BIRDS, BATS AND MINDS
TALES OF A REVOLUTIONARY SCIENTIST
DONALD R. GRIFFIN

BY CAROLYN A. RISTAU

VOL. 3
Birds, Bats and Minds: Tales of a Revolutionary Scientist, Donald R. Griffin

VOLUME THREE of THREE

Carolyn A. Ristau
ACKNOWLEDGMENTS

With heartfelt gratitude, I thank the many who have helped in this enterprise. I have interviewed approximately 40 persons, many by telephone and a few by Zoom or in person and/or email, which has always been a most enjoyable and enlightening experience for me. My interviewees typically seemed to enjoy the interchange as well and recalled many significant and pleasant times with Donald Griffin. Most reviewed and corrected sections of the book about their work and interactions with Don.

For their interviews and correspondence, I specifically thank the following: Gregory Auger, Colin Beer, Marc Bekoff, Jack W. Bradbury, Gordon Burghardt, Julia Chase-Brand, M. Brock Fenton, Owen Floody, Carol G. and James L. Gould, Ann Graybiel, Beverly Greenspan, Alan C. Grinnell, Sharon and Vasken Hagopian, Carl D. Hopkins, Ronald P. Larkin, Irina Mikhalevich, Katherine Boynton Payne, Irene M. Pepperberg, David Pye, Arthur Reber, Paul Rozin, Hans Ulrich Schnitzler, James C. Simmons, Gayle Speck, Alan Steinbach, Eric Strauss, José and Susi Torrê-Bueno, Peter Tyack, Charles Walcott, Peter Waser, Elizabeth (Betsy) Weaver, Timothy C. Williams, Wendy Williams, Yossi Yovel and two persons who wish to remain anonymous.

Though not interviewees, others have also helped provide information and guide me to resources. These include Arthur N. Popper and David Lee.

In helping me deal with the philosophical issues, I particularly thank Colin Beer and Irina Mikhalevich for their comments. Colin also sent me manuscripts and Irina led me to many contemporary references. Misinterpretations are mine.

Those who contributed photographs are sincerely thanked and acknowledged at their photographs and in the Photography Credits section. M. Brock Fenton diligently helped to identify bat researchers in photos.

I am grateful for the assistance of Don Griffin’s daughters, Janet Abbott and Margaret Griffin (Weldon) for sharing information and manuscripts and for commenting on some chapters. I likewise thank Gregory Auger, who shared photographs and memories of the many hours spent with Don studying bats and beavers during Don’s “retirement.” During these years, Don was living in Lexington and then Barnstable, Massachusetts and affiliated with Harvard University’s Concord Field Station.

The Columbia University Library allowed me access to their online library facilities which were essential to this project, and I am most grateful. I thank Andrew Rowan for inviting me to submit my book to the Well Being International Studies Repository.

For writing suggestions, though I may not have followed their good advice, I thank Catherine Loveland Aubin, Frans de Waal, Michael Prime, Patricia Stokes, Wendy Williams, and most particularly, Jack Bradbury and Tim Williams, who had the fortitude to read through drafts of the entire book and offer advice, corrections, anecdotes and information about research. They are both former Griffin graduate students and knew him well. Their suggestions were invaluable. Bernard Starr helped guide me through the complexities of self-publishing. Finally, I deeply thank my husband, William (Bill) D. Knight, who not only read through and commented upon much of the book, but showed me endless patience and encouragement as I spent countless days and weeks and years on this work and not engaged in the many other adventures we could have undertaken together.
PHOTOGRAPHIC CREDITS FOR VOLUME THREE


Cover
Front:
- Design by Christopher W. Robbins
- Top Left: Elephants bonding (by Nachiketha Sharma) Asian elephants (Elephas maximus)
- Top Right: Flying bat (by Nuwat) Adobe Stock File # 136172389 (Greater Short-nosed Fruit Bat, Cynopterus sphinx). Like most fruit bats, this species does not echolocate.
- Bottom Left: Migrating Canada Geese (by Dennis Donohue) Adobe Stock File # 308035291
- Bottom Right: Close-up: Honey bees on a comb (by Dmytro Smaglov) Adobe Stock File # 50392142

Back:

Chapter 16
- Neurophysiological Approaches, p. 332.
- Evolutionary Ecology of Thinking, p. 354.
- Comparative Approaches to Animal Thinking [Griffin is a member of this group], p. 374.
- Communication as Evidence of Thinking, p. 390.


Chapter 18
Part 2 - Bees

Part 4 - Beavers
- Swimming Beaver. (by Mirage3) From Deposit Photos: 54254085_L.jpg
- Don Griffin at a beaver lodge in Mink Pond. (by Gregory Auger)

Part 5 - Bats
- Trout Pond. (by Gregory Auger)
- Don at Trout Pond. (3 photographs by Gregory Auger)
- Trout Pond set-up. (by Gregory Auger)
- Bat Conservation International – Echolocation Symposium Attendees - Austin, Texas – April 2002. (by Gregory Auger) With thanks to M. Brock Fenton for researching the attendees’ identities.
- ... And at the BCI 2002 Conference: Don Griffin and Alan Grinnell, Don Griffin and Elizabeth Kalko (2 photos by Gregory Auger)

Chapter 19
- Don Griffin, “musing,” October 1999 (by Gregory Auger)
CITATION PERMISSIONS
(for Volumes One and/or Two and/or Three)


Chase-Brand, J. (in prep). Autobiographical ms. (Text excerpts)


Williams, J. M. (2014). Bats, birds, and boys: Thirty-five years in the lives of a family of field biologists. Harrison Publishing House. (Text excerpts)
CONTENTS - BRIEF  
(for three volumes)

VOLUME ONE

Acknowledgments  v  
Photographic Credits for Volume One  vi  
Citation Permissions  vii  
Contents – Brief (Three Volumes)  ix  
Contents – Detailed – Volume One  xi  
Introduction  1

PART ONE – EARLIEST TIMES  
(The Making of a Future Man: The Scientist, Donald R. Griffin)

Chapter One - Spirits of the Ancestors: The Redfields and the Griffins [Brief Version]  1-5  
Chapter Two - “Home”: A Reserved Village in Cape Cod [Brief Version]  2-11  
Chapter Three - Formative Years  3-17  
Chapter Four - Wandering in the Marsh  4-27  
Chapter Five - Early Writings: A Teenage Emerging Scientist  5-55  
Chapter Six - Young Men and Flying Bats  6-63

PART TWO – YEARS at CORNELL and HARVARD with BIRDS and BATS

Chapter Seven - Flying with the Birds  7-97  
Chapter Eight - Military Secrets  8-129  
Chapter Nine - A Flying Professor at Cornell: Early Years  9-141  
Chapter Ten - Fish-Catching Bats and Echolocating Birds? Later Years at Cornell  10-179  
Chapter Eleven - Bat (and Bird) Science at Harvard … And Afterwards  11-215  
Appendices - Volume One  
   Appendix - Chapter Two – [Full Version] “Home”: A Reserved Village In Cape Cod App-283  
References - Volume One  Ref-297

VOLUME TWO

Acknowledgments  v  
Photographic Credits for Volume Two  vi  
Citation Permissions  vii  
Contents – Brief (for three volumes)  viii  
Contents – Detailed – Volume Two  x

PART THREE – AT ROCKEFELLER UNIVERSITY: BEFORE THE REVOLUTION

Chapter Twelve - A Tropical Paradise, A Bucolic Field Station, and Early Years at Rockefeller University  12-3  
Chapter Thirteen - Afghani Art and Russian Bats  13-151
Chapter Fourteen - Behind The Man: Significant Women  14-161
Chapter Fifteen – Before the Cognitive Revolution (Griffin Lab Life And Research)  15-199
References – Volume Two  Ref-225

VOLUME THREE

Acknowledgments  v
Photographic Credits for Volume Three  vi
Citation Permissions  vii
Contents-Brief (for three volumes)  viii
Contents – Detailed – Volume Three  x

PART FOUR – REVOLUTIONS AND AFTER

Chapter Sixteen - Revolutions  16-3
Chapter Seventeen - Philosophical Issues – A Brief Foray  17-81
Chapter Eighteen - The Return: Bees, Beavers, Bats, Elephants … and Consciousness in Barnstable  18-103
Chapter Nineteen – A Life Well-Loved: A Broader View  19-177
Appendices  195
  1. Donald R. Griffin Timeline: Life, Honors and Major Projects  App #1-197
  2. Timeline: Griffin’s Students and Associates (Cornell, Harvard and Rockefeller Universities)  App #2-201
  4. Members of The Rockefeller University Animal Behavior Group (Donald Griffin, Peter Marler and Fernando Nottebohm Laboratories 1966 to 1991)  App #4-212
  5. Glossary of “Griffin Terms”  App #5-214
  6. Griffin's Publications  App #6-218
  7. Video, Photographic and Manuscript Collections Concerning Griffin  App #7-226
  8. Selected Memoirs about Donald R. Griffin – List  App #8-228
References – Volume Three  Ref-245
CONTENTS – DETAILED

[A selected list of subsections in chapters]

for VOLUME THREE of THREE

Acknowledgments v
Photographic Credits for Volume Three vi
Citation Permissions vii
Contents – Brief (for three volumes) viii
Contents – Detailed – Volume Three x

PART FOUR – REVOLUTIONS AND AFTER

CHAPTER SIXTEEN – REVOLUTIONS 16-3
CHAPTER SIXTEEN - PART 1 - CULTURAL AND SCIENTIFIC REVOLUTIONS (1960s and ‘70s) 16-3
The ‘60s Revolution - Musically, Culturally, Politically
The Popular Scene in the USA
The "Popular" Revolution at Rockefeller University
Women in Science - At RU and Beyond

Revolutions in Many Fields and Universities
Physics in Revolution
Biology in Revolution
Psychology in Revolution
The “Garcia Effect” (Vehemence in an Elegant Apartment)
Humankind: Unique and the Apex
Animals in the Field and the Lab
“Misbehaving Animals” Challenge the Laws of Learning
The “Garcia Effect” (Tasty Water or Noisy Water?)
“Garcia Effect” - A Small Result or a Major Heretical Challenge?

CHAPTER SIXTEEN - PART 2 - REVOLUTION IN ETHOLOGY AND THE BIRTH OF COGNITIVE ETHOLOGY 16-21
A Quieter Revolution Brews: Griffin: From Physiology to Thinking
The Path
Philosopher Thomas Nagel asks, “What is it Like to Be a Bat?”
Griffin’s Status and Critics

Critics’ Arguments and Griffin’s Responses
1) Behaviorism
2) Anthropomorphism
3) Inhibiting Assumptions
   A. Cartesian Views
   B. (Mis)Understanding Innate Behaviors
   C. Prejudice Against Lower Animals
   D. Parsimony Violated
   E. Science of the “Inaccessible”
CHAPTER SIXTEEN – PART 3 - EVIDENCE FOR ANIMAL CONSCIOUSNESS (BY GRIFFIN AND OTHERS)  16-39

Thinking Animals?
Definitions

Strategies for Gathering Evidence of Animal Consciousness

1) Possible Neural Correlates of Consciousness
2) Versatility in Meeting Novel Challenges ("Versatile Adaptability of Behavior to Changing Circumstances and Challenges")
   Finding Food
   Predation
   Constructing Artifacts
   Tools and Devices
   Ability to Learn Concepts
3) Deception and Manipulation
4) Natural Psychologists
5) Animal Communication – Artificial and Natural
   Artificial Language Projects
   Natural Animal Communication

Griffin's Reflections on Cognitive Ethology and Experimental Psychology

Griffin, the Scientific Communities and Animal Consciousness
Conferences: Spreading "Heresy" and Meeting Resistance
[describes major early conferences where Cognitive Ethology was introduced to diverse scientific audiences]

CHAPTER SEVENTEEN PHILOSOPHICAL ISSUES - A BRIEF FORAY  17-81

CHAPTER SEVENTEEN- PART 1- INTRODUCTION  17-81
1. A Brief View of Philosophical Interests in Animal Cognition
2. Cognitive Ethology Derives from Biology
3. Griffin's Strategies for Gathering Evidence of Animals' Mental Experiences

CHAPTER - SEVENTEEN- PART 2- CONSCIOUSNESS: THE NATURE OF ANIMAL MENTAL EXPERIENCES  17-85
1. Defining Consciousness
2. Jakob Von Uexküll’s “Umwelt”
3. Thomas Nagel’s View of Animal Consciousness

CHAPTER - SEVENTEEN - PART 3 - TOWARD A THEORY OF ANIMAL MIND  17-87
1. Functionalism and Neuroscience
2. Folk Psychology
   A. Thought and Language
   B. Theory-Theory and Simulationism
   C. Philosophical Intentionality Applied to Cognitive Ethology
      1) The philosophical term “Intentionality”
      2) Applying the Intentional Stance: Searle vs. Dennett
      3) Intention and Function
D. Other Philosophers’ Views of “Folk Psychology”
   1) How Aware is a Toad? / Levels of Animal Consciousness
   2) Philosophical Behaviorism
   3) The “Eliminativists”

CHAPTER SEVENTEEN - PART 4 - COGNITIVE ETHOLOGY PROCEEDS  17-95
1. Biases in Studying Animal Behavior
2. Further Developments in Understanding Animal Mind
   A. Advances in Neuroscience, but ...
   B. Attempts to Determine the Origins of Consciousness and Cognition, but ...
   C. Attempts to Bridge the Romantic/Killjoy Divide, but ...
3. Now and Next?

CHAPTER EIGHTEEN - THE RETURN: BEES, BEAVERS, BATS, ELEPHANTS ... AND CONSCIOUSNESS IN BARNSTABLE  18-103
CHAPTER EIGHTEEN - PART 1 – INTRODUCTION  18-103
CHAPTER EIGHTEEN - PART 2 – BEES (Apis mellifera)  18-105
1. At the “Sugar Cube” (approximately 1993-1999)
2. To Bees in Princeton, but ...
3. Bees Buzz Quietly (Near-field Acoustics)
   How Does the Honeybee Get Information from Another’s Dance?
   Wiggling Air Particles
   Hairy Bees and Antennae
   “Rube Griffinberg” Apparatus and Then Denmark
   A Dream: A Model Bee
   A Lone Endeavor  (Griffin and Bees at the Harvard Field Station)
   Science Advances
4. Finally, a Robotic Bee

CHAPTER EIGHTEEN - PART 3 - THE PONDS IN HIS LIFE  18-113
   Hinckley Pond
   Mink Pond (A Tale of Conflict and Beavers)
   Trout Pond (Sewage Attracts Mosquitoes Attracts Bats Attracts Scientists)

CHAPTER EIGHTEEN - PART 4 – BEAVERS (Castor canadensis)  18-116
   Beavers in the Night
   “Beloved Beavers”
   “Retiring” with Beavers
   Meeting an Emergency
   Griffin and His Band
   What’s Happening Inside a Beaver Lodge?
CHAPTER EIGHTEEN - PART 5 - BACK TO BATS  18-129
The Return
Into the Night with Bats
The Terminal Buzz
  2001
  2002
    Ever Modifying the Experimental Apparatus
    Presenting Their Research to NASBR
“What is Worth Doing in 2003?”
  “Buzzless” Bats?
  To Make an Insect
  Or “Experimental Echoes?”
2003 - The End of Field Seasons/ Last Conferences
  “Bat Conservation International Meetings” - Austin, Texas (April 2002)
  “Acoustical Society of America (ASA) Meeting” - Austin, Texas (November 2003)
Reflecting
Bat Research Continues

CHAPTER EIGHTEEN - PART 6 - TO BE AN ELEPHANT … AND OTHER RUMINATIONS  18-153
Ruminations and Areas of Future Research
“Philosopause”
Elephants’ Communication and Social Behavior as Evidence for Consciousness
How to do the Research?
  Example: How to Analyze the Calls of a Distressed Infant Elephant?
Endings and Changes
  Continuing Elephant Research and Conservation
  Reflections

CHAPTER EIGHTEEN - PART 7 - CONSCIOUSNESS and ETHICAL CONCERNS  18-159
Introduction- Griffin’s Views on Animal Consciousness - 2003
Post-Griffin: Contemporary Evidence Supporting Animal Consciousness
  Theory of Mind
    [research on great apes and ravens]
  Communication
  Neural Correlates of Consciousness (NCC)
  Very, Very Different Brains
    [research with octopuses, cuttlefish, a caterpillar and a moth]
Cognitive Ethology: A Science that Struggled, Ideas that Spread
Ethical Considerations and Conservation
Griffin, the Public and Animal Consciousness
Finally ...

