

Night Thoughts

George Gaylord Simpson's Reflections on Leaving the
American Museum of Natural History



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Reflections on Leaving the
American Museum of Natural History

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occasional **papers**

Department of Science and Technology Studies

STS Occasional Papers number 7
UCL Department of Science and Technology Studies (STS)
Gower Street, London WC1E 6BT UK
www.ucl.ac.uk/sts/occasional-papers

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ISBN: 978-1-78751-000-5

A CIP Record for this book is available from the British Library.

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Frontispiece image is George Gaylord Simpson in the American Museum of Natural History “at old desk”. Used with permission of the American Philosophical Society Library.

Cover image is George Gaylord Simpson portrait, 1956. Used with permission of the American Philosophical Society Library.

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Forward

Archives always deliver fascinating new things. When I was a summer intern at the American Philosophical Society Library in 1988, as a Master's student, I had the task of sifting and sorting the papers of George Gaylord Simpson. The correspondence was enormous. There also were large collections of manuscripts, photographs, and other artefacts of a distinguished life. Every box - every folder - yielded new discoveries. I knew almost nothing and I tried to soak everything in via pencil and notebook. At the end of each night, I went through my notes again and again so I wouldn't forget.

One item I've never forgotten is the focus of this *STS Occasional Paper*. Simpson's "Night Thoughts" manuscript was written in 1959 on the first day after he ended his employment at the American Museum of Natural History. His time at that museum was definitive: it didn't just form part of his career; at that point in 1959, the Museum was his career. And Simpson was both loyal to it and extractive of its opportunities. But here on the page, one night, he set down his thoughts about the then-current state of affairs. It is visceral. It is harsh. It seeps with pain and heartache.

I've never forgotten first seeing this manuscript. I don't think I'll ever forget its tone and importance to that person on that one day in their life.

So, thanks to my own department, UCL Department of Science and Technology Studies (STS). Over the years, we've produced a series of occasional papers. The series is intended to offer relatively quick, relatively easy dissemination of materials, ideas, and conversations. This is an ideal format for such a manuscript. Scientists tend to treat documents such as this as too "gossipy" to merit publication. Historians, who routinely use manuscripts such as this, tend to want only the interpretation and analysis. Transcription – and the skill in transcribing – tends to live in our profession's interstitial spaces. My own training as a historian was to prefer the empirical to high-order interpretation. I find joy exploring those interstitial spaces, and this project is an object lesson in why we must value such explorations.



Figure 1: "About to take off," Simpson with colleagues at Cruzeiro do Sul on Juruá River in western Brazil. Simpson in centre, handwritten caption on back reads "GGS and friends (Quirino and Raimunda Ruda)". Courtesy American Philosophical Society Library, Simpson Papers, box "Patagonia, Brazil", folder "Brazil 1956".

Introduction

George Gaylord Simpson (1902-1984) suffered an accident in 1956 that nearly killed him. He was co-leading an expedition on the Juruá River in the Amazon Basin, western Brazil. Their goal was to understand the region's Miocene-Pleistocene geology.¹ Three months into its fieldwork, the expedition was returning down river toward Cruzeiro do Sul. Their usual evening routine

¹ For the expedition's plan, see Simpson (1978, pp. 166-170) and a description in *Society of Vertebrate Paleontology News Bulletin* (hereafter *SVPNB*) June 1956, 47:9. Simpson sent letters to his family while in the field, see Laporte (1987, pp. 288-290). This was not Simpson's first tour across Brazil; he spent more than half a year during 1954-1955 studying museum materials, lecturing, and collecting, see Simpson (1978, pp. 159-166). The value of this trip was queried by Simpson's colleague, Norman Newell, who proposed far more accessible locations in the region, but those were tied to patrons in the oil industry, which Simpson was disinclined to rely upon (Laporte 2000: 248-249).

was to tie-up near a patch of high ground, then assistants would clear vegetation to create a campsite. On the evening of 24 August 1956, Simpson was struck by a felled tree. He was knocked unconscious, suffered a concussion, dislocated his shoulder and ankle, and suffered a badly crushed leg. These were catastrophic injuries.

Colleagues rushed Simpson to Cruzeiro do Sul for urgent medical attention. They cared for him around the clock.² Stabilized in Cruzeiro do Sul, Simpson was flown to Manaus and evacuated to New York City. The travel alone took nearly a week.

Simpson's recovery was slow and difficult. At various times, he was confined to bed, wheelchair, and full-leg cast. Ultimately, there were twelve operations on his leg, each separated by several months for healing and evaluation.³ In his 1978 autobiography, Simpson pointed to this accident as having "changed my life radically," directly linking it to being "removed" as chairman of the Department of Geology and Palaeontology at the American Museum of Natural History (AMNH) in June 1958, and precipitating a confrontation that led to his leaving the institution entirely, "in sorrow and in anger".⁴ The circumstances

² Simpson credited his AMNH colleague, George Whitaker, with saving his life. Whitaker (1956) provided an account of the accident.

³ Simpson's recovery was followed in the *SVPNB* February 1957, 49:10; June 1957, 50:6 October 1957, 51:7 February 1958, 52:12-13; June 1958, 53:8-9. For letters during his recovery period, see Laporte (1987, pp. 290-293).

⁴ Simpson (1978: 170, 173). Also described in Simpson to Martha, 8 May 1959, in Laporte (1987: 298). Announcements of Simpson's

surrounding Simpson's departure from AMNH have been the subject of disagreement and speculation. In his autobiography, Simpson (1978) only mentioned his resignation as a fact and indicated his departure was acrimonious. Acknowledging Simpson's resignation took place "in a huff," his biographer, Leo Laporte, (1987, p. 286) complained none of those involved ever explained the incident publicly. Laporte's (2000) later excellent reconstruction, based on interviews, gave voice to numerous colleagues. Curiously, it omitted much of Simpson's view of events. None of Laporte's writings on Simpson refer to the "Night Thoughts" manuscript, crucial as it is for understanding this moment in Simpson's biography.

This paper contributes to the subject of Simpson's resignation from AMNH by transcribing a previously unpublished manuscript written by Simpson at the moment of his departure. This manuscript was preserved by Simpson in his professional archives, and the original is available to scholars at the American Philosophical Society Library. This paper contributes to our understanding of this moment of heated action. It presents Simpson's own argument for his departure. More important for the historian, it reveals aspects of Simpson's professional and personal mentality: how he understood events taking place around him. This gives much-needed dimension to a figure so important to American evolutionary studies in the twentieth century. This *STS Occasional Paper* consists of a transcription of Simpson's thirteen-page handwritten "Night Thoughts" manuscript, together with a context-setting introduction by Cain and supporting notes. Simpson was scathing about several of his

AMNH colleagues. When this transcription project was first considered in the middle 1990s, one of these colleagues was still professionally active: Edwin Harris “Ned” Colbert (1905–2001). Correspondence between Cain and Colbert in 1993 led to Colbert producing something of a rebuttal, or clarification, to “Night Thoughts”. In fairness to Colbert, his commentary has been excerpted in an appendix to this project.

