hundred million years. By 1916 he was ambiguous: preferring one hundred million years, but admitting the possibility of the longer

[15]Thomas Chrowder Chamberlin and Rollin D. Salisbury, *Geology. Vol 2*, 2nd edition (New York: Henry Holt, 1905), pp. 1–81. For a discussion of the development and reception of the theory see Brush, *Geologist Among Astronomers I*; Brush, *Geologist Among Astronomers I*; and Brush, *Fruitful Encounters*, pp. 22–67.

[16]H. L. Fairchild, "Review of *Introductory Geology, a Text-Book for Colleges* by Thomas C. Chamberlin; Rollin D. Salisbury," *Science* 40 (1914), p. 817.

[17]Arthur Holmes, *The Age of the Earth* (London and New York: Harper, 1913), pp. 29–30.

[18]Chamberlin, *op. cit.*, p. 352.

scales and not caring much. And during the 1920s, Chamberlin supported the billion-year time scales for the age of the solar system, while still not discounting the much shorter scales based on the sedimentation calculation.[19]

Chemistry

The next significant event altering the dialogues on the Earth's age was the 1899 publication by the Irish geologist, John Joly, of a new estimate of the age of the oceans.

Although geology and physics took center stage in the age of the Earth narratives, chemistry played a significant role in attempts to answer the question of the Earth's age from the 18th century to the present day.[20] In the early 19th century, refereing to at-the-time most mysterious and most advanced area of research, Charles Lyell suggested that some electro-chemical reactions continually supply

[19] "The estimate of the years thus represented has been put variously from 50,000.000 to 100,000,000, with indeed higher figures as well as lower. Merely to roughly scale the order of magnitude, and without pretense of accuracy, let us take the midway figure of 75,000,000 years as representative."Thomas Chrowder Chamberlin, "A Geologic Forecast of the Future Opportunities of Our Race," *Science* 30 (1909), p. 943. "during its existence, be it a hundred million years-or ten hundred million years, if you please"Thomas Chrowder Chamberlin, "The Evolution of the Earth. Part I," *Scientific Monthly* 2 (1916), p. 418. See Stephen G. Brush, "A Geologist Among Astronomers: The Rise and Fall of the Chamberlin-Moulton Cosmogony, Part II," *Journal for the History of Astronomy* 9 (1978), p. 85 for unpublished manuscript. Thomas Chrowder Chamberlin, "Diastrophism and the Formative Processes. XIII. The Bearing of the Size and Rate of Infall of planetismals on the Molten or Solid State of the Earth," *Journal of Geology* 28 (1920); Thomas Chrowder Chamberlin, "The Age of the Earth from the Geological Viewpoint," *Proceedings of the American Philosophical Society* 61 (1922). For more discussion of Chamberlin's later view see the next chapter.

[20] On general relationships between chemistry and geology see A. A. Manten, "Historical Foundations of Chemical Geology and Geochemistry," *Chemical Geology* 1 (1966): 5–31; Helge Kragh, " The Chemistry of the Universe: Historical Roots of Modern Cosmochemistry," *Annals of Science* 57 (2000): 353–368; David Roger Oldroyd, "The Earth Sciences," in David Cahan (ed.), *From Natural Philosophy to the Sciences: Writing the History of Nineteenth-Century Science* (2003), 125–126. On geochemistry and United States see John W. Servos, "The Intellectual Basis of Specialization: Geochemistry in America, 1890–1915," in John Parascandola; James C. Whorton (ed.), *Chemistry and Modern Society* (1983); John W. Servos, "To Explore the Borderland: The Foundation of the Geophysical Laboratory of the Carnegie Institution of Washington," *Historical Studies in the Physical Sciences* 14 (1983): 147–185; John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton, N.J.: Princeton University Press, 1990), ch. ch 5.

energy to keep the Earth at constant temperature. But even before this early 19th century reference to chemistry, already in 1715 Edmund Halley suggested a chemical method for calculating the age of the Earth. Halley observed that lakes which lack any outflow, but which receive runoff from rivers should increase their salt content. If the rate of this increase were known, the age of the world could be determined. He suggested that the Royal Society start measuring salt content of lakes and oceans so that the rate could be determined.[21]

Halley's suggestion does not seem to have been followed and the idea of determining Earth's age by chemical means was not brought up again until 1876 when T. Mellard Read proposed that the concentration of sulfates and chlorides could be used to determine geological age. Instead of directly measuring the rate of change of those concentrations in the oceans, Read suggested that the rate at which rivers deposit impurities to the oceans could be measured. Inspiration for this came method after reading the Sixth Report of the River Pollution Commission "When I first turned over its pages, it at once occurred to me that here is the thing which a geologists wants for the solution of several interesting problems."[22] Read's method based on chemical denudation as opposed to the mechanical denudation, relied on the substances that were dissolved in water. During the 1870s, Read argued against Thomson's short time scales and, based on his calculations, suggested an age of the Earth around 500 million years.[23]

It was not, however, until 1899 that the chemical means of estimating geological time scale altered the dialogues on the Earth's age after John Joly presented a new

[21] Edmund Halley, "A Short Account of the Cause of the Saltness of the Ocean, and of the Several Lakes That Emit no Rivers; With a Proposal, by Help Thereof, to Discover the Age of the World," *Philosophical Transactions (Royal Society of London)* 29 (1714–1716): 296–300.

[22] T. Mellard Reade, "President's Address," *Proceedings of the Liverpool Geological Society* 3 (1874–1878), p. 212.

[23] *Idem*; T. Mellard Reade, *Chemical Denudation on Relation to Geological Time* (London: David Bogue, 1879); Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (New York: Science History, 1975), pp. 98–100.

calculation of the age of the oceans in a widely read article. The principle behind Joly's method was a simple one:

> Now, if any of the elements entering the ocean is not again withdraw, but is in a word 'trapped' therein, reappears as no extensive marine deposit, and is not laid down sensibly upon its floor, and if the amount of uniformity already defined is accepted, evidently in the rate of annual accretion by the ocean, from the rivers, of this substance and the amount of it now in the ocean, the whole period since the beginning of its supply can be estimated.[24]

All the ifs had to be accounted for and certain correction introduced; however, in the end, Joly arrived at an estimate of around 90 million years, which nicely agreed with what he thought were the other important calculations of the Earth's age. Joly continued to publish on the chemical means of estimating the Earth's age; however, with minor corrections, his argument stayed unchanged.[25]

Joly's arguments gathered a number of replies and were disseminated widely. In America, in addition to the reprint in *Report of the Smithsonian Insinuation*, the article was abstracted in the *American Journal of Science* along with Archibald Geikie's address also on the Earth's age.[26] It was discussed in detail in an address by William J. Sollas.[27] By 1910, George Becker could write "As is well known to all geologists, the very important method of estimating the age of the Earth devised by Mr. J. Joly."[28] Becker and another USGS chemist, Frank W. Clarke (1847–1931), continued to make new investigations using the chemical method at

[24]John Joly, "An Estimate of the Geological Age of the Earth," *Annual Report of the Smithsonian Institution* (1899), pp. 248–9.

[25]Additional papers include: John Joly, "On the Geological Age of the Earth," *Report of the 70th meeting of the British Association for the Advancement of Science* (1900): 369–379; John Joly, "Radium and the Geological Age of the Earth," *Nature* 68 (1903): 526. For detailed discussion of Joly's arguments and their reception, see Patrick N. Wyse Jackson, "John Joly (1857–1933) and His Determination of the Age of the Earth," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 107–120.

[26]anonymous , "Scientific Intelligence," *American Journal of Science* 8 (1899): 382–400.

[27]William Johnson Sollas, "Evolutional Geology," *Annual Report of the Smithsonian Institution* (1900): 289–315.

[28]George Ferdinand Becker, "Reflections on Joly's Method of Determining the Ocean's Age," *Science* 31 (1910), p. 509. Becker was familiar with Reade's work and in the followed the cited sentence by reminding the reader that it was Hale who first suggested this type of a method. The point, again, was not to be historically accurate, but to refer to the community knowledge.

the end of the decade.

F.W. Clarke's popular and influential *Data of Geochemistry* went through five editions and each one treated the age of the oceans.[29] In the first edition of 1908, Clarke discussed Joly's estimates and pointed to the additional investigations needed to improve the data on which the estimates were based. By the time the second edition was published in 1911, Clarke had obtained that data, and the section on the age of the oceans closely followed Becker's argument of the previous year which is discussed later in this chapter. The third edition of 1916 was similar to the second, though it included citations to some criticism of the method used by Clarke and Becker. The material on the ocean's age in the fourth and fifth editions was unchanged from the third.

In the first edition, Clarke did not discuss age determination based on radioactivity. In the second edition he did so, but pointed to Becker's argument against this method: "the discordance between the foregoing computation and other methods of ascertaining the age of the Earth is extraordinary. From chemical denudation, from palaeontological evidence, and from astronomical data the age has been fixed with a noteworthy degree of concordance at something between 50 and 100 millions of years."[30] The third edition updated the discussion with new developments, but the conclusion stayed the same. The following editions kept on updating the discussion but the conclusion remains the same.

The most intense discussion of Joly's method occurred during the first decade after the initial publication and much of it took place on the pages of the London-based *The Chemical News and Journal of Industrial Science*. However, some of

[29]Frank Wigglesworth Clarke, *Data of Geochemistry*, 1st edition (Washington: Government Printing Office, 1908), series U.S. Geological Survey Bulletin, 330. Each edition was published as a bulletin of the USGS. The five editions were Bulletin number 303 (1908), 491 (1911) 616 (1916), 695 (1920) and 770 (1924).

[30]Frank Wigglesworth Clarke, *Data of Geochemistry*, 2nd edition (1911), series U.S. Geological Survey Bulletin, 491, p. 304.

these discussions were repeated in geological literature and in American literature.[31]

Early Radio-activity

The final developments that altered Earth's age were related to the discovery of radioactivity, which was doubly relevant to the Earth's age dialogues. First, it provided the prophesied unknown source of energy that made the cooling globe argument null, even if the nebular hypothesis were accepted. Second, the decay of one element into another at a known rate could be used to directly measure the age of specific minerals. The first such calculations were announced in 1905. Research on radioactivity captured not only scientists' imaginations, but also the public's. Applications of radioactivity to the problems of solar energy and Earth's age were just as widely presented in the scientific press as they were in publications directed at much broader audiences. It was a topic that was difficult to ignore.

Radioactivity—that is, spontaneous emission of radiation by matter—was discovered in 1896 by Henri Becquerel. In 1903, Pierre Curie and Albert Laborde announced that radium continually emitted heat at a rate high enough to melt more than its own weight of ice every hour.[32] Coupled with the findings of Julius Elster and Hans Geitel and others who presented evidence that radioactive elements could be found in the Earth, water, and air surrounding us, the discovery of the heating effects of radioactivity had profound consequences for the discussion of the age of the Earth and the sun. If there were a new source of energy, then all of

[31] For details see Wyse Jackson, *John Joly*. Some examples include Henry S. Shelton, ",," *The Chemical News and Journal of Industrial Science* 99 (1909): 253; Henry S. Shelton, ",," *The Chemical News and Journal of Industrial Science* 112 (1915): 85; Henry S. Shelton, "The Radioactive Methods of Determining Geological Time," *Philosophical Magazine* 30 (1915): 448–448; Henry S. Shelton, "Age of the Earth and the Saltness of the Sea," *Journal of Geology* 18 (1910): 190–3.

[32] Pierre Curie and Albert Laborde, "Sur la chaleur dégagée par les sels de radium," *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 136 (1903): 973–975.

Lord Kelvin's calculations would be immediately nullified. A number of observers were quick to notice this fact.[33] Though this discovery said nothing new about the Earth's age itself, it presented a problem of a different kind. If the radioactive materials were uniformly distributed through the Earth—that is, if there was as much radium at the core of the planet as at the surface—the Earth would be heating up instead of cooling or maintaining steady temperature. To resolve this problem, scientists proposed that radioactive materials were limited only to the crust.[34]

Another development about the radioactive elements added a new dimension to the issue of geologic time. In 1902, working at Montreal's McGill University, Ernest Rutherford and Frederick Soddy suggested that radioactive elements, as they emit energy, transmutate into different elements.[35] Known as the decay or disintegration series, this transformation was not a direct one. Instead, there were intermediate steps—the original elements transformed into a different element that then transformed into another element and so on until the ultimate stable element. During each step of this series, different rays were emitted. The transformations along each step of the decay series took place continuously, however, at different

[33]Joly, *Radium and the Geological Age*; John Joly, "Radium and the Sun's Heat," *Nature* 68 (1903): 572; Ernest Rutherford, "The Radiation and Emanation of Radium," *Technics* (1904): 171–174; George H. Darwin, "Radio-activity and the Age of the Sun," *Nature* 68 (1903): 496; William B. Hardy, "Radium and the Cosmical Time Scale," *Nature* 68 (1903): 548; Robert J. Strutt, "Radium and the Sun's Heat," *Nature* 68 (1903): 572; F. Himstedt, "Über die radioactive Emanation der Wasser- und Oelquellen," *Annalen der Physik* 13 (1904): 573–582. All of those are European, in America see Clarence Edward Dutton, "Volcanoes and Radioactivity," *Popular Science Monthly* 68 (1906): 543–550 also as Clarence Edward Dutton, "Volcanoes and Radioactivity," *Journal of Geology* 14 (1906): 259–268.

[34]C. Liebenow, "Notiz über the Radiummenge der Erde," *Physikalishe Zeitschrift* 5 (1904): 625–626; Robert J. Strutt, "On the Distribution of Radium in the Earth's Crust, and on the Earth's Internal Heat," *Proceeding of the Royal Society of London. Series A* A77 (1906): 472–485.

[35]One biography is John Campbell, *Rutherford: Scientist Supreme* (Christchurch, N.Z.: AAS Publications, 1999). For a good account of Rutherford and Boltwood's involvement in the the early measurement of geological time, see Lawrence Badash, "Rutherford, Boltwood, and the Age of the Earth: The Origin of Radioactive Dating Techniques," *Proceedings of the American Philosophical Society* 112 (1968): 157–169.

rates, and some were much faster than others. Rutherford suggested using the concept of half-life, which measured the amount of time during which the activity of a radioactive element dropped in half. This theory of radioactive decay led Rutherford and others to focus their investigations on identifying the sequence of transformations, attempting to identify all the elements from the first to the last, and to measure half-lives for each step. Those investigations, as a side effect, suggested a way to measure geologic time.

In 1904, during his visit to New Haven, Rutherford met Bertram Borden Boltwood who was interested in rare earth elements and in radioactivity. Boltwood during 1900–1905 conducted a private laboratory with the geologist Joseph Hyde Pratt. During this time, as part of this mining engineering and chemistry consulting business, Boltwood analyzed many ore samples. Rutherford visited Boltwood's laboratory and, after the visit, the two started a correspondence that turned into a warm friendship.[36] In 1906, Boltwood returned to an academic appointment in Yale's physic department, even though he considered himself a chemist—radioactivity was neither chemistry nor physics and it straddled the two disciplines.

Boltwood was persuaded by Rutherford and Soddy's theory of radioactive decay and presented a test for its validity. If the ratios of radium and uranium in the mineral samples, which he studied as part of his work in his lab, were constant, then it would be difficult to explain this coincidence by chance and would be a strong indication of a genetic relationship between the two, therefore providing strong evidence for Rutherford's transformation theory.

In 1905, Boltwood argued that the ratio of radium to uranium in certain minerals indeed were constant. He also noticed that lead was possibly a product of a

[36]Lawrece Badash (ed.), *Rutherford and Boltwood Letters on Radioactivity* (New Haven and London: Yale University Press, 1969).

decay from uranium.[37] He followed this with another observation that the ratio of lead and helium to uranium increased with the geologic age of the minerals in which they were found. In November of 1905, Boltwood wrote to Rutherford that, using the lead method, he had calculated the age of over 20 minerals, yielding ages ranging from 92 to 570 million years, but he did not publish any of the results until 1907.[38]

Rutherford had already suggested that radioactive decay could be used to measure geologic ages. The investigators of radioactive elements, including Rutherford and Boltwood, seemed certain that the rate at which one element decays into another was constant and independent of any physical or chemical conditions. They argued that if a quantity of a given element were known to decay into another element at a known rate and the initial and final amounts of the two elements were known, then, from the amounts and the decay rate, the time passed could easily be calculated. Of course, just like all the other previous methods of calculations of the Earth's age anchored in a simple principle, the devil was in the details. It would be over fifty years before scientists consistently arrived at the same answer for Earth's age using calculations based on this principle.

The first method that Rutherford suggested for measurement of geological time utilized production of helium from radium decay. In 1904, speaking at the St. Louis Universal Exhibition's Congress of Arts and Sciences, assuming that none of the helium (which is a gas) escaped from a mineral sample, Rutherford estimated the sample's age at 40 million years. The next year he published an article in *Harper's Monthly* in which he explained the phenomena of radioactivity and its relationship to the Earth's heat.[39] For Rutherford, using radioactive decay was one of

[37]Bertram Borden Boltwood, "On the Ultimate Disintegration Products of the Radioactive Elements," *American Journal of Science* 20 (1905): 253–267.

[38]Boltwood to Rutherford, 19 November 1905. In Badash (ed.), *Rutherford and Boltwood Letters.*

[39]Ernest Rutherford, "Radium—Cause of the Earth's Heat," *Harper's Monthly Magazine* 110

the applications of his new science. Boltwood was even more pragmatic and was only concerned with the age calculations as evidence for lead being the ultimate product of uranium decay—an idea that he first proposed in a paper read in front of the American Chemical Society.[40] His 1907 calculations of geologic ages were the continuation of his larger project of determining the steps in the disintegration series of uranium, and especially determining its ultimate product. First reporting his age calculation to Rutherford, Boltwood wrote "I think those numbers [calculated ages for minerals] afford a surprisingly good confirmation of the assumption that lead and helium are the disintegration products of uranium only" and only later he noted "[a]nother point in support of the relative values of the ages of the different minerals is the fact that they are (according to my geological friends) not contradicted by the geological data available on the relative age of the different depositions."[41] He reiterated the relationship of the age calculation in the published paper: "It is beyond the writer's province to discuss the data bearing on the geological ages of the different deposits, but he is indebted to Professor Joseph Barrell of Yale University for the statement that, so far as the knowledge of the latter extends, the relative values of the ratios are not contradictory to the order of the ages attributed by the geologists to the formations in which the different minerals occur."[42]

Similarly, Boltwood's interest in geological aspect was motivated by the ability to use geological minerals to aid his investigations of the radioactive decay series. "In considering the available data on the composition of radio-active minerals, with

(1905): 390–396.

[40]Presented February 10, 1905; published as Bertram Borden Boltwood, "The Origin of Radium," *Philosophical Magazine* ser 6 vol 9 (1905): 599.

[41]Boltwood to Rutherford, 18 November 1905 in Badash (ed.), *Rutherford and Boltwood Letters*.

[42]Bertram Borden Boltwood, "On the Ultimate Disintegration Products of the Radio-active Elements. Part II. The Disintegration Products of Uranium," *American Journal of Science* 23 (1907), pp. 83–84. More on Barrell in the next chapter.

the view to discovering the ultimate disintegration products of the radio-elements, it is therefore necessary to give strict attention to the questions of primary and secondary origin of the individual specimens and the geological period at which they were formed."[43] Boltwood was interested in the geological aspects of the minerals only because in as much it helped him in determining the radioactive decay sequence. Geological knowledge assured that all the lead found in the minerals was the result of uranium's radioactive decay.[44] The difficulty in using minerals to prove decay series (and similarly to use them as clocks) was determining the source of the elements in the mineral (did they come from the decay or from another source through its geological history?) and also the history of the mineral (did it lose or gain any quantity of a given element?). Those were the details with which Boltwood and later investigators were preoccupied.

After those initial investigations neither Rutherford nor Boltwood engaged in significant calculations of geological times again.[45]

The 19th century investigators of the age of the Earth took the Earth and its age to mean multiple things. As such, the Earth's age was relevant to their work in various, not necessarily related ways. This was even more so for the scientists during the first decade of the 20th century: the numbers that were cited as values for the age of the Earth were calculations of the ages of the oceans or times since the decay of radioactive elements. The individuals often cited in relation to the age of the Earth, such as Chamberlin and Boltwood, were investigating the Earth's age only as side projects.

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[43]Boltwood, *Ultimate Disintegration Products I*, p. 255.

[44]Boltwood first suggested lead as ultimate product of decay in Boltwood, *Origin of Radium*.

[45]Note, however, Rutherford did engage in the subject briefly at the request of Francis W. Aston, Ernest Rutherford, "Origin of Actinium and the Age of the Earth," *Nature* 123 (1929): 313–314.

G. F. Becker and an Attempt at a New Consensus

How did the scientific community reconcile these new developments with their community knowledge of the Earth's age? The significance of these developments was that Walcott's paper and community knowledge that emerged during the 1890s could not account for these new claims. It was not clear that these new developments challenged the previous consensus (Joly's work seemed to reinforce it and Chamberlin's was ambiguous). New standard relationships among the new and old investigations relevant to the Earth's age needed to be established through an engagement in a new dialogue. Just as previously there was nothing self-evident about the relationship among the investigations, those investigations continued to be about vastly different issues, each based on a wide array of assumptions. Various combinations were logically defensible.