CHAPTER NINETEEN – A LIFE WELL-LOVED: A BROADER VIEW  19-177

The Many Voices
1. CR: I asked my interviewees, “What sort of man was Don Griffin?”
2. CR: I asked my interviewees, “What motivated Don Griffin? What were his goals?”
3. CR: “What were his attributes as a scientist?”
4. CR: “What of Don Griffin as a Teacher, as a Mentor?”
5. CR: I also asked if Don Griffin had been an influence on each person’s life, and if so, how?
6. CR: I queried the “Many Voices” as to their view of Griffin’s major contributions.
7. CR: “What,” said I, to the “Many Voices,” “do you think was Don Griffin’s view of his own accomplishments?”
8. CR: And then, I pushed my patient interviewees yet more, “What of Don’s frustrations and regrets? Do you have any sense of those?”
10. CR: Evaluating Griffin’s Contributions to Cognitive Ethology

The Future Lies Before Us

APPENDICES     195
1. Donald R. Griffin Timeline: Life, Honors and Major Projects   App #1-197
2. Timeline: Griffin’s Students and Associates (Cornell, Harvard and Rockefeller Universities)   App #2-201
4. Members of The Rockefeller University Animal Behavior Group (Donald Griffin, Peter Marler and Fernando Nottebohm Laboratories 1966 to 1991)   App #4-212
5. Glossary of “Griffin Terms”   App #5-214
6. Griffin’s Publications   App #6-218
7. Video, Photographic and Manuscript Collections Concerning Griffin   App #7-226
8. Selected Memoirs about Donald R. Griffin – List   App #8-228

REFERENCES – VOLUME THREE     Ref-245
CHAPTER SIXTEEN

REVOLUTIONS

“Excessive caution can sometimes lead one as far astray as rash enthusiasm.”¹
(by Donald R. Griffin in Listening in the Dark, 1958)

[Chapter Sixteen contains three Parts: This file contains Parts One and Two]
Part One: Cultural and Scientific Revolutions (1960s and ‘70s)
Part Two: Revolution in Ethology and the Birth of Cognitive Ethology
Part Three: Evidence for Animal Consciousness (Griffin’s and Later)]

PART ONE

CULTURAL AND SCIENTIFIC REVOLUTIONS (1960s AND ‘70s)

Overview

The ’60s and ’70s see “revolutions” in many domains, witness the cultural, stylistic, and political changes underway in the USA generally and at The Rockefeller University.

Women in science still meet obstacles, but their role is changing; a study at RU reveals Griffin as one of the most positive in his support of women scientists.

The sciences, too, experience revolutions in thinking and discoveries: In physics, the “standard model” emerges in the ’70s and experimental physicists confirm it. Biology encounters Stephen Jay Gould’s concept of “punctuated evolution,” opposing/elaborating upon Darwin’s theory. Cellular and molecular biology gain even further dominance as “proper science.”

The cognitive revolution in Psychology attracts significant attention. Strict behaviorism is challenged and erodes. New interpretations emerge, many drawn from research at the University of Pennsylvania: Not any stimulus can be associated with any other. Robert Rescorla’s research reinterprets classical conditioning as a cognitive event. General laws of learning do not suffice; each species has its innate biases.

The ’60s Revolution - Musically, Culturally, Politically

The Popular Scene in the USA

The ’60s and ’70s ... there were revolutions in those times ... many. On a popular note, our music had changed. The Beatles and Rock and Roll dominated. A transformed kind of country music in the sounds of Joan Baez, Judy Collins, Odetta and Bob Dylan. Yes, like all events, it had roots—Black American, then white and black American jazz, before that the Negro spirituals and work songs, the folk songs of rural Appalachia. Our songs shouted for love and others of anger and change. “You say you want a revolution. Well ... We all want to change the world ...” sang the Beatles.² Our music made us dance. “If I can’t dance to it, it’s not my revolution,” proclaimed Emma Goldman in an earlier era.³ “... the times they are a-changin.”⁴ “The answer, my friend, is blowin' in
A Revolutionary: Donald R. Griffin

the wind ...” we heard from Bob Dylan in the 60s, suggesting, perhaps, that answers to the wrongs of our time were there somewhere about us, in the wind ... or perhaps, that we pay them no mind: they blow past us, as the leaves in the wind.\footnote{5}

Our politics and social movements were formidable. The country, the world watched the violence outside, then the turmoil inside the 1968 Democratic National Convention. Chicago’s Mayor Daly had called out the police; they tossed gas grenades among the protesters; they beat them with batons. The Democrats had taken a stance supporting the war in Vietnam and the young men who were to go to that war were angry, as was most of the rest of the country by 1968. Many stomped in protest matches around the country. Reputedly, said a general looking at a rally outside the Pentagon window (or perhaps with the President at a White House window?): “Don’t bother about it. Let them march; they’ll think they’re doing something.” Yes, the war went on.

The drug of choice was marijuana, “Mary Jane,” “grass,” “the good weed,” “Pot,” and also LSD, sometimes peyote and the “magic mushroom.” Spiked chocolate chip cookies, brownies with crunches of weed, might be served, knowingly or not by a hostess. If it wasn’t a bad “trip,” which, occasionally, it was, “loving” was the mood engendered, sensual pleasures of sight (light glittering from the leaves), sound (deep bases that resounded through our bones), taste. Grass gave us enormous appetites, especially for sweets ... for sex too. “Make Love, not War” was the refrain in song and spirit. “Love-ins,” multiple partners, shared partners; the “pill” made it less risky for the women. She wouldn’t get pregnant, but STDs\footnote{6} skyrocketed for men and women.

We wore radically different apparel ... The long white kid gloves of the ’50s, the circle pins proclaiming our virginity, and mid-calf skirts had disappeared. Bell-bottomed pants for men, long, flowing, multi-colored and tie-dyed skirts for women ... or mini-skirts, ever so slightly below our bottoms.

Long flowing hair; no tidily ordered, short, neat coifs any longer on women. The men let their hair grow ... and their sideburns and maybe even their beards. Lots and lots of natural materials, seeds, macramé ropes, and beads from exotic places, adorned our necks.

We often lived communally. We grew vegetables and pot ... or tried to. Many of us were “city folk” with little “know-how” of even suburban lawn growing and tending, so any “grass” growing was even more problematic.

That was many of us, not all. Others disparaged those “hippies,” “druggies,” smelly, dirty sorts ... made them all of a kind, though, of course, we weren’t.

**The “Popular” Revolution at Rockefeller University**

Activism and anti-war actions existed at Rockefeller University as well, though not at all to the extent occurring at many other universities. At RU, mostly the graduate students (remember, there are no undergraduates) and postdocs, research associates (RA) and just a few of the younger faculty were involved. I do not recall whether any of the “old establishment” took part. That was generally true at most colleges and universities, though with some very notable exceptions; Noam Chomsky, the noted MIT linguist, was a prominent activist. But as both the war and demonstrations continued, the more established, older faculty participated in the protests at many institutions.

An underground newspaper at RU, staffed by a small coterie of students and some others, reported such matters as the likely lead content in the older apartments owned by RU. These were largely the 4\textsuperscript{th}-floor “walk-ups” on East 65\textsuperscript{th} Street, rented to RU postdocs, RA, as well as the tenants...
who had lived there for decades. The lead deeply worried those families with young children who might eat any falling paint chips (they were old buildings). Another matter caused some consternation in the administration. This was the report, including maps, that indicated the former Standard Oil holdings in the sea outside Vietnam, now belonging to the beneficiaries, the Rockefeller family. Included, of course, were musings about going to war to protect such fortunes.

A silent, candlelight vigil occurred one spring evening outside the campus home of the RU President. The gathering was small, certainly less than 50 persons, as I recall. We were protesting the US involvement in the Vietnam War, possibly just after the invasion of Cambodia was made public. Frankly, at the time, worrying about all the work I had to do, I felt our actions were pathetic, quite useless. Much to my astonishment, years later, in a book written about those times, I found a comment that Nixon had been quite disturbed that such a protest was occurring at The Rockefeller University. The Rockefeller family had generally shown strong support for the war.

Small meetings continued among RU persons opposing the war. Modest posters announcing such were placed on walls of the basement hallways. I once saw a Nobel laureate angrily rip one down. One time, attending a larger meeting outdoors on the grass in the warm spring sunshine, the group was suddenly “rained upon” by the RU sprinkler system. Such afternoon watering was not a usual occurrence at RU.

And then a bigger demonstration. A large hand-painted sheet hung across the RU main entrance gate with “ON STRIKE” in huge black letters. This, too, was another antiwar protest. For one day, some of the RU personnel did not work. Communication systems had been set up, battery-operated, so that no one could “pull the plug” on the event. We could speak with each other via our walkie-talkies, high-tech for that era. We had a backup battery-operated speaker system to address the audience. With binoculars, we could view the further parts of the campus. Much of the equipment was the gear we used in our fieldwork researching animal communication and behavior.

We had read in the newspapers about the folks, often reported to be construction workers, who came with their heavy metal hammers and shovels and sometimes beat up those “un-American,” “lily-livered,” hippy protestors. We were scared … but prepared (or so some thought). A few of the young men, none particularly “strong men,” had held secret meetings and amassed some brooms and related paraphernalia, now hidden around the campus. These were the “weapons” for defense. No women were allowed during that strategizing. The women of the group got to make sandwiches. (The men and women all personally donated funds to buy the food.) The purpose was to “free the dining room personnel” so they, too, could attend the rally. And one young man did help with the sandwich making, Andy, the editor of the underground newspaper.

It was a significant rally. In solidarity with the strike, the Teamsters did not enter any gates of the RU campus. A major leader of their union spoke at the rally, as did, to our initial surprise, David Rockefeller. But he had substantial security for the campus. Men in beige coats and dark glasses “mixed” in among the large RU group gathered on the lawn. Others patrolled the flat roof areas of the campus buildings with their own communications. We were surprised by them and their “stuff” and they seemed similarly surprised by us and our “stuff.” No emergencies arose. The sun shown on the people, the lawns, the brightly flowering gardens. Speeches were made. And the next day, the New York Times ran an article on the front page of the second section about the “Strike” at Rockefeller University.
The official Rockefeller University Newsletter also reported the event. In their draft version, a “luncheon on the grass” was served. Needless to say, that wording didn’t survive our editing, but then neither did any phrase akin to “freeing the dining room staff” survive theirs.

The war continued (the US first entered in 1965); research continued; and, finally, the US ended its involvement in the war (1973).

At the time, I was not yet a member of Don Griffin’s lab; I was part of the Marler section of the RU Animal Behavior group. Marler was away from RU during all this. I do not recall Griffin’s role, but I would imagine, he would try to avoid political matters. However, recall his activism while at Harvard, supporting Marcus Singer, the Jewish Cornell physicist who had been targeted in the McCarthy era. At Rockefeller, he had been instrumental in assisting Janna Lipmanova, the young woman bat scientist, escape from Russia to the USA and helping find an academic “base” for her here. Internal RU politicking did involve him, for example, as he stood with several of the old guard protesting the intended elimination of the entire philosophy group. That was 1976 when RU President Seitz and the Board of Trustees decided to revert to a biomedical research model, as the university had been before changing from an institute to a university. Central to the decision was the far greater ease in securing funding for such research than for philosophy and other “academic” studies. RU relented, but almost all the philosophers left anyway. On a personal level, Griffin could be an activist, and had been, but avoided politics at most levels.

**Women in Science - At RU and Beyond**

The status of women, in society and, specifically, in science, was an issue of those times and today. Charles Darwin’s ideas are, of course, relevant. His evolutionary theory drives the quest for the evolutionary origins of many of humankind’s abilities and for the likelihood of consciousness in animals. He had other views not so warmly entertained by at least half the human race.

*The chief distinction in the intellectual powers of the two sexes is shewn by man’s attaining to a higher eminence, in whatever he takes up, than can woman — whether requiring deep thought, reason or imagination, or merely the use of the senses and hands.*

These were Darwin’s deep-seated beliefs even though he knew well and interacted respectfully with many accomplished female writers and thinkers as well as his intelligent, well-read wife and other female family members.

Presumably, by these days, male biologists and academic colleagues still entertain many of Darwin’s attitudes but have rejected these misogynist ones. Yet the role of women in science does not convey a sense of equal acceptance or status, though their lot is improving. During my interviews for this biography, I was quite taken aback by some experiences of the women scientists with whom I spoke, including information that emerged about Rockefeller University. Spoiler alert: as you may have already surmised from reminiscences I’ve reported, Don Griffin lands far more positively than most male scientists of his generation.

**Florence Rena Sabin**

Rockefeller University is a “mixed bag” in its relationship with women in science. To be sure, RU first had a woman as head of a department in 1925, when it was still the Rockefeller Institute for Medical Research. This was Florence Rena Sabin (1871-1953) who had also previously been the first woman to hold a full professorship at the Johns Hopkins School of Medicine and the first woman elected to the National Academy of Sciences; she retained the position of being the sole
female member of the Academy for 20 years. Her prior medical education was possible only because she, along with thirteen other women in her class, had been admitted to Johns Hopkins School of Medicine because of a donor’s stipulation requiring the admission of female students. This, after first graduating with a BA from Smith College (1893), one of the “seven sisters” and an institution that was a potent force in women’s intellectual and professional development over long decades. Florence Sabin’s field of study was cell biology until she “retired” back to Colorado and then became a fierce advocate of stronger public health laws; in particular, she saved innumerable lives from the scourge of tuberculosis.¹²

**Margaret Floy Washburn**

Margaret Floy Washburn (1871-1939) was another formidable voice for women in the field of science; her area was comparative psychology. Her book, *The Animal Mind: A Text-book of Comparative Psychology* (1908), became the standard comparative textbook for 25 years, with subsequent editions after 1908. Washburn considered that animals were conscious and that their consciousness could be studied experimentally. Teaching at Vassar College, another of the “seven sisters,” for most of her career, she had neither the institutional support common in a large university setting nor the research assistance from graduate students. Male colleagues had such advantages, but they were at universities where she was not welcome because she was a woman. So, she worked with undergraduates and they conducted studies together.