Who was Simpson?

At the time of the Brazil expedition, Simpson was a highly respected senior figure in vertebrate palaeontology.⁵ An expert on fossil mammals, he had authored hundreds of research publications, collected widely across the Americas, and worked through countless museums around the globe. Simpson was a leader of his discipline, receiving international praise as a synthesizer of knowledge on topics such as mammal taxonomy (Simpson 1945), evolutionary studies (Simpson 1944b, 1953c), biogeography (Simpson 1953a), and methods of analysis (Simpson and Roe 1939). Prolific, Simpson was also a public scientist with widening scope (Simpson 1949, 1953a) and a popular science writer with growing commercial reach (Simpson 1951, 1953b).

At AMNH in 1956, Simpson was chairman of the Department of Geology and Palaeontology, appointed in 1944. He started at AMNH in 1927, and he rose through the scientist ranks. Fiercely loyal to the institution, Simpson did not always rate highly AMNH’s management. In the early 1940s, he fought bitterly with

⁵ Whittington (1986) provides a detailed Simpson biography, plus a nearly exhaustive bibliography.

its newly appointed director, Albert Aide Parr (1900-1991). Parr sought to reorganise the institution to better reflect what he took to be contemporary trends in the life and natural sciences. He called it 'modernisation'. As part of this reorganisation, Parr proposed, in 1942, the abolition of the Department of Vertebrate Palaeontology. He said this would remove an artificial dichotomy between ancient and contemporary animals. Simpson and others objected vociferously, noting palaeontology's interdisciplinary connections with geology and biology.⁶

Difficulties surrounding this argument led Simpson to consider a change of institution, receiving an offer from Yale University as a Sterling Professor. Instead, in a flush of patriotism he rarely discussed later, Simpson enlisted into military service in 1942. Working in intelligence, he served as an officer in North Africa and Italy. Returning to AMNH in December 1944, Simpson found a much less antagonistic working environment. Rather than lose Simpson, Parr chose to reverse his plans, and Simpson made important gains during negotiation. His department was expanded - the Department of Geology and Palaeontology now included vertebrate and invertebrate palaeontology as well as mineralogy - and Simpson was named chairman.

It's fair to say AMNH managers and the director did not rate Simpson highly as a chairman. Simpson's style of management drew from the generation of palaeontologists preceding Simpson at the AMNH, such as the executive model of Henry Fairfield Osborn (Simpson 1944a, Rainger 1991) and the delegating model

⁶ Paleontology's interdisciplinary links with biology and geology have been studied elsewhere, such as University of Chicago (Rainger 1993).

of Roy Chapman Andrews (Gallenkamp 2000). Colleagues remembered Simpson as inattentive and hands-off. At times, he was described as negligent; at other times, simply absent or oblivious while leaving others to pick up the slack (Laporte 2000: 245-254). To be sure, Simpson had strategic plans and fought his department's corner. However, he never allowed chairmanship to interfere with his research progress or his expedition travel. Whatever his international prestige, Simpson proved an unimpressive local manager. This frustrated Museum management, especially Parr, and it built up pressure for confrontation as internal complaints about Simpson increased.

Crucially, museum management was changing during Parr's tenure as AMNH director (1942-1959). Departments lost autonomy to the central administration; meanwhile, chairs were expected to be on hand to actively administer their units and to actively participate in central activities rather than devoting their time and attention on research. From Parr's perspective, his trouble with Simpson had several sources. First, Simpson was regularly absent from AMNH on research or collecting trips. Simpson delegated most things to Edwin Colbert. However, Simpson did not hesitate to reverse decisions when he thought appropriate. Colbert complained about unfairness, carrying the load but having no real power. Parr found a confusing line of authority, and this hampered progress. Second, when he was present, Simpson was inattentive to operations, largely leaving colleagues to solve institutional and professional problems on their own. Third, Simpson continued pre-war strategic priorities grounded in collection, preparation, and curation towards systematics research and paleontological problem solving. Parr deemphasised expansion of the collections, deemphasised evolution and systematics research, and generally tried to move the museum away from its traditional position as a research centre

(Parr 1959, Kennedy 1968). Parr wanted AMNH to take a lead in science education, with modern exhibitions and innovative programmes that spoke to postwar curriculum debates. He was responding to shifts in funding patterns involving large museums (more government support; less private philanthropy) and to shifts in the expectations of patrons (more responsive research and more education). He wanted his scientific teams in residence, available to act, not foraging in obscure localities while sending home more and more crates of inventory.

Discussions with Simpson about stepping aside as chairman led nowhere. He simply would not resign the post. Simpson's desire to remain chairman rose from his personal understanding of the tacit professional hierarchy within the institution. Chair was "top dog", he argued. A chair was understood to be the most eminent person in the peer group. There can be no doubt Simpson thought himself to be that person. Likely, so did everyone else at AMNH. This signal of status and reputation plainly mattered to Simpson. Probably it mattered to him more as a signal externally than internally. Perhaps, Simpson took the view that, given Parr's emphasis, the only way he could preserve autonomy was as chair. When in a lower role, he could be bossed around, assigned to projects he had no interest in undertaking. As chair, he would set the agenda. In addition, he simply was not sympathetic to the transitions underway in AMNH management, strategy, or institution-wide processes. He continued to delegate and defer, believing he enjoyed autonomy at the strategic level; the rest would sort itself without him.⁷

⁷ Laporte (2000: 249-251) discusses concerns within the department over Simpson's managerial style, and he reports on some of the

Life changing injuries in the field

Simpson's accident was in 1956. Improving slowly, he returned officially to work part-time in 1957, and full-time in 1958. Maintaining the position as chair seemed important to Simpson's own sense of recovery: reporting for work and returning to service marked his improvement.⁸ That he had been the "victim" of an accident weighed heavily on Simpson. This was not his fault, and only a cruel person would kick a man when he was down.