George Ferdinand Becker (1847–1919) was the scientist who at the end of the first decade of the 20th century attempted to reevaluate the new claims about Earth's age and establish a new consensus.[46] Becker graduated from Harvard in 1868 where his initial interest in natural sciences turned toward physical ones. After graduating, he went to Germany, where he received a Ph.D. from Heidelberg in 1869. During his time in Europe, he also studied at the Royal School of Mines in Berlin. Robert Bunsen praised him as "among my most active and gifted students."[47] Upon returning to America, Becker found employment first in the industry and then as a professor at California State University at Berkeley, where he met Clarence King. Once the USGS was established in 1879, King made Becker one of his first appointments to the Survey, where he served as a

[46]For biographical information, see George Perkins Merrill, "Biographical Memoir of George Ferdinand Becker, 1847–1919," *Memoirs of the National Academy of Sciences* 21 (1927); Arthur Louis Day, "Memorial of George Ferdinand Becker," *Bulletin of the Geological Society of America* 31 (1919): 14–25.

[47]Merrill, *Becker*, p. 3.

geologist-in-charge or chief of division for the rest of his career.[48] Becker was one of the few early USGS geologists to hold a Ph.D. and his view of the relationship of geology to other branches of learning was was grounded in the mathematical and quantitative approach of physics and chemistry, as such it differed from the ecological views of McGee and Powell. Lord Kelvin was Becker's scientific role model. Until his physio-chemical work was halted in 1892, Becker was in charge of geophysical work at the Survey (in that capacity he supervised Barus's work). In 1900, he again reestablished the geophysical laboratory at the USGS and actively lobbied for the creation of an independent geophysical laboratory by the Carnegie Institution of Washington.[49]

Like other members of the USGS, Becker was not constrained by the disciplinary division that were beginning to dominate scientists working within the universities.[50] Because of his training, he saw connections between geology, astronomy, physics, and chemistry. His colleagues found, at times, his use of mathematics intimidating.[51] In fact, Becker thought that "Aside from its biological aspects, geology is mainly physico-chemical study and, if it is ever satisfactorily developed, will appear as a branch of astro-physics."[52] He thought that the Earth was the only cosmic object available for direct investigation by humans, and eventually other objects (e.g., Mars) would receive similar treatment and the study of the Earth will lose its privileged status. Becker thought that geology as practiced

[48]With the exception of 1892–1894 when he did some work in South Africa.

[49]Servos, *To Explore the Borderland*.

[50]For example, George Ferdinand Becker, "Geology and Cosmogony," *The Cosmopolitan; a Monthly Illustrated Magazine* 16 (1893): 255.

[51]Arthur Louis Day, "George Ferdinand Becker," *American Journal of Science* 48 (1919), p .17.

[52]Becker, "Remarks on Geophysics" manuscript n.d. [1890s]. Science: geophysics. Box 26. GFB. This view is repeated in Becker "Genesis," 1913. Science: Earth—Age, Origin, and Composition. Box 26. GFB; George Ferdinand Becker, "Half a Century in Geology: Progress Made Since Agassiz Discovered Glacial Period," *New York Times* (1909): 4. This position was echoed by Becker's colleague Charles R. Van Hise see Charles Richard Van Hise, "The Problems of Geology," *Journal of Geology* 12 (1904): 589–616.

during the 19th century was like astronomy practiced by Tycho Brahe: it was gathering the data to be interpreted into systematic laws by the likes of Kepler.[53]

If much of geology was more properly part of astronomy, other parts of it seemed to be in the domain of physical chemistry: "There is some misapprehension as what [Arthur Louis] Day and I mean by the loose term Geophysics. It is practically synonymous with the physics and physical chemistry of *extreme conditions*, the behavior of matter under great pressures and high temperatures such as exist in the interior of the Earth (or any other cosmic body)."[54] It was to the investigation of mineral substances under those extreme conditions that Becker dedicated much of his time. For Becker, the solution of many of geology's problems depended on a better understanding of those extreme conditions, many of which were the same physical properties relevant to the calculation of the Earth's age.

Throughout his career Becker often commented on topics very close to the question of the Earth's age. However, he did not directly address the Earth's age until the 1890s and only over a decade later did he publish articles on the topic addressed to scientific audiences. One of the topics engaged by Becker was the question of the Earth's rigidity, which was a fundamental assumption of the calculations of the Earth's age by Thomson and King, but which was also a central issue much more important for American geologists' discussion of isostasy.[55] He even saw King's investigation into the Earth's age as being primarily about the structure of the planet, and he was critical of Fisher's critique of King, but did not defend King's calculation of the Earth's age, but only his argument of the its ridging.[56]

[53]For example, Becker, "On Chamberlin's 'Origin of the Earth"' Science: Earth—Age, Origin, and Composition. Box 26. GFB.

[54]Becker to J.S. Billings, 4 Jan 1906. Box 18. General Correspondence 1901–1907. GFB. Emphasis in the original.

[55]George Ferdinand Becker, "An Elementary Proof of the Earth's Rigidity," *American Journal of Science* 39 (1890): 336–353; for discussion on isostasy see Oreskes, *op. cit.*, ch. 2–3.

[56]"The investigation of Messrs. King and Barus mentioned above revitalizes an argument for

Becker may have stayed out of the Earth's age dialogues because, despite his high admiration for Kelvin, he disagreed with his later calculations of the Earth's age, though he praised Kelvin's for limiting geological speculation, and stating that both groups of scientists arguing from different premisses "reached[ed] results not entirely discordant."[57]

In a note intended for publication in the popular *Cosmopolitan Magazine*, Becker addressed the new efforts by Kelvin and Murray to calculate the Earth's age and the challenge to Kevin from Perry. Becker concluded that, based on chemical evidence, the Earth's core was metallic; therefore, calculations which assumed that the Earth was composed of rock were incorrect. He suggested that the physicists (that is King, Kelvin, and Tait) who were suggesting a twenty-million-year-old Earth, should check the results of their calculation to avoid concluding that the Earth's age is much younger than the sun, which Becker thought to be one hundred million years old.[58] Even though Becker was drawn to geology by Clarence King, and his education was in chemistry and physics, during the 1890s he preferred the hundred-million-year time range for the age of the Earth. It seems that the concordance of the results from the different methods made him defend those results. He was able to point to the assumptions and shortcoming of calculations of scientists who disagreed with him (such as the older Kelvin, King, and Boltwood), while being satisfied with equally uncertain assumptions he made in his own calculations. Such treatment does not represent any dishonesty on the

the solidity of the Earth, put forth long ago by Lord Kelvin"George Ferdinand Becker, "The Interior of the Earth," *North American review* 156 (1893), p. 447; George Ferdinand Becker, "Fisher's New Hypothesis," *American Journal of Science* 46 (1893): 137–139. George Ferdinand Becker, "Note on Computing Diffusion," *American Journal of Science* 3 (1897): 280–286.

[57]George Ferdinand Becker, "The Age of the Earth," *The Cosmopolitan; a Monthly Illustrated Magazine* 16 (1894), p. 512.

[58]G.F. Becker "New Light on Earth's Age" MS. Science: Cosmopolitan magazine articles 1893–96. Box 26. GFB. The paper to which Becker refers are William (Lord Kelvin) Thomson and J. R. Erskine Murray, "On the Temperature Variation of the Thermal Conductivity of Rocks," *Proceedings of the Royal Society of London* 58 (1895): 162–167; William (Lord Kelvin) Thomson, "The Age of the Earth," *Nature* 51 (1895): 438–440.

part of Becker, but represents the fact that the dialogues on the Earth's age were multiple separate conversations and it was impossible to logically connect all the different assumptions. Becker in his own understanding made some of the facts coherent, the ones which were of most interest to him, but after settling on those propositions, others were no longer coherent with them.

Becker was one of the initial driving forces behind the establishment of the Geophysical Laboratory at the Carnegie Institution of Washington. While making his argument in support of that institution, he enumerated outstanding problems in geophysics. Though Becker sometimes included the age of the Earth in a list of outstanding problems in geophysics, when arguing for the Geophysical Laboratory, the Earth's age was either not mentioned or was a consequence of another investigation. The age of the Earth was never a prominent problem on this list, but it was always only one step away from the suggested investigations. On at least one occasion, Becker did include it as one of the outstanding problems of geophysics.

John Servos argues that by 1900 the geological community itself was fragmenting into a constellation of specialties.[59] Two groups of geologists, one focused on geophysical investigations and the other on geochemical investigations, fought for the control of the proposed Geophysical Laboratory. (The "geochemists" eventually won out.) Servos stresses that the age of the Earth played a central role for the "geophysicists," who included Becker, Barus, and Chamberlin. He is correct in stating that those scientists were involved in the age of the Earth debates, whereas the other group which included Charles R. Van Hise, Joseph P. Iddings, and Arthur Day, was not. But as I have argued here, the Earth's age was not the primary problem for Becker, Barus, or Chamberlin. Servos claims that for Becker the fundamental problems of geophysics were "distribution of terrestrial densities,

[59]Servos, *Intellectual Basis*.

the cause of crustal deformations, and the age of the Earth."[60] In support of this he cites an address that Becker delivered at the Congress of Arts and Sciences in St. Louis and an article he wrote in response to the publications of Barus, King, and Fisher. However, the St. Louis address was not used for the justification of the CIW Laboratory. Becker did not even mention the Earth's age in the 1893 article in which he explicitly discussed the issues raised by the "geochemists" in relation to the interior structure of the Earth. On the other hand when explicitly addressing the problems for the new laboratory, Becker wrote of "three of the many great questions of geophysics, namely the distribution of terrestrial density, upheaval and subsidence, and vulcanism" and said nothing about the Earth's age.[61] In an early manuscript when outlining what he though the new laboratory should be, Becker wrote "the goal of geophysical institute should be to achieve for terrestrial physics and chemistry what Laplace accomplished for Celestial Mechanics."[62] Later, he provided a formula for the calculation of the Earth's age, which he claimed "it will take only a few minutes to recompute the age of the Earth on the hypothesis here described" once the proper data are supplied.[63] Those two statements, and many similar ones found throughout his writings, support viewing Becker as searching

[60] *Idem*, p. 157.

[61] Becker, *Geology and Cosmogony* (Servos incorrectly cites this article as titled "Age of the Earth."); George Ferdinand Becker, "Present Problems of Geophysics," in Howard J. Rogers (ed.), *Congress of Arts and Science. Vol IV* (Boston and New York: Houghton, Mifflin and company, 1906): 508–522; also available as George Ferdinand Becker, "Present Problems of Geophysics," *Science* 20 (1904): 545–556. George Ferdinand Becker, "Appendix 1 to the Report of Advisory Committee on Geophysics: Project for a Geophysical Laboratory," *Year Book - Carnegie Institution of Washington* 1 (1902), p. 45. It is true that in the specific problems to be investigated the advisory committee on geophysics suggested investigations which could have been applied for age of the Earth calculations; however, the Earth's age is never mentioned by the committee and only mentioned in a reprinted letter from Lord Kelvin who makes a reference to "good work by Dr. Carl Barus and Mr. Clemence [sic] King."Robert Simpson Woodward et al., "Report of the Advisory Committee on Geophysics," *Year Book Carnegie Institution of Washington* 1 (1902): 26–43; anonymous , "Appendix 2 to the Report of Advisory Committee on Geophysics: Letters from European Scientists Relative to Research in Geophysics," *Year Book - Carnegie Institution of Washington* 1 (1902), p. 59–60.

[62] Becker, "Sketch for a Geophysical Laboratory. n.d. Folder: "Science: institutions for scientific research in physics, geophysics and chemistry." Box 26. GFB.

[63] Becker, *Reflections on Joly's Method*, p. 511.

for basic laws of geophysics and viewing the age of the Earth as one particular problem, the resolution of which would be a mere application of those basic laws. Servos is right in that Becker was very interested in the interior of the Earth and its history, however, he incorrectly equates that interest with the age of the Earth problem.

Writing on science, Becker often warned about too much speculation while he thought that scientists should pay closer attention to historical precedents than to their own theories.[64] It is for the lack of historical exposition that he criticized Chamberlin and his new theory of planetesimal hypothesis. Not only had Chamberlin not provided adequate historical review of the topic, Becker argued, but seen in light of historical developments, Chamberlin's planetesimal theory was not a rejection of the nebular hypothesis, but merely a version of it, long ago anticipated by the likes of Laplace and Kant. Additionally, Chamberlin presented Laplace's theory in a way that, according to Becker, its author would have disagreed with.[65]

In 1907, Becker confronted the challenge of new estimates of the duration of geological time based on radioactive measurement. Still full of questions after studying the current literature, Becker directly asked the investigators of relevant research. He wrote to Bertram B. Boltwood, "I am trying to make up my mind as to the geological and cosmogonical importance of radio-activity."[66] Becker raised a number of issues, for example, that the chemical combinations of radioactive elements possibly had an effect on their activity. He also noted that the claimed amount of heat generated by radium does not make sense with the experimental

[64]Becker, *Geology and Cosmogony*; George Ferdinand Becker, "Kant as a Natural Philosopher," 5 (1898): 97–112. Letter exchange with Boltwood. Reviewing a book proposal of Joseph Barrell, he advised rejecting it because too speculative. Becker to C.W. Hayes. 16 Mar 1904. Box 18. General Correspondence 1901–1907. GFB.

[65]Becker, "On Chamberlin's 'Origin of the Earth'." Folder "Science: Earth—Age, Origin, and Composition." Box 26. GFB. Merrill claims that Becker was very interested in Chamberlin's theories though he disagreed with them, Merrill, *Becker*, p. 8.

[66]Becker to Boltwood, 23 November 1907. Box 18. General Correspondence 1901–1907. GFB.

setup reported: "It would seem to me that if a tube of radium salt well packed in cotton and storing up heat at a rate of 100 calories per hour would soon set the cotton on fire." He also pointed to the lack of spectra of radioactive elements in the sun, thus questioning the argument that those elements provided sun's energy. In the end, Becker simply did not trust the new law of radioactivity, especially since it suggested time scales fifty times longer than those calculated by Walcott and others.

Boltwood's carefree reply did little to reassure Becker. For example, to Becker's query about Rutherford's postulation of an intermediate product between uranium and radium that did not emit any radiation, Boltwood replied by calling it "merely a thrust in the dark" which was "perfectly natural and logical" guess in view of the evidence at the time, but which, however, has been shown wrong by the evidence now available. Referring to another question by Becker about the effect of water on radium, Boltwood dismissed the biggest authority on the subject: "Rutherford's statement that dry radium compounds lose less than a hundred-millionth part of their emanation is undoubtedly an exaggeration."

Boltwood questioned recently published reports of chemical influence on radioactive properties by pointing out that those reports disagreed with all other publications and that before they could be taken seriously, they would have to be verified. Boltwood also dismissed other reports brought up by Becker: one by Crookes because there had not been a follow up in over a year; another by pointing to a possibly faulty experimental method; and another experiment by stating, "I feel confident that if properly tested it would be found to show the normal activity corresponding to its composition."[67]

As to the presence of uranium in the sun, Boltwood admitted that he only consulated two (rather out-of-date sources) which had claimed that there was evi-

[67]Boltwood to Becker, 25 November 1907. Box 18. General Correspondence 1901–1907. GFB.

dence for it. Boltwood admitted that more data were needed before geological ages could be calculated with certainty, and that cooperation of geologists, chemists, and physicists was required. However, he did not comment on the order-of-magnitude difference between his calculations and the ones cited by Walcott (he only noted "that the presence of radioelements introduces a new and important factor" for cooling globe calculations).

In a follow-up letter, Becker pointed to newer sun spectrum data that questioned the validity of the older data referred to by Boltwood. Also, using Boltwood's own method, Becker computed ratios of lead to uranium from different samples for the same location and found large disagreements.[68]

Boltwood seems to have taken Becker's second later more seriously and talked to Yale geologists, Herbert Ernest Gregory and Joseph Barrell, on whose authority he questioned the geological age of the mineral used by Becker and further presented experimental difficulties, which made measurement of uranium lead ratio too uncertain to attempt for the particular minerals used by Becker.[69]

The overall sense of the exchange was that, from Boltwood's point of view, the sample selection and experimental procedures were complex and one had to have specific level of expertise to obtain meaningful results—being an expert specialist he could dismiss contradictory reports as resulting from improper experimental procedure. Boltwood also knew the individuals involved in the research and was familiar with the fast pace at which results were presented (often results and arguments took place in forms of letters to the editor, which were faster to print than full research articles). He knew that in the community of researchers of

[68] Becker to Boltwood, 30 November 1907. Box 18. General Correspondence 1901–1907. GFB.

[69] Boltwood to Becker, 22 December 1907. General Correspondence 1901–1907. GFB. A decade later Barrell recalled that "he stated to Professor Barrell [. . .] that no real conflict appeared to exist between the geological facts and the new physical evidence. The conflict was only with one of the several interpretations of that evidence, though one which was generally accepted." Joseph Barrell, "Rhythms and the Measurement of Geological Time," *Bulletin of the Geological Society of America* 28 (1917), p. 750

radio-activity, new, often wild, claims were being made constantly and only the claims that have been independently verified needed to be taken seriously (hence his dismissal of a report that was printed over a year ago and did not receive any replies). He was not an expert on geological matters. He referred to geologists who had given him answers with enough ambiguity to allow for interpretation in line with his thinking. He was not researching the age of the Earth, and he had nothing to say on other "classical" ways of calculating it (with the exception of cooling globe calculations, which were nullified by radioactive heat). For Boltwood, the new methods were still very imperfect and much more work had to be done, but the main conclusion—it is possible to use radioactivity to date geologic mineral and they point to great ages—was certain even if a lot of the details needed to be worked out.

From Becker's point of view, Boltwood seemed to have been cherry picking research reports, dismissing any that were contradictory. Boltwood was lightheartedly interpreting as exaggerations published claims by Rutherford—an established authority in the field and Boltwood's collaborator. Boltwood was not familiar with the literature, that was most relevant to his work as Becker saw it (that is calculation of geological time). For Becker, Boltwood's work was very uncertain and its main conclusion could be questioned before all the details were worked out.

Becker also wrote to other scientists attempting to determine whether there was evidence of radioactive elements in the sun or if the minerals that had radioactive elements were warm.[70]

Becker found a number of inconsistencies in the radiometric research and his concerns were dismissed by one of the leaders of the new science. Becker had mul-

[70]Becker to J.S. Ames, 29 Nov 1907. Box 18. General Correspondence 1901–1907. GFB; J.S. Ames to Becker, 7 Jan 1908. Box 19. General Correspondence 1908–1915. GFB; Becker to WE Hidden, 29 Nov 1907. Box 18. General Correspondence 1901–1907. GFB; Becker to P.G. Nutting, 30 Nov 1907. Box 18. General Correspondence 1901–1907. GFB.

tiple trusted geological means pointing at a mid-range duration of geological time; his reservations about the revolutionary claims of this very new method based on properties, which were far from understood, was very justified. In short, Becker was not convinced of the usefulness of radio-activity to geology. He attempted to provide material for new consensus by writing a paper in which he evaluated the new radiometric methods and rejected them. In this paper, he dismissed radioactive dating by stating that the half-lives may not be constant. Similarly, he dismissed the heating effects of radioactive decay by arguing that the radioactive elements must be distributed only closely to the surface of the Earth. Becker's main argument, however, was evidence showing that the salt content of the oceans method for estimating age of the Earth was trustworthy. The method also produced numerical results that were similar to earlier calculations based on sedimentation rates. Becker even recalculated the age of the Earth as a cooling globe. After adjusting the parameters involved, even this previously hostile method produced results in line with his geological estimates.[71] Just as Walcott had previously, Becker stated that the physical method, this time based on radioactivity, must simply be somehow incorrect.[72]

Whereas Becker's paper on the age of a cooling globe was addressed to scientists in general and was published in the journal *Science*, his paper evaluating radioactivity was addressed to geologists and Becker promised to "first make an attempt to sketch in outline such features of that subject [radioactivity] as seem to me of especial interest to us."[73]

In the article, Becker questioned uranium as a true element and thought about it as a compound of lead and helium. He gave an extensive review of methods of

[71]George Ferdinand Becker, "Age of a Cooling Globe in Which the Initial Temperature Increases Directly as the Distance from the Surface," *Science* 27 (1908): 227–233.

[72]George Ferdinand Becker, "Relations of Radioactivity to Cosmogony and Geology," *Bulletin of the Geological Society of America* 19 (1908): 113–146.

[73]*Idem*, p. 113.

determining age of the Earth using cooling globe, denudation, age of the oceans methods, and George Darwin's calculation, concluding that the range 50–75 million years was comparable with this diverse group of calculations. He was happy to consider radioactive dating methods but stated, "One condition of its [radioactive dating] acceptance would clearly be that it should give periods of the same order of magnitude as is indicated by purely geological data."[74] That is, he would accept the methods only if they were in agreement with the results that he took as firmly established. The use of radioactive dating would be not as much in dating the age of the Earth, but in dating specific rock samples that now could be dated out of the context of their place in the geological column. Becker presented his own calculation of specimens from the Llano County (Texas). First, he gave geological evidence that the samples were fitting to be used with Boltwood's method. Next, however, Becker argued that different minerals of the same geological age pointed to vastly different ages expressed in years which would seem to invalidate Boltwood's method. Becker then reported Boltwood's reply to his objection:

> Mr. Boltwood informs me, however, that [. . .] none of these minerals is really suitable for throwing any definite light on the question of the uranium-lead ratio for Llano county, since all of the specimens show signs of incipient or advanced alteration; but, according to theory, the state of combination is without influence on radioactivity, so that the only alteration which would affect the matter must involve the addition or abstraction of uranium or lead, and mere hydration, for example, should be without effect.[75]

Becker was taking radioactive dating at its face value as a straightforward, one-step calculation, and so did not accept the explanation of Boltwood for whom the whole process was a complex, multi-step, just-right analysis of just-right samples.