Margaret Washburn had been psychology’s first woman Ph.D.; she was awarded the degree by Cornell University (1894). She also became the first woman President of the American Psychological Association (APA). In his 1984 comprehensive history of comparative psychology, Donald Dewsbury included Washburn as the only woman among the 38 scientists he highlighted.¹³ A scientific recollection of her contributions noted the disappearance of animal mind and consciousness for almost 100 years and recognized Donald Griffin, a century later, championing the rebirth of scientific interest in animal mentality.¹⁴

**“The Times They Are A-Changin’”**

During the time Don Griffin was on the Rockefeller faculty (1965-1986), significant changes were underway for women, both in the nation at large and, though slowly, at RU, as well. NOW (the National Organization for Women) was founded in 1966; the inimitable Betty Friedan, author of *The Feminine Mystique*, was its first president. She drafted NOW’s original Statement of Purpose:

> The purpose of NOW is to take action to bring women into full participation in the mainstream of American society now, exercising all privileges and responsibilities thereof in truly equal partnership with men.¹⁵

At Rockefeller, some of the women created their own group, possibly officially affiliated with NOW, and held fairly regular meetings. Among some of these women, there was also the recognition that men, too, were constrained by society, often facing enormous pressure to be the breadwinner of a family and to be highly successful in that effort.

In at least some ways, Rockefeller University was more progressive about women’s status than were Harvard and numerous other institutions. Tim Williams, who first joined Don Griffin as a graduate student at Harvard, recalled some office-sharing rules. Initially, he was in an office with a
female graduate student. Don told him privately not to let this be too widely known, "because there was a Harvard rule that a male and female could not co-inhabit the same office space."\textsuperscript{16}

During Tim’s undergraduate days at Swarthmore College, the campus was divided into two halves by a walk, men’s dorms to the west and women’s to the east. He recalled, "For two hours on Sunday (I think), the sexes were allowed to visit each other’s dorms, but the room door had to be opened at least 6 inches and all 4 feet had to be on the floor (I am not kidding)." In my (CR) own case, at Vassar College, in the early 1960s, I remember being called to task. One weekend, my father had brought me some items and had stayed a while to visit. I had committed some infraction, either forgetting to leave the dorm room door open or, perhaps, his visit extended beyond the designated male visiting hours.

One of Tim’s much loved Swarthmore biology professors, Norman Meinkoth, and his wife, had, during the depression, worked as high school teachers in Indiana. But they had to marry secretly, “and work in different towns because only one of a married couple could be employed as a teacher in the state.”

After Tim received his Ph.D. with Griffin at RU (1968), he held a postdoctoral position at Woods Hole Oceanographic Institution (WHOI). He recalled that in the ’60s and early ’70s, women were not allowed on WHOI ships. “The excuses were that women were bad luck (!) or that the Navy rules prohibited women.” This was about the time (1972) that I (CR) had joined as a volunteer aboard the WHOI Atlantis II vessel to assist Don Griffin and John Teal’s project studying bird migration over the ocean. The “no-women” exclusion rules had recently changed, but I was newly pregnant and thought it prudent not to mention that fact when I volunteered. As I expected, when I arrived at the ship in Bermuda, much more obviously pregnant, I was permitted to join the expedition. All went well.

As earlier noted, another RU expedition, this a 1971 student field course to Africa, also encountered the changing times. Initially, the group’s leaders and mentors had not intended to include women students; they were a “distraction” to the men’s work; lodging logistics would be a nuisance. (Griffin, as far as I know and can recall, was not involved in this exclusionary decision.) But this was an RU project, a US government-funded event, so ... one woman grad student, Beverly Greenspan, Don’s student, was finally permitted along. (Further details in Chapter 12.\textsuperscript{17})

Tim Williams also recalls the situation at WHOI in which a researcher and his wife were co-principal investigators on a grant from the Navy. But, since the male was employed by WHOI, she could not also receive a salary, even though it was budgeted in the grant. The couple simply left and walked down the street in the town of Woods Hole to the Marine Biology Lab of Boston University. That institution happily accepted the scientist couple ... and the grant and sizeable overhead to the university. WHOI changed its regulations soon afterwards.

**Some Women Scientists’ Experiences**

It is hard to imagine now the systemic bias against women in science as late as the 1960s, so I include a few examples.

**Ann Graybiel:** The prominent neuroscientist Ann Graybiel,\textsuperscript{18} when an undergraduate research assistant in Don Griffin’s Harvard lab, was happy to be invited by Griffin to spend part of a summer at Simla in Trinidad. She was an assistant for Don and his grad student Rod
Suthers, studying bats. "I even had the opportunity to do my own project" with echolocating birds, the oilbirds (Steatornis caripensis), she reminisced.

But later, she did not encounter such encouragement. Two years after receiving her Ph.D. at MIT, she joined the MIT neuroscience faculty.

_**I had no start-up money. They gave me a room in the small place where they kept the monkeys. I used to clean the floor myself.**_

At the time, and for a long time, she was the only female faculty member in a six-floor building.¹⁹

**Julia Chase Brand** has distinguished herself, in her early days and then again during her retirement, as a distance runner pioneering efforts to allow women into running competitions. She was “an 80 mile a week runner, but the only race available to “girls” was the 880, which is two measly laps.” ... As “the pioneer distance runner” who challenged the AAU ban on women’s distance running, in 1961, she illegally entered a men’s road race. By doing so, she forced “the AAU to back down and allow women to run cross country for the first time.”²⁰ Her civil disobedience had clearly demonstrated that women were not weak and delicate flowers, capable of running only two laps.

Her second career was in science. As a beginning grad student in 1966 in Physiology, at Indiana University, she hoped to do original work in women’s exercise physiology. At that time, there was little research on female athletes. There were also few women scientists; there hadn’t been any in the department for 20 years, though several were entering with Julia. In her autobiography (in progress),²¹ she recalls the overt harassment she experienced. During lab work, whenever she started an incision, a male colleague would bump her elbow, while hissing, “Wrong sex, wrong sex.” Others hid pornographic photos in her textbooks.

Although she wasn’t stuffed in with monkeys, she initially had a desk stuck against the wall in a hallway, but then graduated the following year to a small room of her own. The male students had rooms with windows, filing cabinets and typical office equipment. But no coed fraternizing was to occur.

So, what of the room of her own?

_It was a windowless room with flaking grayish pink paint. In front of the furnace door was a 2'x3' foot wooden table, and from the ceiling hung a bare light bulb with a string. In the far corner stood two gauze-wrapped right legs that apparently had been overlooked when used body parts were incinerated at the end of the gross anatomy course the previous semester – perhaps they were intended as company for me?²²_

Those legs gained some notoriety. Reporters came to photograph her and her advisor after they had presented a co-authored paper demonstrating that “blind as a bat” bats weren’t blind at all; they could visually discriminate patterns.²³²⁴ The newspaper photo of Julia Chase in her office included the severed legs. This photo finally embarrassed the Department into moving her desk into the regular graduate student office with the male grad students. Now, she, too, could be part of the chats and gossip about grant opportunities and departmental politics.

Rutgers University later hired her on their faculty, but, like Ann Graybiel, and unlike the male hires, she was given neither lab space nor start-up money. Her choice of scientific direction did shift back to bat echolocation as well as explorations of the faint but audible sounds that rats
can use for navigation. After years next on the Barnard College faculty, in her 50s, she entered med school and trained as a psychiatrist, which became her next career. And in 2011, on the 50th anniversary of the race she ran in civil disobedience as a woman entering a men-only race, she ran again in the competition. Now it was officially open to women too.25

The RU Report on Women

In 1972, a report was written at Rockefeller University, “An Inquiry into the Status of Women in Science.” HEW (the US Department of Health, Education and Welfare)26 had required that Federal contractors file salary and job category data from women and minority groups. Since RU received many federal grants, this was necessary to continue receiving them. RU “passed with flying colors,” President Seitz announced. But ... that was for nonacademic personnel. What of the academic? Clearly, the proportion of women and minorities did not reflect those in the general population. The RU admin announced that it was fine in this regard as well. They could show that so few qualified women and minorities existed and that RU was exhibiting no discrimination. As evidence, they pointed to the members of the National Academy of Science: two women, no blacks, and 40+ male RU professors. No discrimination.

But there was considerable interest in the matter by at least some at Rockefeller University. The women wondered, why don’t women succeed? And if they don’t, why does RU spend so much effort and funds to educate women graduates destined never to become successful scientists? About one in four RU graduate students does not graduate. “They include married women who are obliged to interrupt their studies because of pregnancy or whose husbands’ work takes them to other parts of the country.” The numbers also include those, both male and female, who do not consider themselves and RU a good match for various personal and academic reasons. But “outright academic failure is rare.”27

The administration showed sufficient interest to form a committee to interview RU personnel about the matter. The interviewers were younger faculty members and graduate students. Those interviewed included “15 laboratory heads, 17 working women, 9 female grad students and 8 male junior faculty members.”28

At Rockefeller and beyond, the problem was easily displayed by a few statistics. As is usual, women’s salaries were lower than men’s, even for the same academic position. In 1971, the highest rank held by a female RU graduate was an associate professor (1/17 or 6%). 39% of the male graduates held that rank or better. At RU itself, there was a similar story. “The two female associate professors with tenure have been at rank 7 and 14 years, while 6 of the 23 men still at RU who were associate professors in 68-69 have been promoted.” (4-5 years duration) The women’s salaries, though at rank about twice as long as the average male, were 91% of the male mean.

Why is this the situation? Sex discrimination, women’s inferiority or women’s competing demands? Two of the lab heads said blatant sex discrimination had existed in the past but was no longer a factor. Among the other comments offered by the lab heads, were that women did not work as hard as men, because they “felt less pressure,” “were weaker,” “had less energy,” were “less driven.” “... The physical demands of raising a family were [considered] incompatible with the undivided commitment required by first-rate science.” That reason alone, most lab heads felt, was “sufficient to explain the few women of distinction in science.”29 The fact that some few women do
have children and become “first-rate” scientists, indicated merely that it could be done, but was rare.

About half of the more junior men and women did feel that more subtle forms of sex discrimination were operating. Seven of the nine female grad students planned to have children and that a husband would share 50-50 in family duties. “These women were highly ambitious and considered their work the most important thing in their life ...”30 All but two of the younger men in the survey intended to share family responsibilities. 1/3 of the lab heads explicitly rejected the concept of flexible working schedules and likewise implied that such young “sharing” men would find no place in their laboratories and “would never make their mark on science.”31"

The generation gap was strong. The two senior women who had successfully combined career and family32 felt it could be done only in exceptional instances. Among other attributes, it required luck, choice of a supportive husband, being a member of a supportive laboratory, giving up much, such as any hobbies, and being an excellent organizer and time manager.

Other factors were noted as contributing to women not “rising.” Among them was “mobility;” the threat of moving may provoke a higher salary. A woman at that time usually moved where her husband’s job was located, not her potential job location. Even the mere fact that a young woman was marriageable or married was sufficient for many men to think her commitment to science might not continue, and thus she was not worthy of promotion.

How then can a university help to promote the role of women in science? Donald Griffin and the two senior women were the only lab heads who thought that the feasibility of part-time work should be explored, but neither thought it a good solution to being a serious scientist. They believed that it was far better to be in a laboratory where flexible work arrangements were possible. Psychology and theoretical physics are such fields, whereas neurophysiology might require 18 hours of continuous lab work.33

Since family responsibilities were seen as such potent constraints on scientific commitment, assistance with child care was suggested by the interviewing committee as a means to aid both women and the emerging generation of young men who intended to share home responsibilities. Rockefeller had a nursery school, but not for the youngest children. It also was only five days a week, and “...young faculty members work day in and day out.”34 Thus, expanded facilities, including summertime, would be helpful. Alternatively, a supplement to ease the cost of outside child care could help.

The report, though so disheartening and demoralizing, did suggest a change might be forthcoming, given the starkly different attitudes of the older and newer generations of scientists. The 15 male lab heads, had, with one exception, married women who stayed at home during their childbearing years. At least five of those men divorced and later married professional women. The ambitious young men and women seemed intent both on scientific careers, but also on sharing family responsibilities.

**Women Scientists in Griffin’s Lab.**

As is noted in the report, Don Griffin was the only male lab head who was open both to exploring part-time scientific work and also thought flexible scheduling was a reasonable alternative. Dr. Christiana Leonard, the report’s author, once remarked that in her time at Rockefeller, Don Griffin always seemed to have more women scientists than did other labs,
suggesting a supportive attitude and atmosphere. As a lab member, I agreed but was less sanguine about the situation. Perhaps, women were not driven to enter a field such as neurobiology where, at least for men, the job prospects were more numerous and secure, and the science was highly respected. Not having the demands put on men, a woman could follow her interests. Perhaps.

I am aware of two women graduate students, Jean B. Harrison and Kathryn Ralls in Griffin's Harvard laboratory; both received their Ph.D.s with him. Previously, when Don was a professor at Cornell, the only woman graduate student I know of was Ann M. Rawson, the wife of another Griffin grad student, Kenneth S. Rawson. Kenneth did receive his Ph.D., but Ann did not. I think, but I do not know for certain, that she stopped her studies to raise a family.

At RU, Don supported several women scientists, in the least with lab space and equipment, and sometimes salary as well. (That's not to say that he was the only RU professor who had female graduate students and junior faculty.) Among the female recipients of Griffin's largesse were Dr. Katherine Ralls, Dr. Julia Chase, myself (Dr. Carolyn Ristau), grad students Marilyn Yodlowski and also Beverly Greenspan who was mentored primarily by Jocelyn. Don helped Julia Chase, then on the Barnard College faculty, when she was trying to record rats' vocalizing; initially, the sounds were considered possibly ultrasonic. He gave her office space and lent her the necessary high-tech equipment. While President of the Harry Frank Guggenheim Foundation, he encouraged work in Cognitive Ethology. Among the grant recipients in that field were the husband-wife team of Dorothy Cheney and Robert Seyfarth, studying vervet monkey semantic communication, Irene M. Pepperberg, working on parrot cognition and language, and my own research on injury feigning and cognition in plovers.

Personally, Don's first wife and mother of his four children had planned to follow a career in biology. To Griffin's disappointment, after the children were in school, she opted to become a secondary school biology teacher instead. His second wife, was the noted marine scientist, Jocelyn Crane.

RU Changes

The times changed, attitudes changed, and Rockefeller University changed. And the RU Report? Hopefully, it helped to instigate some of the changes at RU. It was intended to be widely circulated at the university; it wasn't. Written almost 50 years ago, it was formerly available at The Rockefeller Archive Center in Sleepy Hollow, New York. At the beginning of the Covid-19 pandemic, all the RU documents were removed, pending further decisions as to location.

Among the RU changes over the 50 years were the actions implemented by David Baltimore, who served briefly as the President of RU (1989-1991). He is reported to have added about 100 new assistant professorship positions, receiving many applications, and personally reviewing each appointment. This resulted in approximately 50 female faculty being added to the RU roster. Later, in 1998, Rockefeller University created the “Women and Science Initiative.” Among its goals were showcasing the contributions of women scientists and creating a program of support for women scientists. More than $32 million was raised to pursue such goals. Equality has not been achieved at RU, but progress has definitely been achieved.
Revolutions in Many Fields and Universities

Physics in Revolution

Among the sciences, there were revolutions, too. Atomic physics now had quarks! Murray Gell-Mann and George Zweig independently posited quarks’ existence and made very specific predictions, an essential aspect of their daring to suggest such a new entity. And quarks were found! Using high-speed particle accelerators in the U.S. and Europe, the quarks’ existence was experimentally verified. No longer were the elemental particles of the atom’s nucleus understood to be protons (positively charged particles) and neutrons (heavier neutral particles) with negatively charged electrons orbiting outside. Now, scientists recognized that the protons and neutrons were composed of much smaller entities, quarks, with fractional positive and negative charges. As experiments continued on the accelerators, a veritable zoo of very short-lived, extraordinarily small particles was discovered.