A grudging Parr was willing to leave Simpson in place provided the department worked well enough. This compromise failed later in 1958 after Simpson told Parr he wished to accept an eight-month residence to work in Buenos Aires.⁹ Parr did not want to approve Simpson's leave, but any option to do so was lost when Colbert strongly protested and brought the leadership question to

disagreements among the staff in how to move the institution forward. His data comes from interviews with the staff. I have not examined institutional archives on this point. Neither Parr nor Colbert have unified archival collections to collate against Simpson's papers at the American Philosophical Society.

⁸ For example, see memorandum to Parr, et al from Simpson, 24 January 1958 (in Simpson Papers, folder "Parr, Albert") in which Simpson reports he is working at least half time for the Museum at this point and recovering slowly.

⁹ Simpson was an expert in the paleontology of Patagonia with several major expeditions to the region and substantial collecting experience (e.g., Simpson 1934). The request was made to his new chairman, Colbert, on 5 September 1958. Correspondence in Simpson Papers APS, series 1, folder: "Colbert, Edwin #1").

a head. Colbert's fury allowed Parr to box Simpson in: as a matter of best practice, chairs could not be long absent from the museum. A leave request would only be considered if Simpson resigned as department chair.

Though he complained about autonomy, Simpson finally accepted he had been cornered and resigned his department chairmanship in the summer of 1958. Colbert was appointed as his successor. There is no evidence that Simpson had yet made a decision to leave the museum. Instead, believing there had been a *quid pro quo*, Simpson immediately requested extended leave to take up the offer from Buenos Aires. This was made to Colbert, who passed it to Parr. Parr declined Simpson's request. He said Simpson "had long been absent" from AMNH, and he was needed in New York. Parr was not swayed when Simpson reminded him that his recent absence had been the result of his injuries. No doubt, Parr had taken a long view. Simply put, he wanted his staff in residence contributing to the institution's current mission.

Simpson was incandescent. He firmly believed he had been guaranteed autonomy in exchange for his resignation as chair, and now this guarantee meant nothing. He complained of bad faith on Parr's part. He spoke of collusion between Parr and Colbert. Though their relationships had been breaking down since Simpson's accident, Parr's decision to decline Simpson's request to take up a fellowship in Buenos Aires was the immediate provocation behind Simpson's decision to resign entirely from AMNH. That decision was made in September 1958.¹⁰

¹⁰ Later in September 1958, Simpson made a second request to absent himself from the museum, proposing travel to London to attend

Before informing the director of his decision, Simpson worked to arrange a suitable alternative. He wrote to some of his closest colleagues about prospects, and he asked them to make discreet enquiries. He considered returning to former offers, such as Yale. He also considered simply retiring and moving to his New Mexico summer home for a period without employment. Simpson need not have worried. An offer from Harvard quickly arrived.

Alfred Romer was director of the Museum of Comparative Zoology (MCZ). He was one of Simpson's lifelong friends and colleagues. He offered Simpson an Agassiz Professorship at the MCZ, with related appointments at Harvard University. Among his MCZ colleagues would be another of Simpson's closest professional colleagues, Bryan Patterson, and his former AMNH colleague Ernst Mayr.

The MCZ offered an exhilarating change.¹¹ "Everyone at Harvard is most cordial, too, and I actually can't(?) hardly wait to get cracking there," Simpson wrote after a preliminary visit.

300th anniversary celebrations of the Royal Society. He was still furious, and he did not resist the temptation to provoke. "I presume that there would be no serious objection from you or from the director if I planned to spend my vacation in Europe in 1960, but in view of our recent discussion of the inadvisability of absenting myself from the museum, I would like to be reassured on that point." (23 Sept 1958 Simpson to Colbert in Simpson Papers APS, series 1, folder: "Colbert, Edwin #1")

¹¹ Simpson to Martha, 8 May 1959, in Laporte (1987, p. 298). Announcements of Simpson's move are found in *SVPNB* June 1959, 56:6 and 8-9 October 1959, 57:5.

Simpson told his mother that this new job "pays well and has no duties except to write and do such research as I please!"¹²

Simpson was more frank with his sister. "I'm lucky to be getting [the] hell out," Simpson wrote (Laporte 1987: 297). Writing to his family later, Simpson elaborated. "It's a hard decision after 31 years (exactly, today, now I think about it)," however, the new job offered "More money and literally no duties - just to sit and think if so disposed, occasionally to say a kind word to students (but no teaching!) and other faculty. Free, too, to come & go as I please...."¹³

Simpson accepted the Harvard offer in November 1958, after visiting Boston. He resolved to leave AMNH at the end of the fiscal year (30 June 1959), and this quickly became generally known within the museum. He said the delay was so he could finish teaching commitments at Columbia University (Laporte 1987: 293-295); however, the real reason likely was associated with the formalities of his appointment at the MCZ.¹⁴ His

¹² Simpson to his mother, 27 November 1958, in Laporte (1987: 295).

¹³ Simpson to Martha, 1 Nov 1958, in Laporte (1987, p. 294). Parr rebutted in the museum's Annual Report 1958-1959 when announcing Simpson's resignation. Simpson's appointment was, Parr wrote, "a signal honour in the academic world" and it "will enable him to spend as much time as he chooses in research." (p.35)

¹⁴ Simpson had no fondness for teaching, (Laporte 1987: 295), so the classroom was not a major factor in his delay. Simpson returned to Columbia University in 1960 to deliver the Jesup Lectures for 1960. These lectures led to his book, *Principles of Animal Taxonomy* (Simpson 1961b). On Simpson, the Jesup Lectures, and the

formal resignation from AMNH was submitted only in April 1959. In his notice to Parr, Simpson commented on the drag his administrative duties had been on his attention to research. “Thirty-odd years of that is enough, and the Museum even with recent improvement in this respect does not really and adequately provide relief of its senior staff members from sheer routine.” He also described how his accident had curtailed future plans for expeditions. “...I feel strongly that the Museum should maintain an active field program in fossil mammals and I see little hope of sustaining this under present circumstances”.¹⁵

The decision to leave his situation at AMNH lifted Simpson’s spirits dramatically,¹⁶ and it ignited his research programme. During his convalescence, Simpson mostly produced book reviews and revised earlier work (Table 1). Perhaps the most original work done during his convalescence led to the edited volume *Behavior and Evolution* (Simpson and Roe 1958). With the MCZ move, Simpson vigorously returned to alpha-taxonomy, broadened the scope of his analytical projects, and restored his role as authoritative commentator about his specialties. He also made use of new opportunities, such as working with his wife and with Harvard geneticist Richard Lewontin to revise *Quantitative Zoology* (Simpson, Roe, and Lewontin 1960). The Cambridge academic environment also induced Simpson increasingly to

Columbia Biological Series, see Cain (2001).