In the end, Becker was quite forceful in his conclusions:

> But I find no convincing evidence that the law of decay is so simple as is assumed [. . .] On the whole, then, the surface temperature gradients, taken in connection with the age of the Earth as determined stratigraphically, or

[74] *Idem*, p. 133.
[75] *Idem*, p. 134.

> from the sodium content of the ocean, or from the theory of a cooling Earth, do not indicate that the excess of temperature within the Earth is due in any large measure to radioactivity. [...] It does not seem to me that geologists can possibly accept the age of minerals as determined from the uranium-helium or uranium-lead ratios, which do not seem consistent and far longer than stratigraphy could admit.[76]

There was a lot of evidence which Becker saw as independently confirming each other, that pointed to a shorter time scale, which by far outweighed this one new method, which seemed highly problematic, untested, and inconsistent. Becker argued that radium was confined to a thin layer near the surface because if it were distributed through the planet, the core of it would be liquid, which he claims was not the case. Becker was very thorough in his evaluation of previous evidence and concluded that calculations based on radically different methods pointed to the Earth being 50-75 million years old. The large uncertainty was due to the imperfect data. But F. W. Clarke was already at work providing new, more accurate data, that would improve Joly's calculations.

Becker was sympathetic to Joly's method, however, he thought that the rate of deposition of sodium into the ocean had been changing in a predictable way. It used to be at a higher rate than currently observed.[77] Becker argued that the rate of deposition was not constant, but rather decreased asymptotically as the land was worn away. He reduced Joly's calculation to a formula which, once proper data were supplied, "it will take only a few minutes to recompute the age of the Earth on the hypothesis here described."[78]

The data were published by Clarke and Becker followed with calculations of the Earth's age based on the data.[79] Becker recognized only three ways of measuring the age of the oceans: by the sedimentation method, the secular cooling, and the

[76] *Idem*, p. 135.
[77] Becker, *Reflections on Joly's Method*.
[78] *Idem*, p. 511.
[79] Frank Wigglesworth Clarke, "A Preliminary Study of Chemical Denudation," *Smithsonian Miscellaneous Collections* 56 no .5 (1910) and George Ferdinand Becker, "The age of the Earth," *Smithsonian Miscellaneous Collections* 56 no. 6 (1910).

ocean salt content methods. (Darwin's dating of the Earth-moon system he mentioned as closely related.) However, he was aware of the radioactive calculations suggesting long-time scales, and it was exactly their refutation that this publication sought. Becker aimed to provide "convincing reasons for adhering to ages within a hundred million years, or even within the two hundred, they [geologists] may partly repay the heavy debt due by them to Kelvin and his intellectual heirs."[80] In this publication he presented again his earlier calculation of chemical denudation and secular cooling with the new data and arrived at the conclusion that the Earth was 60–65 million years old. He concludes "it follows that radioactive minerals cannot have the great ages which have been attributed to them."[81]

This was Becker's last published report on the topic, though he continued to be interested in it. There survive incomplete notes of further consideration based on additional data obtained by Clarke in 1912.[82]

Upon closer investigation, Becker does not fit into any pigeonhole. He was a geologist trained in chemistry, he aspired to the precision of astronomy, and he used advanced mathematics in his arguments, yet he was sceptical both of the short time scales argued by King and Kelvin and the long ones of Boltwood and Rutherford. He preached against speculation in science, but his judgment of what was speculative and what was not were consequences of his understanding of the world. Though believing himself to be providing honest investigations, the results reaffirmed his already established beliefs.

Becker, however, failed to reestablish consensus among American scientists. There were a number of utterances that ignored him and others that contended with him. In 1893, Walcott provided all the responses the community wanted, but

[80]George Ferdinand Becker, "Halley on the Age of the Ocean," *Science* 31 (1910), p. 2.
[81]*Idem*, p. 28.
[82]Becker "Age of the Earth with Clarke's data for sodium" MS, 1912. Science: Earth—Age, Origin, and Composition. Box 26. GFB.

in 1908 Becker did not. For example, Becker dismissed the Chamberlin-Moulton cosmogony; that is, he did not provide a ready-made reply for its challenge. His dismissal of radioactivity was also unsatisfactory, as radioactivity was too popular a topic to be rejected based on some as-yet-unknown error.[83] Nevertheless, Becker's paper was very popular, the Geological Society of America, in whose Bulletin it was published, soon ran out of offprints.[84] What the paper showed was that, if this was the best effort to establish consensus, it failed short of its goal. Though many geologists were sympathetic to Becker's arguments and used the paper to justify their beliefs, others were not.

For example, Charles Schuchert initially seemed to have been impressed with Becker's results and wanted to use Becker's paper in teaching his students. Similarly, Frank Wigglesworth Clarke continued to support Becker's conclusions in his influential *Data on Geochemistry* into the 1920s.[85] And in 1916, Henry Fairfield Osborn, based on discussion with Becker and Clarke, dismissed determination based on radioactivity and instead cited as most reliable the geo-chemical methods.[86]

Walcott's paper did not receive any replies that challenged it, but Becker's did. Becker's colleague, Arthur L. Day, called the paper "of uncommon interest" and "of extraordinary character," but also pointed out that it was suggestive and speculative. He did not criticize Becker directly, but he did not offer support either.[87] Others were less sympathetic. Alfred Church Lane attacked Becker's

[83]For the popularity of radioactivity in both scientific and popular contexts, see Lawrence Badash, "Radium, Radioactivity and Popularity of Scientific Discovery," *Proceedings of American Philosophical Society* 122 (1978): 145–154; Lawrence Badash, *Radioactivity in America: Growth and Decay of a Science* (Baltimore: Johns Hopkins University Press, 1979). See also, Luis Andres Campos, *Radium and the Secret of Life* (Ph. D. diss.), Harvard University, 2006.

[84]Edmund O. Hovey to Becker, 18 November 1909. Box 19. General Correspondence 1908–1915. GFB.

[85]Schuchert to Becker, 13 Sep 1910, Box 19. General Correspondence 1908–1915. GFB. Frank Wigglesworth Clarke, *Data of Geochemistry*, 5th edition (1924), series U.S. Geological Survey Bulletin, 770.

[86]Henry Fairfield Osborn, "The Origin and Evolution of Life Upon the Earth (Lecture I, Part I)," *Scientific Monthly* 3 (1916): 5–22.

[87]Arthur Louis Day, "Geology and Radioactive Substances," *Science* 28 (1908): 526–7.

attempt to recalculate Earth's age based on a cooling globe.[88] Lane noted that the formula used by Becker applied only if the surface of the Earth was held at constant temperature, which Lane claimed had not been the case through geological time. Lane also objected to Becker's treatment of temperature gradient with which Lane was familiar, based on his own investigations of mines in Michigan. Just as to Becker, Boltwood's calculations seemed speculative, to Lane, Becker's were "mere speculative hypothesis."[89] In 1910 Chamberlin in his *Journal of Geology* published a short note by H. S. Shelton who questioned the use of salinity of the seas as a measure of geological time: "The method is liable to a number of uncertainties and it would not be wise to lay too great stress on this particular estimate."[90] The attacks continued throughout the decade culminating with Joseph Barrell's investigation of geological time.[91] Carl Snyder also asked about Becker's lack of consideration of alternative cosmogonies, explicitly asking about Chamberlin's.[92]

Even more telling are references to age of the Earth made by scientists not directly involved in dialogues on the topic. In a 1914 textbook on physiogoegraphy, the discussion of the Earth's age presented the state of the debate as it was during the 1890s, even though the citation included Becker's paper.[93] In 1918, paleontologist Frank Knwolton, writing on geological climates, noted that astronomical calculations for the age of the sun point toward ages of less than fifty million years, while geological estimates based on radioactive dating point to as much as over a 1000 million years."Certainly there must be some readjustment or mutual con-

[88]Alfred Church Lane, "Schaeberle, Becker and the Cooling Earth," *Science* 27 (1908): 589–592.

[89]Lane, *Schaeberle*, p. 592.

[90]Shelton, *Age of the Earth*, p. 193.

[91]Barrell, *Rhythms*. For details of Barrell's critique see next chapter. See also, E. M. Kindle, "Inequalities of Sedimentation," *Journal of Geology* 27 (1919): 339–366.

[92]Snyder to Becker, 2 Sep 1908. Box 19. General Correspondence 1908–1915. GFB.

[93]Ralph Stockman Tarr, *College Physiography* (New York: Macmillan, 1914). The book appeared posthumously under the directorships of Lawrence Martin, therefore it is unknown wether the citation was added by Martin or by Tarr. Regardless, a textbook published in 1914 (and reprinted in 1924) did not include discussion of radioactivity of salt clocks.

cessions between astronomers and geologists before these widely discordant figures can be brought into harmony!"[94] He did not mention any other ways of estimating Earth's age including Becker's. It is not only the estimates that were discordant, but the reports were even more so. The authors did not cite the same sources, they did not describe the same problems, and often were not aware of whole sets of literature. This is highly contrasted with the situation before 1900 when all accounts were in reply to the same community knowledge. The next chapter surveys some of those semi-independent disciplinary approaches to the question of the Earth's age.

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During the first decade of the 20th century, the scientific community lost its consensus answer to the question "how old is the Earth?" as a result of three independent developments. First, T.C. Chamberlin's cosmogeny which was an alterative to the widely accepted Nebular hypothesis, challenged the fundamental assumption of Kelvin's calculations of the Earth's age, namely that the Earth started as a molten sphere and has been cooling ever since. Chamberlin argued that instead the Earth formed by an aggregation of small "planetismals" over a long period of time. The new cosmogony did not provide an obvious new way to calculate the Earth's age. The next development was the popularization of the measurement of the ocean's age by John Joly. This development fit well within the previous discussion of the earth's age as the methodology itself bore resemblance to the geological denudation and sedimentation methods as did the results. Finally, the discoveries associated with radioactivity were the most significant as they both nullified the cooling earth calculations by providing a new source of heat, and provided a totally new way to directly measure ages of geological samples. However

[94] Frank Hall Knowlton, "Evolution of geologic climates," *Bulletin of the Geological Society of America* 30 (1919), p. 542.

those new calculations were far from being universally accepted as trustworthy and the scientists making them, Rutherford and Boltwood, were not very interested in arguing for the correctness of those calculations which were not central to their own research.

Combined, those developments created a situation where the old community knowledge was no longer adequate. New community knowledge must emerge from a community in dialogue. Since there was not enough discussion of the Earth's age in light of the new developments for new community knowledge to emerge, George Becker decided to provide the seeds for new consensus knowledge. Throughout his career Becker was not primarily interested in the Earth's age, but was often interested in topics closely related to it and even opined directly on the Earth's age in publications aimed at non-scientific audiences. At the end of the decade he published a series of articles aimed at reconciling the new developments with the earlier knowledge on the Earth's age. His conclusions received mixed responses: some scientists were persuaded by them, while other objected publicly. Becker's failure to bring about a new consensus is a sign of the loss of a common response given by either the scientific community at large, or geological community in particular.

This chapter showcased the limitations of community knowledge, which cannot be used to create a response to new knowledge claims such as the new utterances on the cosmogony, age of the oceans, and radioactivity. New knowledge claim must be evaluated by individuals and new community knowledge must emerge out of dialogues among individuals. The case of George Becker provides another illustration of individual understanding—his views on the Earth's age were the consequence of his particular education, experiences and considerations. It also highlights a few points about the dialogues on the Earth's age during this period: there were a multitude of discussions closely related to the Earth's age, but which could be discussed without explicitly bringing up the issue of geological time, like the discussion of the Earth's rigidity. Consequently, we must pay close attention when evaluating those utterances and disentangle the various dialogues contributing to the discussion of the Earth's age. Furthermore, when Becker finally did opine on the Earth's age, he did so on the pages of the popular *Cosmopolitan Monthly*, giving further evidence that the topic was one that the scientists claimed as one of their own, and that non-scientists were very interested in it, but that the scientists often did not discuss it in scientific forums. Finally, Becker's attempt at bringing about a new consensus shows first, that the scientific data did not point to a particular conclusion. The effort itself was an attempt to provide a response which could be used by geologists in discussion of the Earth's age. However, Becker was responded to as if he were speaking representing the community and his utterance did not become community knowledge.

Chapter 5

Fragmentation

In the 1900 presidential address of the Geology Section of the British Association for the Advancement of Science by William J. Sollas, "Evolutional Geology" he discussed the evolution of the Earth and the details of the various age of the Earth dialogues from astronomical, biological and geologic considerations.[1] His address followed one given the previous year by Archibald Geikie, also on the topic of the Earth's age.[2] In America, G. K. Gilbert gave an address on the Earth's age as the retiring president of the American Association for the Advancement of Science.[3] Two decades later, general scientific societies, the BAAS in Europe and American Philosophical Society in the U.S., again approached the question of the Earth's age and Sollas again participated. However, those later discussions were different. This time, instead of integrating the different approaches to the question of the Earth's age, Sollas limited his discussion to the topic of his own expertise. The same was the case with the APS symposium, which included disjointed disciplinary presentations.

[1]The address was reprinted in William Johnson Sollas, "Address of the President of the Section of Geology of the British Association. I," *Science* 12 (1900): 745–756; William Johnson Sollas, "Address of the President of the Section of Geology of the British Association. II," *Science* 12 (1900): 787–796; William Johnson Sollas, "Evolutional Geology," *Annual Report of the Smithsonian Institution* (1900): 289–315.

[2]Archibald Geikie, "Address of the President, Geology Section," *Report of the 69th Meeting of the British Association for the Advancement of Science* (1899): 718–730.

[3]Grove Karl Gilbert, "Rhythms and Geologic Time," *Science* 11 (1900).

The individuals involved in the debates had their unique reasons to be interested in Earth's age, but they were increasingly separated from each other, both institutionally and intellectually. The discussions of the different approaches to calculating Earth's age were increasingly detailed as well and required specialized knowledge that drew on separate sets of literature, further making the research inaccessible to scholars from other fields. While individual researchers were at least nominally aware of the other lines of research, individuals who were not familiar with the age of the Earth literature were presented with a confusing situation of hardly noticeable, and fragmented, non-interacting discussions that included the discussion of radioactivity, and related but separate discussion of the application of radioactivity to questions of geological time, astronomical discussion of the age of the sun and stars, and investigations of the Earth's age based on rates of evolution.

Radio-active Dating

European researchers were the ones who applied radioactivity to the measurement of geological time for the first two decades of the 20th century. Just as measuring geological time by denudation and sedimentation was not a single method but a family of methods, so, too, application of radioactive materials to the problem resulted in multiple methods. From the very beginning to the present day there have been a number of methods based on different element decay series. Most of them relied on comparing the quantities of two elements from a decay series. For example, the helium/uranium method utilized the rate of production of helium (α-particles) from the decay of uranium. Since the rate of helium produced during the decay of uranium was measured experimentally, if the amount of helium and uranium in a given mineral were also measured, the age of the mineral could be calculated. This was exactly the calculation first introduced by Rutherford during his Silliman Lecture at Yale in 1905.[4]

Another method, also suggested by Rutherford during his Silliman Lecture, was the lead/uranium method. Whereas helium, as a gas, potentially could escape and therefore posed a problem for the first method, lead, the final product of uranium decay, remained at the site of production and hence was more suitable for measuring geological time. This second method was used by Boltwood.

Another method of an entirely different kind was advocated by the same John Joly who earlier brought to prominence the ocean salt content method. Despite using radioactive phenomena to study geological time, Joly did not abandon his shorter time scale. He could not accept the slow rates of sedimentation suggested by an over billion-year-old Earth. Instead, he criticized the assumption that the decay rates of uranium have been constant throughout history. Joly argued that the small, dark rings in some granite minerals resulted from radioactive decay of small zircon crystals at the center of the rings. The intensity of those rings, or halos as they were known, corresponded to the length of time that the radioactive elements in zircon have been decaying. Working with Rutherford who experimentally produced artificial halos, Joly tried using the halo method (also called the pleochroic halo method) to date minerals.[5]

Perhaps the strongest proponent for applying the radiometric methods to the question of geological time was Arthur Holmes (1890–1965), about whom Cherry

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[4]Ernest Rutherford, *Radioactive Transformation* (New York: Charles Scribner's Sons, 1906), pp.187–192. Other application of the He/U method were Robert J. Strutt, "On the Radio-Active Minerals," *Proceedings of the Royal Society of London. Series A* 76 (1905): 88–101; Robert J. Strutt, "On the Accumulation of Helium in Geological Time," *Proceedings of the Royal Society of London. Series A* 81 (1908): 272–277.

[5]For more information, see Patrick N. Wyse Jackson, "John Joly (1857–1933) and His Determination of the Age of the Earth," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 107–120. Few of the publications in which Joly discussed radioactivity and geological time were, John Joly, "Radium and the Geological Age of the Earth," *Nature* 68 (1903): 526; John Joly, "The Distribution of Radium in the Rocks of the Simplon Tunnel," *Science* 26 (1907): 518–519; John Joly, *Radioactivity and Geology* (London: Constable, 1908); John Joly, "The Age of the Earth," *Annual Report of the Smithsonian Institution* (1911): 271–293;John Joly, "The Age of the Earth," *Nature* 109 (1922): 480–485.

Lewis writes that "[f]or nearly 50 years [he] pursued these goals [of searching for a geological clock] almost single-handedly."[6] Holmes during his undergraduate studies was the first person to use the uranium-lead ratios to measure geologic time. In 1913, he published a book *The Age of the Earth* in which he argued for an Earth 1,600 million years old. He spend the much of the 1910s employed as a geologists for various commercial companies, but also obtained his Ph.D. from Imperial College in 1917. In 1924 he was offered a professorship in geology at Durham University where he investigated measurement of geologic time and was an early proponent of the theory of continental drift.

Holmes's significance in the development of radiometric dating is undeniable, and he was the first person to make the search for measure of geologic time his primary research interest. Unlike all the other researchers described so far, from his time as a student, Holmes was interested in using radioactive dating to obtain measures in years for different geological ages. His first publications were on the topic. Holmes followed on Boltwood's investigations and found Boltwood's results to be concordant with his own. He also suggested possible reasons why the radiometrically derived ages could be so much longer than ones based on sedimentary measures (that is, he explained why using sedimentation rates was misleading). However, I will make clear in this and the next chapters that the establishment of radiometry as the preferred method for measuring geological age was not single-handedly accomplished by Holmes.[7]

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[6]Cherry L. E. Lewis, *The Dating Game: One Man's Search for the Age of the Earth* (New York: Cambridge University Press, 2000), p. 5. See also, Cherry L. E. Lewis, "Arthur Holmes' Vision of a Geological Timescale," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 121–138.

[7]Arthur Holmes, "The Association of Lead with Uranium in Rock-minerals, and Its Application to the Measurement of Geological Time," *Proceedings of the Royal Society of London. Series A* 85 (1911): 248–256; Arthur Holmes, "The Duration of Geological Time," *Nature* 87 (1911): 9–10; Arthur Holmes, *The Age of the Earth* (London and New York: Harper, 1913).

Joseph Barrell and Geological Dating

Joseph Barrell (1869–1919) originally trained as an engineer and after obtaining his masters degree in 1893, taught mineralogy, mining, geology and zoology at Lehigh University. Taking a leave from his teaching duties, in 1900 he obtained a Ph.D. in geology from Yale where he accepted a faculty position in 1903 and became professor of structural geology in 1908.[8] Barrell was interested in a number of related topics that included the origin of the Earth (he modified the Chamberlin-Moulton planetesimal theory), sedimentation and erosion, isostasy, and palaeoclimatology. His research was regarded as theoretical and Barrell's main contributions were analysis and synthesis of the work of others. He was elected to the National Academy of Sciences shortly before his death in 1919.[9]

Barrell initially became interested in the question of geological time when, in 1906, he was considering the exact mechanisms of sedimentation and erosion and challenging several accepted notions of those mechanisms. The next year, when Boltwood consulted Barrell about Becker's query, Barrell informed Boltwood that no conflict appeared between geological time and the ages calculated by Boltwood.[10]

From the beginning, Barrell was attracted to viewing geological history in terms

[8]For biographical information, see anonymous , "Joseph Barrell (1869–1919)," *American Journal of Science* 48 (1919): 251–280; Herbert Ernest Gregory, "Memorial of Joseph Barrell," *Bulletin of the Geological Society of America* 34 (1923): 18–28; Charles Schuchert, "Biographical Memoir of Joseph Barrell, 1869–1919," *Biographical Memoirs* 12 (1927): pp40.

[9]However, not everyone approved of Barrell's research. Becker, reviewing a book, advised its rejection, writing "Mr. Barrell shows a regrettable disregard for literature, an unwise choice of a method of determining the quantitative composition of rocks from slides, and above all undue tendency to speculation. In my opinion, his theories are strained, and the facts, do not support them. It appears to me that in his best interest Mr. Barrell should be discouraged from speculating with insufficient premises." Becker to Hayes, 16 March 1904. Box 18. General Correspondence 1901-1907. GFB.