The quite revolutionary, but modestly labeled, “standard model” of atomic physics emerged in the ’70s and the experimental physicists confirmed it. The theory describes three of the four fundamental forces and classifies the elementary particles. These forces, along with gravity, drove the evolution of our universe and keep it together: the “strong” nuclear force, recognized in the late 50s and early 60s, binds the atom’s nucleus together; the weak force, discovered only in the 70s, is the mechanism of interaction between the subatomic particles and responsible for radioactive decay; the familiar electromagnetic force is light and electricity and magnetism; and, finally, there is the mysterious force of gravity, then and now still baffling. It would not fit neatly into the standard model. Physicists’ quest for the grand unifying theory of everything was (and is) simply stymied by gravity, as well as by other phenomena.

And in the ’70s, computer technology advanced enormously, making possible the gargantuan calculations and analyses required for the sub-atomic physics work and other science, including explorations into outer space. A Russian cosmonaut was the first man in space in 1961, while the competitive Americans planted a flag on the moon in 1969. In the ’70s, Stephen Hawking theorized the existence of Black Holes, dense pockets of space with extraordinarily strong gravitational fields, so strong, no light could escape from them; no light could “see” them ... until 2019. In April of that year, an international team of astronomers first revealed a black hole 55 million light-years away and the fiery gases blazing about it.

Biology in Revolution

Biology saw its own revolution. Darwin’s 1859 theory of evolution had postulated survival of the fittest through the gradual change of organisms as they competed with each other and as the environment changed. Eventually, the extraordinary diversity of life on our contemporary earth came to be. But others in the ’70s, in particular, Stephen Jay Gould, proposed the concept of “punctuated evolution.” By this theory, species can remain pretty much at a standstill, at a stasis over generations. Suddenly, a significant environmental change can provoke a rapid change in characteristics needed to survive and so species diverge. That’s also a closer approximation to our
findings in the fossil records ... many similar specimens over time; then a sudden, substantial
difference.40

Cellular and molecular biology had likewise been leaping forward and assuming a dominant
role in the field of biology. Natural observations of animal behavior had been struggling for
acceptance as “proper science” even decades before when the young Donald Griffin was a graduate
student; it became even less appreciated.

Psychology in Revolution

The “Garcia Effect” (Vehemence in an Elegant Apartment)

Psychology, too, underwent a revolution. As a Psychology graduate student at the University
of Pennsylvania, I was privy to some of the turmoil. I recall, in particular, an evening in an elegant
townhouse in downtown, central Philadelphia. That street, Delancey, was quiet, removed from the
city’s hubbub. Its three-story, upscale, well-preserved old brick buildings were flanked by leafy
trees lining the paths. Here was the home of Professor Francis W. Irwin, a quite reserved and
somewhat formal, older faculty member. Officially part of the clinical faculty, he focused on the
study of motivation. His clear thinking and ideas were, however, sought by all; his comments were
always welcomed on any manuscripts about to be sent for publication.

On a Friday evening, once each month, a soiree of sorts was held at his house, open to the
faculty and graduate students. The apartment was a refined, graceful home, its walls a sophisticated
gray, its furnishings in fine taste. Initially, beer and pretzels were served to “lubricate the
discussion,” but these morphed over time to include wine and cheeses. Quite a different fare and
style from our typical graduate student existence. Helping set up would be one of the women
graduate students, invited by Frank Irwin to be a co-hostess for a semester or so. We did consider it
an honor to fill that role.

And the focus of the evening? A “big issue” in “philosophy of science or in the strategy of
research in experimental psychology”41 suggested either by Professor Irwin or a topic selected by a
U Penn faculty member, invited to be the evening’s “featured guest.” That evening it was the “Garcia
effect,” named for the scientist who first conducted certain unsettling experiments (published in
1966).42 The “insurrectionist” John Garcia, was a warm, outgoing, bushy-bearded, slightly rotund
man in his late ’40s-early ’50s. He had recently spoken at Penn. I recall, too, strolling after his
lecture from the Biology building, the venue for his talk. That site was in itself a bit of mystery, for
the essence of his address was especially provocative for the field of psychology, not biology.
Perhaps, grounding his findings in the seemingly “more objective” field of biology was specifically
intended. Amongst a small group of Psychology faculty and grad students ambling along the path,
one heard much discussion, rather heated, a good bit of perplexity, and some disbelief in Garcia’s
findings.

Within the civilized ambiance of Frank Irwin’s apartment, disagreements arose again, far
more vehement than the usual open-ended conversations on the Friday evenings there. The
renowned Professor Richard L. Solomon, usually friendly and outgoing, was standing and most
angrily almost shouting, yes, actually shouting, something akin to, “You are undermining the very
foundations of psychology! You are removing the very basis of understanding upon which our
graduate students rely. Decades of work should not be challenged by some bizarre finding, some
peculiar situation.” This was directed to a few faculty members who were championing Garcia’s research, among them, as I recall, Professor David Williams, an experimental learning theorist.

**Humankind: Unique and the Apex**

To understand the context of the turmoil, let us step back. As in all revolutions, beliefs develop beforehand; they fester or bloom according to one’s bias. Events occur, some noted, some not, some exaggerated. So, with this revolution.

How far back to go? Aristotle was, among his other talents, a naturalist and astute observer. Watching animals, he noted similarities to humans, but to him, animals were far more lowly creatures. His is the still-potent “Scala Naturae,” the hierarchical ranking of man and beast, with humans at the apex. Though Aristotle saw some “psychological continuum,” we humans alone are rational creatures and moral beings. A student disagreed with him. Non-human animals reason, sense and feel emotions as do humans, claimed Theophrastus. He became a vegetarian, not wanting to unjustly rob the life of such beings. But it was Aristotle’s view that prevailed in the West for nearly 2,000 years.

The world’s religions held vastly different views: from the Jainists who literally would not harm a fly, to Buddhists who envisioned reincarnated beings, and to Christians and Jews and Muslims who, depending on their sect, could find passages in their holy writings promoting man as the hierarchical head, and/or the protector or “user” of animals. But man was a separate entity; never was an animal a moral being.

The separation of man and animal was vividly exhibited in the thinking of Descartes. Humankind had a soul. Animals did not; they were machines; we would say “robots,” who did not experience emotions or pain. Neither were they rational entities. And without a soul, they could have no moral sense.

That view prevailed in popular philosophy and in scientific endeavors. Humans could commit many actions in the name of science. Using animals as models for human diseases was acceptable, was a positive way forward. The inconsistency of our thinking about animals was usually not considered. Somehow, animals shared enough similarities with our structures and bodily processes that they could serve as models for our health and diseases, but the similarities abruptly ended for mental processes. The fact that brains, made of the same organic materials in man and beast, many possessing similar structures, using neurons to send signals, did not seem to provoke much consideration or concern.

**Animals in the Field and the Lab**

Science continued its endeavors. Some asked, “How does an animal perceive the world?” It was Jakob von Uexküll (1909) who conceived of an “Umwelt,” a world view, specific to each species, dependent on their sensory capacities, their needs, their motivations, and their means available to interact with the world, including both the social and environmental. He, too, got to be forgotten, particularly in the USA. Although various European ethologists conducted field experiments, they were particularly concerned about the animal’s behavior in its natural world. In the USA, interest was growing to conduct very carefully controlled experiments with animals. Try to isolate the factors responsible for and impacting any behavior. Experimentation in laboratory settings became the accepted norm. And, since the belief was strong and growing, that the same
general principles applied throughout humans and animals, one could experiment with any animal and find laws. In particular, general laws of learning could be found that would apply to all species, including humankind. That movement was “Behaviorism,” J. B. Watson, its formulator, and B. F. Skinner, its popularizer and leader. Its roots lay in the positivist philosophical tradition. Behavioristic views dominated, indeed swamped, experimental psychology for decades in the 20th century.

At the same time, European studies of behavior were emphasizing the instinctual aspects. Konrad Lorenz and Nikolaas Tinbergen proposed models, quite hierarchical, to explain the control of motivation and behavior. The rift between the two continents of thought became intertwined with sociological/societal issues. Europe was regarded as a more stratified, controlled society, whereas the USA was a land where change was the essence; theoretically, anyone could, with effort, learn and become whatever he chose.48

That capacity for change was intrinsic to Behaviorism as well. To the Skinnerian, the behaviors of organisms, animal or human, were “shaped” by reinforcement. Organisms performed behaviors for which they were specifically rewarded (reinforced). Watson had asserted:

*Give me a dozen healthy infants, well-formed, and my own specified world to bring them up in and I’ll guarantee to take any one at random and train him to become any type of specialist I might select – doctor, lawyer, artist, merchant-chief and, yes, even beggar-man and thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors.*49

In actuality, he was not quite so extreme, noting in the very next sentence, “I am going beyond my facts, and I admit it…” But that sentence is typically forgotten. To the behaviorist, the mind was a “tabula rasa,” a “blank slate” onto which experience would write. Again, a concept initiated by Aristotle who wrote of the “unscribed tablet.”50

Heresy was in the making. There had been other views by scientists, largely considered “unscientific” by the dominant professionals. In 1925, Tolman had closely studied the behavior of animals, even rats, even in the controlled conditions of the lab, and had introduced the concept of “purpose” to describe their behavior. He had tried hard to describe such purposes in “objective” terms, i.e. observable behaviors. But he’d used a word, “purpose,” describing a mental state, rather than an analysis in terms of stimuli and conditioned responses to those stimuli.51 Hebb furthered the “revolution,” as he himself termed it, suggesting the need to study those things that Behaviorism had cast aside, e.g. “mind,” “self-awareness,” “consciousness.”52 And the more thoroughly one investigates, the more one finds hints of the heresy from other scientists but phrased as carefully as possible to be “acceptable” and publishable in the scientific climate of the times.

**“Misbehaving Animals” Challenge the Laws of Learning**

Sometimes, the “general laws of learning” seemed not to be working. There was a captive chimpanzee “rewarded” by desirable food when it gave correct responses in the scientist’s experiment. Strangely, it simply stockpiled the food pieces and when the experiment was completed, it gave the “rewards” back to the scientist. How peculiar! The only plausible interpretation, since it was still hungry, was that it wanted to continue the “game,” the social interaction with the scientist. This, in a time when such “social rewards” were not considered in the context of experimental design and interpretation.

And then there were the pigs. We realize, now, that pigs are quite intelligent beasts. The Brelands were a couple who made a living by training animals, including pigs, to perform in ways
that people might pay to watch. They both had Ph.D.s in psychology and significant experience in laboratory animal studies. The pigs, very “tractable” animals, were given a seemingly simple task. A pig was to pick up a coin, later several, bring each coin, one at a time, back several feet to a “piggy bank,” (a container with a slot), and then drop each coin into the slot. After the coins were all deposited, the pig received a food reward. This worked, but only for a short while. Very soon, the pig, instead, began digging into the ground with its snout, or “rooting,” as the behavior is termed. Or it would toss the coin into the air, “root” around once it fell, and “root” some more. It wouldn’t stop those actions. It happened with each successive pig. Despite the pig’s intelligence and impressive abilities to solve problems presented to them, in this case, no amount of reward would change its activities. Why? “Rooting” was an innate behavior that was part of a sequence involved with foraging for food. The pigs were drifting gradually into behaviors completely different from those that were conditioned. The Brelands termed the changing behavior “Instinctive Drift.”

Similarly, despite whatever training, raccoons would repeatedly rub together the coins they were to deposit in a “bank.” When raccoons forage naturally, they “rub” and “wash” crayfish or other prey with their highly dexterous hands. And chickens would “dance” and “scratch” for their reward, but not learn to hold still. And so forth ... for many species. Clearly, the Brelands suggested, the white lab rat “cannot reveal everything there is to know about behavior.” The Brelands considered their discoveries as indicating “a clear and utter failure of conditioning theory.”

The Behaviorists had “abolished” instinct from their theories. Furthermore, asserted the Brelands, the Behaviorists had also adopted certain tacit assumptions. In particular, Behaviorists assumed that the animal brought to the lab is a “tabula rasa,” that species differences are minor, and that any responses can be about equally well conditioned to any stimuli.

Not too many people knew about the Brelands’ findings at the time. Those who did thought them rather peculiar and then passed on to something else.

The black activist Frantz Fanon wrote of matters such as these in many of his writings. One quote is particularly salient, though he intended it as a comment about societal prejudices, in particular racism.

_Sometimes people hold a core belief that is very strong. When they are presented with evidence that works against that belief, the new evidence cannot be accepted. It would create a feeling that is extremely uncomfortable, called cognitive dissonance. And because it is so important to protect the core belief, they will rationalize, ignore and even deny anything that doesn’t fit in with the core belief._

**The “Garcia Effect” (Tasty Water or Noisy Water?)**

Perhaps, now, we can better understand that evening’s vehement discussion at Professor Irwin’s home. The rebellious acts of John Garcia were simply two experiments he had conducted. He had published the results, tersely worded, comprising only two pages each, in the new journal _Psychonomic Science_, just begun in 1964. It was not yet a prestigious journal and lasted only until 1974 when the growing society expanded its publications into a half dozen journals. In 1974, It was the voice of the Psychonomic Society which had broken away from the dominant, mainstream American Psychological Association, to distinguish itself as a group of serious, experimental psychologists, not applied psychologists, not clinical psychologists. Compared to other psychological journals, the original _Psychonomic Science_ was also far less concerned with theory.
Working on a military project concerning the effects of radiation, Garcia had noticed that rats exposed to X-rays would avoid flavored water they had drunk before the exposure. Similarly, rats “poisoned” by exterminators, if still alive, will avoid the “poisoning bait,” but not the place it was found. Puzzling. So, Garcia and his colleague, Robert Koelling, conducted an experiment. Some rats drank “bright, noisy water” (water in the presence of a bright light and a clicking sound) while others had “tasty” water. The drinking was followed by radiation or a toxin, or by immediate or delayed shock. These “consequences” are termed “reinforcements” which, in this case, were negative, in contrast to rewards, which are “positive reinforcements.” The timing of reinforcement was critical to the Behaviorists’ view, for a neural association was assumed to occur between a stimulus and the reinforcement that closely followed it. More than a half-second’s delay was deemed to be deleterious to forming a strong association.

But Garcia got peculiar results. As expected, the associations arising from the electrical shock depended strongly on the length of delay, with stronger associations formed when the shock was immediate. The other results? Yes, peculiar. By the tenets of Classical (Pavlovian) conditioning, any stimulus can be conditioned (associated) with any reinforcement. That’s basic. But that was not happening.

If the rats had been exposed to “bright, noisy water,” they would inhibit drinking when those stimuli were present, but only if they had been shocked. Those who had been nauseated by either the toxin or radiation, when tested days later, simply drank the “bright, noisy water.” But if the nauseated animals had been exposed to “tasty” water (salty) in association with their nausea, they strongly inhibited their drinking when they were tested days later, fully recovered from that nausea. And that nausea was not immediate; it can take hours for the effects to develop after radiation or ingesting a toxin. That’s no “half-second” between the stimulus of salty water and the effects of reinforcement, the nausea.

In the other seminal paper, the authors specifically investigated the delay over which the rat would associate an internal gastric malaise with a gustatory stimulus. This time apomorphine was administered which produces an immediate gastric disturbance, while the associated taste was saccharine water. Rats still exhibited suppression of drinking even with a delay of up to almost 3 hours between experiencing the sweet taste and the stomach upset.

“Garcia Effect” - A Small Result or a Major Heretical Challenge?