¹⁵ Simpson to Parr, 17 April 1959, in Simpson Papers APS, folder: “Parr, Albert”.

¹⁶ Simpson to Martha 25 June 1959 in Laporte (1987: 300-301). More letters in early days at Harvard in Laporte (1987: 304-309).

reflect on subjects outside his particular expertise. For example, he engaged the extra-terrestrial life debate (Simpson 1964), and he wrote more substantially in history of science, especially on Darwin (Simpson 1961a).

Interpreting "Night Thoughts"

If the manuscript's introduction is accepted on face value, the purpose of "Night Thoughts" was documentation for later autobiographical work. Importantly, it was not sent to a typist for drafting. Moreover, there is no evidence it was circulated among friends; even Simpson's wife and closest confidant, Anne Roe, may not have seen it. Simpson had strong attachments to the AMNH, as is shown clearly in his post-final remark. As he penned "Night Thoughts" perhaps he was seeking closure to a central chapter of his life.

In "Night Thoughts," Simpson centralized three circumstances underlying his resignation. First, there were "changed conditions of work" at the Museum. In particular, he pointed to an intense antagonism between himself and museum director Albert Parr over the nature of Simpson's responsibilities in (as distinct from "for") the Museum. Parr wanted Simpson to be "at his desk" so that he could oversee department and curatorial operations and also be available to the administration. Parr believed Simpson was neglecting chair's responsibilities. When Parr insisted Simpson turn down the invitation to Argentina, Simpson believed he had lost professional autonomy completely. To him, this was unacceptable.

Exacerbating this feeling, Simpson continued, was a growing environment of "bad faith" surrounding his resignation from the chairmanship. As "Night Thoughts" describes, Simpson clearly felt cornered when Parr insisted he step down. Simpson believed

he would be exchanging his resignation for a return of his autonomy, only to find later that such was not the case. Indeed, more constraints seemed to be placed on him as the months passed in 1958. He saw his rapport slipping among the museum trustees, and Simpson must have sensed his reputational capital at an all-time low.

Intensifying the environment of bad faith was a personal hurt that Simpson felt when sympathy (or enough sympathy) from Parr failed to appear following his emergency return from Brazil in 1956. Repeatedly in “Night Thoughts,” and elsewhere, he criticized Parr for being insensitive and uncaring when it came to acknowledging the pain Simpson incurred “in the service of the Museum”. Parr made no social calls to the patient, Simpson wrote, neither did he seem to take a genuine concern in the patient’s progress. Simpson pointedly remembered it was Parr’s secretary who sent him flowers during his early hospitalization.

An “unsatisfactory departmental condition” was the third central circumstance identified in “Night Thoughts”. Fiercely independent, Simpson plainly admitted that there was considerable “strain” in having a new “top dog” in the department after June 1958. That the successor was his long time colleague, Edwin Colbert, perhaps made matters worse. An obvious choice, Colbert was second in rank within the department, and he routinely assumed administrative control while Simpson was away. While Simpson’s future was in question, Colbert’s future was bright. Beyond a growing research programme and being the Museum’s current top expert on fossil reptiles, dinosaurs, and amphibians, Colbert had been elected to the National Academy of Science (1957) and to the presidency of the Society for the Study

of Evolution (1957).¹⁷ He recently had completed a term as editor of the journal, *Evolution* (1949-1952), and he was planning a role as editor of the new museums journal, *Curator* (first issue 1959, published by the American Association of Museums). His extensive other activities, such as AMNH exhibitions and fieldwork as well as teaching at Columbia University, are described in his autobiography (Colbert 1980).

In "Night Thoughts," Simpson pondered collusion between Parr and Colbert in regards to his resignation. Simpson also clearly noted his suspicion a distinct change in patronage was underway in which he was being overlooked and Colbert sponsored. To make matters worse for Simpson, he believed it was through his own efforts that vertebrate palaeontology survived as a discrete entity at AMNH. In the year he stayed at the museum following his resignation as department chair, Simpson saw nothing but an erosion of his past successes, blaming Colbert for significant "concessions" to the Museum's administration.

"Night Thoughts" adds key information to Simpson's biography beyond its relevance to the Museum resignations. For example, Simpson describes his occasional negotiations with Yale University regarding his possible appointment as a Sterling Professor. How serious those plans were - especially in 1942 at the moment Simpson volunteered for military service, in part, to escape an earlier clash with Parr - previously has not been recognized except by Laporte (2000). Other comments about employment pepper Simpson's manuscript. Something also is written about the circumstances of Simpson being offered the Agassiz professorship at the Museum of Comparative Zoology,

¹⁷ Simpson had a backstage hand in each of these, he said.

contrasting the second-hand comments from Mayr (Laporte 2000).

“Night Thoughts” is a manuscript written from a particular point-of-view by a particular person at a particular point in their life. It must not be read as impartial or fair to others. Yet, the modern tendency to dismiss documents such as this with a label such as “bias” must be avoided, too. Readers have the opportunity to witness through these pages an intensely emotional moment in biography. It is a moment in which a person constructed a boundary between one part of their life and another. Perhaps, it offered closure. Perhaps, justification. It’s easy to imagine Simpson finishing the last page, closing the folder, then standing up from his desk thinking “it’s finished”.

Table 1: Simpson's publishing productivity (1951-1967)

This table shows the quantity and diversity of Simpson's publications between 1951 and 1967, including the year of his accident (1956) and his AMNH resignation (1959). The number of items published is counted from the bibliography in Whittington (1986). In parentheses is the number of journal pages in all items for the year. Books are listed as "bk". For instance, the data "5 (25+bk)" translates to "five items published in this category and year, producing 25 pages plus one book".