[10]Joseph Barrell, "Relative Geological Importance of Continental, Littoral, and Marine Sedimentation," *Journal of Geology* 14 (1906): 316–356; Joseph Barrell, "Rhythms and the Measurement of Geological Time," *Bulletin of the Geological Society of America* 28 (1917). Barrell, *Rhythms*, p. 750, in this publication Barrell incorrectly gives that year as 1906.

of rhythms and cycles.[11] This view culminated in Barrell's one major contribution to the discussion of geological time, an article titled "Rhythms and the measurement of geological time" published in 1917.[12] This article was not about physics or radioactivity, it was a geological argument presenting geological evidence for long-time scales. However, there was no geological way to measure the duration of time; instead, there was a physical way that was compatible with the long-time scales expected by geological evidence. It seems Barrell was triggered to write on the use of radioactivity to measure geological time by the work of Arthur Holmes, whose conclusions on the subject Barrell wanted to relay to the American audience.[13]

In general, Barrell's article was an attack on the overattachment of geologists to the uniformity of geological processes. Barrell placed much stress on the rhythmic nature of geological change and pointed to numerous "revolutions" in the geological record and to the pulsatory nature of erosion processes. Barrell started his article by claiming that rhythms were used to measure time. To measure geological time, long rhythms were needed. He claimed that "[t]he doctrine of uniformitarianism has ignored the presence of age-long rhythms, and where they were obtrusive has sought to smooth them out."[14] He pointed out that both the rates of sedimentation and denudation were not simple aggregates of continuous steady processes. However, unlike earlier works, his presentation was much more specific in accounting for the unconformities (i.e., breaks in the geological records). He finally concluded that "it seems that geologic time is certainly much longer—perhaps ten or fifteen times

[11] Boltwood to Becker, Dec 22, 1907. Box 18. General Correspondence 1901–1907. GFB. anonymous , *op. cit.*, p. 271.

[12] Barrell, *Rhythms*.

[13] Holmes' relevant works are Holmes, *Age of the Earth (1913)* Arthur Holmes, "Radioactivity and the Measurement of Geological Time," *Proceedings of the Geologists Association* 26 (1915): 289–309. On Holmes' influence on Barrell see: Barrell, *Rhythms*, 750–1; Ellis L. Yochelson and Cherry L. E. Lewis, "The Age of the Earth in the United States (1891–1931): From the Geological Viewpoint," in C.L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001), p. 147.

[14] Barrell, *Rhythms*, p. 746.

longer—than the estimates based on strictly uniformitarian interpretation."[15]

Barrell framed the history of the Earth's age discussions in terms of the physics-versus-geology narrative, and he aimed to unify the two disciplines. He aimed to "bind the geological and physical arguments into a unity—the geological data giving evidence of a highly variable rate and imperfection of record, the physical evidence supporting the assumption of a constant rate for radioactive processes and giving the magnitude of the framework into which the geological picture must be set."[16] He reexamined the Llano district data that led Becker to reject radioactivity in 1908, and found Becker's analysis in error further claiming that "[an] hypothesis gains greatly in strength when what appeared to be conflicting evidence is resolved into supporting evidence."[17]

Barrell presented a view of geological history that was ill served by uniformitarian assumptions. As the title indicated, Barrell presented evidence that Earth's history was marked by pulsating activity: faster at times, slower at others. He drew his conclusion based on analysis of present-day processes that showed great variability. From geological evidence, Barrell argued that different processes, many of which may be present during all times, have dominated at specific geological ages. For example, he presented detailed analysis of denudation by rivers and stream. He concluded that, for presently observed running water, no general rule could be made: there were too many variables, ranging from slope, to soil type, to humidity. The averages used by previous scientists who took denudation as a means to estimate the Earth's age, hid those complexities. He wrote, "The mean rate of denudation is a factor which, then, in the very nature of things, is subject to great variation through geologic time and is therefore wholly unsuited to

[15] *Idem*, p. 749.
[16] *Idem*, p. 751.
[17] *Idem*.

serve as a method of measurement."[18] Crucially, Barrell not only pointed out the problems with the measurement he opposed, but also explained to many readers' satisfaction how those methods greatly underestimated the time required.

Similarly to the denudation analysis, Barrell provided a corrective to the over-simplified calculations of the duration of time based on sedimentation rate. Again, he considered not only averages but the actual conditions, both of the materials carried into the sea and of the seas themselves to conclude that the process cannot be treated as uniform in the simple sense of the theory (the same rate as currently observed). Barrell followed the theoretical discussion by including a number of specific sites where he claimed this process took place.

The biggest significance of Barrell's paper was that it provided a method that would reconcile the accepted facts about rates of sedimentation, about the general nature of geological processes, and the order-of-magnitude-higher time scale resulting from radiometric methods. The core of Barrell's argument was that in addition to the accepted discontinuities in the geological column, there were many more micro-discontinuities that explained why time recorded by sedimentation was much less than required. For example, Barrell argued that the deposits under consideration were made in shallow seas, the base level of which oscillated. That is, throughout history, land rose and subsided. Consequently, the coastline moved back and forth. Land which at one point was below water level, and received deposits as the base level rose and the coastline recessed, later would be above sea level and denuded. This denudation would erode the previously deposited sediments resulting in a disconformity.

Barrell's argument preserved accepted knowledge about the rates of sedimentation (they did not have to be modified ten-fold, as did the age of the Earth), it preserved the general mode of geological change (geological action was uniform,

[18] *Idem*, p. 776.

however the record left behind was not uniform), and it allowed for the much longer time scales demonstrated by radiometric calculations.

Barrell presented evidence that the known and accepted geological processes result not in continuous depositions at a set rate but rather resulted in rhythmic process during which alternating periods of depositions and removal. The resulting geological formations represented only a partial record of the time passed. Barrell's analysis of the details of the processes of sedimentation, deposition, and erosion had as its goal showing that the geological methods of estimating duration of time were based on a flawed premise that a given thickness of deposits corresponds to a set duration of time.

Next, he proceeded to evaluate the arguments made by Walcott and the British geologist W. J. Sollas in detail "since their work has had great influence on geologic thought in these matters."[19]Starting from the general consideration of rates in section I of his article, Barrell went through a litany of reasons why the rates used by Walcott were all too high. Additionally, arguing from geologic research done since 1893 (in large part by Charles Schuchert about whom more follows in the next chapter), he pointed to many time periods with no deposits, where Walcott thought that the deposits were continuous.

Barrell also reviewed attempts to determine the ocean's age by arguing that measuring its increase in salt content suffered from many of the same objections as measuring the rates of denudation. He concluded that "[a]s a basis for attempting to measure the age of the earth, it is, however, defective only in lesser degree than the methods resting on the measurement of stratigraphic sections."[20] Again, he

[19] *Idem*, p. 815. Sollas addressed the question of the age of the earth and the oceans on numerous occasions, initially he argued that all the estimates supported about 50 million year old Earth, later he extended this estimate to closer to a 100 million years. Sollas, *Address I*; Sollas, *Address II*; William Johnson Sollas, *The Age of the Earth and other geological Studies* (London: T. Fisher Unwin, 1905); William Johnson Sollas, "The Anniversary Address of the President," *Quarterly Journal of the Geological Society of London* 65 (1909): l–cxxii.

[20] Barrell, *Rhythms*, p. 835.

presented a laundry list of the familiar objections. He criticized the assumption that rates of sodium extraction from sedimentary and igneous terrene were the same, and that the rates have been constant throughout geological time.

Barrell took most effort in directly replying to Becker. "The subject should not be dismissed so briefly, however," justified Barrell, "since the chief publications in American geological literature in the past 10 years on the subject have been those of Becker, and unless the validity of his assumptions is carefully examined, his arguments will appear to possess considerable force."[21] Barrell devoted four pages to criticizing Becker's assumptions that the ratio of total oceanic sodium to total oceanic chlorine had remained the same through the geologic time and that the rate of sodium supply from igneous rocks decreased through time logarithmically.

Finally, he turned his attention to the calculation of the Earth's age based on loss of primal heat. He dismissed the 19th century discussions, deferring to the new source of heat: radioactivity which "gives such an embarrassingly large quantity of heat that" researchers had to assume that the heat generating radioactive minerals were restricted to the outer 40 miles of the Earth's crust.[22] Again, he directed most of his attention on Becker's recent calculations. Barrell criticized Becker for his use of Barus's calculations of fusion rates and for the too-artificial assumption of uniform densities of the crust. Barrell also attacked Becker for assuming uniform initial temperature distribution and considering only two factors: addition of radioactive heat and loss by conduction to the surface. Barrell believed that there was evidence for convection within the Earth. He summarized Becker's argument by stating that "assumption is built on assumption into a many-storied structure and the whole rests on a foundation of quicksand. That it is a castle in the air and can not reflect the conditions of nature is indicated by various well

[21] *Idem*, p. 836.
[22] *Idem*, p. 839.

established inferences."[23]

After this extensive discussion and dismissal of attempts to measure geologic time, Barrell turned his attention to attempts to do so using radioactivity. He started by giving an overview of the basics, such as a definition of half-life. For a detailed discussion, Barrell deferred to Arthur Holmes: "The following three topics are quoted entire, as they give in brief space the essentials of the methods and the original article will be seen by just a few American geologists. The importance of the whole subject from a geological standpoint is such that this presentation should be given in an American geological publication."[24] This introduction was followed by nearly fourteen pages of verbatim citation from Holmes. By doing so, Barrell showed Holmes's importance in promoting the use of radiometric methods as well as suggesting that even as late as 1917 American geologists were still disconnected from significant portions of European research.

The three sections cited from Holmes gave three methods of using radioactive material for the purpose of measuring geologic time: accumulation of helium, pleochroic halos, and accumulation of lead. When Barrell returned to writing his own article, he attacked Becker again, this time on the topic of the age of the Llano series that Becker attempted to calculate to show the unreliability of the radiometric methods. Barrell spent twelve pages refuting Becker's arguments. Statement by statement, he showed how Becker was missing vital evidence, relying on out-of-date sources, misreading his sources, and at one point even ridiculed Becker stating, "[h]ow different this is from the facts has been shown on the previous pages. Becker, for some unknown reason, avoided using two ratios of Boltwood's list and apparently made an arithmetic blunder in recomputing the lead-uranium

[23] *Idem*, p. 841.

[24] *Idem*, p. 845. The article from which is is citing at length was Holmes, *Radioactivity and Measurement*.

ratio from a third."[25]

Barrell started the concluding section of his paper with the reference to the conflict of physicists and geologists. "In the last third of the nineteenth century physics, in the embodiment of its leaders, Kelvin, Helmholtz, Tait, and others, spoke with assurance on the limits of geologic time. Geologists sought to meet their demands, in so far as they could, but such men as Huxley, Geikie, Goodchild, and others, giving greater weight to the geologic evidence, refused to accept the restrictions which were set."[26] By this time, such a reference to the disciplinary conflict seemed to have become nearly obligatory. After making it, Barrell argued that convergence of evidence showed his view to be correct.

Throughout the article, Barrell went to great lengths to present geological evidence for and against the various estimates of geological time. He explicitly stated the goal of his article: "it is hoped that the problem [of radioactive methods] will be directly attacked, and from the standpoint of geological science, rather than left almost wholly as accidental investigations of the radio-chemist."[27] Barrell argued that researchers with proper knowledge of geological sciences and directly interested in the results of measuring geological time should take up radioactive methods.

At the very end of his long article, Barrell touched on related subjects: the length of time for evolution to take place and the source of stellar energy. His discussion there was rather general and speculative. In the previous sections citations permeated every statement and specific data were given; here those were lacking.

In the decade after the publication of Barrell's paper the article was cited numerous times. Looking at it with hindsight, it is easy to see a number of scholars who have recognized Barrell's significance. However, looking at it from the point

[25]Barrell, *Rhythms*, p. 868.
[26]*Idem*, p. 871.
[27]*Idem*, p. 874.

of view of the contemporaries, those voices were lost in the chatter of scientists. It was not the case that no one paid attention to Barrell's research; it was that no one paid attention to the people who paid attention. While there was a small group of researchers who were increasingly convinced of the usefulness of radiometric methods, this group was not recognized as expert on the subject of geological time by the many other scientific communities at large.

Among the citations to Barrell, there were ones that only concentrated on numerical answers. For example, as already mentioned, Knowlton in 1919 cited Barrell in support of over a billion year time scales. Others were less concerned with numerical numbers for the Earth's age. Some, like Reginald A. Daly, also in 1919, referred to Barrell's estimation of the date of glacial periods. T.C. Chamberlin in 1922, though he did not discusses radioactivity in this particular essay, cited Barrell as one of the representatives of a new school of thinking on geologic time.[28]

A larger number of researchers used Barrell's claims about the nature of formation of deposits. In 1919, E. M. Kindle cited Barrell in support of his own argument that sedimentation was not a uniform process; however, he made no menton of the relationship to the measurement of the Earth's age. Similarly and in the same year, E. C. Case cited Barrell as stressing the importance of small uncomformities. F. Bascom, in an 1921 article in which he applied Barrell's reinterpretation of erosion history to a new region, simply echoed some of Barrell's calculations. Finally, in 1924, Eleanora Bliss Knopf followed Barrell and Bascom in discussing erosion of the Appalachian highland, but made no reference to any numerical calculations of geological time.[29]

[28]Knowlton, *op. cit.*; Reginald A. Daly, "The Coral-reef Zone during and after the Glacial Period," *American Journal of Science* 48 (1919), p. 138; Thomas Chrowder Chamberlin, "The Age of the Earth from the Geological Viewpoint," *Proceedings of the American Philosophical Society* 61 (1922), p. 249.

[29]E. M. Kindle, "Inequalities of Sedimentation," *Journal of Geology* 27 (1919), p. 365; E. C. Case, *The Environment of Vertebrate Life in the Late Paleozoic in North America: A Paleogeographic Study* (Washington, D.C.: Carnegie Institute of Washington, 1919), p. 13; F. Bascom,

The importance of Barrell's explanation of the formation of small disconformities was additionally expressed by Charles Schuchert, who earlier in 1910 wrote to Becker praising his article against long time scales, but was later cited by Barrell as a convert to those time scales. Schuchert described his conversion as follows: "For a long time [I] was unable to reconcile such vast lengths of time with the comparatively few known great unconformities and the thickness of known formations [. . .] like all geologists, [I] had been thinking of geologic time as something like 100 million years in length."[30] The problem faced by Schuchert was that the longer time scales, implied that a large amount of the geological record was missing. Though Schuchert was making an overgeneralization when he wrote that all geologists believed the Earth to be 100 million years old, he and other geologists were quite sure of their calculations of the rates of sedimentation and deposition and did not want to give them up. However, after prolonged discussion with his Yale colleague, Schuchert came to agree with Barrell that there are many short breaks in the geological record that are of a different nature than the traditionally accepted disconformities.[31]

Writing in 1920, Herdman F. Cleland presented yet another attitude toward questions of the Earth's age that had persisted since the last century when he wrote that the "[e]stimates of the length of geological time are so uncertain that little dependence can be placed on them, but it is, nevertheless, interesting to speculate upon [length of geological time]." He included Barrell's estimate for the duration of the Pleistocene Ice Sheets (1,500,000 years), noting that "[a] few years ago Barrell's estimate would have seemed extravagant" but it was no longer,

"Cycles of Erosion in the Piedmont Province of Pennsylvania," *Journal of Geology* 29 (1921), p. 543; Eleanora Bliss Knopf, "Correlation of Residual Erosion Surfaces in the Eastern Applachian Highlands," *Bulletin of the Geological Society of America* 35 (1924): 633–668.

[30]Charles Schuchert, "Unconformities as Seen in Disconformities and Diastems," *American Journal of Science* 13 (1927), p. 260.

[31]*Idem*. Though Schuchert was not immediately persuaded to the multi-billion year time scales, see 211.

though he did not commit himself to it either.[32]

However, Barrell's arguments were not universally recognized, as there were papers that discussed the age of the Earth which did not cite him, including one by William Duane addressing radio-active approaches to the problem explicitly.[33] To be fair, the paper was rather brief and did not include any citations. H.V. Ellsworth complained that "geologists and mineralogists have done little or nothing in connection with the development [of the radiometric methods]."[34] He did not cite Barrell, perhaps justifiably, as Barrell, though he argued for the use of radiometric methods, himself did not employ them.

In summary, the significance of Barrell's paper was in providing a strong response to Becker and other proponents of short time scales. He gave geologists geological reasons for a much longer time scale, while still maintaining the validity of many previously held beliefs about rates of sedimentation. Barrell also provided a sympathetic introduction to radioactivity and the work of Holmes for American geologists. That is he provided the intellectual foundation for a new community knowledge: responses to previous research, statement of methods, scope of questions. However, none of those factors were enough in themselves to make the long time scales and the radiometric method community knowledge. This again demonstrates the differentiation between individual understanding and community knowledge. Individual understanding can be expressed in utterances such as an article.[35] Community knowledge, on the other hand, can only be established by investigating the responses to that original utterance and as historians we can

[32]Herdman Fitzgerald Cleland, "A Pleistocene Peneplain in the Coastal Plain," *Journal of Geology* 28 (1920), 705–6.

[33]William Duane, "The Radio-Active Point of View," *Proceedings of the American Philosophical Society* 61 (1922): 286–288.

[34]Hardy Vincent Ellsworth, "Radioactive Minerals as Geological Age Indicators," *American Journal of Science* 9 (1925), p. 127.

[35]Though one always must look for an individual speaking as a community member and sperate the two voices.

only learn of it by analyzing responses. For a new consensus on the Earth's age to establish itself two things had to happen: a specialist community of individuals had to develop its own specialist community knowledge, and a wider community of scientists in general had to defer to this specialized community on questions about the Earth's age. Barrell's article went some ways towards the first step, but did little towards the second. This is evident not only from the citations this paper did and did not receive, but also from the future attempts at the Earth's age research, some of which I mention in the conclusion of this chapter and explore more fully in the next chapter.

Although historian Patrick Wyse Jackson claims that Barrell's paper "achieved classic, almost cult, status among geologists," historians have seemed to give much less attention to Barrell than to other figures involved in the Earth's age dialogues such as Holmes, Sollas, Joly or Becker. For example, despite mentioning the paper neither Wyse Jackson, Dalrymple, nor Yochelson and Lewis summarized the argument presented in it, even though they do so for many other papers, and none of them mentioned the importance of the reconciliation of current sedimentary rates with the extended radiometric ages.[36]

Astronomy

Astronomical questions had been related to the discussion of the Earth's age since the initial involvement by William Thomson in the 1860s. In addition to the theory of the formation of the Earth itself, which was the foundation for one of the calculations, Thomson calculated the age of the sun. Additionally, George

[36]Patrick N. Wyse Jackson, *The Chronologers' Quest: The Search for the Age of the Earth* (New York: Cambridge University Press, 2006), p. 195. Yochelson and Lewis, *op. cit.*; G. Brent Dalrymple, *The Age of the Earth* (Stanford, Cal.: Stanford University Press, 1991). Wyse Jackson's claim of the classic status of Barrell's paper is confirmed by a search of the Web of Science database which shows that the article has been continually cited in the geological literature with the top three year of citations occurring during the 1990s six citations per year.

Darwin calculated the Earth's age based on his theory of Moon formation by fission. Darwin's astronomical calculations gave a wide range for the possible age of the Earth; however, during the 19th century, only the lowest value was cited in support of the shorter time scales. Darwin revised his estimates of the Earth's age during the 1905 presidential address to the BAAS, where he concluded that the Earth might be as many as 1000 million years old.[37]

The questions of the internal state of the sun and its source of energy and age did not return to prominence in astronomical research until the third decade of the 20th century. Discussions about the age of the universe were in nearly complete isolation from discussions of the Earth's age.[38]

Although most astronomical discourse did not intertwine with discussions of the Earth's age, there were some notable exceptions. The astronomer Thomas Jefferson Jackson See (1866–1962) perhaps was not that notable for his scientific achievements (despite a very promising start, by the 1910s he was ostracized by most astronomers). However, he remained respected in the popular press and continued to prolifically publish in a number of academic and popular publications. For example, in 1907 he published an article in *Popular Astronomy* in which he calculated the Earth's age according to Kelvin's formula to be about 100 million years. He dismissed influence of radioactivity because the phenomena were not understood enough and he proclaimed that "Several years ago when the enthusiasm over the radium discoveries was at its height there were those who admitted a terrestrial history of a thousand million years. But mysterious as radium still

[37]George H. Darwin, "Address by the President of the British Association for the Advancement of Science," *Science* 22 (1905): 257–267. For his earlier calculations, see George H. Darwin, "On the Precession of a Viscous Spheroid, and on the Remote History of the the Earth," *Philosophical Transactions of the Royal Society of London* 170 (1879): 447–538; George H. Darwin, "A Tidal Theory of the Evolution of Satellites," *The Observatory* 3 (1879–80): 79–84.

[38]Karl Hufbauer, "Astronomers Take Up the Stellar-Energy Problem, 1917–1920," *Historical Studies in the Physical Sciences* 11 (1981): 278–303; Helge Kragh, "Cosmic Radioactivity and the Age of the Universe, 1900–1930," *Journal for the History of Astronomy* 38 (2007): 393–412.

remains, it is doubtful if such a view is generally held today."[39]

In 1901, Robert Woodward published an astronomical calculation related to the Earth's age. Woodward, who participated in the Washington symposium on the Earth's age, and who was the astronomer to the USGS, argued against Laplace's assertion that there would be no perceptible change in the duration of the day as a result of the Earth's contraction due to cooling. He also calculated the lengthening of the day as a result of an accumulation of meteorites, concluding that it was even less than the shortening of the day due to cooling. The geologist Alfred C. Lane also thought that meteor dust could be used to measure geological time; however, in a more geological manner, he suggested measuring the accumulation of nickel from meteors in the arctic ice.[40]

The greatest discussion of issues related to the age of the age of the Earth from astronomers came from discussion of interdisciplinary efforts dealing with the solar system.[41] One of the individuals deeply involved in those debates and perhaps the most significant of American astronomers to opine on the Earth's age was Harlow Shapley (1885–1972). Just as Barrell aimed to introduce the geological

[39]Thomas Jefferson Jackson See, "The Age of the Earth's Consolidation," *Popular Astronomy* 15 (1907), p. 550 the same argument is repeated in Thomas Jefferson Jackson See, "On the Temperature, Secular Cooling, and Contraction of the Earth, and on the Theory of Earthquakes Held by the Ancients," *Proceedings of the American Philosophical Society* 46 (1907): 191–299. For biographical information, see Charles Peterson, "The Education of an Astronomical Maverick: T. J. J. See and the University of Missouri," *Journal for the History of Astronomy* 35 (2004): 293–304; Thomas J. Sherrill, "A Career of Controversy: The Anomaly of T. J. J. See," *Journal for the History of Astronomy* 30 (1999): 25–50.