Garcia & Koelling drew no grand generalizations from these results, though natural selection was mentioned as possibly affecting mechanisms that “associate gustatory and olfactory cues with internal discomfort.” Others did emphasize the broader implications, in particular, Paul Rozin and related views from Sara Shettleworth and Martin Seligman, all scientists associated with the University of Pennsylvania Psychology Department. In Rozin’s view, the “Garcia effect” aka “conditioned taste aversion” was not an isolated phenomenon. It should be construed as “belongingness” or an inbuilt tendency for certain categories of stimuli to be associated more readily with only certain relevant events. Yet more broadly, belongingness should be understood as “an example of a general adaptational principle.” Rozin develops these ideas yet more fully in considering the evolution of intelligence.

Organisms may not only learn some things more easily but may learn some things differently: “the “laws” of learning need not be the same in the feeding system and other systems.”
An example is birds which are generally more visually oriented than rats, and will associate an internal alimentary disturbance more readily with color rather than odor, but can learn either.\textsuperscript{61} Another persuasive example derives from honeybees which can “project” the sun’s path later in the day, but only if they have been exposed to part of the arc of the sun’s movement in the sky. Bees raised without sun exposure have difficulties navigating.\textsuperscript{62}

That evening at Professor Irwin’s home, Richard Solomon clearly understood the potential challenge of Garcia’s work if it was not merely a unique finding. Solomon himself was a major innovator in the field of psychology. He was admired, as well, for emphasizing the positive contributions of other colleagues and those he worked with. Typical of this generosity, in order to foster the future careers of his young students, he would often put his or her name as the first author of a paper, more properly attributable primarily to himself.\textsuperscript{63}

He and his students, working with rats and dogs, conducted ground-breaking research that had profound impacts on clinical psychology and the understanding and treatment of both ordinary human behaviors and their disorders. Among his achievements, applicable to humans, was an analysis of “avoidance learning.” Animals, humans too, after several associations, learn to avoid a signal of an impending negative outcome (electric shock for the dogs in the lab’s experiments). The dogs tend to maintain that avoidance over many “trials,” even though they haven’t again experienced the negative outcomes. Such behavior is clearly both potentially useful and deleterious. It can lead to what Solomon and his team labeled “learned helplessness.” As demonstrated by students in his lab, dogs, and, by extension, humans too, could “learn helplessness.” In the experiments, some dogs received shocks from which they could learn to escape, while others received the same number and duration of shocks, but could not escape. Even in new situations, “learned helplessness” became a way to deal with the circumstances. The “helpless” dogs would not explore potential means of escape. Again, the findings readily applied to real-life situations, such as human depression, anxiety, phobias, shyness and loneliness, which could all be exacerbated by “learned helplessness.”\textsuperscript{64,65} (Fifty years later, the authors state that the passivity to prolonged aversive events was actually the “default” response but organisms can learn to overcome such passivity.\textsuperscript{66})

In yet other work, another Solomon student, Robert Rescorla, challenged interpretations of the traditional “Pavlovian” (“classical conditioning”) and “instrumental” (“operant conditioning”) learning models. Pavlov had demonstrated that a tone that preceded food to be given to a dog soon evoked apparently the same responses that the dog made to the food, i.e. the dog began salivating to the tone. During “operant conditioning,” championed by B. F. Skinner, an animal’s (or human’s) particular behavior is “shaped,” meaning it occurs more frequently, after being “reinforced,” i.e., rewarded. According to learning theorists, just about \textit{all} human behavior was to be explained by this process. Both Pavlovian and operant conditioning were considered automatic mechanisms; the term “reflex” was often used for Pavlovian conditioning.

In an ingenious set of extraordinarily well-designed and well-controlled experiments, Bob Rescorla showed that something far different from “automatic” association was occurring during the pairing. Typically, a signal (the CS or conditioned stimulus, e.g., a tone or other neutral stimulus) was paired with an outcome (the US or \textit{un}conditioned stimulus, e.g., a shock). Making sure to associate each tone and shock an equal number of times, he set up two conditions. In one, the tone was always followed by a shock; in another, the tone was only sometimes followed by a shock and
sometimes not. The tone acquired far more “strength” or we might say “fearfulness” in the first case than in the second. That may seem sensible, but utterly contradicted the “reflex” analyses of Behaviorism. The number of pairings of tone and shock was identical, remember, and by Behavioristic theory, it was the number of pairings that determined “reflex” strength. But, in the first case, the tone was a “predictor” of shock. In the second case, the tone merely predicted the possibility of a shock. In both cases, the tone had information value; this was putting cognition into the head of an animal. Robert Rescorla went on to become a major contributor to the field of psychological learning theory, and after a stint on the Yale faculty, returned to the “hotbed” of the University of Pennsylvania Psychology Department.

Also at Penn on his sabbatical was Ulric Neisser, working on a book. That book was Cognitive Psychology, published in 1967, transforming the field of human cognitive psychology. From his lifetime of research and thinking, he was termed by many, the “Father of Cognitive Psychology.” His central point was the need for an “ecological psychology,” one that got outside the laboratory, one that studied all aspects of human memory, learning, and cognition from the vantage point of humans operating in their environment, their world, solving their real-life problems.

In summary, the field of Learning Psychology was jolted: The “Garcia Effect” was not a trivial matter; innate predispositions impacted possible learning. Classical Conditioning was not “automatic;” cognition intervened. The traditional “Laws” of Learning had been challenged successfully. Even human cognition had been challenged.
PART TWO

REVOLUTION IN ETHOLOGY AND THE BIRTH OF COGNITIVE ETHOLOGY

Overview
Griffin's thinking has evolved from a physiological to a mentalistic approach, beginning with his childhood wonderings about animals and their lives through his "behavioristic indoctrination." Von Frisch's extraordinary 1940s' discovery of honeybees' ability to convey food location information via their dances still has an impact on Griffin's thinking. It culminates in his public statements of animal consciousness, most notably in his 1976 book, *The Question of Animal Awareness* (QAA). Discussions with the philosopher Thomas Nagel are another impetus for the book.

Many criticize Griffin's ideas and interpretations. He replies. The issues include behaviorism, anthropomorphism, misunderstanding of innate behaviors, the limitations of parsimony, the science of the "inaccessible" (i.e. another's conscious content).

A Quieter Revolution Brews: Griffin - From Physiology to Thinking

The Path
"The greatest and most powerful revolutions often start very quietly, hidden in the shadows. Remember that."  

Many, including myself, consider that Griffin's major act of revolution was in 1976 when he published a small book: *The Question of Animal Awareness: Evolutionary Continuity of Mental Experience* [QAA].

But how did Griffin come to this act of revolution? There were decades of his "quieter" acts earlier in his revolution. In demeanor and style, he did not appear a "revolutionary." In 1976, Donald R. Griffin was among those still wearing a jacket and tie, at least in his Rockefeller University laboratory. In the field, a floppy, protective field hat, and sturdy, beige trousers were his attire. Classical music was Don and his wife's choice for concerts. But he was, nevertheless, also a revolutionary ... with long-enduring impact.

Recall his earlier, in my mind, only slightly less cataclysmic revolutions: the astounding findings of bats' extraordinarily sensitive and accurate sonar abilities used not just for navigating the perils of dark caves, but to locate and capture flying insect prey. These entailed totally unimagined senses. Recall the variety of environmental cues that allowed birds to "home" and to navigate while migrating. As Don pursued these investigations, his scientific attitudes were changing. Remember that he had begun, as a young boy, exploring the natural world of the animals who lived near him. This was a particularly easy and appealing activity during the many years when the family home was on Cape Cod, surrounded by meadows, marshes, rivers and ponds. In some publications, written at a much later date, he recalls how, in those young years, he wanted to know what it was *like to be a particular animal*. Later, as an undergraduate at Harvard and a burgeoning young scientist, he was enmeshed, of necessity, in the scientific attitudes of his times. Though he and Professor George W. Pierce had other hopes and other interpretations of the bats' ultrasonic vocalizations, they were conservative in their published conclusions. But he kept questioning the extent of the bats' abilities, and later of the birds. Gradually, even to a “tough” empirical scientist,
the animals he studied revealed more and more abilities; Griffin asked deeper questions about the nature of those abilities. His own expressed scientific attitudes began to shift as can be revealed in a careful reading of his work; a Ph.D. thesis by Richard Nash describes in detail the shift from a more physiological approach in his thinking to a more cognitive one. This was the slower, quieter part of his revolution. But I believe that his curiosity about animal “inner” life was simmering for many decades. The scientific attitudes and practices about publishable material were highly constraining for public statements and writings, but hints were available.

I will mention a few examples. I opened this chapter with an admonition from Griffin from Listening in the Dark, made by him in 1958. I repeat it now: “Excessive caution can sometimes lead one as far astray as rash enthusiasm.” It was cautious to write only in terms of “stimuli” and innate physiological processes. But even in 1958, Griffin was venturing beyond that in his published work.

He reflects in the Prologue to Echoes of Bats and Men, a shorter 1959 version of the highly successful, yet more technical book, Listening. Both books describe bats’ astonishing echolocation abilities:

_I always found small mammals enough like ourselves to feel that I could understand what their lives would be like and yet different enough to make it a sort of adventure and exploration to see what they were doing._

In a 1988 article in American Scientist, Griffin considers his fascination with animals and curiosity about their inner lives as reasons for the work he chose to do:

... As far as I can recall, I became a scientist, because I was fascinated by animals and wanted to know what it would be like to be animals of various sorts.

... It seemed easier to imagine being a mammal or a bird, so I concentrated on them and especially on their migration and orientation ...

Why migration and orientation? It seems to me that such studies could fairly readily be undertaken by the enthusiastic, but practical, teenager. Birds and bats were easily accessible; they lived near where he did. He banded birds, then bats. He then displaced the bats and looked for their return or searched for them another season or year in other locations. All he needed as equipment were bird bands, available from the Federal government for a small fee, and “assistants”/friends he seemed able to cajole into helping him. Such friends might even have a car to carry them all. (When he was slightly older and allowed to drive himself, his supportive, indulgent parents lent him the family’s Model T Ford to provide needed transportation; finally, they just gave it to him.) Don could gather incontrovertible data: a number on a band on a captured bird or bat, recorded for an identified species at a specified location, date, and time and recovered by recapture, again indicating similar types of information. No one could doubt such data, as they might for verbal descriptions of social or much other behavior.

Jack Bradbury, a former DRG “bat” grad student, and subsequently a researcher of many animal species and a longtime friend of Don’s, offers a further suggestion of the genesis of QAA. Any sort of navigation, as well as echolocation itself, requires decision-making: where and when to fly, to turn? How to avoid obstacles, where to go to catch a flying insect? From there, Bradbury sees a potential progression into DRG’s concerns about the animal’s processes of mentation, of their “inner life.” Could be. I think, however, Griffin’s was a much broader interest, begun very early on, about the mental state, the thoughts, the goals and even the feelings of that set of other beings around us, non-human animals. These were the beginnings within a young boy as he
enthusiastically traipsed after those animals of the fields and waters near his home in Scarsdale and then Cape Cod.

As to Griffin’s adult professional years, recall the earlier discussions about bats’ adaptability and ability to learn. For example, while on the Harvard faculty, he had noted the bats’ rapid progress in distinguishing between the edible and inedible bits tossed in the air by his “mealworm gun.” Much earlier, bats’ learning capacity was recognized by Griffin in his 1958 *Listening* book. He did not use terms like “awareness” and “thinking” in that book, at least as noted in the detailed index. Yet one finds items such as “Learning...” even “possibility of in Protozoa.”

In more private writings, Griffin was yet more open in his opinions and speculations about the inner lives of animals. A 1959 letter to the NIH neurophysiologist Wade H. Marshall dealt with the treatment of animals used in scientific research. Griffin notes, “Our scruples are very largely anthropomorphic.” He argues that no physiologist could make a good case of there being any great difference between the physiological likelihood of pain or suffering experienced by a chimpanzee or a rat when applying the same procedures on each. But “we shudder more for the primate than for the rodent.”

Within the same letter, Griffin expressed his views that “consciousness is present as a continuum roughly paralleling the refinement of nervous systems ...” But he saw “no qualitative distinction” anywhere from “man to coelenterate.” (Coelenterates are a group of simple animals, including hydra, jellyfish, sea anemones, and coral animals.)

In May 1969, writing to the RU physiologist Carl Pfaffman, Griffin commented on plans for an NBC TV film on Aggression.

_There is a strong implication in the outline that animals do not “resolve differences” without actual violence and injury. Nothing could be farther from the truth, of course. I should think that a program of this type could well emphasize the gestures and social relationships in mammals at least that appear to limit the occurrence of fighting._

This is not straight “physiological” thinking by Griffin.

In January 1970, another “reveal” of DRG’s thinking, Jim Gould had just completed a "grueling" round of interviews for potential admission into the Rockefeller University graduate school program, the last being with Donald Griffin. As previously noted, Gould, as an undergraduate, had conducted research with colleagues on the controversy concerning stimulus control of the honey bee’s waggle dance. After the interview, as Jim was rushing to catch a plane and as the RU elevator doors were starting to shut, Don asked him, “Do the bees know what they are doing? “I hope not,” a startled Jim feebly replied. He had no idea that Griffin was thinking along such lines.

Over the years, Jim Gould asked Griffin several times about what started him thinking about animal minds. Jim claims that each time he got a different answer. From Gould’s perspective, it was the culmination of many events personally experienced by Don: For example, Griffin was always impressed with his “beloved beavers,” intrigued by the dams they built and their ingenuity in making repairs. But, to Griffin’s dismay, he recognized how psychologists and ethologists continually underestimated animal capacities. Even the concept of “cognitive maps” which seemed useful to Griffin, was not even discussed by many others. That insight was first inferred for rats by Tolman back in 1930 and was used also to describe the representations of chimpanzees.
Gould offers his personal favorite version of Griffin’s various given reasons for starting to think about animal thinking. It had to do with Griffin watching fiddler crabs on the beach in the company of a “remarkable scientist;” that scientist was Jocelyn Crane, who became Don’s wife. The male fiddlers were fighting each other, each brandishing its one huge claw, pushing each other with it, “handshaking” so vigorously that one crab could lift the other off the beach. But it seemed that none were ever hurt; they appeared to be largely unperturbed. “Why do they do it?” mused Griffin. “Because they enjoy it!” pronounced Jocelyn. Griffin claimed to have never given the matter of animal minds a thought before then\textsuperscript{91} ... a tale we know was untrue but a gentlemanly honoring of his scientist wife and her impact on his thinking.