Columns:

- 1: year of publication
- 2: total number of items published in the year (n)
- 3: book reviews and memorials
- 4: alpha-taxonomy
- 5: beta- and gamma-taxonomy including synthesis of primary literature
- 6: commentary about field, news, extension of research, progress
- 7: commentaries in non-paleontological subjects

1	2	3	4	5	6	7
1951	9	4 (7)	1 (19)	1 (21 +bk)	2 (26)	0 (0)
1952	10	5 (34)	1 (12)	2 (25)	2 (5)	0 (0)
1953	17	8 (11)	0 (0)	3 (72+bk)	3 (4+bk)	1 (2)
1954	9	5 (9)	1 (4)	2 (23)	1 (3)	0 (0)
1955	5	2 (2)	3 (65)	0 (0)	0 (0)	0 (0)
1956	7	3 (7)	0 (0)	1 (28)	1 (2)	0 (0)
1957	9	5 (7)	2 (74)	1 (20)	0 (0)	0 (0+bk)
1958	13	11 (28)	0 (0)	2 (49)	0 (0)	0 (0+bk)
1959	22	7 (15)	5 (39)	4 (85)	4 (36)	2 (12)
1960	14	4 (13)	1 (1)	2 (76+bk)	6 (32)	0 (0)
1961	15	8 (21)	1 (5)	2 (46+bk)	2 (21)	2 (20)
1962	12	2 (3)	1 (55)	2 (57)	3 (28)	4 (13)
1963	11	6 (12)	2 (14)	0 (0)	0 (0)	3 (38)
1964	17	7 (13)	1 (21)	2 (17)	1 (31)	5 (25+bk)
1965	22	6 (9)	3 (6)	1 (7+bk)	0 (0)	10 (92+bk)
1966	15	6 (6)	0 (0)	3 (19)	0 (0)	6 (21)
1967	10	2 (2)	2 (37)	1 (259)	0 (0+b)	4 (40)

Editorial Notes

Written in Simpson's own hand, "Night Thoughts" is a 13-page manuscript included in the George Gaylord Simpson Papers (series #3, folder: "Night Thoughts...") at the American Philosophical Society Library. It is transcribed and published here with the Library's permission.

Explanation of editorial marks: The original text appears in regular type. Text deleted by Simpson is struck out. Text inserted by Simpson is set between ^- and -^ symbols. Text underlined by Simpson is underlined here. Cain's editorial comments are in [brackets]. The character "&" by Simpson is replaced here with "and" throughout. Page numbers mark the end of pages in the handwritten original, meaning [page 1] marks the end of Simpson's page 1.

"Night Thoughts on Leaving the American Museum"

[start of transcription]

1 July 1959

This is the first day since 1927 that I have not been employed by the American Museum of Natural History, and I am writing this memo for my own benefit, aware that distant memory has a way of reinterpreting events.

Although the situation has become far more complex, its outer form, at least, was first shaped by the loss of rapport between the then director of the Museum, Bert Parr, and me. In the first year and a half (more or less) after my injury (24 August 1956) on the Juruá [River], Bert hardly demonstrated even perfunctory interest in my survival or welfare. He once had his secretary send flowers to the hospital and once spoke to me there briefly by telephone. After my first stay at the hospital I occasionally got in to the Museum and on one of these occasions I made an appointment to

see the Director (he never sought me out until later and for other reasons, as below). We talked briefly in his office, and his only expression of concern was to remark on how well I was doing. (I was in a full-leg case, in a wheel chair, and in constant, intense pain.)

A first complication stemmed from the report of an AIBS-NSF committee on support for systematic zoology.¹⁸ Ernst Mayr was chairman and I was one of several other members. The report was drafted by Ernst, criticized by all of the other committee members, rewritten by Ernst, and signed by all of us not long before I went to Brazil in 1956. Among other things, this report recommended that federal grants for facilities in systematic zoology be given to museums through the NSF, a recommendation that was accepted and put into effect by the NSF. Bert objected very strenuously to this recommendation and action, chiefly on the grounds – which I consider ridiculous – that they favour inefficient, poor, ^-small-^ museums at the expense of the efficient, good, ^-large-^ ones (i.e., his), even though it was recommended that the latter have preferential treatment. Bert reacted with almost psychopathic violence, [page 2] making special trips to present his objections to the NSF, to other museum directors, and to all who would listen. In the summer of 1957 and through the winter of 1957-58 Bert wrote me rather voluminously about this, voicing extreme annoyance, insisting that as a non-administrator I had no right to an opinion on the matter,

¹⁸ Material relating to this report is located in Simpson Papers APS, series 1, folder: AIBS. This includes a 12-page memo from Parr to Simpson (1 April 1958) critical of the Committee's report.

but tending to "excuse" me as misled by Ernst, although I pointed out that I had read and in part revised the statement that I signed and that I did assume full responsibility for the opinions so obnoxious to Bert. In this connection (and no other) he also once visited me while I was in bed at home – the only such visit he made either to home or hospital throughout the period of my total disability.

In early 1958, while temporarily out of the hospital between operations but still in a cast, I several times visited the Museum with wheel chair and crutches. On one such occasion Bert came into my office and had a long talk urging me to resign as chairman of the Department. His main arguments were: (1) that changing conditions regarding exhibition, research support (including his NSF battle again!), and general administration were going to throw unusually heavy burdens on chairmen which I would ~~probably~~ not be competent to bear; and (2) that I was accustomed to spend much time ^-on Museum business-^ away from the Museum and that except for short (on the order of weeks) field trips and regular vacations this would no longer be permitted to chairmen but would be permitted to curators who were not chairmen.

As Bert later emphasized, he advised and urged me to resign as chairman and did not order me to do so. But my decision had to be based ~~on~~ mainly on these factors: (1) that the Director expressed the opinion that I was not capable of [page 3] performing the future duties of a chairman; (2) that as chairman I would have to end my field and foreign programs but was assured that I could continue them as nonchairman curator; and (3) that urgent "advice" from the man who in fact was empowered to

discharge me ^-(and who had in fact discharged curators who ignored his advice)-^ obviously had the force of an order. It therefore ~~seemed obvious~~ was plain to me that I did not ~~in fact~~ really have a choice, and ~~that if I [illegible]~~ and after consideration for a day or two I resigned as chairman. The resignation was immediately accepted, without expressed regrets or appreciation for past service, and Ned Colbert was appointed chairman.

In the Fall of 1958 I was well enough to return to regular duty at the Museum. In the meantime, I had received an invitation to spend about 9 months, starting in January 1960, in Argentina at Argentine expense, under conditions that would enable me to carry on the Museum's program there. On the basis of Bert's assurance that such absences would be all right if I were not chairman, I had tentatively accepted. On returning to the Museum I reported the matter to Bert. He said that he would not order me to refuse the invitation, but that he strongly advised me to do so. I reminded him of his previous assurance that such absences would be permitted if I resigned as chairman and that I had resigned on that basis. I suggested that the previous assurance had the [appearance?] of bad faith in the light of this development. His only reply was that he was sorry but that if he insisted on going he would permit this, but that in his opinion if I did so my position would probably be abolished! In other words, I was perfectly free to go, but I would be fired if I did!