[40]Robert Simpson Woodward, "The Effects of Secular Cooling and Meteoric Dust on the Length of the Terrestrial Day," *Astronomical Journal* 21 (1901): 169–175; Alfred Church Lane, "Meteor Dust as a Measure of Geologic Time," *Science* 37 (1913): 673–674.

[41]For analysis of this community, see Ronald E. Doel, *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Research, 1920–1960* (New York: Cambridge University Press, 1996). For detail of many of the debates, see Stephen G. Brush, *Nebulous Earth: The Origin of the Solar System and the Core of the Earth from Laplace to Jeffreys* (New York: Cambridge University Press, 1996) and Stephen G. Brush, *Fruitful Encounters: The Origin of the Solar System and of the Moon from Chamberlin to Apollo* (New York: Cambridge Univ. Press, 1996). See also, Stephen G. Brush, "Is the Earth too Old? The Impact of Geochemistry on Cosmology, 1929–1952," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 157–175.

community to the new science of radioactivity and its bearing on the question of geological time, Shapley used Barrell's article to introduce the astronomical community to what he perceived as the new geological thinking on the Earth's age and its relevance to astronomical questions. Shapley immediately noticed that the billion-year-old Earth posed a problem for the age of the sun: there were no known sources of energy to have sustained the sun's output at the current level, including radioactive decay of heavy elements. Shapley considered the geological evidence to be secure enough to suggest that there must be some unknown source of energy powering the sun.[42]

The question of solar energy was, in general, rather related to question of the Earth's age. During the 19th century, gravitational contraction was believed to be the source of solar heat and the issue was initially investigated by William Thomson, Hermann Helmholtz, and others. With the discovery of radioactivity, a number of scientists suggested that radioactive decay might power the sun. However, it was only toward the end of the second decade of the 20th century that astronomers seriously started looking into the question of solar energy, pushed to it by arguments about the age of the sun and other stars. During his investigations into stellar evolution, British astronomer Arthur Eddington, who now occupied George Darwin's chair in astronomy at Cambridge, came upon an astronomical objection to the short ages of the stars. He pointed out that if the sun's age were 20 million years, then stars that were 100 times brighter than the sun could only have lasted 100,000 years if contraction were the only source of energy. By 1920

[42]Harlow Shapley, "The Age of the Earth," *Publications of the Astronomical Society of the Pacific* 30 (1918): 283–298. Shapley also addressed the question of the Earth's age in Harlow Shapley, "Age of the earth," *Scientific American Supplement* 87 (1919): 34–5; Harlow Shapley, "The Age of the Earth," pages 90–100 of *The Universe of Stars, Radio Talks from the Harvard Observatory* (Cambridge, Mass.: Harvard Observatory, 1926). For biography, see JoAnn Palmeri, *An astronomer beyond the observatory: Harlow Shapley as prophet of science* (Ph. D. diss.), University of Oklahoma, 2000; Bart J. Bok, "Harlow Shapley 1885–1972," *Memoirs of the National Academy of Sciences* (1978): 241–291.

Eddington, Russell and Shapley had additional arguments for the longevity of the stars and used them to argue that astronomers should take up the problem of stellar energy.[43]

Theories of the formation of the solar system were also related to the Earth's age. For example, James Hopwood Jeans and Harold Jeffreys were highly critical of the Chamberlin-Moulton planetesimal hypothesis and on numerous occasions invoked questions of time in their arguments. Jeans at one point suggested that the solar system started to form only about 560 millions ago, while Jeffreys suggested a figure of around three billion years, which agreed with a figure arrived at by an American astronomer, Henry Norris Russell, who attempted to arrive at the age of the Earth's crust based on the abundances of radioactive materials in it.[44]

The astronomers' discussion displays yet even more dialogues which were entangled with the discussion of the Earth's age. The discussion of the source of energy of the sun was most directly related to question of geological time and despite its importance during the mid-19th century, discussion of sun's source or energy (and with it, its age) returned to prominence only during the 1920s. The age of the sun question was even less important to the astronomers than the question of the Earth's age was to the geologists. By 1920, the astronomical community still did not have a community knowledge about the question of the sun's age and its relationship to the Earth's age. The more interwound interaction between the two ages during the 1930s is briefly covered in the next chapter.

[43] Hufbauer, *op. cit.*

[44] For more details, see Brush, *Fruitful Encounters*, pp. 68-81; James Hopwood Jeans, *Problems of Cosmogony and Stellar Dynamics* (Cambridge: Cambridge University Press, 1919), p. 286; Harold Jeffreys, "On the Early History of the Solar System," *Monthly Notices of the Astronomical Society* 78 (1918): 424–441; Henry Norris Russell, "A Superior Limit to the Age of the Earth's Crust," *Proceedings of the Royal Society of London. Series A* 99 (1921): 84–86. Jeans during the 1920s switched from the last defender of the short age of the sun, to advocate that the sun was trillions of years old.

Paleontology

For Charles Darwin, the limited time afforded by William Thomson's calculations was the most serious objection to the evolution of life by natural selection. By the end of the 19th century, paleontologists and zoologists, like the geologists, convinced themselves that 100 million years were sufficient for evolution to take place, though the short age of the Earth was at times used to support faster modes of evolution, such as mutation theory.[45] However, when the restriction of the 100 million years seemed to have been lifted by John Perry's modification of Kelvin's calculation, one of the few British zoologists who adhered to Darwin's original notion of evolution, Edward Bagnall Poulton (1856–1943), jumped at the occasion to proclaim that the time restraints placed on biological evolution were now removed. He then proceeded to evaluate the time required to account for the evolution of the Earth's life forms. At no point did he try to place a numerical value on the rate of evolution, but he pointed out that most life forms observed today, existed in essentially the same form in the fossil record. He concluded that the time period in which fossiliferous rocks were found had to be multiplied many times to account for the evolution of today's life forms.[46]

It seems that in Europe, and especially in Britain, the topic of the age of the Earth and the time required for evolution may have been avoided because of the trouble caused by Darwin's time speculations. In the United States, the age of the Earth question did not seem to play a role in debates over evolution, especially prior to the 1890s. In general, the topic of the age of the Earth was quite removed from the communities of scientists investigating living organisms present and past. Nonetheless, for some individuals the question of the age of the Earth was relevant.

[45] Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983), pp 23–24, 201–202.

[46] Edward Bagnall Poulton, "Address to the Zoological Section by the President of the Section, I," *Science* 4 (1896): 625–637; Edward Bagnall Poulton, "Address to the Zoological Section by the President of the Section, II," *Science* 4 (1896): 668–680.

William Diller Matthew (1871–1930) was trained as a geologist. He received a geology Ph.D. from Columbia College in 1894. His interest in paleontology came initially from his interest in geology. Though he spent his career working for the Department of Vertebrate Paleontology at the American Museum of Natural History under Henry Fairfield Osborn, his research was in both paleontology and geology and he was competent in both fields. One of the early problems that Matthew engaged was biostratiagraphy. The problem was central to Osborn's research program in evolution, which required correlation of stratigraphic information from America and Europe.

In 1914, Matthew wrote, "There is as yet no satisfactory way of estimating the age of the Earth and the length of geologic periods [. . .] the translation into years is a matter of wide divergence of opinion and no real proof that any of the results are even approximately correct."[47] Matthew thought the situation was so dire that he attempted a calculation based on palaeotological evidence for the rate of evolution. He fully admitted that this was a very speculative endeavor, but it was no more speculative than the other methods.[48]

For Matthew, the age of the Earth was only of minor importance. He was not attached to any particular value. In his calculation, he arrived at relative time ratios of geological periods and used an estimate of the absolute age of one of the periods to calculate the total time. Even though his calculation were totally dependent on that estimate, Matthew's argument would stand unfazed if that estimate were to change. This is what seems to have happened in 1917 when Barrell argued for a much longer time scale and Matthew was immediately persuaded. The longer time scale did not change anything about his own beliefs. What Matthew

[47] William Diller Matthew, "Time Ratios in the Evolution of Mammalian Phyla. A Contribution to the Problem of the Age of the Earth," *Science* 40 (1914), p. 232.

[48] The rate of evolutionary change would become more important during the evolutionary synthesis of the 1930s and 40s. Vassiliki Betty Smocovitis, "Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology," *Journal of the History of Biology* 25 (1992), p. 23.

did care about were relative ages of geological horizons and their global correlation. He attempted to calculate the age of the Earth only because he thought the rate of evolutionary change was a more uniform process than the various geological processes employed in the previous calculations.

Matthew, like the previous generation of scientists, was more interested in obtaining an answer that would be accepted by the wider community than in convincing others of his own particular way of understanding the problem. In 1918 he thought that the publication of Barrell's article would bring a new consensus to the community. About the article Matthew wrote: "I was greatly impressed with its importance. I believe it will be a classic—as much as Walcott's essay of twenty-five years ago."[49] He was so sure that Barrell would bring new consensus that he urged Osborn to use the long time scales in Osborn's American Museum of Natural History. Based on discussion with Becker and Clarke, Osborn dismissed long time scales as late as 1916, and instead cited as most reliable the geo-chemical methods. However, by 1928, he accepted long time scales and referred to Barrell as a "brilliant geologist."[50]

Personally, Matthew favored long time scales prior to Barrell's publication, but he bracketed his individual understating. He wrote: "For various reasons, I am disposed to believe that the relative length of the Paleozoic should be revised upward," but he was willing to accept shorter estimates as not unreasonable.[51] Later he explained his reasons: "I had not employed such large absolute figures because I was not satisfied that the radioactive data were sufficiently consistent to be dependable."[52] Barrell's paper did not only convert Matthew and reassure

[49]W.D. Matthew to H. F. Osborn, July 1, 1918. H.F. Osborn Administrative Correspondence folder 4, general correspondence box 70, folder 70, WDM.

[50]Henry Fairfield Osborn, "The Origin and Evolution of Life Upon the Earth (Lecture I, Part I)," *Scientific Monthly* 3 (1916): 5–22; Henry Fairfield Osborn, "Present Status of the Problem of Human Ancestry ," *Proceedings of the American Philosophical Society* 67 (1928), p 152.

[51]Matthew, *op. cit.*, p. 235.

[52]W.D. Matthew to H. F. Osborn, July 1, 1918. H.F. Osborn Administrative Correspondence

him, but it also provided him with the belief that others will be converted to the radiometric method of estimating the Earth's age. This example shows how an individual whose understanding did not change significantly, changed his addressed utterances based on his perception of community knowledge.

Other paleontologists were less optimistic about using paleontological data to arrive at Earth's age. John Mason Clarke wrote, "I may as well frankly say at the beginning that there can be little hope of arriving either at a reliable or an approximate conclusion as to the age of the Earth through this palaeontological channel, unless the study of the chronological development of life may in some way afford a measure of the rate of vital processes and thus the measure of some short span or infinitesimal fraction of Earth history."[53] Clarke did not think that it will be possible to establish the rate of evolution, and the efforts that went to such attempts did not warrant their work, except for auxiliary results rising from those efforts.

The Symposium at the American Philosophical Society

When in 1897 Isaac Minis Hays started to perform the duties of the Secretary and the Librarian of the American Philosophical Society, the Society, despite its rich heritage was on the verge of obscurity. Its meetings were hardly ever attended by more than two dozen people, and sometimes as few as half a dozen showed up. At the same time, the American Association for the Advancement of Science, National Academy of Science and many disciplinary societies were drawing crowds of hundreds and even thousands. One of the ways in which Hays tried to uplift the APS was by establishing an annual General Meeting. Those meetings were

folder 4, general correspondence box 70, folder 70, WDM.

[53]John Mason Clarke, "The Age of the Earth from the Paleontological Viewpoint," *Proceedings of the American Philosophical Society* 61 (1922), p. 272.

successful in drawing hundreds of attendants.[54]

Despite rejuvenating the APS, the Society was far from its old glory and Hays was on a constant lookout for ways to engage more of its somewhat lethargic members. In 1921, after just reading about a very successful symposium on the age of the Earth at the meeting of the BAAS, Hays suggested to the APS president, William Berryman Scott, that the Society should organize a similar symposium on this side of the Atlantic. Scott, who during the 1890s did not believe that the age of the Earth could be calculated, agreed, though he was doubtful that four or five speakers could be found.[55]

The committee on the General Meeting approved the proposal for the symposium on the age of the Earth. Four speakers were suggested (B.B. Boltwood, Harlow Shapley, Charles Walcott, and John Mason Clarke) to take up the matter from the points of view of radio-activity, astronomy, geology, and paleontology, respectively. Scott and the astronomer Henry Norris Russell were appointed to the subcommittee on the symposium.[56] From the original list, only Clarke accepted. Walcott declined to participate but suggested that Scott write to Chamberlin, who agreed to participate, though he was not sure if his health would allow him to attend the meeting in person.[57] Russell suggested and contacted Yale mathematical astronomer Ernest William Brown and Harvard Medical School biophysics professor William Duane as the other two participants.[58]

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[54]Whitfield J. Jr. Bell, "Minis Hays, Secretary, Librarian, and Benefactor of the American Philosophical Society," *Proceedings of the American Philosophical Society* (1975), pp. 404–5.

[55]Hays to Scott. 25 Oct 1921. V.B.2 Hays Correspondence 1920–1921. APS. Scott to Hays. 27 October 1921. V.A.4 William Scott Correspondence 1921–1925. APS. In 1897, Scoot claimed that "Geological chronology can be relative only" William Berryman Scott, *An Introduction to Geology* (New York: Macmillan, 1897), p. 221. The same conclusion was reached in William Berryman Scott, *An Introduction to Geology*, 2nd edition (New York: Macmillan, 1907). The second edition was still printed without a change in 1921.

[56]4 & 25 Nov 1921; 12 Jan 1922; 15 Feb 1922. Minutes of Committee Meeting of the APS. APS.

[57]Scott to Clarke, 6 February 1922. V.A.4 William Scott Correspondence 1921–1925. APS.

[58]Scott to H.H. Donaldson. 6 Feb 6 1922. V.A.4 William Scott Correspondence 1921–1925. APS.

The efforts to organize the symposium sheds some light on the place of the question of the Earth's age in American science. First, it was the British symposium that prompted a struggling American scientific society to mimic its success. Next, the original suggestion of Walcott and Boltwood as participants in the symposium who had last published on the Earth's age in 1893 and 1907, suggests that Walcott's paper was still well remembered and that there were not any new individuals who obviously had carried the debate past what took place fifteen years earlier. The two men who had been the most forceful, Becker and Barrell, had passed away. The choice of Shapley (and the nomination of Russell to the subcommittee) reflected familiarity with the recent energy of the sun debates. From the people who have participated in dialogues on the Earth's age, two others seem missing. Those are Frank W. Clarke the geochemist and William Matthew the paleontologists. F. W. Clarke's absence probably was due to the lack of discussion of the problem from the chemical point of view in the BAAS symposium, as well as chemistry's general exclusion from the narrative of the disciplinary conflicts on the Earth's age. Matthew's exclusion was possibly the result of the very close relationship of Scott and Henry Fairfield Osborn who by this time had started to separate from Matthew. Last, the individuals who finally participated are also revealing. Chamberlin represents the few enthusiastic and knowledgable researchers about the topic. Clarke, who in November 1922 wrote a letter to the editor of the *New York Times* protesting about misrepresentation of questions of geological time in the popular press, represents the popular engagement with the topic.[59] And Duane and Brown, whose eventual contribution to the symposium amounted to saying "we don't know and we don't care," represent the majority of the scientific community's attitude toward the question.[60]

[59] John Mason Clarke, "Letter to the Editor," *New York Times* (1921): 103.

[60] It is possible that individuals were unwilling to participate in the symposium because it was organized by the APS, however, I have found no evidence to this effect.

The symposium took place on 22 April 1922. Scott read Chamberlin's paper at the meeting and afterward the society decided to publish the paper in its *Proceedings*.[61] Brown was reluctant to submit his remarks for publication, but did so "in spite of [his] opinion on the question."[62] The final publication seems to have been delayed because Duane took a long time to submit the manuscript and return proofs despite not making any changes to either. Hays asked him to expand on his "abstract," but Duane cited lack of time and energy as reasons why he did not do so.[63]

Chamberlin did take the occasion of publication to revise and expand his paper. He was quite excited about the revisions and asked for 500 offprints of the article. He grew quite impatient when the paper was not published by the end of the year, going as far as to inquire if it would be possible to publish his paper separately as he had a "special reason why [he] would like to distribute [the offprints] now."[64]

The papers finally appeared in the December issue (published in January 1923) of *The Proceedings of the APS*. Chamberlin started his paper in a metaphorical manner, retelling the story of a disciplinary relationship over the last century in the debates on the Earth's age. Representing geology by a horse, from the beginning he warned

> Kelvin checked him too high. A reasonable check should have given him good form and some sense of restraint, but checked too high, he took to short mincing steps. As a result he's in poor shape to swing into the great pace of the new leaders. It is too much to expect him to recover his natural step at once, but he will in time. For the present, he will need a touch of the whip now and then to make him keep pace. Let this be gentle and considerate, because of his age and his past service, but let it be persuasive.[65]

[61]Chamberlin to Scott. 18 April 1922. V.B.3 Secretary Correspondence. Box 1922 A–N. APS.

[62]Brown to A.C. Abbott. 22 May 1922. V.B.3 Secretary Correspondence. Box 1922 A–N. APS.

[63]Hays to Duane, 5 May 1922, V.B.3 Secretary Correspondence. Box 1922 A–N. APS; Duane to Abbott 4 Aug 1922, V.B.3 Secretary Correspondence. Box 1922 A–N, APS.

[64]Chamberlin to Secretaires of the APS. 13 June 1922. V.B.3 Secretary Correspondence. Box 1922 A-N. APS. Chamberlin to Secretaries of the APS. 2 Jan 1923. V.B.3 Secretary Correspondence. 1922 O-Z and 1923 A-H. APS.

[65]Chamberlin, *Age of the Earth*, p. 248.

What Chamberlin meant was that geologists were reluctant to accept the longer time scales suggested by the new radiometric methods, still not fully having recovered from the debates of the last century. He briefly reviewed the other methods (including his own attempt to estimate the Earth's age from the planetesimal hypothesis which was most likely two or three billion years).[66] The rest of the paper was devoted to showing that geological evidence was, in fact, in line with those extended time scales. He argued that there had to be corrections to the calculations from currently observed rates of denudation and sedimentation. The second argument against the earlier calculations was that those calculations assumed that the post-Cambrian time occupied some known (and significant) amount of the total age of the Earth. Chamberlin pointed out that it was currently believed that the Precambrian was much longer than previously assumed and, further, that there is no observed evidence for guessing the depth of the geological column. The expanded part of Chamberlin's paper was on the salt method of determining ocean's age. The conclusion of this discussion stated that "The science of hydrogeology, of which oceanology is only a part, is not yet ready to render a verdict; it has more need of a court of inquiry than a place on the witness stand."[67]

Clarke, in his own paper, gave thoughtful review of the problem of using the rate of evolution, concluding that it is impossible from an investigation of biological change to infer anything about the age of the Earth. Along the way, he mentioned that "the interpreters of radio-chemistry—we thank them for giving us what we already had. There is time enough," stating that the length of geological time is enough for the process of evolution to have taken place.[68]

Ernest Brown, who obtained his Ph.D. under George Darwin and was an expert

[66] Thomas Chrowder Chamberlin, "Diastrophism and the Formative Processes. XIII. The Bearing of the Size and Rate of Infall of planetismals on the Molten or Solid State of the Earth," *Journal of Geology* 28 (1920): 665–701.

[67] Chamberlin, *Age of the Earth*, p. 271.

[68] Clarke, *Age of the Earth*, p. 275.

in lunar theories, was rather brief in his remarks, stating that there is little, if any, way that astronomy can contribute to the discussion of the age of the Earth or the solar system. He wrote that Darwin's theory of moon formation has been shown to be incorrect. Only physical considerations and pure speculation were useful in those discussions.[69] In the briefest of the communications, William Duane simply listed the numerical estimates of ages of various minerals based on uranium/helium and uranium/lead ratios.[70]

Overall the effectiveness of the symposium for the APS is not clear. Of all the articles published, the one by Chamberlin has received the most citations, especially from supporters of the radiometric measures.[71]

The APS symposium was organized around the principle that there were multiple approaches to the question of the Earth's age. It was organized not out of desire to really answer the question, but to increase attendance at the annual meeting. The attitude of the speakers is representative of the scientific community in general, and it seems that few, like Chamberlin, were very enthusiastic toward the topic, while most did not have much to say. We may conclude that, while the discussion of the question of the Earth's age remained in principle familiar to the members of the scientific community, its details were far from familiar and united.

Conclusion

During the first 20 years of 20th century the scientific community gained a new way of answering the question of the earth's age. The new community knowledge answer was that there were multiple methods, stemming from multiple disciplines,

[69]Ernest William Brown, "The Age of the Earth from the Point of View of Astronomy," *Proceedings of the American Philosophical Society* 61 (1922): 283–285.