**Philosopher Thomas Nagel asks, “What is it Like to Be a Bat?”**

Don notes, in the Preface to *The Question of Animal Awareness* (1976), a significant impetus for his writing the book at that particular time. Griffin was on the RU faculty and, for a year (1973-1974), the philosopher Thomas Nagel was a visiting faculty member.\textsuperscript{92} At the time, Don was thinking about animal awareness but had not yet “gone public” with the matter. Griffin writes that Nagel “supplied an immediate spur” during that time “when he raised the question of whether animals have mental experiences.”\textsuperscript{93} The issue provoked intense discussions between Nagel and Griffin and within a broader audience of psychologists, ethologists and philosophers, in particular, philosophers of science and of mind. Nagel’s public views were expressed in the widely quoted, influential and provocative article, “What is it Like to Be a Bat?” (1974).\textsuperscript{94} Dennett (1991) considers that paper to “have become perhaps the most famous and influential paper in the philosophy of mind, widely known outside of philosophical circles.”\textsuperscript{95}

Nagel had already presented the ideas to a smaller audience of philosophers at the University of North Carolina Chapel Hill colloquium in 1971, impervious to critics of that talk, such as Daniel Dennett. Nagel had simply *assumed* that bats and other animals, possibly all organisms, were conscious; they had mental experiences. He was not certain whether some simpler organisms had conscious experiences, and he thought that it was most difficult to say generally what provided evidence for consciousness.\textsuperscript{96}

As Dennett notes, Nagel’s choice of animal subject was a sage one. If Nagel had mused about what it was like to be a spider, many might wonder if it was anything at all. And if the wondering had been about a chimpanzee, its perceived closeness to humans and human experience might have made a presumption of mental experiences highly likely to many. Thus, a conscious chimpanzee might not be particularly puzzling. Humans likewise seem to think they know their pet cat’s moods and inclinations. But a bat? It is a mammal, like humans, yet there is an enormous gap; it “sees” a world through echoes of its sounds.\textsuperscript{97}

And, says Nagel, we can never know a bat’s experiences. Those experiences are *subjective*, known only to the entity that has them, and impervious to an outside observer, to an *objective* view. Thus, it is impossible for us to imagine *what it is like to be* a bat. Its senses are different; its brains are different. Though sonar is a form of perception, Nagel states that “there is no reason to suppose it is subjectively like anything we possess.”\textsuperscript{98} (Back to that statement soon). Crucially, Nagel’s criterion for mental experience, for consciousness, is “that there is something that it is like to be a bat.”\textsuperscript{99} He is not asking how a bat behaves, nor how well we can imagine hanging upside down and other characteristics of a bat’s behavior. Says Nagel, “I want to know what it is like for a *bat* to be a bat.”\textsuperscript{100} But since Nagel considers it impossible for us to know the bat’s subjective experience, then
that experience is an *inaccessible* state. Thus, we must acknowledge that there may be *nothing* that it is like to be a bat.

That is the major point of controversy for Dennett and some other philosophers, as is their view that scientists *can* already answer many of the interesting questions about what it's like to be a bat. Scientists know, for instance, that a bat can sense objects, even those as small as insects. Dennett and others objected to Nagel’s lack of arguments supporting the existence of conscious experience in animals. But none of these criticisms delve into Nagel’s concept of the essential subjectiveness of a bat’s consciousness.

I have dwelt on Nagel’s arguments for two reasons: Firstly, they were such a direct impetus to Griffin’s publishing QAA at that particular time, and secondly, they played such a central role in philosophy by setting a criterion for consciousness as “something that it is *like to be* that organism.” Nagel was emphasizing the utterly subjective nature of consciousness, the “hard problem” of consciousness as labeled by Chalmers\(^\text{101}\) (to be discussed later). To some philosophers and ethologists, e.g., Colin Beer,\(^\text{102}\) that high criterion for assigning consciousness to an entity meant that the whole enterprise of attempting to study animal consciousness was impossible. (See further discussion in Chapter 17.\(^\text{103}\)

Despite such concerns by others, Griffin agreed with Nagel’s assumption that animals have mental experiences, but Griffin strongly believed that we could attain, at least, a glimpse of what their felt experiences might be like. In particular, he tried to persuade Nagel that, like bats, blind humans rely on a limited type of echolocation both to navigate and to use before they touch objects; thereby, they gain a sense of those items. Humans make audible sounds to do so, and listen for the echoes; they do not do nearly as well as bats and dolphins. But in Nagel’s view, because bats are so different from humans in their reliance on echolocation, the two are not comparable. Sighs Don, “The best I could do was to persuade Nagel to include a brief footnote about human echolocation in his paper.”\(^\text{104}\)

As was described in the chapter involving the history of bat research,\(^\text{105}\) we see that studies since the 1970s have revealed the extraordinary detail that bats can detect in their worlds through sonar … minuscule objects, huge objects, texture, shape, all integrated to create a 3-D perception of the world. Selective attention both emphasizes relevant aspects and ignores irrelevant stimuli, for them, as it does for us. For Griffin, for myself and many other scientists, such information is a window into a bat’s mind. Nagel did suggest that bats could sense these various attributes of objects, even to the extent of creating a three-dimensional world, but nevertheless, he insists that the subjective experience of the bat is still missing.\(^\text{106}\)

Nagel’s writings have inspired biologists other than Griffin to “take him on.” Jim Simmons did so in a 2018 lecture titled “What is it like to be a Bat.”\(^\text{107}\) He spoke of the auditory and neural mechanisms underpinning the bat’s attention, i.e., the bat’s awareness and its focus on certain aspects of the environment and not others when it constructs a three-dimensional representation of the world. Nagel, I’m sure, would respond in the same way about the absence of subjective understanding.
Griffin Changes His Public Voice – Why?

Don Griffin wrote two memoirs that I have referenced numerous times. In the first, in 1985, he pondered, at the end of that essay, why he had hesitated so long in expressing his views about animal awareness and thinking.

Beginning in the mid-1970s, I have undergone one further change in scientific outlook. Slowly over the years, I had become more and more dissatisfied with the reductionistic and behavioristic viewpoints of my colleagues in biology and psychology. In particular I had begun to doubt the wisdom of totally ignoring the possibility that animals might experience conscious thoughts and subjective feelings. This has led me to attempt to launch a sub-discipline of cognitive ethology.108 ...

... I have often wondered in recent years why it took me so long to speak up on this subject. I believe the reason was my early indoctrination in the positivist climate of science at Harvard and elsewhere in the 1930s. Many scientific developments and much shaking up of prior ideas were necessary before I was ready to think seriously about the thoughts and feelings of animals. Hindsight is always easy, and perhaps I am simply swimming with a changing tide in the history of ideas. It does seem that my firsthand involvement in several surprising discoveries is what prepared me to shift my thinking into new and I hope fruitful channels.109

Those discoveries were foremost his research with bats. In particular, he was impressed in that summer of 1950, when he recognized and even gathered early data establishing that flying bats used echolocation to detect and capture insects on the wing. Recall that Griffin considered this his most significant research achievement.110

Von Frisch’s work of the 1940s111 had also simply astonished Don as it revealed the honeybees’ ability to communicate to others via a deceptively simple “waggle dance.” The recipients could then fly the correct distance and direction to the specified location for food. Griffin was instrumental in bringing von Frisch to the USA to lecture about his research and in convincing Cornell University (then Griffin’s academic “home”) to publish a translation of von Frisch’s honeybee studies. When Griffin was interviewed for an RU Research Profile (1985-1986), he credited von Frisch’s discoveries as inspirational for him, setting off ideas that then simmered for a long time. Simply “shaken” from his “reductionistic complacency,” by von Frisch’s findings, Griffin began to wonder “whether animals might not be doing a lot of things we never imagined they could do.”112

In the later memoir of 1998, Don additionally emphasized dissatisfaction with reductionist behavioristic attempts at explaining animal behavior.113 That view of Griffin is corroborated by others who knew him114 and by his numerous (civilly and politely) exasperated statements in QAA and his following books.

Griffin’s Status and Critics

Finally, in 1976, Griffin came to publish his views in The Question of Animal Awareness (QAA), a very short book, only 105 pages long with another 27 pages of references and an index. Colleagues were outraged. Some thought he was becoming senile; he, all of 61 at the time.115,116 This was an internationally renowned, hard-nosed scientist whose careful, detailed study of bats’ sonar capacities had been cited over and over, inspiring the work of many. He had been a full professor at Harvard, chair of their Biology Department, enticed by RU President Detlev Bronk to create and direct a new Institute, The Institute of Animal Behavior (IRAB) at Rockefeller
University. Awards ... innumerable!!117 Suggesting that animals might be aware, be conscious, might think, was heresy. And that scientists should be studying such supposed phenomena? ... an enormous step backward for the field! His critics made reference to Romanes, the post-Darwinian chronicler of animal thinking who overstepped, who reported not only carefully observed behaviors and animal problem-solving, but anecdotes from almost any source.

After publishing QAA, Griffin's private and public views grew yet more and more accepting of the likelihood and extent of animal cognition and awareness. Even the titles of his three books about Animal Consciousness portray this shift as do the discussions within those books. The first book, in 1976, was titled *The Question of Animal Awareness: Evolutionary Continuity of Mental Experience* (QAA) and was substantially revised in 1981. The second, in 1984, an even more massively revised and enlarged book, was simply *Animal Thinking* (AT). And the third in 1992 was *Animal Minds* (AM), with an updated and revised edition in 2001, retitled to be: *Animal Minds: Beyond Cognition to Consciousness* (AM-C), his *magnum opus*. Yet, his very last publication,118 published posthumously in 2004, was quite constrained. This was a paper with Gayle Speck in the journal *Animal Cognition*, the very journal that had begun publication in 1998 with his writing as the lead article.119 The endless critiques by reviewers before the 2004 publication forced "toning down," if the paper was to be published.

Likewise, the resistance to his thinking simultaneously grew still more formidable and also, in different quarters, slowly more accepting. Yet, even within his own laboratory, not all the lab members accepted Griffin's views. Ronald Larkin, who was Griffin's enthusiastic partner in radar studies of avian migration, was asked by Don about his views on animals' awareness of their activities. Ron reported that he gave a decidedly negative reply, considering the matter a non-productive area for research. After that, Don did not again raise the issue with him.120 Recall the incident with Jim Gould, who was taken aback after his graduate school interview with Griffin, when Don asked him, referring to the honeybees' waggle dance, "Do the bees know what they are doing?" Jim recalls a lame reply: "'I hope not.' I had no idea that he was thinking along those lines."121 Jim did acknowledge, decades late, that he had come to accept that bees were conscious, but he "wouldn't give Don the satisfaction of admitting such."122

Other associates were far more accepting. Gordon Burghardt is proud to claim he was Griffin's first post doc in Cognitive Ethology (1977-78), while working also with the chemosensory physiologist RU Professor Carl Pfaffman at the other end of Smith Halls' 5th floor. Burghardt was interested in snakes' chemical perception, but curious about animal sentience, in particular, the consciousness and behavioral control questions raised by a two-headed black rat snake he studied. Griffin's promoting concern with such topics encouraged Burghardt (and others) to dare study topics such as play behavior, including its cognitive and emotional components.123

Griffin's student, Peter Tyack worked directly with Roger Payne, studying the communication and social behavior of humpback whales (*Megaptera novaeangliae*). Peter Tyack agreed that whales were conscious entities, though the nature of whales' consciousness was not the focus of his studies.

I arrived from an earlier, largely behavioristic graduate training in experimental and physiological psychology, but my own Ph.D. thesis work and postdoctoral studies had shifted to studies of communication. Then I had written extensive reviews of the ape language and cognition
projects; I was both wary of unsubstantiated claims while also fascinated by some remarkable ape abilities. So, I was both impressed by Griffin's daring in assuming such widespread animal consciousness and concerned about approaches to gathering evidence. We agreed on many issues and happily argued about some as well.

Within the broader scientific community, even Griffin's harsh critics did acknowledge his eminence within science, particularly in the field of animal orientation and navigation. No one contested that he was highly regarded and had already achieved a half-century of productive research along with recognized honors for his work. In The New York Times Obituary, Griffin was credited as "the only reason that animal thinking was given consideration at all." Peter Marler, his colleague in the RU Animal Behavior group said,

That someone of Don's reputation dare to reopen such questions... sparked a minor revolution among zoologists about whether or not the animal is a proper subject for research.

Although Griffin's first "consciousness" book, QAA (1976), managed to arouse much interest and controversy, some reviewers remarked that the book was too short. Don had tried to include much more, but apparently, the editor was becoming exasperated with Don's continual additions. Brock Fenton, a bat researcher taking his sabbatical (1976-1977) at RU in Griffin's group, recalled how he had tried to tell Don about a journal article describing some new research. Don waved his hands, saying, "No, the editor says I'm not allowed to read anymore!"

One reviewer, indeed a friend of Don's, Jack Hailman, was also critical of the book and wanted "one argued more closely, more widely and more deeply to reflect the author's marvelous scientific intellect." Griffin took these and the other critiques to heart. Not daunted, he greatly expanded the pertinent evidence from animal behavior and communication in the enlarged QAA-2 (1981) and particularly in Animal Thinking (1984) and the subsequent Animal Minds and its revised expansion. For example, in the original QAA, evidence of awareness from communication had focused to a great extent on the honey bee's waggle dance and the ape "language" and cognition projects. Afterwards, the organization and content of the books changed significantly, including a comparative review of evidence suggesting animal consciousness. These included anecdotes as well as data from rigorous observations and experimental findings. Critics still perceived Griffin as "iconoclastic" and challenging accepted science; several of the critiques are noted in an excellent review/critique of Griffin's work by the environmental philosopher Eileen Crist. However, as Colin Beer commented, Griffin's including new lines of evidence and a richer database in Animal Thinking made a greater impact on the field; the later Animal Minds had an even greater effect.

In QAA, Griffin described the philosophical concern, the solipsistic position, that we cannot demonstrate the reality of another human being's inner experiences; we face a similar or greater problem with animals. But he entreated the reader, the scientific community and the layperson to "accept the reality of our own mental experiences, even without rigorous proof." In later editions, he was forced by critics to deal with more philosophical and conceptual issues concerning animal mental experience, but never sufficiently in the views of some. For example, in a Harvard University Press in-house review of a proposed revision of Animal Thinking, Colin Beer noted that these theoretical issues were arranged as the book's "end-pieces," the introductory and final chapters. In between were the instances of animal behavior that, in Griffin's assessment, support animal awareness. But, in Beer's view, Griffin should "represent the conceptual problems more
thoroughly,” so that he balanced advocacy with “critical ballast.” Although Beer recommended that the revised edition be published by Harvard University Press,134 there was no such edition. Instead, the University of Chicago Press published the next book, Animal Minds (1992) and the next, updated and enlarged revision (2001). Both books dealt with expanded philosophical considerations. An additional chapter in the 1992 edition on the “Significance of Animal Consciousness,” included philosophical and ethical issues and a section about the scientific significance of animal sentience. The 2001 edition was yet further expanded with two chapters: “The Philosophical and Ethical Significance of Animal Consciousness” and the “Scientific Significance ....” However, whatever the critics’ views of the essential issues, Griffin saw his central task as convincing people that animals are conscious, or, at least, are most likely to be, and that scientists should be studying such mental states. At the time, the specific concepts an animal might be using were of interest, but not his central concern.

Critics’ Arguments and Griffin’s Responses

In QAA, Don first conceived his task as:

... I shall concentrate on images, intentions and awareness of objects and relationships in the outside world rather than on feelings and purely subjective qualities. My reason for this choice is that I can see more realistic hopes of developing objective methods for gathering satisfactory data about the former than about what psychologists have called "raw feels" (Tolman, 1932).135,136

He added that he would return briefly to considering subjective feelings later in the book.

The kinds of arguments advanced against Griffin’s ideas about animal awareness and consciousness were basically of a few sorts. And, as Jim Gould has noted,137 Don would delight in turning the criticisms back against the critics. Colin Beer, in suggesting approaches that Griffin could take to avoid being vulnerable to hostile critics, then remarks: “On the other hand, Don Griffin seems to suffer criticism gladly; indeed, he seems to thrive on it.”138 Eileen Crist139 has outlined the argument types in her helpful review of Don’s books on consciousness and the responses to them by the scientific community. I rely heavily on her work in the following discussion.

1) Behaviorism:140

Griffin deemed that most critics were worn down by the long reign of behaviorism and indoctrination into that mindset. Thus, they described behaviors as unconscious reactions, as a series of “mindless” rather than “mindful” actions. Griffin considered most behavioral scientists as “inclusive behaviorists,” for they were willing only to describe observable behaviors, but not delve into the mental experiences associated with them. To such scientists, animal awareness was either “epistemologically inaccessible” or “empirically nonexistent.”