As further evidence of bad faith, and of antagonism toward me personally, I pointed out that I had been told that the chairman could not be long absent from his [page 4] desk in New York, but that since becoming chairman Ned had been away with the director's approval, more than half the time and was about to go

to Brazil for a long period under circumstances similar to those of my proposed trip to ~~Brazil~~ Argentina. Bert simply insisted that the circumstances were not parallel, that I had long been absent, and that now for an indefinite period I must resign myself to staying strictly at my desk to compensate for this. I pointed out that I was being asked to compensate for an absence due entirely to my being injured in the service of the Museum. Bert reiterated (he went over and over his quite evasive [illegible strikeout] arguments: ^-He later admitted that he had been evasive, and said this was because he was acting as Aleck White's hatchet man. Actually the last straw in this situation was Bert's assuring me that I was extremely fortunate – I would never had to do another day's work! Just check in every day at the Museum until I could retire on pension. This disgusted me and made it impossible for me to stay at the Museum.-^ it did not matter why I had not been doing my work, the only pertinent fact was that I had not.

This attitude, the evident breach of faith, and the changed conditions of work under Ned as chairman all made me feel that I could no longer work for the Museum and I resolved to leave it definitely at the end of the ~~next~~ current fiscal year (30 June 1959). I started at once looking for another job [illegible strikeout], but in agreement with Anne, I decided to resign even if I had no job.

Months later, after I had told Bert that I was leaving and after Bert himself knew he was to be fired as director, he told me that orders to induce me to resign as chairman and to keep me ~~working at my desk~~ in New York had both come from Aleck White [Alexander M. White], the president of the Museum [from 1951]. He added that Aleck had hoped that some means would be found to remove me from the staff entirely not on any grounds of dissatisfaction with

past services but solely because (a) I was permanently crippled and so less useful, and (b) I had one of the two highest curatorial salaries (about [page 5] half Bert's salary) and could economically be replaced by a cheaper man. Bert said he had refused to consider removing me from the staff, but had gone along with the other orders from Aleck. I have no way of ~~judging~~ knowing whether these were indeed Aleck's orders. Quite possibly they were, because Aleck later did say to me that he felt it was best for both institutions if I left AMNH for Harvard. In any event, even by his own story Bert agreed about the chairmanship and the denial of freedom to work for the Museum anywhere but in New York, and he was responsible for the conditions that did in fact persuade me to leave the Museum altogether, and which he well knew would so persuade me.

Aleck's late action in removing Bert from the directorship, whatever its motives, was in its own way just such a ruthless and unilateral decision as had been applied to me through Bert. This also helps me to believe Bert's statement that the actions toward me were initiated by Aleck. (It is amusing that Bert came to me complaining of Aleck's treatment of him and asking my sympathy – without any evident ~~motive~~ awareness that there was a close parallel with the action toward me in which Bert had concurred!)

By this time I was definitely committed to going to Harvard, but I would not have stayed at AMNH even if I had known that Bert was to lose the directorship. The unfair and, as I still feel, heartless treatment ~~toward~~ of me had probably been initiated and ^-had-^ certainly ^-been-^ approved by the president and trustees, regardless of Bert's responsibility in the matter, and the, to me, unsatisfactory departmental situation would not be corrected by

having a new director. [page 6]

I must therefore briefly note the departmental situation. Ned came to the Museum a few years after me and is a few years younger. He was thus always in my shadow, and it is only human that this irked him. Our personal relationships were always cordial, but never warm. I also received certain honors that might have gone to Ned had he happened to be the older man and higher in rank. ^-(Yet I can't resist interpolating that when I began to receive such honors I was myself a quite junior staff member and not in any way Ned's administrative superior.)-^ I tried to compensate by pushing Ned both on the staff and elsewhere and seeing that he had all facilities for work and recognition of it, but this was not enough. (For instance, I arranged that Ned have the largest share of laboratory assistance and field funds. I had him rather than myself sent at Museum expense to foreign meetings; I engineered his election to the National Academy; when he was put out at my being invited to Brazil I arranged for a similar invitation to be given him – and so on through the years. Perhaps I even antagonized him by being too helpful, but he surely would not have had many advantages if I had not gotten them for him and would then have surely been still more antagonized and with better reason.)

In any event, Ned was evidently disgruntled and twice ^-(that I am aware of)-^ in the past five or six years he organized a sort of cabal in the Department when I was away or ill, with the openly avowed purpose of becoming chairman in my place. Both times he dropped the matter when I returned and took a firm stand. Of course I cannot really judge how justified the conviction was, but I did honestly believe the Department was better off under me, and I was sure that [page 7] I was better off – one cannot be positive

how pure one's motives are!

I have no ~~positive~~ concrete evidence, but with this background I feel an inner conviction that Ned was involved in the decisions and actions that led to his replacing me as chairman and finally to my leaving the staff altogether. Since Aleck was receptive to both ideas and Bert at least to the first, probably this would have happened in any case. By the same token, ~~they~~ Aleck and Bert would have welcomed and acted on any suggestions along this line overtly or implicitly made by Ned, and this is what I think actually happened. (Because I do not wish to embarrass anyone without useful result and because I am still most anxious to maintain "correct" relationships with Ned, I have not felt like making direct inquiries.)

It is inevitable that Ned should feel or ~~think~~ I should feel or, most likely, both ^-of us should feel-^ the reversal in our roles, Ned plainly made an effort to be fair and not unduly commanding toward me, and I sincerely tried to accommodate to his being my administrative superior. Wherever the feeling originated, however, there certainly was a strain in his now being top dog. Although I had always tried to be more than equitable in sharing assistance and facilities, I now found that my own participation in them was [illegible strikeout] reconsidered and sharply reduced. In the final ~~disagreements over freedom of action~~ developments [illegible strikout] that led to my leaving the staff, Ned told me privately that he felt I could and should be free to pursue my work as I thought best and, in the specific instance that brought this into the open, that I should go to Argentina in 1960, but when we discussed this together with the director, Ned told Bert that he agreed with [page 8] him, i.e. that I should not go. [Illegible strikeout]

It was also inevitable that Ned and I had different ideas about departmental policy. I had always felt that within the broad outlines of Museum welfare, my main duty was to the Department and that in dealing with the administration I should always press for the advantage of the Department as a whole and of each individual in it. Ned ~~had~~ has a more cooperative attitude toward the administration, to put it in one way, or ~~was~~ is more subservient, to put it in another. He is not inclined to object to or even to query any suggestions contrary to the interests of the Department and its members. Among his early administrative acts were agreements to transfer of a large special fund out of department control, to reduction in department personnel, to space reallocations that markedly reduced our facilities and raised acute problems of placing even our reduced staff, to a lower budget, and other almost crippling administrative demands. Perhaps I could not have avoided making these concessions ^- had I still been chairman-^, but I would ~~not have~~ have fought against them ^-and would almost surely (as often in the past) have saved something for us.-^ I could not help feeling that Ned was not inclined to fight for ^-(or even to think of)-^ his own – or those whom, in my opinion, he should have considered his own ^- as apart from himself-^. It was undoubtedly this greater docility that led the administration to prefer Ned as chairman – just as (although I have no admiration for the director's personal treatment of me) it was Bert's lack of docility that led to his discharge as director. I even prefer a man with bad principles to one who has no principles.