[70]Duane, *op. cit.*

[71]For example, Arthur Holmes, "The age of the earth," *Nature* 112 (1923): 302–303; Theodore William Richards and L. P. Hall, "The atomic weight of uranium lead and the age of an especially ancient uraninite," *Journal of the American Chemical Society* 48 (1926): 704–708.

arriving at multiple answers. This knowledge emerged as many of the varying, semi-independent dialogues each received some support and some challenges. Even if there were some groups of scientists who were satisfied with one particular approach to the question of the Earth's age (e.g. Holmes, Barrell, Chamberlin who all accepted the radiometric techniques) they were not recognized by either geologists or scientists in general as speaking with a voice representing the whole community.

The confusion from the beginning of the 20th century was resolved, but the answer this time was even more disappointing than the one from the 1890s. Around 1920, as there was no one research community that held jurisdiction over the age of the Earth question, anyone and everyone could take a stab at it. Boltwood and Holmes used radiometric methods, Barrell based his arguments on geological processes, Matthew used paleontological data, and astronomers such as Shapley and Russell contributed to the dialogues based on their own disciplinary research. Although by the mid-1920s a number of scientists were convinced that the Earth's age was in the billions of years and radioactive methods were best at determining the ages of individual rocks, it was not clear to the scientific community in general that this was or ought to be the dominant view not limiting itself to the radiometric methods. This is evident from the facts that when in 1925 the National Research Council formed a committee on the age of the Earth, this committee investigated multiple points of view. The previous year, the well-known geologist George Perkins Merrill, writing a history of the previous century of American geology, in the last chapter addressed the question of the Earth's age. The chapter started with a footnote which explained that the subject of the Earth's age was "one of great popular interest and it has been thought advisable to give the accompanying résumé compiled from readily available sources."[72] Merrill discussed the

[72]George Perkins Merrill, *The First One Hundred Years of American Geology* (New York: Hafner, 1924/1969), p. 648.

Earth's age only as an afterthought, yielding to popular demand, and not digging deep to understand the issue. The chapter itself ended with discussion of Becker's 1910 contribution to the debate; however, there was one final footnote that noticed a "recent (1917)" paper by Barrell. Merrill was not much more decided on the topic two years later when in the NAS memoir of Becker he wrote "which of the two [Becker or Barrell's approach to Earth's age] credit is to be given for nearest approach to actual facts is as yet impossible to say."[73] Merrill's ignorance of the status of discussion and the NRC's committee's approach are evidence that during the mid-1920s no single approach to the Earth's age was accepted.

[73]George Perkins Merrill, "Biographical Memoir of George Ferdinand Becker, 1847–1919," *Memoirs of the National Academy of Sciences* 21 (1927), p. 8.

Chapter 6

Rise of a New Common Answer

The final chapter in the account of attempts to answer the question of the Earth's age describes the establishment of radiometric methods as the primary methods for determining geologic time and of establishment of a community of researchers who were recognized by the scientific community at large as the experts on the subject of the Earth's age. My main argument in this chapter is that this final success of the radioactive method was due to two parallel developments, both tied to the National Research Council, one well recognized in current historiography, the other, nearly forgotten (respectively, the Subcommittee on the Age of the Earth, and the Committee on the Measurement of the Geological Time). I will argue that both were equally important, even though each accomplished very different goals.

Andrew Abbott argues that there is an asymmetry in how a professional community portrays itself to the outside world and how it functions within the professional workplace. To obtain jurisdiction over some set of problems professionals must present themselves as a homogenous entity to the public. Among themselves, however, the profession is very heterogenous with many tasks being performed by individuals without the credentials that the professionals claim are necessary.[1] The homogenous-versus-heterogenous distinction also applies to scientific communities.

[1]Andrew Abbott, *The System of Professions: An Essay on the Division of Expert Labor* (Chicago: University of Chicago Press, 1988).

More important here is general asymmetry of what a community must do to function in solving specific problems, and what it must do to be recognized as the (only) community that can and ought to solve the specific problems. This asymmetry helps us comprehend how between 1925 and 1935 a community of researchers obtained jurisdiction over the problem of the Earth's age.

There were two parallel developments that made this happen. The first one was the actual development of the community with the appropriate community knowledge that was robust enough to sustain a research program. The second development was to have this community recognized as holding the jurisdiction over the question of the Earth's age and geologic time; that is, adding the claim to general scientific knowledge that questions about the Earth's age are to be deferred to this community.

It is essential to notice the two *distinct* sets of community knowledge I just described corresponding to the community responsible for the new research program and the wider scientific community. Each individual is a member of multiple communities and each of those communities has its own community knowledge. Some of those communities are nested; A. C. Lane, for example, was a geologist, which also made him a scientist. There is a distinction between speaking as a geologist and speaking as a scientist.[2] Up to this point when discussing community knowledge about the Earth's age, I was discussing general scientific knowledge—that is, what an individual could say about the Earth's age as a general scientist—because there was no community knowledge on the age itself. Each new utterance represented an individual understanding. Beginning in the 1920s, a community of researchers started to collaborate on this problem, and as the result of this collaboration, for the first time, a new set of knowledge emerged. However, the

[2]It is quite likely that the community knowledge about what it is to be a scientist held by geologists was different from the one held by biologists or physicists. This difference is not significant to the discussion at hand.

emergence of this new specialist community did not necessarily alter the general scientific community knowledge on the topic. A group of researchers were specializing in using radiometric methods for the measurement of geologic time. However, if a general scientist, such as a geologist not involved in that research, was asked a question about the Earth's age by another scientist or a non-scientist, that geologist, drawing on the existing general scientific community knowledge, would not know that this particular community was researching the Earth's age. If he did know, he would not know whether this community's research was to be valued more than some other community's research. This was the situation during the 1920s described in the previous chapter.

In the previous chapters I tried to provide a comprehensive survey of the discussion of the age of the Earth, at least enumerating all the principle individuals involved. This approach would be impractical for the present chapter for two reasons: the number of individuals has increased considerably and the geologic time research project has grown in size to incorporate a wide range of research, including multiple publications by an ever increasing number of researchers. Just as in the previous chapters, the amount of the technical details presented is the minimum necessary to understand the dialogues within the scientific communities. Readers interested in additional technical details of the history of radiometric dating will find G. Brent Dalrymple's *The Age of the Earth* an excellent source.[3]

NRC and Cooperative Research

Changes in organizational structure of American science were again the background to the debates on the Earth's age. Around the turn of the century, research universities replaced federal agencies as the primary seats for scientific inquiry. World War I brought on further changes. The National Research Council was

[3]G. Brent Dalrymple, *The Age of the Earth* (Stanford, Cal.: Stanford University Press, 1991).

established to coordinate scientific war efforts and after the war was transformed into a peacetime institution. It became one of the most important organizations in interwar American science. The NRC itself did not conduct any research, but it was the central planning institution that decided what research should be performed. It not only directed support to certain research ventures, it actively sought scientists willing to take on projects favored by the NRC leaders. The favorite projects were ones involving "borderline science." The NRC leaders believed cooperation among scientists from different fields counteracted the undesirable effects of specialization and isolation among scientists by providing cross-fertilization and the discovery of new research topics.[4] The age of the Earth question fit the NRC's interests well: it was a widely known question, one which scientists wished to have resolved, and it was also a borderline question spanning multiple fields.

However, the efforts to sustain the borderline projects were usually not successful. In his influential study "Managing Cooperative Research and Borderline Science," which examines the NRC during this time, Glenn Bugos reports that the NRC's top-down organizational efforts to create research at the interdisciplinary level were unsuccessful, but that the efforts of individuals were more so.[5] The NRC was not the only institution interested in bringing in cooperation among practitioners of separate disciplines. As Robert Kohler well documented, the Rockefeller Foundation under the leadership of Warren Weaver, pursued transdisciplinary and holistic science. Weaver supported individuals, not necessarily for their projects but for their shared vision of transdisciplinary science. Yet most of his projects failed to successfully create any lasting enterprises. For what would become known

[4]Glenn E. Bugos, "Managing Cooperative Research and Borderland Science in the National Research Council, 1922–1942," *Historical Studies in the Physical and Biological Sciences* 20 (1989); Nathan Reingold, "The Case of the Disappearing Laboratory," *American Quarterly* 29 (1977): 79–101; Karl T. Compton, "Specialization and Cooperation in Scientific Research," *Science* 66 (1927): 435–442; Robert Kohler, *Partners in Science: Foundations and Natural Scientists, 1900–1945* (Chicago University Press, 1991), ch. 4.

[5]Bugos, *op. cit.*

as molecular biology, he supported research of individuals who were investigating different problems in different context but no coordinated effort arose out of that support. The most successful transdisciplinary endeavors resulted around not an intellectual concept (there were none shared by the participants) but by researchers sharing a physical instrument.[6] Research into the question of the Earth's age provides further examples of the failures of transdisciplinary efforts, but it also provides an analysis of what caused some of the failures while other efforts succeeded.

Lane's Committee

Two committees of the National Research Council are relevant here. One was chaired by Adolph Knopf and remembered for the contribution by Arthur Holmes. The other was chaired by Alfred Lane. The differences between the committees are pronounced in the current historiography. Whereas Holmes is a subject of numerous articles and a popular biography and Knopf's committee is featured in all the discussions of the history of the age of the Earth, Lane and his committee are virtually forgotten.[7] When Lane is mentioned, erroneous information is given such as that Lane worked for the Canadian National Research Council, that his interest in the age of the Earth was the result of the publication of Knopf's Bulletin, and that Lane's biggest achievement was his contribution to that Bulletin.[8] Yet, as I will show, Lane's contemporaries perceived him as a rather consequential

[6]Kohler, *Partners in Science*, especailly chs. 11-13. On the successful role of instruments in creating transdisciplinary communities see also Peter Galison, *Image and logic: A material culture of microphysics* (Chicago University Press, 1997).

[7]Cherry L. E. Lewis, *The Dating Game: One Man's Search for the Age of the Earth* (New York: Cambridge University Press, 2000).

[8]Respectively, Patrick N. Wyse Jackson, *The Chronologers' Quest: The Search for the Age of the Earth* (New York: Cambridge University Press, 2006), p. 237; Ronald E. Doel, *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Research, 1920–1960* (New York: Cambridge University Press, 1996), p. 81; and Rexmond C. Cochrane, *The National Academy of Sciences: The First Hundred Years, 1863–1963* (Washington, D.C.: National Academy of Sciences, 1978), pp. 313–314.

contributor to the research on geologic time.[9]

If Adolph Knopf's recollection is to be trusted, The Committee on Measurement of Geological Time by Atomic Disintegration was started by Andrew Cowper Lawson "to see what [the measurement of geological time by atomic disintegration] is all about."[10] Even if those were not exactly Lawson's words, the sentiment seems likely. As I have argued in the last chapter, during the first half of the 1920s, there was not only no consensus on the question of the Earth's age and geologic time, but many scientists were quite ignorant of the dialogues that did take place on the topic among the various small groups of individuals working on the question. The Committee on Measurement of Geological Time by Atomic Disintegration was established in 1923 within the Division of Geology and Geography. Alfred Lane reluctantly accepted the position as the chair. He would hold this position for the next twenty-three years.

Alfred Church Lane (1863–1948) obtained his Ph.D from Harvard in 1888 and, in the years leading up to it, was an instructor of mathematics at that institution. For the rest of his career he brought a quantitative approach to many geological problems, including that of geological time. Lane spent his early career at the Michigan Geological Survey, and later was a professor of geology at Tufts College.[11] He had been interested in questions of measuring geological time since at least the beginning of the century and suggested doing so by using unconventional methods, such as the accumulation of meteoric dust.[12] In 1907, speaking to the AAAS, Lane

[9]Robert Leslie Nichols, "Memorial to Alfred Church Lane (1863–1948)," *Proceedings of the Geological Society of America* (1953): 107–118; Paul Eugene Fitzgerald, "Alfred Church Lane (1863–1948)," *Bulletin of the American Association of Petroleum Geologists* 32 (1948): 2168–2170; Oscar Brauer Muench, "Determining Geologic Age from Radioactivity," *Scientific Monthly,* 71 (1950), p. 300; Walter H. Bucher, "National Research Council and Cooperation in Geological Research," *Bulletin of the Geological Society of America* 53 (1942), p. 1336. His importance can also be judged from the numerous acknowledgments in paper by scientists writing on the topic e.g. Kenneth Knight Landes, "Age and distribution of pegmatites," *American Mineralogist* 20 (1935), p. 81.

[10]Adolph Knopf, "Measuring Geologic Time," *Scientific Monthly* 85 (1957), p. 229.

[11]Nichols, *op. cit.*

[12]Alfred Church Lane, "Report of the State Board of Geological Survey for the year 1901,"

assumed the Earth to be 100 million years old. In the same talk, he considered the chemical evolution of the Earth's oceans: one of the major research questions still to be worked out. The next year he disagreed with Becker's recalculation of the age of the Earth as a cooling globe, challenging not as much Becker's final numerical answer, but his assumption of an Earth with a uniform diffusion rate and his ignorance of newer cosmogonies (such as that of Chamberlin), which would nullify Becker's calculations.[13] He continued attacking the sodium method as late as 1929.[14] By 1915, Lane expressed increased scepticism about the measurement of the Earth's age, calling numeric calculations "largely guess work," while claiming that it seemed more likely that the Earth was closer to "millions of millions" of years old rather than "millions." Lane cited Arthur Holmes at one point in his paper; however, the application of radioactivity which had the most significance was its contribution to the temperature gradients used in the traditional cooling globe calculations.[15]

Lane became the chairman of the committee applying radiometric methods in 1923, but even as late as 1926, Lane still admitted the possibility that the decay rates of radioactive elements could vary through time.[16] By the 1930s Lane publicly stated that radiometric methods were the best methods of estimating geological time and the Earth's age and gave an age around 1.5 billion years.[17]

The other members of the committee included Hardy Vincent Ellsworth, a

Annual Report Michigan Geological Survey (1902), p. 243; Alfred Church Lane, "Meteor Dust as a Measure of Geologic Time," *Science* 37 (1913): 673–674.

[13]Alfred Church Lane, "Schaeberle, Becker and the Cooling Earth," *Science* 27 (1908): 589–592.

[14]Alfred Church Lane, "The Earth's Age by Sodium Accumulation," *American Journal of Science* 17 (1929): 342–346.

[15]Alfred Church Lane, "On Certain Resemblances between the Earth and a Butternut," *Scientific Monthly* 1 (1915): 132–139.

[16]Alfred Church Lane, "Discussion: Geologic Age Calculations," *Science* 64 (1926): 119.

[17]Alfred Church Lane, "Science Service Radio Talks: The Age of the Earth," *Scientific Monthly* 32 (1931): 362–365; Alfred Church Lane, "Scientists Determine New Figure for Age of Earth," *Science News-Letter* 26 (1934): 167

chemists serving as a mineralogist with the Geological Survey of Canada who researched rare earth and radioactive minerals and became interested in applications of atomic disintegration to geologic time; Frank Hess, a USGS economic geologist with an interest in rare earth elements; Samuel Lind, a radiation chemist at the U.S. Bureau of Mines where the next committee member, R. B. Moore, was also a chemist; and Roger Clark Wells, a chemist for the USGS who was deeply fascinated by the decay of uranium. Starting in 1925, the committee expanded and included Knopf. Perhaps unsurprisingly, all of the committee members supported the long time scales.[18]

The work of Lane's committee was not explicitly interdivisional. Its stated goal was to bring to the attention of geologists the relevant work done by chemists and physicists. The committee did not publish any bulletins and restricted itself to composing informal annual reports. Especially during its early years, it had very little NRC funding. At a time when the NRC was actively seeking to finance borderline science with hundreds of thousands of dollars, Lane secured various outside sources of funding, including a $5,000 gift from an anonymous acquaintance from Boston.[19]

Historians well recognize that scientific publications give little light on how

[18]Maurice Hall Haycock, "Memorial of Hardy Vincent Ellsworth," *American Mineralogist* 38 (1953): 427–431; Edward P. Henderson and Marjorie Hooker, "Memorial of Frank L. Hess ," *American Mineralogist* 54 (1969): 626–634; Keith J. Laidler, "Samuel Colville Lind," *Biographical Memoirs of the National Academy of Science* 74 (1998): 1–18. Frank L. Hess, "The Age of the Earth," *Scientific Monthly* 20 (1925): 597–602; Roger Clark Wells, "Ages of Minerals and Rocks Based on Radioactive Changes," in Edward Wight Washburn (ed.), *International Critical Tables of Numerical Data, Physics, Chemistry and Technology* (New York: McGraw-Hill, 1926): 381–384.

[19]NRC's annual budget was around one million dollars. National Research Council (NRC), "A History of the National Research Council, 1919–1933," *Reprint and Circular Series / National Research Council* (1933), p. 9; Kohler, *Partners in Science*, pp. 102–103. The anonymous donor was Everett Morss. Correspondence related to Morss' donation and the NRC appropriation to the committee can be found in the folder Committee on Measurement of Geologic Time by Atomic Disintegration: 1924–1929. Central Policy Files, 1924–1931. NAS-NRC Archives (Washington: D.C.). The committee's annual reports provide information on other sources of funding secured by the committee.

scientists arrive at their conclusions, but rather show how they persuade others that their findings are correct. It is therefore, intriguing to read the reports of Lane's committee, which without much fanfare announced the thinking of its chairman. Of course, the report was created with a specific agenda in mind, but this goal was rather different from one motivating a scientific article or personal correspondence. The first report of the committee presented in 1924 was the shortest of all at only four pages. Going through it in detail will be helpful in understanding the work of the committee and the work of the community, as well as the difference in what Lane accomplished from efforts such as those by Knopf's committee.

The first paragraph of Lane's 1924 report listed the important contributors in the application of atomic disintegration to the questions of geological time: Joly, Rutherford, Strutt, Boltwood, Mügge, Holmes, and Barrell. Next, he confirmed the fragmentation of research: "But while the physicists and chemists have added much to our knowledge during the past decade, geologists have as yet made but little use of the important data thus contributed."[20] Hence this committee was formed. "It appeared at once" that the first order of business should be compiling a bibliography that once completed "will be sent to workers in this field, as a means of getting in touch with what is being done."[21] The problem was the lack of awareness of relevant research by the relevant researchers. Lane's committee decided it was most important that all the researchers be aware of what is going on. While not explicitly stated, the consequence of the action taken is that this committee would decide which work was relevant and who were the relevant researchers who would receive this bibliography.

In addition to knowing the relevant literature, the researchers would need ma-

[20]Alfred Church Lane, *Report of the Committee on the Measurement of Geological Time by Atomic Disintegration* (Washington, D. C.: National Research Council. Division of Geology and Geography, 1924), p. 1.
[21]*Idem.*

terials for analysis. For obtaining those materials, the committee secured a small grant of $150 dollars. Also, two scientists offered to share the specimens they had in their possession. This shows that even such small grants could contribute to a significant development in scientific research.

Next came a more substantial problem that investigations of geological time had to address. The fractions of radium, uranium, and lead in the minerals to be measured were much smaller than most geologists knew how to measure. Lane listed the individuals who had tried several techniques that were not successful. The report also suggested other studies and listed them with names of the researcher who would undertake them. The nature and tone of those announcements were very different than that of a scientific article. This was not a presentation of completed experiments, but rather an effort to keep everyone informed of the activities of the other researchers, presumably so that work would not be duplicated and that those researchers could be contacted.

In addition to the uranium-lead method, the report next touched on the pleochroic halo method, concluding that its usefulness was currently an open question and that the committee was providing some assistance to one L.E. Spock who was investigating just this question.[22]

Lane, as chairman, maintained heavy correspondence that unfortunately does not seem to have survived in any archive. However, he often shared what he deemed appropriate from this correspondence through the bulletins. For example, he reported that Joly believed the discrepancy between the results of lead-thorium and lead-uranium methods was one of the major problems in the area. However, H.V. Ellsworth was currently working on just this problem "and it has been the pleasure of this Committee to testify to the importance of his work." Lane not only

[22]For descriptions of the different radiometric methods see the section on radioactive on page 164.

reported who was working on what, he further evaluated this work and promoted some as significant, while downplaying others. Through this, as well as other activities as chairman, he exerted considerable influence by setting the expectations of the members of the community. By differentiating among the quality of work done, he further made possible the building of community knowledge by narrowing, from tens or even hundreds of investigations and publications, the pieces of research that one ought to be familiar with, and also provided a mechanism to reduce the number of conflicts within a community. If results of one investigation were at odds with results of another investigation, but one investigation was more trustworthy than the other, then there was no reason to panic. Again, it was important that Lane dealt with a multidisciplinary community of researchers who were not familiar with, or even interested in, the research methods or questions of other members of this community. By evaluating the work being done Lane made it possible for the community to build up community knowledge. This was in contrast to the situation during earlier decades when different researchers made contesting claims with no way to resolve them.

Lane also mentioned the limitations of the committee's work, outlining the work that yet was to be done. For example, he listed further applications of a method developed by Schlundt and Moore. The committee wanted to solicit cooperation from researchers who might be interested in pursuing this avenue of work. Recruiting new scientists was a major undertaking of the committee. Scientists were recruited for specific tasks that Lane thought would advance research in the use of radiometric methods. When a new researcher joined, he was initiated into the community and its knowledge.[23]

Only after those comments had been made did the report present a summary of the six current methods for using atomic disintegration for the purpose of measur-

[23]See, for example, the case of Alfred Nier discussed below.

ing geological time: lead-uranium method, lead-thorium method, helium-uranium method, helium-thorium method, radium method, and the pleochroic halo method. This summary was only an enumeration of the methods, it was not meant as an introductory text for novices in the field, but a list for individuals who already shared some basic knowledge on the topic. However, as is clear from the rest of the report, some individuals did not possess all the basic knowledge. The audience for this report were experts in their particular field but at the same time, they were novices in research on geologic time.