As part of this line of criticism, explanations were offered by critics in terms of “associative learning.” But as various philosophers of science have argued, “It is agreed upon that associative processes can be constructed post-hoc for every experimental outcome.”141 Associative learning (as applied to animals’ problem-solving abilities) “does not produce clear behavioral predictions that can be falsified...” Thus, such hypotheses are weak as explanations.142 Although these particular quotations are recent, that general sense of frustration with the never-ending “associationist”
arguments was certainly held by Griffin and many others attempting more mentalistic understandings of animal behavior.

2) Anthropomorphism:

Claims of anthropomorphism were another set of wrathful judgments hurled against Griffin. But to Don, these arguments simply exposed the fear of anthropomorphisms that such critics held. They feared attributing specifically human traits to animals. But as Griffin emphasizes in QAA and even more forcefully in his subsequent writings, he is not claiming that conscious attributes would be the same in non-humans, but making analogies to humans is a starting point for further investigations. There is no reason to believe that any mental experiences animals may have must be similar to our own. To claim such, states Griffin, is truly anthropomorphic, a conceit asserting that only humans can have conscious experiences; that no animals can have even simple thoughts about what is important to them.

[This view has come to be termed “anthropocentric” with humans as the touchstone for consciousness. In further developments, some do accede to the possibility that animals might have some mental capacities. But any such abilities must be understood and judged relative to the “gold standard” of human mentality. Yet another biased and limiting view of potential animal cognition and sentience.]

In Animal Minds (both editions), Griffin cites the detailed analyses of the philosopher J. A. Fisher who concluded that there is no fundamental philosophical basis for the taboo against anthropomorphism as conceived by most scientists. Jonathan Bennett had earlier come to similar conclusions. Bennett was among the group of philosophers who interacted with investigators in the new field of Cognitive Ethology.

When we humans communicate to other humans what an animal might be experiencing, we need words to do so. But by Griffin’s using such terms, the critics were quick to claim that he was asserting an identity in conscious states. In later writing, Don used terms like “critical anthropomorphism.” Still later, he emphasized these points by creating new labels and distinguishing between anthropomorphism, a slippage into “mechanomorphism,” (his term for behavioristic, reflex-like explanations of behavior) and a “balanced ‘zoomorphism’” which he espoused.

3) Inhibiting Assumptions

A. Cartesian Views:

In Griffin’s view, certain assumptions held by many of his critics also blocked or inhibited inquiry. Crist discusses those assumptions in her review, and I shall again draw upon her work. Prime among these was the uniqueness of humans, the radical separation of man and beast. These old Cartesian views certainly held less sway than in the days of Descartes and during the height of Positivism and Behaviorism, but they persisted. Hence, as we shall see, combating Cartesianism was part of the reason for Griffin’s intense interest in animal communication, including the artificial language projects that permitted apes and some other species to communicate to a degree with humans. Scientists were perennially searching for the ultimate barrier that separated humans from others, but barriers such as tool use had already fallen away, and cracks were appearing in assertions that language was a uniquely human attribute.
B. (Mis)understanding Innate Behaviors

Genetically based, even learned behaviors, likewise dominated his critics’ thinking in rather peculiar ways. Many assumed that if a behavior was genetically based, it was automatic and rigid. This bias existed, despite all the evidence to the contrary, even in 1976, of both the voluntary control of many such behaviors and the frequent learning involved. Even certain environmental contingencies were sometimes needed for specific behavioral expressions; genes indicated probable behavioral development when the organism could experience specific conditions. My favorite example of this was conducted decades ago. A female rat was housed in a cage totally devoid of any objects to carry. Such carrying experience was necessary, as I recall, for the eventual appropriate caretaking of the young, including moving and carrying them. The deprived rats found a solution: they carried their tails.\textsuperscript{152}

Another tactic of critics was to find an example of innate, stereotyped behavior used in a situation where it is counterproductive or ineffective in that circumstance. The apparent lack of cognition in that scene is then used to deny the animal’s overall cognitive and/or conscious abilities. Beaver’s dam-building and repair were common targets. As Griffin reports in \textit{AM-C},\textsuperscript{153} in some studies, the sounds of rushing water were played over a loudspeaker, often situated on a dam site.\textsuperscript{154,155,156} The sounds at least attracted the beavers’ attention, and beavers did place piles of sticks near the speaker, but often only after a considerable period of time. That behavior does seem a useful stimulus-provoked response to a puzzling situation, a helpful, likely innate reaction. However, it is also the case that beavers choose appropriately sized stones and modify pieces of wood to plug holes in their dams as necessary. (Beaver “engineering” is discussed more fully in Chapter 18.\textsuperscript{157})

And as to consciousness, whether or not a behavior was genetically determined, an organism could, nevertheless, be \textit{aware} of what it was doing: A bird building its nest could be aware of what and how it was constructing that nest.

C. Prejudice Against Lower Animals

Finally, a third “killer” assumption was prejudice against consciousness in lower animals, particularly invertebrates and especially insects. But, as Colin Beer observes, Don was taking “the whole animal kingdom as his province,”\textsuperscript{158} not merely the mammals or even just vertebrates. Griffin is careful to offer innumerable examples, particularly in his later books, of very complex and adaptive behavior by insects and other invertebrates. These include the various hunting strategies employed by some predatory, jumping spiders of the genus \textit{Portia}, who trap and devour other insects, particularly some spider species\textsuperscript{159,160,161} (to be described more fully later). As Griffin remarks, if these were the activities of a monkey, there would be little hesitation in considering them as conscious, planned, variable and adaptive hunting techniques, but scientists found it difficult to do so for a spider.\textsuperscript{162}

Further to Don’s interest in insects were, of course, the honey bees, which, he noted, were literally inspirational for him.\textsuperscript{163} They employed their waggle dances not only to communicate the distance and direction of food but with versatility. The dances might indicate the location of water for a thirsty hive, or of pollen or nectar, whichever was needed (pollen providing protein for the bees), or of a potential new hive site when the community members were gathered in a swarm and in the process of moving. On this issue, a few critics, in particular, Adrian Wenner, persisted in
claiming the bees do not use potential information from the dance to reach the food source the scouts had visited.\textsuperscript{164} (This issue has already been discussed in detail.\textsuperscript{165})

D. Parsimony Violated

Violation of parsimony was the fourth deeply held conviction expressed by some critics. Griffin deals with this concern in at least two ways. Firstly, he recognizes that an animal faces an often-unpredictable world. From an evolutionary point of view, it would appear far simpler and aid an animal’s survival, if it could consciously think about alternative choices, rather than rely on innumerable rigid programs created during evolution that might be able to deal with a situation. The problem is especially acute when an animal confronts a novel circumstance. And secondly, consider what is understood about the “Law” of parsimony. Occam’s Razor, often invoked in the same breath, is a possible means of approaching a difficult issue in science, not either the goal or the actual explanation. Our bodies and those of animals are very complex. For example, we have several backup systems to protect our breathing and other essential physiological functions. Though, as scientists, we would prefer the aesthetics of simple explanations, that is almost never the case.

E. Science of the “Inaccessible”

And perhaps most adamantly expressed was the conviction of his critics that investigating animal mind is scientifically impossible, because conscious mental states are “fundamentally inaccessible,” or, even further, they are inaccessible because such states do not exist in animals. The impossibility of ever understanding the essential subjectivity of a conscious state, is, likewise, Nagel’s concern. In 1929, the early behaviorist psychologist, Watson, had written that mental activities were subjective and beyond the reach of science.\textsuperscript{166} Other behaviorists had hoped some means of dealing with such matters could be developed, but, as Griffin notes in QAA,\textsuperscript{167} later behaviorists remembered only the rejection of mental states. In the second edition of Animal Minds, Griffin considers the “animal cognition” research that was underway, much begun after QAA. Among other studies, he notes the work of Edward Wasserman, an experimental psychologist well known for his laboratory studies of pigeon cognition. One of Wasserman’s papers is titled “Conceptual Behavior in Pigeons”\textsuperscript{168} Conceptual “behavior?” Mental terms were to be avoided at all costs. This was just one example of the many in animal cognition studies in which animals were granted only “unconscious cognition.”\textsuperscript{169}

Don Griffin remarks that the same “inaccessibility” pertains to the consciousness of other humans. Yet we go about our daily lives, making inferences about others’ beliefs and motives, inferences necessary for our survival. We have to make some best estimates of what another is thinking (the philosophical problem of “Other Minds”). Similarly, with animals, can we not draw plausible inferences, based on animals’ behaviors, as to their mental states, their goals and wants and understandings? The essential challenge, states Griffin, is to develop ways to best make such inferences. Always, he takes care to claim only that the inferences are suggestive evidence for a particular mental state. He never claims that he or anyone can find irrefutable evidence that a specific behavior is conclusive proof of an accompanying mental state.
ENDNOTES
ENDNOTES FOR PART ONE BEGIN HERE

1 Donald R. Griffin (DRG), 1958a, *Listening in the Dark*, p. 69. [Pagination is the same for Arabic numerals in the 1974 reprint.]


3 This phrase and slight variants are commonly attributed to Emma Goldman (1869-1940), an activist of the late 19th and early 20th centuries. Goldman scholar, Alix Kates Shulman, cannot, however, find it in Goldman’s writings. Shulman notes that a similar sentiment was expressed in Emma Goldman’s autobiography (1934), p. 56. Discussion in “Dances with Feminists” by Alix Kates Shulman, December 1991.


5 Mick Gold, 2002, p. 43. ‘The answer, my friend, is blowin’ in the wind’ has been described as “impenetrably ambiguous” ... notes Mick Gold in a magazine article about Bob Dylan in 2002. Yet Dylan himself interpreted the words when the song was published in 1962 in *Sing Out*, a quarterly journal of folk music and folk songs, published from 1950-2014. “It ain’t in no book or movie or TV show or discussion group. Man, it’s in the wind.” Like a sheet of paper, it falls, and nobody picks it up and then it flies off again. For Dylan “some of the biggest criminals” are the ones who ignore the wrong they see. Quoted in Michael Gray’s 2006 *Encyclopedia of Bob Dylan*.

6 STD refers to sexually transmitted diseases, such as the “clap” (Gonorrhea) or worse, syphilis, or worse yet, AIDS which was beginning to spread without much awareness in the late ’60s.

7 Unfortunately, despite my searches, I cannot find a reference for this comment nor do I recall anything more about the source.

8 I have been unable to locate this news article on the New York Times website.

9 Griffin’s assistance to Janna Lipmanova and her subsequent work with the US aerospace program is described in Volume Two - Chapter 13, “Afghani Art and Russian Bats,” section, “A Woman Scientist Escapes the Soviet Union and Saves the US Space Program?”


13 Elizabeth Scarborough, September 2010.

14 Elizabeth Scarborough, September 2010.

15 NOW, n.d.

16 Timothy C. Williams, personal communication, October 2, 2020, E-mail. Unless otherwise specified, the quotations from Williams and his recollections are all contained within that E-mail.


18 See Appendix in Volume Two for a very brief biographical sketch of Ann M. Graybiel.

19 Ann M. Graybiel, (n.d.).

20 Julia Chase, personal communication, October 18, 2021, E-mail.

21 Julia Chase n.d., ms. in preparation for autobiography

22 Julia Chase, n.d., ms. in preparation for autobiography.

23 See further discussion of aspects of this work conducted by them at Simla in Trinidad.

24 Roderick Suthers, Julia Chase and Barbara Braford, 1969.

25 Jeré Longman, October 25, 2011. The 2011 Thanksgiving Road Race in Manchester, Connecticut was the 50th anniversary of the same race Julia Chase-Brand ran in 1961.

26 HEW was a cabinet-level department of the US government from 1953 to 1979, when, under President Dwight D. Eisenhower, the Department of Education split from HEW and the Department of Health and Human Services was formed. Information was retrieved on September 25 from HHS.gov.


28 Christiana M. Leonard, 1972, p. 3.


32 These senior women were Professors Marilyn Farquhar and Rebecca Lancefield.
35 Janet Abbott, personal communication, May 28, 2005, E-mail. Included in the email was a copy of the speech that Janet gave at her mother’s memorial.
36 Further discussion of the lives of these women and others significant in Griffin's personal/professional life may be found in Volume Two - Chapter Fourteen, "Behind the Man: Significant Women."
37 Josef Michl, personal communication, September 28, 2021, conversation. Professor Michl was formerly a member of the Rockefeller University faculty.
39 Sharon Ford Hagopian and Vasken Hagopian, personal communication, March 12, 2019. Telephone interview. The Hagopians, are both physicists from the University of Florida, Tallahassee, working also on nuclear projects at CERN and the Fermi Laboratory. They related to me the discoveries in atomic and nuclear physics. Any errors are mine.
40 Max Roman, n.d.
41 Francis W. Irwin, 1963, Handout announcing the meeting of “Dr. Irwin's Informal Friday Night Seminar” for October 11. Document in private collection of Carolyn A. Ristau.
45 René Descartes, 1649; translated and reprinted 1989.
46 Jakob von Uexküll, 1909.
48 I purposefully use the pronoun “he” and the nouns “human” and “animal” rather than “he” or “she” and "human” and "non-human animals" for they reflect the customs and beliefs of the times.
49 J. B. Watson, 1930, p.82.
50 Aristotle, (c. 350 B. C.), De Anima, p. 429b29–430a1 In Hicks, R. D. (Ed.), 2015.
51 Tolman, 1925.
52 Donald O. Hebb, 1960.
55 Frantz Fanon, 2008.
56 John Garcia and Robert Koelling, 1966, p. 123
58 Paul Rozin and James W. Kalat, 1971, p. 481.
61 H. Wilcoxon et al, 1971. Bobwhite quail (Collinus virginianus) were the subjects in these experiments.
64 Kendra Cherry, 2019.
65 Martin E. P. Seligman and Steven F. Maier, 1967.
66 Steven F. Maier and Martin E. P. Seligman, 2016, p. 349. In this publication, published 50 years after the original 1967 article, the authors state that their “original theory got it backwards.” Citing neurophysiological evidence, the authors now claim that passivity to prolonged aversive events is the unlearned, default, innate response.
68 Paul Rozin, personal communication, April 23, 2021, E-mail.
69 Ulric Neisser, 1967.
70 Ira Hyman, 2012.
ENDNOTES FOR PART TWO BEGIN HERE

72 DRG, 1976, Question of Animal Awareness (QAA).
73 Note quotations in the following paragraphs from Griffin’s 1959, *Echoes of Bats and Men* and a 1988 article in *American Scientist*.
74 Richard Nash, 2016.
79 Jack Bradbury, personal communication, March 5, 2022, E-mail.
80 Volume One- Chapter 11, Part 1, “Griffin’s Lab: The People The Projects,” in section, “The Shape of Things: Capturing Adhesive Rolls and Mealworms and Getting Better at It (Wings Help).”
81 DRG in 1974 (also 1958a), *Listening in the Dark*, p. 408.
84 Donald R. Griffin to Carl Pfaffman, 17 March 1969, Series 1, Box 11, Folder 113, Rockefeller University Laboratory of Animal Behavior, Griffin, Correspondence, Donald Redfield Griffin Papers, Rockefeller University Archives, RAC.
85 This incident and Gould’s undergraduate research are described more fully in Volume Two - Chapter 12, Part 5, “Griffin Lab Research: Early RU Years,” section, “Research by Griffin Lab Members,’ Subsection, “James (Jim) L. Gould (And Bees That Lie.”
90 Edward C. Tolman, 1948.
92 Encyclopedia, February 12, 2021. Most of Nagel’s academic career was at Princeton University and later at New York University.
95 Daniel C. Dennett, 1991a.
97 Daniel C. Dennett, 1991a, p.1-2. The ideas in the preceding paragraph are drawn from Dennett’s paper.
100 Thomas Nagel, 1974, p. 439.
104 DRG, 2003, GEOMAY03, unpublished draft of paper later appearing in German in a popular magazine or book.
105 Volume One - Chapter 11, for example, Part 2, section, “And Then There Was Jim Simmons.” See also Part 4, “The Present And Future For Bat Echolocation Research (and devices).”
107 James A. Simmons, 2018. A short precis of the talk is available in this reference as is a link to a video of the lecture.
108 DRG, 1985, Memoir in Dewsbury, p. 140.
109 DRG, 1985, Memoir in Dewsbury, p. 140.
110 Alan D. Grinnell, personal communication, April 30, 2019. Telephone interview.
111 Waggle dance, n.d. Karl von Frisch had first conducted field experiments with honey bees as early as 1923. He had noticed that bees in the hive were taking some kind of cues from returning foragers that excited them to leave the hive and find an artificially placed food source.