All these and other circumstances made me feel that ^-(regardless of who was director)-^ I could not long endure a situation in which decisions for the Department were made by

Ned and my ~~own decisions~~ ^-plans for my own work-^ had to be approved by him. [page 9]

Once my decision to go to Harvard was final, Ned's actions had no further bearing on it, but some of them did reassure me that the decision was wise. I will only mention a petty example: when he knew I was leaving, Ned had an inventory made of all that he considered Museum property in my rooms or control, having these listed without my ^-prior-^ knowledge by those working with me (George Whitaker, Mary Patsuris, and Eunice [Lugo??]), having a careful recheck made when I actually shipped my things to Harvard. Included on Ned's lists were a number of things that had been given to me ^-by individuals outside the Museum-^ for my personal use and even some I had purchased myself ^-with my own money-^. I of course left them all, and anything else to which I could not prove title. This did not amount to much, but the attitude gave me a sad and sour feeling after 30-odd years of trustworthy efforts for the Museum.

Now I shall go back and annotate the decision to go to Harvard, as distinct from the decision to leave the Museum, or in addition to that. When I left Yale in 1926 I had expected to return there in 1927. The tentatively promised Yale appointment was, however, delayed under circumstances that annoyed me, and before it was finally official, I accepted what then seemed only an about equal appointment at the American Museum. A few years later I was offered a better job at the Univ[ersity] of Chicago, but I was now so engrossed at AMNH that I did not seriously consider it. (AMNH did meet the modestly higher salary offered ^-considerably less than my secretary now gets-^, but I would have stayed anyway.) A later offer of a departmental chairmanship at Toronto was so

unattractive to me that it was never carried even [page 10] to a discussion of salary or other details.

When Parr first came to AMNH in 1942 one of his more precipitate first actions was to abolish the Dept. of Vertebrate Paleontology ^-(which had been the most productive in the Museum for [is] first 50 years)-^ out of hand, divorcing me from paleontological facilities and from those whom I considered my real colleagues and putting me under Harold Anthony, whom I did and do admire greatly as a man but not, I must confess, as an administrator or a scientist. At the same time I was offered a much better job at Yale (a Sterling Professorship) and I agreed that I would go there unless AMNH met what I felt were minimal requirements to give equal opportunity there. The important requirements were: (1) reconstitution of a department including at least all vertebrate paleontology (Bert himself broadened this, somewhat against my judgment, to include also invert[berate] pale[ontology] and mineralogy); (2) to make me chairman (which even then Bert was disinclined to do, but I was the senior staff member in the combined field and, rightly or wrongly, I felt that I could better insure a progressive program than the other possible candidates at the time); and (3) to meet the offered salary at Yale. This was bitterly opposed by Childs Frick on the Management Board (and indeed he resigned from that Board in consequence), but to my surprise the Board voted to accept my conditions – so much to my surprise that I had already made all arrangements to move to Yale. My family actually did so, as I had entered the army ^-while these negotiations were going on, and I-^ expected to return to New Haven, not to New York.

In fact I was so sure that Frick would carry the Board that in

making those demands I thought I was paving the way for reunion of vertebrate paleontology [page 11] under some other head (one of Ned's disappointments that help to understand and forgive later machinations) rather than with my own participation. Bert fought for the arrangement I insisted on. He still thoroughly disagreed with the idea of having a separate department for paleontology, but I convinced him that the Museum could not otherwise hold first class palaeontologists. Since he had gone to bat for me and my ideas, even against his own judgement, I could hardly do anything but stay to back him up although by then I would have preferred to go to Yale. This early, strong and [illegible strikeout] disinterested action by Bert won my great admiration, and it [illegible strikeout] somewhat compensates his later inhuman attitude toward me.

~~When~~ ^-Not long before-^ both Yale and I thought I would be going there, they ^-had-^ fired Ed Lewis and there was a rumor in the profession that he was fired so that I could go there. This was absolutely false. The decision to fire Ed was made on independent grounds entirely, and I had nothing to do with it or [had] any knowledge whatever of it until after the action was complete. Nevertheless this influenced me when the next move was made. Joe Gregory was hired at Yale in Ed Lewis's place, and when the tenure-or-out time arrived for him I was told that he would not be given tenure, and ^-I was-^ again invited to go to Yale on any realistic terms I wanted. Again the rumor arose that I was forcing out a younger colleague, and this time I know that the ~~feeling if not~~ ^-rumor was started-^ ~~was at least intensified~~ and spread by Ned Colbert. He became very emotional about Joe, and demanded that I do what I could to keep him at Yale. (Again, poor Ned was in an emotionally ambivalent situation, because he certainly felt he would be better off if I left AMNH and yet [page

12] felt that if I did so in this way I would be damaging a colleague whom he valued more highly.)

Again I confess that I was sorely tempted, but the plain fact was that if I agreed to go to Yale Joe would be out through no fault of his. I could not do it. I wrote that I could not accept an arrangement in which I seemed to be replacing Joe, and that if they did not give Joe tenure Yale's reputation would suffer among palaeontologists. I never received a reply or acknowledgement of my letter, but they gave Joe tenure. It is ironic that he voluntarily left not long thereafter.

When in the fall of 1958 I definitely decided to leave AMNH I of course hoped to obtain suitable work elsewhere. I had ~~tentative~~ offers of just one year each at three institutions ^-([unreadable] at Palo Alto, Inst. for Adv. Studies at Princeton, and UCLA)-^ and had tentatively accepted the first of them with the idea that I could thus at least gain time without abandoning my profession. Yale was not among them, and under the circumstances I felt I could not approach Yale or expect anything there. I discussed the whole situation frankly with Al Romer, with the idea that he could more tactfully than I make it known among the profession that I was available. Perhaps I was naïve, but I honestly had no thought of getting a job with Al, himself, especially as Bryan Patterson, whose field is almost exactly the same as mine, had gone to Harvard not long before (and ~~in large part on~~ ^-with-^ my recommendation). I was truly astonished a few weeks later when Al offered me an Agassiz Professorship, with all proper assurance that this did not in any way jeopardise Pat's position or anyone else's. Of course I accepted with pleasure and gratitude. [page 13]

Finally, I should note with emphasis that I did not go to Harvard either as a mere escape from AMNH or faute de mieux. I would have left AMNH in any case, even without another job in sight. It was just good luck, and friendly help, that I did [illegible strikeout] so soon have a job in sight and that the job was irresistibly attractive in its own right. Thus for general consumption I could quite honestly state the truth: that I was leaving to accept a job more attractive to me and one which AMNH could not match.