For a community to be productive, its members must share the expectations of the other members of the same community. They must reply with common utterances to common questions. They need to agree to what is obvious and what is surprising, what needs to be said, and what needs to be omitted. Such a community can function only if a geographically or institutionally isolated community member shares the knowledge that enables her utterances to be accepted by the community. It was to the establishment and maintenance of this common knowledge that the work of Lane was dedicated.

In contrast to Knopf's committee, discussed next, which took as its goal production of a monograph containing knowledge, Lane's primary effort was the production of ephemera aimed at facilitating the dialogue among scientists in various fields. Lane's primary product was the annual report. A major part of it was composed of summaries of relevant scientific work from around the world from the previous year. Because the researchers were scattered across academic disciplines as well as across the world, Lane saw it as essential to provide its members with a common background to which they would be responding. Knowing new research in the field, however, was not sufficient for a functioning community. The new publications needed commentary on which lines of research seemed profitable and which were problematic, which research was of high quality and which needed scrutiny.

Lane's reports provided such a commentary.

Lane's work was facilitating dialogue within a specific community and not directly producing knowledge. The publications of the committee were mimeographs produced in about one hundred copies and, for the most part, distributed directly by Lane to the interested parties. They were composed not of fully formed articles or even notices, but rather snippets that made sense only to the members of this specific community that already shared some knowledge. For example, the first two sentences of the report for 1925 were

> Before going into details it may be well to sum up the salient results of the year. It appears even clearer that the ratio of lead to uranium does increase in the older uraniferous minerals, and that if lead from other sources can be eliminated the relative age of different minerals can be estimated.[24]

No time was spent on formalities, and no explanation given for why the issue of lead content was so significant that the results needed to be mentioned as the very first item in the report. The audience of the report already knew.

Lane created a communication tool aimed at maintaining a community of researchers interested in geologic time by ensuring all the community members knew what needed a reply and what kinds of replies were expected. His annual reports, which by the 1930s grew to be over a hundred pages long, included extensive annotated bibliographies of relevant works, summaries of works in progress, Lane's own editorial comments on the direction of research, and even excerpts from correspondence that included informal discussion of research not likely to be found in an academic journal. Lane seems to have been in contact with the majority of investigators of radiometric dating in the world. He was the clearinghouse for rock specimens and a general gatekeeper for the community. His correspondence and the annual reports replaced face-to-face dialogues. The bibliography, the annotations,

[24]Alfred Church Lane, *Report of the Committee on the Measurement of Geological Time by Atomic Disintegration* (Washington, D. C.: National Research Council. Division of Geology and Geography, 1925), p. 1.

and the summaries were the technologies that ensured that all the interlocutors shared the same knowledge and were replying to the same utterances. Unlike published books or journal articles, the reports did not follow a prescribed genre. They had a place for personal annotations and by design, they were to serve only for the present moment, each year to be replaced by a new one. They were addressed to a specific interlocutor—another researcher who was participating in this particular research at this particular time. Unlike journal articles, they were not directed to what Bakhtin would call the superaddressee—someone who stands beyond space and time, someone who will read and understand after we no longer exist. Their goal was *not* to stand the test of time, just the opposite. Their goal was *not* to be judged by history, but to be forgotten by it.

Lane's efforts resulted in truly inter-field endeavor. The affiliations of the committee members and the bibliography reflected participation from geology, chemistry, and physics. But the annual reports were not the only way in which Lane contributed to investigations of geologic time. He actively campaigned for research relevant to the age of the Earth and enrolled new scientists in this research. For example, when the physicist Albert Nier arrived at Harvard he had no interest in the age of the Earth; it was Lane and Gregory Baxter, another committee member, who convinced Nier to employ his new mass spectrometer to analyze isotopes of lead relevant for radiometric dating. The effort was an interdisciplinary collaboration: Lane and other geologists provided the mineral samples; Baxter, a chemist, prepared them; and Nier, a physicist, analyzed them. The results were published in chemical and physical journals, but were made known to all interested parties by Lane's annual report. Nier's work made the direct measurement of isotope abundances possible, thus transforming the research field.[25]

[25]Michael A. Grayson, "Professor Al Nier and His Influence on Mass Spectrometry," *Journal of the American Society for Mass Spectrometry* 3 (1992): 685–694; Alfred O. Nier, "Variations in the Relative Abundances of the Isotopes of Common Lead from Various Sources," *Journal of the*

NRC Bulletin No. 80

The NRC was organized into disciplinary divisions that were composed of numerous committees, but a number of the committees were established to span multiple disciplinary divisions. There were three committees within the NRC that were ideally suited to investigate questions of the Earth's age. Each one had a different efficacy in achieving cooperative research. At least in the case of the Earth's age, the more explicit the effort to bridge the gap between research fields, the less fruitful the result. The only one of the committees that has a significant presence in current historiography is the one which published a monograph.

I now discuss the best remembered of the three committees—the "Subcommittee on the Age of the Earth." It was part of the explicitly interdivisional "Committee for the Physics of the Earth" established in 1926 within the Division of the Physical Sciences with the cooperation of the Division of Geology and Geography and the American Geophysical Union. Chaired by Adolph Knopf, the committee originally included in addition to Knopf, three other Yale faculty: Charles Schuchert, Alois Kovarik, and Ernest Brown (the same who participated in the APS symposium).[26] The British geologist, Arthur Holmes, and A. C. Lane were soon added. The goal of Knopf's committee was to prepare a report that would present the current state of knowledge on the subject. This report was eventually published as the NRC Bulletin no. 80.

Although he did not conduct any research in the area himself, Adolph Knopf (1882–1966) was named the chairman of the subcommittee on the Age of the Earth. His connection to the topic was through the people surrounding him. He was a

American Chemical Society 60 (1938): 1571–1576; Alfred O. Nier, "The Isotopic Constitution of Radiogenic Leads and the Measurement of Geological Time. II," *Physical Review* (1939): 153–163. For details, see Alfred O. C. Nier, interview by Michael Grayson and Thomas Krick as the University of Minnesota, Minneapolis, Minnesota, 7 and 8 April 1989 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0112).

[26] J.S. Ames to C. Schuchert. 22 Dec 1926. Box 19. CSP.

student of Andrew Lawson who for a time headed the Geology and Geography Division of the NRC and was a member of the other committee on geological time. Based on conversation with Boltwood, when Knopf went to Yale he became interested in the question of geological time. His wife, the geologist Eleanora Bliss Knopf, utilized Barrell's method.[27]

The subcommittee on the age of the Earth met on June 6, 1927, and it enumerated the different methods that had been used to measure the age of the Earth and geologic time. The list was subdivided into sections. The first was "Methods of the 'hour glass' type;" it included the Earth's cooling and the sun's shrinking as well as "The journey of the Solar System from the Milky Way." The section on "Radioactive Disintegration of Elements" included eight methods. "Debit and Credit Methods" included Joly's sodium method. Charles Schuchert marked this section as assigned to Adolph Knopf. Schuchert indicated that he was responsible for the next section on "Rates of Erosion," as well as the section on "Organic Evolution." The remaining three sections were accumulation of meteorites, cyclic methods, and atmospheric cycle and accumulation. Altogether there were forty-four methods listed. Schuchert marked the halo method as not reliable and the Uranium-Lead method as the most reliable. Next to "Organic Evolution," Schuchert wrote his own name and noted that there were no quantitative methods.[28]

Knopf contributed to the Bulletin a short chapter on the age of the ocean based on the accumulation of sodium in which he questions the assumptions on which Joly's and Clarke's calculations were based. Knopf did not present any new analysis or reinterpretations of the data, but rather cited previous objections to this method by Barrell and Lane, and pointed to the uncertainty of the assumptions.

[27]Robert G. Coleman, "Memorial of Adolph Knopf (Dec. 2, 1882–Nov. 23, 1966)," *American Mineralogist* 53 (1968): 567–576.

[28]List of Methods of Measuring Age of the Earth and Geologic Time [Notes from Committee Meeting]. Box 20. CSP.

He concluded that "[t]he most that can be said is that the estimate of 100 million years for the age of the ocean is probably a minimum."[29]

Alois Francis Kovarik (1880–1965) for two decades was a physics professor at a small school in Iowa during which time he earned a Ph.D. from the University of Minnesota. He later become a fellow with Hans Geiger in Rutherford's Laboratory and finally joined Yale's physics faculty where he befriended Boltwood. In his contribution to the bulletin, Kovarik provided an introduction to the data and methods required to perform the calculation of mineral age based on rates of atomic disintegration.[30]

Ernest William Brown (1866–1938) was a mathematician astronomer who devoted his life to the calculation of lunar theory and similar problems of planetary motions. He joined the Yale faculty in 1907. While he occasionally worked on problems separate from his main research agenda, the question of the Earth's age does not seem to be connected in any way to his research or interests.[31] In the short article published as part of Bulletin 80, Brown argued that there was no purely astronomical way of calculating the Earth's age (by which he meant no method based on mathematical calculation of planetary motions), but the data available were consistent with the billion year time scales suggested by other contributors to the volume.[32]

Charles Schuchert (1858–1942) was a paleontologist and paleogeographer who was appointed to Yale in 1904. Like Barrell, Schuchert was attracted to rhythms and periodicity in geological history.[33] In 1910, he gave lengths of geological times

[29] Adolph Knopf, "Age of the Ocean," in *The Age of the Earth* (Washington, D.C.: National Research Council, 1931), pp. 71.

[30] Alois Francis Kovarik, "The Age of the Earth-Radioactivity Methods of its Determination," *Scientific Monthly* 32 (1931): 309–318.

[31] Frank Schlesinger and Dirk Brouwer, "Ernest William Brown," *Biographical Memoirs of the National Academy of Science* 21 (1941): 243–73.

[32] Ernest William Brown, "The Age of the Earth from Astronomical Data," in *The Age of the Earth* (National Research Council, 1931): 460–466.

[33] Adolph Knopf, "Charles Schuchert 1858–1942," *Memoirs of the National Academy of Sci-*

in accordance with Walcott while being very impressed with Becker's paper and using it with his students.[34] However, by 1915, citing British geologist Alfred Harker, in his geology textbook Schuchert seemed to side with the over billion year time scales suggested by radiometric dating, stating "Even if finally it shall turn out that the physicists have to reduce their estimates as to the age of certain minerals and rocks, geologists nevertheless appear to be on safer ground in accepting their estimates than those based on sedimentation, chemical denudation, or loss of heat of the Earth."[35] Joseph Barrell's arguments removed most doubts that Schuchert had, and, during the 1920s, Schuchert openly supported long time scales.[36] However, although he still did not believe that there could be enough of the geological column to account for an Earth of over a billion years, he proffered slightly shorter time scales.[37] Therefore, he admitted to having been "surprised by his own results" of the calculations he did for the committee.[38] Schuchert started those calculations by assuming 500 million years for the time since the beginning of Cambrian and then concluded that, indeed, there was enough thickness of the strata and there were enough breaks to reconcile the geological record with the data obtained by radiometric methods.

Schuchert also briefly discussed calculations of the Earth's age based on biological considerations. Here he cited calculations of Matthew and J. M. Clarke's APS symposium paper with whom he worked before going to Yale. He concluded with

ences 28 (1952), p. 372.

[34]Charles Schuchert, "Paleogeography of North America," *Bulletin of the Geological Society of America* 20 (1910): 427–606. Schuchert to Becker. 13 September 1910. Box 19. General Correspondence 1908–1915. GFB.

[35]Charles Schuchert, *Text-book of Geology, Part II. Historical Geology* (New York: Wiley, 1915), p. 984; Alfred Harker, "Geology in Relation to the Exact Sciences, With an Excursus on Geological Time," *Nature* 95 (1915): 105–109.

[36]Charles Schuchert, "Unconformities as Seen in Disconformities and Diastems," *American Journal of Science* 13 (1927): 260–262.

[37]Charles Schuchert, *Outlines of Historical Geology* (New York: John Wiley and Sons, 1924), pp. 104–105.

[38]Charles Schuchert, *Outlines of Historical Geology*, 2nd edition (New York: John Wiley and Sons, 1931), p. 14.

Clarke's conclusion that the rate of evolution cannot be used to measure geologic time.

The final contributor was Arthur Holmes who prior to joining the committee had just published a new book dealing with the Earth's age.[39] Holmes's contribution was disproportionately larger than any other contribution and took up nearly 350 pages of the 470-page volume. It also considerably delayed its publication. In his contribution, Holmes proceeds from an introduction to the basics of physics of radioactivity to detailed discussions of and refutation of the halo method which Schuchert describes as the best known to the geologists, and detailed description of the minerals that can be used for radiometric dating. The largest part of Holmes's section was devoted to a survey of world mineral calculations based on the uranium-lead method, which was followed by a brief discussion of the uranium-helium method. In the end, Holmes considered few consequences of the long geological time scales. Finally he extended special gratitude to Knopf, Lane, and R. W. Lawson. Lawson in particular he thanked for discussion and two decades of collaboration. Of Lane he wrote "[he] has helped very materially by keeping me in touch with American investigations while they were in progress; by his stimulating criticism of current views and by the encouragement that flows like a tonic from his friendly letters."[40] Holmes thanked Knopf for having "shouldered more than the usual burdensfor which an Editor is expected to be responsible," including recalculating many of the calculations using a different value for some of the constants, and "completing the bibliographic details of many of the references."[41]

The final report of the committee was published in 1931 as NRC Bulletin No. 80

[39] Arthur Holmes, *The Age of the Earth: An Introduction to Geological Ideas* (London: Ernest Benn, 1927).

[40] Arthur Holmes, "Radioactivity and Geological Time," in *The Age of the Earth* (1931), p. 455.

[41] *Idem*, p. 455–456

at a cost of nearly $5000, equal to the division's annual appropriation for all of its publications.[42] Lane, even before he saw it, called Bulletin 80 a "magnum opus" and was "yearning to see it." After publication of the report, the committee dissolved. Lane asked all of its members to join his and asked Knopf to be the chairman, but Knopf turned him down. Lane also suggested dropping "by Atomic Disintegration" from the name of the committee.[43] Three of Knopf's committee members joined Lane's committee which did shorten its name to "Committee on the Measurement of Geologic Time." It was no longer necessary to state by which method; by 1932 only one method was clearly preferred.[44]

The mission of Knopf's committee was to bring together representatives of multiple approaches and present a unified answer to the age of the Earth question in an NRC Bulletin that combined all disciplinary approaches. However, this cooperative effort failed, and the report of the committee contained separate sections describing the methods of measuring the age of the Earth based on various disciplinary approaches. Knopf explained: "In assembling the results of the work of the committee, it has been thought best, because of the extreme diversity of the topics considered in this report, to present them under the respective names of the five members of the committee responsible for them."[45]

The work of Knopf's committee did little in prompting cooperative research but although the results were not integrated, the radiometric method of measurement of time was clearly the preferred one. Not only did its discussion occupy over 80%

[42] Adolph Knopf et al., *The Age of the Earth* (Washington, D.C.: National Research Council, 1931). Minutes of meetings of the Division of Physical Sciences, 1930–1934 pp. 5-6.; 1931. F.K. Richtmyer to V. Kellog . 18 October 1930. NRC-DPS. Also, "The Physics of the Earth," n.d. ibid.

[43] Lane to Schuchert. 13 May 1931. Box 23. CSP.; Lane to Schuchert. 5 May 1931. Box 23. CSP.

[44] Alfred Church Lane, *Report of the Committee on the Measurement of Geological Time* (Washington, D. C.: National Research Council. Division of Geology and Geography, 1932), pp. 1, 32-33.

[45] Knopf et al., *op. cit.*, p. v.

of the volume, but even authors who were evaluating the other approaches deferred to the radioactive methods. Although the committee was organized according to the view that there are multiple ways of calculating Earth's age, the final report made it evident that only one of them was productive and accepted by the experts writing the report.

The bulletin provided a new standard text on the topic. It included discussion of all the previous methods considered and all the authors either explicitly used measurements based on uranium-lead ratios, or deferred to them. Whereas for the previous two decades in scientific discussion there were various murmurs about Earth's age, mostly missed by the majority of scientists, here was a shout that presented an overwhelmingly detailed, coherent, and (at least comparatively) unified account of the research on the topic.

The previous self-contained utterances were the size of a scientific article, the problem of the Earth's age was so conceived that an individual (of course drawing on the research of others) could attempt to answer the question fully in the span of about 30 pages. By 1931 the self-contained utterance, Bulletin No. 80, was nearly 500 pages long and Holmes alone cited nearly 700 sources. Earlier, each article contained the entirety of the project of calculating the Earth's age. By now, investigations of geological time were done by researchers who were publishing small pieces of the much larger project. Such collaborative work required a number of shared assumptions and coordination of efforts that could not have be done by publishing a textbook volume summarizing the present state of the field.

The NRC Bulletin No. 80 is generally regarded as marking the triumph of the radiometric method and I agree that it established the scientists using radiometric methods as the ones with pre-eminent jurisdiction to conduct research on the question of the Earth's age. But establishing a community's right to a research problem is not sufficient to make its research successful, especially if the community crosses institutional boundaries. At a minimum, those researchers also have to engage in fruitful dialogue.

Recalling the events later, with a tone of self-congratulation, Knopf wrote "Between the beginning of the present century and 1930, an age of the Earth of 100 million years had become generally accepted. In that year it was suggested that in the light of the new discovery of geology and radioactivity, the Earth is at least 2000 million years old."[46] But he did not forget about Lane and generously recalled his accomplishments and the importance of the annual report in particular. "Under the able chairmanship of A.C. Lane, this committee promoted the fruitful cooperation between the investigators of the widely different disciplines."[47] In the preface the the Bulletin No. 80, Knopf thanked A. C. Lane who has "given valuable aid to the work of this [Knopf's] committee by advice and informal cooperation."

The activities of Lane and his committee also parallel NRC's "Committee on Common Problems of Genetics, Paleontology, and Systematics" studied by Joe Cain. This committee for a time was led by Ernst Mayr who established a successful interdisciplinary community by organizing conferences, sending circular letters, and producing mimeographed reports.[48] However, unlike Lane's committee, this one was soon transformed into a more formal arrangement with a scientific society and a journal. Cain documents a famous scientist's efforts, which led to the establishment of a formal society.

[46] Knopf, *op. cit.*, p. 225. Of course, Knopf was aware that the billion year time scales were not first suggested in 1930 and in the body of the same article he presents more factual history.

[47] Knopf, *op. cit.*, p. 229.

[48] Joe Cain, "Epistemic and Community Transition in American Evolutionary Studies: the 'Committee on Common Problems of Genetics, Paleontology, and Systematics' (1942–1949)," *Studies in History and Philosophy of Biological and Biomedical Sciences* 33C (2002): 283–313. See also: Joe Cain, "Common Problems and Cooperative Solutions: Organizational Activity in Evolutionary Studies, 1936–1947," *Isis* 84 (1993): 1–25; Joe Cain, "Exploring the Borderlands: Documents of the Committee on Common Problems of Genetics, Paleontology, and Systematics, 1943–1944 ," *Proceedings of the American Philosophical Society* 94 (2004).

Age of the Earth during the 1930s

In 1936, chaired by T. S. Lovering, "The Interdivisional Committee on Borderland Fields Between Geology, Physics and Chemistry" was established within the NRC as an explicit effort to explore the disciplinary borderlands. However, the age of the Earth question, which clearly lay at the intersection of geology, physics, and chemistry, was not a subject of its activities. Two years later, the committee produced a mimeographed preliminary report (a planned final version was never published) and referred the reader to the "excellent work" of Lane's committee in this area.[49] When in 1942, Walter H. Bucher in "National Research Council and Co-Operation in Geological Research" discussed all the subcommittees that were part of the committee on the Physics of the Earth, including the subcommittee on the age of the Earth, he did little more than enumerate them. He described in much more detail Lane's committee praising it: "We have a right to be proud of what has been accomplished under [the committee's] auspices."[50] Lane's mimeographed reports were singled out as a great accomplishment of the committee. Bucher further credited the work of this committee for the recent embrace of a research program in radiometric dating of geological time by the Council of the Geological Society of America.

Those contemporary accounts are evidence of the perceived importance of the work of Lane's committee. Another piece of evidence in support of my claim that there were two parallel developments and that both were necessary is further corroborated by the fact that other cooperative efforts failed.

It is the responses to a knowledge claim that determines the meaning of that claim. That is why Walcott's utterance on the Earth's age, which was cited only

[49]Thomas Seward Lovering, *Report of the Interdivisional Committee of the National Research Council on Borderland Fields between Geology, Physics and Chemistry* (Washington, D.C.: National Research Council, 1938), p. 46.

[50]Bucher, *op. cit.*, p. 1339.

approvingly, attained a canonical status, while Becker's claim, which was challenged as well as supported, did not become part of community knowledge. After the publication of Bulletin No. 80, there were no challenges to the claims it raised and one can find many approving statements such as the one from the 1934 annual report of the National Academy of Science that claimed "[a]tomic disintegration has come to be regarded as an acceptable method for such age determination."[51] However, even more telling of the status attained by a community of experts investigating geologic time by atomic disintegration is the deference of the astronomical community.

The problem that the Earth was too old for astronomers already started to be noticed in 1915 by F. A. Linedemann and Shapley, as discussed in the previous chapter.[52] However, the problem became more severe during the 1920s and 1930s. At the same time that Holmes, Lane and their colleagues were advocating ages of our planet at over 1.5 billion years, astronomers, based on the new data and theories of the expanding universe, started to believe that the universe's age should also be in a similar neighborhood. Obviously, the Earth could not be older than the universe, and likely it should be much younger. During the 1920s the similar ages of both made many astronomers uncomfortable, the problem worsened during the 1930s and 1940s when radiometric calculations of the Earth's age extended past 3 billion years, the making the Earth older than the universe. The astronomical community struggled to account for this discrepancy. Unlike previous debates where two methods gave conflicting results and supporters of one method pointed to faults in the calculations of the other groups, this time astronomers did not challenge the calculations of the Earth's age, but rather started searching for faults within their own calculations. At one point some of them went as far

[51]National Academy of Science, *Annual Report of the National Academy of Sciences* (Washington D.C.: National Academy of Sciences, 1934), p. 30.