Postfinally – I have no resentment against AMNH or any individuals there. I love the institution to which I have devoted nearly 32 years – ~~most~~ ^-practically all-^ of my adult life – and I hope it will prosper and continue its great work, as I am sure it will. ~~In the long run~~ Its life does not depend on any ^-one-^ individual. I do feel that several people there came in the end to treat me without human understanding and even with some bad faith and unfairness, but in the long run they cannot harm the institution ~~fatally~~. Indeed ~~in other ways~~ even they have greatly helped the Museum, which they plainly love as much as I do. If, as I feel, their treatment of me has been unjustified, the fault is in ~~their own weaknesses and which~~ such weaknesses as we all have, and in their characters and mine. As Don Quixote (or was it Sancho Panza?) said, “We are all as God made us, and some of us even worse.”

[end of transcription]

Postscript: Colbert's reply

It must be remembered “Night Thoughts” reflects Simpson’s individual impressions of his institutional circumstances, and they reflect only one perspective in this period in the history of AMNH and the personalities involved. Further scholarship will add more. In 1993, Cain shared Simpson’s “Night Thoughts” manuscript with Colbert. As one of Simpson’s closest colleagues at AMNH and someone named in “Night Thoughts,” Colbert was asked for his comments.

Colbert said he had not previously seen this manuscript. He said “Night Thoughts” badly misrepresented his own actions and intentions, suggesting the manuscript was “put down in the immediate heat of [Simpson’s] anger and frustration”. It did not “reflect justice on his memory” (27 July 1993 Colbert to Cain). In fairness, Colbert previously chose not to comment on Simpson’s departure (e.g., Colbert 1980: 129 simply notes it as a matter of fact), and so he deserves the opportunity to reply. His substantive comments are provided here. Colbert sent his initial reflections in July 1993. Comments in a more formal voice came in September 1993. Colbert asked for these to be included in the archives folder

where Simpson's manuscript is preserved. The original correspondence is held by Cain; copies have been donated by Cain to the Library for cross-referencing purposes.

Parr died in 1991. He did not know of Simpson's "Night Thoughts". His substantial reactions to Simpson's departure are not recorded.

Excerpt 1: Colbert to Cain, 27 July 1993

[start of transcription]

I hope I never was the ungrateful wretch that he depicts. Indeed I feel pretty sure that in our early years at the Museum we were good friends. His negative feelings toward me were largely, I think, the result of the chairmanship "problem." I fully appreciated all he did to help me, and I hope he might have remembered my help to him. Such as running the department in his absence (always with advice from him when possible) taking over his Columbia classes, helping to put some of his publications through the press, staying with him at his apartment when his wife was away, and so on.

Those of his charges that are very serious simply are not true. I never organized a "cabal" against him in order to become Chairman of the Department. I did not indulge in "machinations" for which he was willing to "forgive" me. Back-stairs shenanigans have never been my style. Nor did I ever spread rumours concerning his negotiations with Yale. These were obviously figments of his imagination, based upon I don't know what. Perhaps on nothing.

I never did lust for the Chairmanship of the Department – and he admits he has no concrete evidence that I did.

In fact, when George came back to the Museum after his long hospitalization, I urged the director to let me step aside as acting Chairman, so that George could resume these duties. I was denied – the Chairmanship was thrust upon me.

I tried to do my best, and it was very embarrassing to have to

assume a position above George. I admit that I was not as forceful as he was: I was never a table-thumper, but I don't think I was as subservient as he imagined. I went to the mat on occasions with the Director and even with the President of the Trustees, but we managed to stay friendly.

Two small points.

I was not "put out" as he assumes by his invitation to Brazil.

And I am completely puzzled by his tale of my having had an inventory made of objects in his office. I have no recollection of such a thing. Perhaps some listing was made in connection with his move to Harvard, which George misinterpreted.

Well, I am sorry to have written such a long letter. But I feel I should put my side of the case before you. And I think the case boils down essentially to George's resentment at having been requested to relinquish the Department Chairmanship.

[end of transcription]

Excerpt 2: Colbert to Cain, 21 September 1993

[start of transcription]

When George Simpson returned to the American Museum, after a long absence – the result of being struck down by a falling tree in the Brazilian jungle, and the consequent hospitalization that assured his partial recovery from the accident, I had been serving at the request of the Director of the Museum, Dr. A. E. Parr, for the better part of two years as Acting Chairman of the Department of Vertebrate Paleontology.

At the time I was quite willing to step aside from departmental administration and resume my former full-time role as Curator. I made this clear to the Director of the Museum, as well as to George Simpson. But the Director informed me that I could not do this; the Museum Administration had firmly decided that I should be the Department Chairman.

I told the Director that Dr. Simpson would not like this arrangement, and of course Simpson did not. Nor did I; it was awkward and embarrassing for me.

The Director assured me that he would make it very clear to Dr. Simpson that the transfer of the Chairmanship to me was entirely an Administration decision, and that I had not part in it. Evidently, he did not make the matter clear to Simpson, or perhaps Simpson did not believe what he was told.

However that may be, Dr. Simpson convinced himself that I was part of a plot to undermine him. Nothing could be farther from the truth.

I therefore state upon my word of honor that:

Transcription

- (1) I never organized a “cabal” in the Department to promote myself as Chairman.
- (2) I was never involved in the decision, made by the Administration, to transfer Chairmanship of the Department from Dr. Simpson to myself.
- (3) I never made any suggestion for such a transfer to the Administration.
- (4) I never indulged in so-called “machinations” on my own behalf.
- (5) I never started or promoted a rumor that Joseph Gregory was being forced out at Yale to make a place for Dr. Simpson.
- (6) I was not “irked” by being in Simpson’s “shadow.” From my earliest days at the American Museum I accepted George Simpson’s scientific eminence as a fact of life.”

Further than this there is nothing I can say. Those who read “Night Thoughts” and my statement will have to believe either the one or the other of us, so far as my actions are concerned. Unfortunately George Simpson and Albert Parr are dead; their views and their actions in this unnecessarily sad affair must depend on interpretations made by the historians of vertebrate paleontology.

[end of transcription]

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