[52]F. A. Lindemann, "The Age of the Earth," *The Observatory* 38 (1915): 299–301.

as to abandon the expanding universe idea and started advocating a steady state universe. Historians have already recognized that age of the Earth was part of the history of astronomy.[53] I am emphasizing here that the astronomers' response to the age of the Earth research (their deference to it) is a sign of the acceptance of the radiometric methods and the geological time community as holding jurisdiction over the topic of the Earth's age.

While much of the astronomical discussion was taking place in Europe, the debate also was present in America. At a joint symposium of the Astronomical Society of the Pacific and American Physical Society in 1935 on the geological and cosmic time scales, six papers were presented. George D. Louderback read a paper on "The Age of the Earth from Sedimentation" in which he questioned the adequacy of sedimentation methods as appropriate for measuring the geological time. However, he concluded that whatever results can be obtained were in line with radiometric dating. Robley D. Evans read a paper on the age of the Earth from radioactive disintegration, concluding that the Earth is somewhere between 1,850 and 3,500 million years old. B. Gutenbergy approached the question of the Earth's age by looking at changes in temperature and elasticity and was satisfied that the Earth's age is in the range of $10^9 - 10^{10}$ years. Three more astronomers tried to reconcile those ages with the age of the universe.[54]

However, just because the radiometric methods became community knowledge as the methods to be used in measuring geological time, that did not mean there was a consensus on the matter. The method of Laplace, treating the Earth as a cooling sphere, was not forgotten as Arnold N. Lowand used it to calculate the history of temperature distribution in our planet. However, unlike Lord Kelvin, he

[53] Stephen G. Brush, "Is the Earth too Old? The Impact of Geochemistry on Cosmology, 1929–1952," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 157–175.

[54] George D. Louderback et al., "The Geologic and the Cosmic Age Scales," *Science* 82 (1935): 51–53.

included the additional heat from radioactivity and, rather than use the calculation to arrive at the Earth's age, used the 1.6 billion years as the assumed age of the Earth in his calculation.[55] Herbert P. Woodward writing to Schuchert to thank him for a copy of the Bulletin and for citing him in it continued "I should like to return to further study of the Earth's age as determined by the denudation-deposition method. It seems to me that we have a method of calculating the Earth's time which is nearly as reliable as any other method, and that one of the first problems is to get acceptance of the validity of this method by other workers in the field."[56] Woodward was pushing for using denudation as a measure for the age of the Earth and saw Schuchert as his sympathizer. Even during the 1930s there were still dissenting views, just as a decade earlier. The difference was that Woodward was aware that his view did not correspond to the community knowledge on the age of the Earth, which now accepted that radiometric methods were the proper ones for the measurement of geologic time, and Woodward presented his views accordingly.

Consensus is always a discordant consensus, there is no shared belief, there is not a unanimous one thought. There are always many voices representing individuals' understandings and even disagreeing among the community members. But there is a consensus nonetheless because the community members recognize that they are expecting the other community member to expect them to hold a given belief. Dissenting voices, such as Woodward's here, provide an answer to the question asked at the outset of this study: "What does it mean for a community to know something?" In part, it means that a member who is dissenting is positioning his or her utterance knowing that they are disagreeing with what the community knows.

[55] Arnold N. Lowan, "On the Cooling of a Radioactive Sphere," *Physical Review* 44 (1933): 769–775; Lowan, *op. cit.*.

[56] Woodward to Schuchert, 19 July 1931. Box 23. CSP.

Conclusion

Why should we pay attention to Lane? I claim that while historians have attended to scientists' activities in knowledge production as well as institution and discipline building, those aspects of academic work provide only one part of the story. A second part is provided by viewing science as an act of dialogue. It is the maintenance of this dialogue that Lane facilitated.

There are a number of individuals, such as Arthur Holmes, who have contributed to the development of scholarship with their intellectual products. There are also a number of well-known individuals, such as George Hale, whose impact on academic pursuits was much more pronounced through their organizational and administrative efforts. Hale is remembered for establishing lasting institutions such as *The Astrophysical Journal*, Yerkes and Mount Wilson Observatories, the NRC itself, and, I would argue, the discipline of American astrophysics can be considered a palpable result of Hale's activities.[57] However, unlike Holmes and Hale, Lane did not leave behind anything tangible.

In 1949, geologist Esper Larsen eulogized Lane with these words: "Largely through his efforts and the work of his committee we now have an excellent, though still imperfect, time-scale for the geological column. Dr. Lane stimulated and coordinated the work of his Committee so that it became one of the most successful of those of the [National] Research Council."[58] In praising Lane, Larsen was not referring to intellectual contributions or formal institutions, he was referring to providing a means for an interdisciplinary community to exist in dialogue. Lane's work in facilitating the dialogue within his community suggests that in our inves-

[57]On Hale's achievements, see Helen Wright, *Explorer of the Universe: A Biography of George Ellery Hale* (New York: American Institute of Physics, 1966/1994). On Hale and the formation of the NRC, see Reingold, *op. cit.*; Daniel J. Kevles, "George Ellery Hale, the First World War and the Advancement of Science in America," *Isis* 59 (1968): 427–437.

[58]Esper Signius Larsen, Jr, "Memorial of Alfred Church Lane," *American Mineralogist* 34 (1949), p. 251.

tigations of interdisciplinary efforts we must not limit our gaze to discipline and institution building. In our investigation of scientific practice, we must include the notion of dialogue.

Conclusion

The historiography of the age of the Earth question grows directly out the histories written by the participants in the original events of the late 19th century. It is those histories that present the disciplinary conflict thesis. Yet, as I argued here, this conflict thesis does not match the historical events, and it does not even correspond to the understanding of the situation by the historical actors. This conflict narrative became part of the community knowledge and the mythology of the scientific communities involved in geological time. This myth is present in introductions and conclusions of public addresses and in histories given in textbooks, it does not appear in research papers or private correspondence. As such it should be taken for what it was and not as representing the actual history or the beliefs of the scientists presenting it.

The first significant historical account of research on the question of Earth's age was the 1968 article, "Rutherford, Boltwood, and the Age of the Earth: The Origin of Radioactive Dating Techniques" by Lawrence Badash. It sets out the basic shape of the account for the dialogues on the Earth's age in the 19th and 20th centuries. Badash presents his history in disciplinary terms starting with the challenge by William Thomson to uniformitarianism followed by the acceptance by the geologists of the limitations of a 100 million year old Earth. Next in Badash's account is the discovery of radioactivity and Boltwood and Rutherford's measurements of geological age using radiometric techniques which, however, resulted in only limited new research by Arthur Holmes. The geologists finally accepted radiometric methods after the publication of the 1931 bulletin by the National Research Council subcommittee on the age of the Earth. Summing up, Badash wrote "the radioactive dating techniques pioneered by Rutherford and Boltwood had reached maturity, and maturity brought with it the blessing of the geologists." That is, Badash identified the chronology and many of the individuals involved in the debates on the Earth's age still accepted by historians today, including this author. However, what exactly is "maturity" and "blessing"?[1]

The best and most detailed account of the debates on the Earth's age was published in 1975 by Joe Burchfield. Burchfield centered his account on the role of William Thomson (Lord Kelvin). His analysis revolves around the concepts of authority of Thomson, physics, and quantitative methods which triumphed over the geologists and their methods in late 19th century Britain. As discussed in the introduction, Burchfield also presents history in disciplinary terms; however, during his analysis he recognizes the many exceptions to this account.[2] Since the publication of Burchfield's work, historians significantly expanded the details of our knowledge on the research about the age of the Earth during this time period, but the basic account set out by Badash and Burchfield has not been significantly altered. Dalrymple provides a detailed survey of the methods used.[3] Stephen Brush attempts to shift the focus from disciplinary conflict of physics vs. geology, to a criticism of uniformitarianism based on the principle of conservation of energy; however, his discussion is still presented in disciplinary terms.[4] The collection *The*

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[1]Lawrence Badash, "Rutherford, Boltwood, and the Age of the Earth: The Origin of Radioactive Dating Techniques," *Proceedings of the American Philosophical Society* 112 (1968): 157–169.

[2]Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (New York: Science History, 1975).

[3]G. Brent Dalrymple, *The Age of the Earth* (Stanford, Cal.: Stanford University Press, 1991).

[4]Stephen G. Brush, "The Age of the Earth in the Twentieth Century," *Earth Science History* 8 (1989): 170–182; Stephen G. Brush, *Transmuted Past: The Age of the Earth and the Evolution of the Elements from Lyell to Patterson* (New York: Cambridge University Press, 1996); Stephen G. Brush, "Is the Earth too Old? The Impact of Geochemistry on Cosmology, 1929–1952," in C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (2001): 157–175.

Age of the Earth: From 4004 BC to AD 2002 has added a number of valuable case studies and provided further details, yet again no challenges to the overall account are presented.[5] A number of other studies illuminate the details of various individuals involved. Also, there is no lack of popular accounts of various aspects of the search for the age of the Earth.[6] In many ways this historiography mirrors the professional historical discussion of the topic. The history of the Earth's age debates is of high interest and there are a number of popular accounts of it. It is often mentioned in other histories which primarily do not deal with the question of the Earth's age. There are relatively few historians investigating it, and in their accounts the narrative of the conflict of physicists vs. geologists dominates even though many details are available to show the shortcomings of such an account.

The account presented here adds to this historiography in two distinct ways. First, I provide new details to the collection so far amassed. I have consulted new archival sources and have added details surrounding some of the important events in the debates on the Earth's age such as the 1893 Washington symposium and the individuals associated with it, the involvement of George F. Becker in the debate, and the NRC committees on the Earth's age. I have made arguments for the significance of under-appreciated actors like Joseph Barrell and Alfred C. Lane as well as the discipline of chemistry. But, more fundamentally, I have attempted to reevaluate the disciplinary conflict story.

The new account proceeds as follows. In United States, after the Civil War scientists were aware of the age of the Earth debates which were taking place in Britain during the middle third of the century. During the 1890s, in particular

[5]C. L. E. Lewis and S. J. Knell (eds.), *The Age of the Earth: From 4004 BC to AD 2002* (London: Geological Society, 2001).

[6]E.g. Claude C. Jr. Albritton, *The Abyss of Time: Unraveling the Mystery of the Earth's Age* (San Francisco: Freeman, Cooper and Company, 1980); Patrick N. Wyse Jackson, *The Chronologers' Quest: The Search for the Age of the Earth* (New York: Cambridge University Press, 2006).

after the 1893 symposium of the Washington Academy of Sciences and the paper on the Earth's age by Charles Walcott later that year, a new temporarily stable configuration of community knowledge emerged stating that there was a conflict about the Earth's age between the physicists and geologists, however, that the Earth's age was around 100 million years, which was long enough for geology. This community knowledge of the general scientific community did not mean that all or even the majority of the individuals believed these two claims to be the case. Most scientists had little interest in the question of the Earth's age and the ones who did hold a strong opinion held a range of beliefs. What this new community knowledge did mean was that when answering the question "How old is the Earth?" scientists answered using this community knowledge and when addressing each other, they structured their utterances as if their addressee accepted the community knowledge.

During the first decade of the 20th century, new calculations of the age of the oceans by Joly, Becker and Clarke reinforced the older estimates. The new cosmogony advocated by Chamberlin and the discovery of heating effects of radioactivity nullified the basic assumptions underlying the calculations of the Earth's age based on a cooling globe. The calculations of ages of minerals based on radioactive decay by Boltwood and Rutherford suggested the age of the Earth to be an order of magnitude larger than previously accepted. Most importantly, there were only a limited number of responses to this range of new claims, and those responses lacked unison. Consequently, the old consensus was lost and it was not replaced by a new one.

During the first two decades of the 20th century, a number of individuals continued to hold specific beliefs about the Earth's age, but the general scientific community and specific disciplinary communities did not hold any community knowledge on the topic. That is, if one were to conduct a survey among the members of those communities asking “what do the geologists (or physicists ,chemists, astronomers, paleontologists) believe about the Earth’s age?” one would not get a uniform answer. This situation lasted well into the third decade of the 20th century. During the 1920s, as a result of work by Arthur C. Lane and his NRC committee, a new stable configuration of community knowledge emerged and, symbolically archived jurisdiction of the question of geological time with the publication of NRC Bulletin no. 80 in 1931.

The main conclusion of my study is that the inadequacies in the earlier historiography result from inadequate understanding of group knowledge and consensus. The solution I propose, presented in the simplest terms, is that we need to view the historical actor as composed of dialogically interacting multiple subjects: the individual who strives to understand and multiple community members who strive to communicate. Furthermore, I introduced the concept of an imagined community member—the image that an interlocutor has of an anonymous member of a given community, and most importantly its knowledge, assumptions, and expectations. I have argued that viewing the discussion of the Earth’s age as a disciplinary conflict does not match the historical record, although the discussion of the Earth’s age starting from the 1890s was often presented by historical actors in disciplinary terms. However, such discussion was usually contained in introductions and conclusions of addresses before scientific societies and other wide audiences such as the Walcott address of 1893. Statements from those sources should not be taken as directly representing the situation as the actors perceived it, but rather as the actors speaking as community members addressing the imagined community member. To search for the individual understanding, we must look beyond those quotable introductions of presidential addresses and look inside them. We must look at the connections between the different utterances of the same individual, like King’s short article on the Earth’s age and his Catastrophism talk from two decades earlier. Sometimes, individuals share their individual understanding in print more readily, like McGee who plainly described his own philosophy of science and we can see how his utterances on the Earth's age fit into his broader comprehension of the world. But, as I have argued, such open sharing of understanding runs counter to the goal of science which is creation of community knowledge and socializing individuals to speak as community members, not as individuals. I have shown how expressions of this community knowledge are disjointed from the beliefs of the individuals, for example, by noticing the myriad of meanings attached by various scientists to Walcott's paper while it functioned as community knowledge during the 1890s. I have also shown how fundamental community knowledge is to the possibility of scientific work. First, without shared community knowledge, the dialogue splinters into non-interacting voices, as it did during the 1910s and early 1920s after Becker's failed attempt to bring a new consensus. Second, the work of Lane in his committee, which succeeded by establishing community knowledge, was required before the research program of mid century could produce the answer accepted to this day. The value of canonical texts, like those of Walcott and Bulletin No. 80 can also be seen as a testament to importance of community knowledge.

Though my analysis started with the goal of understanding the relationship of the disciplinary narrative to the historical event, I challenged earlier understandings of the dialogues the Earth's age in other ways. The advent of radiometric methods of determining the age of geological samples did not immediately solve the problems of the Earth's age, as is sometimes reported. The period between 1910 and 1925 was identified by disciplinary division much more strongly than the preceding period as evidenced by the separate dialogues existing within the geological, chemical, radioactive, astronomical and paleontological communities. In my analysis, I also tried to highlight the role of chemistry which has been under-represented in current historiography. During the early 20th century, chemistry was just as significant to the Earth's age debates as physics and geology and more significant than astronomy and paleontology. Final, I have recognized the significance of Joseph Barrell and Alfred Lane to the dialogues on the Earth's age. Though Barrell's 1917 article is sometimes mentioned by historians, his significance in showing to American geologists how the billion-year-old Earth was compatible with geological knowledge is not stated. Alfred Lane is hardly ever acknowledged, yet I show how instrumental he was to building the community of researchers of geological time and how widely he was recognized by his peers for doing so.

The broader claim which I make is the importance of asking the question "What do we mean by a group of people knowing something?" Asking this question can help us to untangle the historical record which otherwise is difficult to make sense of. I have argued for a particular answer to this question, that we can analyze historical discourse in terms of categories of individual understanding and community knowledge. The questions remains whether the historical episode which I have described was unique and whether the divisions into individual understanding and community knowledge will be applicable to other areas.

There are a number of observations that seem relevant. Throughout the entire period covered by this dissertation, there were two groups of individuals who were pushing for the discussion of the question of the Earth's age: the non-scientists who were interested in the questions of the Earth's age and a few isolated scientists who were investigating the question. There was not a community of scientists investigating the question until the 1920s. The question "How old is the Earth?" was on the one hand very easily understood by anyone, on the other hand it had multiple meanings for the various individuals involved. It would be deceptively simple to compare the numerical answers to the question provided by various individuals with each other. Just as the question had multiple meanings, so too, behind the numbers given in answers lay various meanings, like the initial calculations of the cooling globe versus the calculation of the time required for the deposition of the geological column. Yet other numbers represented things like the age of the oceans, or the time required for evolution of life to take place. Finally, even the number given as representing the geological age of the Earth needs to analyzed as either a calculation of the duration of time since the beginning of the Cambrian (Phanerozoic eon) or a calculations of the total age of the Earth.

The wide appeal of the question and the seeming commensurability of the answers made it possible for a dialogue to take place among the various individuals investigating the Earth's age. This dialogue likely would not have taken place if the individuals had not described their research as research on the Earth's age and had not expressed their answers in a number of years. That is, if Thomson had written about a model of a cooling sphere and if Walcott had written about the reconstruction of history of rates of denudation, then it is possible that the connections between the two would not have been made.

What seems unique about the question of the Earth's age is that it could be investigated by a number of individuals drawing on different sets of knowledge and that the individuals could express their findings in a way that made it seem as if they were discussing the same problem.

The conditions that seem to be necessary for emergence of community knowledge start with the separation of the private and the public spheres for discussion. The public sphere necessary for community knowledge to emerge has to be composed of individuals who do not judge themselves as they would in an interaction of individuals. That is, there has to be an assumption that one's simply being a member of the group gives another member of the same group sufficient information about how to address that individual. Community knowledge as described here seems to be dependent on specialization of knowledge. That is, not all members of the community are equally expert on the different aspects of knowledge. There has to be an idea of continuity of knowledge claims and that one adds onto the previous knowledge rather than creating understanding de novo. Finally, a set of technologies make community knowledge possible. Those are communication, computation, and storage technologies. The communication technologies overcome the locality of the utterance in two ways. First, by being written or otherwise inscribed the utterance may be transported to a different locality. Second, inscription fixes the record of the communications, that is, it enforces a uniform response. Individual understanding could take place without communication although, of course, one is always a community member, if by nothing else than by the language one speaks. The computation technologies are the ones that allow distributed manipulation of knowledge claims. This is required for knowledge claims to build on each other, but it also specifies the way in which knowing the world is possible. An individual depends on the cognitive abilities of the human body, an ability which differs from individual to individual. The storage technologies make possible both passage of knowledge claims through time and extending the scope of materials that can be computed. Many technologies used in modern science serve several of these functions. I suspect that things like the scientific article, the map, logic, a diagram, ideas of objectivity could all be analyzed as such technologies. In the end, all the aspects of the dialogues on the Earth's age were historically contingent; however, the most essential ones seem to be ones that are common to modern science more broadly.

Various aspects of scientific knowledge have been investigated by historians: its production, its role in society, its claims to authority. What I propose is an investigation of scientific knowledge from the perspective of the aims of comprehension of the individual and the knowledge community. When are the two goals overlapping, when do they differ, what are the consequences both for individual and for the community? How does this scientific knowledge, which privileges communicability, compare to the other ways of comprehending the world such as folkways, religion, art, or mysticism?

This study suggests that the knowledge which can be communicated cannot satisfy one's desire to understand and that one's individual understanding cannot be communicated. The communicable knowledge serves the needs of a community which exists in multiple places through time. The rise of knowledge communities and technologies associated with them has made possible knowledge systems which are inaccessible in full to the individual, yet which require individuals to sustain them. The tension between the individual and community is not that the community is against the individual; rather, the individual must be a member of some community (and in the modern world multiple communities). The tension is between attempting to limit oneself to only knowing, or to trying to communicate understanding. What we should realize is that both are needed and there are limits of both. Thankfully, the modern world, despite its pretensions, has never been disenchanted.[7] There is no need to re-enchant it, just to recognize that each utterance is not only from a fellow community member but also from an individual (uttering at a specific time, from a specific place); it is alright to allow this inaccessible individuality of each utterance to be misunderstood.

[7] Joshua Landy &Michael Saler (ed.), *The Re-Enchantment of the World: Secular Magic in a Rational Age* (Stanford, Cal.: Stanford University Press, 2009); Wiktor Stoczkowski, *Des hommes, des dieux et des extraterrestres : ethnologie d'une croyance moderne* (Paris: Flammarion, 1999).

List of Abbreviations

Below is the list of abbreviations for cited archival sources.

APS—Archives, American Philosophical Society. American Philosophical Society, Philadelphia, PA.

CSP—Charles Schuchert Papers. Manuscript Group 435. Manuscripts and Archives, Yale University Library, New Haven, Conn.

GFB—George Ferdinand Becker Papers. Manuscript Division, Library of Congress, Washington, D.C.

NRC-CPF—Geology and Geography: Committee on Measurement of Geological Time by Atomic Disintegration: 1924–1929. Central Policy Files, 1924–1931. National Academies of Science–National Research Council Archives, Washington, D.C.

NRC-DPS—Committee on Physics of the Earth: 1927–1938. Division of Physical Science, 1919–1939. National Academies of Science–National Research Council Archives, Washington, D.C.

WDM—William Diller Matthew Papers. Vertebrate Paleontology Archives, American Museum of Natural History, New York

WJM—W. J. McGee Papers, Manuscript Division, Library of Congress, Washington, D.C.

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