113 DRG, 1998a.

114 Timothy C. Williams, March 28, 2021, personal communication, E-mail. This is one example that many viewed Griffin as exasperated with reductionistic explanations of animal behavior.


117 See Griffin Timeline in Appendices for a list of Griffin’s awards.


119 DRG, 1998b.

120 Ronald D. Larkin, personal communication, June 14, 2019. Telephone interview.


123 Gordon Burghardt, personal communication, June 17, 2022, E-mail.

124 Carolyn A. Ristau and Donald Robbins, 1982.


126 Carol Kaesuk Yoon, November 14, 2003.


130 Marc Bekoff, 1993, p. 166.

131 Eileen Crist cites several critics, namely Colin G. Beer, 1977 and 1984; Robin Dunbar, 1984; Frank Graham, 1984; Louis M. Herman, 1985 and others in the course of her review. I refer the reader to her excellent paper. I would add other critiques/discussions of interest about Griffin's views of Cognitive Ethology, including Marc Bekoff & Colin Allen (1997), Dale Jamieson & Marc Bekoff, January 1992, and my own, including Carolyn A. Ristau (2013) among those of others.


133 DRG, 1976, QAA, p. 7.


139 Eileen Crist, 2008, p. 5, p. 7-9. (Online article has no page numbers; these are approximate numbers from creating pages beginning with “1.”)

140 DRG, 1976, QAA, p. 63-68. Griffin’s discussion of Behaviorist Objections and the others listed in my description are part of Chapter 5 in QAA, titled “Objections and their Limitations,” p. 63-79. Later books entail further elaborations of Griffin’s arguments.


144 DRG, 1976, QAA, p. 6.


149 For other philosophers' involvement with issues pertinent to animal cognition and sentience, see Volume Three – Chapter 17, “A Brief Foray Into Philosophical Issues Concerning Cognitive Ethology.”
151 Eileen Crist, 2008.
152 Unfortunately, I cannot find the relevant reference. Lacking such, the reader may choose to disregard my memory from decades ago, or entertain it as a possibility.
153 DRG, 2001, AM-C, p.107 is a description of these experiments. Beaver lodge and dam building are discussed p. 99-112.
156 Richard, 1980.
157 Volume Three – Chapter 18, Part 4, “Beavers …”
160 Wilcox and Jackson, 1998.
161 Wilcox and Jackson, 2002.
163 Fulvio Bardossi and Judith N. Schwartz, 1986.
165 Volume Two – Chapter 12, Part 5, “Griffin Lab Research: Early RU Years,” section, “James (Jim) L. Gould (And Bees That Lie).”
166 J. B. Watson, 1929.
167 DRG, 1976, QAA, p. 64.
CHAPTER SIXTEEN

REVOLUTIONS

“Excessive caution can sometimes lead one as far astray as rash enthusiasm.”
(by Donald R. Griffin in Listening in the Dark, 1958)

[Chapter Sixteen has three Parts: This section contains Part Three.

Part One: Cultural and Scientific Revolutions (1960s and ‘70s)
Part Two: Revolution in Ethology and the Birth of Cognitive Ethology
Part Three: Evidence for Animal Consciousness (Griffin’s and Later)]

PART THREE

EVIDENCE FOR ANIMAL CONSCIOUSNESS (BY GRIFFIN AND OTHERS)

Overview
Griffin deals with defining “conscious,” “mind” and related matters. He creates a Table of “O.K” to “Taboo” terms used by comparative psychologists and others studying animal behavior as they attempt to avoid mentalistic phrases.

He suggests strategies for gathering evidence of animal consciousness: 1) Possible Neural Correlations of Consciousness, 2) Versatility in Meeting Novel Challenges (e.g. predation, tool making and use, learning concepts, constructing cognitive maps, etc.), 3) Deception and Manipulation, 4) Natural Psychologists (i.e. social behavior and relationships), and 5) Artificial and Natural Animal Communication. Griffin is especially impressed by the possibilities of two-way communication between humans and animals (e.g. apes and a parrot).

Griffin reinterprets various results from experimental psychology research to offer support for animal consciousness.

In major conferences he spreads his “heresy” and encounters resistance. Griffin remains perplexed as to why so many of his fellow behavioral scientists and biologists cannot be convinced of animal consciousness.

Thinking Animals?
Some observations could not forever be ignored. Victorian gentlemen walked in the woods and hunted. To do so, required knowing much of the hunted animal’s behavior. Where does it hide? What lures it out? How and what does it hunt? What seems to alert others of a hunter’s presence? The gentleman saw that his own and the animals’ hunting success was a challenge for each. Furthermore, the hunter and his family often had other pets besides their hunting dogs ... and the humans typically became attached to the animals, spent a fair amount of time observing and interacting with them. These pets were not such simple entities; they were often difficult to understand; they were intriguing.
Science was also finding animal behavior intriguing. Darwin’s *Origin of the Species* (1859) laboriously detailed the evolution of beast and men. He was met with outrage, of course; the thinking was anti-religious. It showed no sharp boundary between mankind with a soul and the beast without one. In fact, Darwin did not mention a soul. That book, that thinking, as well as that of his contemporary and proponent of evolution, Alfred Russel Wallace, gradually gained more acceptance. Even in Darwin’s *The Descent of Man* (1871), his last two chapters deal with the mental life and mental evolution of animals. Writing at length about such animal mental powers as curiosity, attention, imagination, reason, abstraction, and self-consciousness, he summarizes: “…the difference in mind between man and the higher animals, great as it is, certainly is one of degree and not of kind.”

Yet another book Darwin wrote was largely ignored; it was even more diabolical. In his 1872 book, *The Expression of the Emotions in Man and Animals,* Darwin proposed that animals, like man, could not only think, but furthermore, they experienced emotions. Many a drawing portrayed a similar snarl or gaze of contentment for chimpanzee and human. *Origin of the Species* remained a known work, but *Expression of the Emotions* began to disappear among the many forgotten books of humankind.

**Definitions**

Griffin struggles first with a definition for “Consciousness,” which like “awareness,” “sentience” and “mind,” seem to have no satisfactory definition and none agreed upon by all who use the terms. In QAA, he offers some “rough-and-ready unsophisticated definitions” which he believes will suffice in these early stages and will suggest possible ways to explore the field of ethology, or rather his newly termed field of “Cognitive Ethology.” As for “mental experiences,” he states the “obvious fact” that “all of us have them.” He emphasizes our ability to think “about objects and events remote in time and space,” thus distinguishing the events from simple ongoing sensations. A “mind” has such experiences. For Griffin, “awareness is the whole set of interrelated mental images of the flow of events …” In QAA, Don suggested that “The presence of mental images, and their use by an animal to regulate its behavior, provide a pragmatic, working definition of consciousness.” He further emphasizes that he shall … concentrate on images, intentions and awareness of objects and relationships in the outside world, rather than on feelings and purely subjective qualities. My reason for this choice is that I can see more realistic hopes of developing objective methods for gathering satisfactory data about the former than about what psychologists have called “raw feels” (Tolman 1932).

He notes that he shall return briefly to animals’ subjective feelings. By 2002, Don used “conscious” to mean “subjectively experiencing feelings or thoughts, however simple and basic or elaborate and subtle they may be.” And in his last paper, with Gayle Speck (2004), he wrote “Consciousness is the subjective state of feeling or thinking about objects and events.” In short, his definitions all hovered around the same “common-sense” meanings.

Despite the confusion in use of terms by many, particularly of “consciousness,” all agreed there were many levels and possible attributes of consciousness. Griffin’s modus operandi was “here’s a working definition;” let’s get on with it and not get stymied by “paralytic perfectionism.” Others, however, considered the whole enterprise deeply flawed if all could not agree upon definitive statements and empirical definitions. I shall discuss later both some distinctions Griffin
made about levels of consciousness and some philosophical concerns about the need to create a conceptual framework for Cognitive Ethology.

Despite the taboo (Griffin’s term) against psychologists, ethologists and other purveyors of animal behavior to deal with animals’ consciousness, such scientists actually employed a range of concepts that ranged from acceptable to taboo. Don set them handily in a table from “O.K.” to “Taboo.” Near the O. K. end were terms such as “Pattern Recognition and “Neural Template,” through some middle ground entries as “Expectancy,” “Internal Image,” “Understanding” and “Intention” all the way to concepts of “Mind (Mental),” “Choice,” “Free Will” and “Consciousness.” Again, in short, it was very difficult for animal behavior researchers to totally avoid thoughts of animal thoughts, so these various attempts and terms had emerged.

### Strategies for Gathering Evidence of Animal Consciousness

But Griffin wished to gather evidence for animal consciousness and mental experiences. What strategies might be employed? Beginning in QAA, and stated more explicitly in his later writings, Griffin proposes essentially three promising approaches. Eileen Crist (2008), among others, reviews some of Griffin’s examples, while Carolyn A. Ristau wrote later reviews (2013 and 2022) discussing Griffin’s strategies and evidence and updating the field. Marc Bekoff and Chris Allen undertook similar work reviewing and analyzing Griffin’s approach.

Griffin’s three primary approaches are (1) possible neural correlates of consciousness (NCC), 2) animals’ versatile behavior in meeting novel challenges and (3) animal communication, which Griffin considers a potential “window” into animal minds. He states his overall strategy explicitly in his last publication (2004) describing the latest findings that support animal consciousness:

*Though no single piece of evidence provides absolute proof of consciousness, this accumulation of strongly suggestive evidence increases significantly the likelihood that some animals experience at least simple conscious thoughts and feelings.*

He proposed that the task for those who wished to understand animal mentation and behavior was to adopt the truly neutral stance of assuming that pA = .5, where pA is the probability of awareness given the evidence provided by an experiment or, more generally, by the entire body of accumulated evidence. Evidence can either decrease pA probability towards the “straitjacket” of extreme behaviorism or increase it towards a likelihood of animal awareness and consciousness. A pA value of 1 meant the evidence was sufficient to establish that the animal was aware. It is, in effect, “proof” and Griffin is careful to emphasize that it is most unlikely to be able to prove that an animal is conscious in some specific instance. To assume that animals are not conscious, as Griffin’s critics tended to propose, is already biased, as is the other assumption of presumed consciousness. Griffin does note that the assumption that an animal might be conscious can lead to more open-ended and productive means of scientific experimentation and interpretation. Griffin’s books and writings about consciousness contain examinations of the existing literature on animal behavior that support interpretations of animal awareness. I will now examine his classes of evidence.

1) **Possible Neural Correlates of Consciousness**

In QAA, Griffin writes only briefly about neurophysiological evidence, but greatly expands such treatment in subsequent books and articles. His first examples (in QAA) entail “split-brain” human patients who have had their Corpus Callosum cut, that is, the connecting neural tissue
between the two hemispheres has been severed. One hemisphere, typically the left in humans, can
use language while the other usually not at all or minimally. But that less dominant hemisphere, cut
off from the language centers, can still solve problems and indicate such by other means, e.g., by the
humans pointing to their responses to problems set for them. Griffin links the functioning of the
subordinate, language-free hemisphere to the possible functioning of wordless animals. Some
prominent neurologists have come to similar conclusions, specifically Eccles (1974), Pribram (1971)
and Gazzaniga (1975).

In his later books, Griffin stresses how “all known structures and functions of nerve cells
seem to be much the same wherever we find them, whether in different parts of a human brain or
in other brains.” No structures, processes, or neurotransmitters have been found unique to
humans that are necessary for consciousness. Some parts of the brain such as the cerebral cortex or
reticular activating system seem to be more involved with consciousness, but many brain areas
partake in conscious states and activities. Later neurophysiological research has suggested other
areas as core, for example, the thalamic complex, including the brainstem reticular
formation. In his last book, Griffin notes the work of Panksepp, as well as others, that emphasizes
deep, old subcortical systems generating affect and emotional expression. Since then, further
confirming evidence has been garnered. The findings are of particular significance since they
expand the likelihood of consciousness, indeed of affective consciousness, including pain, to reptiles
and amphibians. The work promotes Griffin’s views of widespread consciousness.

More contemporary work has further refined and expanded the findings, but I simply
cannot review the entire area in these writings.

Griffin was particularly interested in studies attempting to find neural correlates of
conscious states. In writings of 2002, he relates that neuroscientists consider the most likely
correlates of consciousness to be “widely distributed, but coordinated activity engaging large areas
of the brain.” What of differences in brain activity that involve consciousness and those that don’t?
Griffin stresses again that contemporary knowledge indicates that “conscious” brain activity simply
does not require structures or processes present in human’s, but absent in animal brains.

I cannot review all of Griffin’s considerations of the neurophysiological research that he
interprets as supporting the likelihood of consciousness and conscious thinking in animal species.
For that, I refer you to his last book, Animal Minds: Beyond Cognition to Consciousness (2001), his
posthumously published papers “Windows on Animal Minds” (2009, submitted 2002) and the last
paper by himself and Gayle Speck (2004). Some updates to Griffin’s evidence for animal thinking
can be found in publications by myself and others.

Some areas of neuropsychological research particularly interested Don Griffin; I shall cite a
few. Certain electrical neural activities, termed ERPs or Event Related Potentials, are brain waves
not directly affected by sensing, but by internal processes of the brain. Some, such as the P300
wave, seem to be associated with conscious thinking. Could such waves be detected in animals and
under what conditions? Recording the waves does not require invasive procedures; they can be
recorded from the scalp. Indeed, such brain waves have been recorded in various species.

In Griffin’s view, technological advances in brain imaging, specifically positron emission
tomography (PET) and functional magnetic resonance imaging (fMRI) provide promising areas for
research. These techniques create three-dimensional visual images of relative activity in different
portions of a brain area under examination. PET images indicate metabolic rate by measuring
radioactively labeled glucose uptake, while fMRI’s portray local blood flow. Both types of scans are presumed to reflect activity or information processing in the nearby neurons, the PET scans being more sensitive.³³

Rare cases of “blindsight” occur in humans with extensively damaged visual cortex on one side of the brain. They report seeing an object located in a position such that its input is to the “good” side of the visual brain region. They do not report seeing the object when its image projects to the damaged brain area. Nevertheless, the subjects can perform detection and other tasks with the damaged side as though they are “seeing.”³⁴ Research has been conducted with monkeys by creating such lesions, achieving similar results. Since the monkeys, of course, do not speak, ingenious experiments elicit responses from monkeys interpreted as meaning that they “see” or do not “see.”³⁵ Philosophers of mind have likewise been intrigued with the phenomenon of blindsight and its relation to consciousness.³⁶

To Griffin (and others), the activity of “Mirror Neurons” also suggest conscious awareness. In experiments with monkeys, a group of brain cells might fire when a monkey sees another monkey or human perform an activity (e.g., picking up an item). The same group of cells fire when the monkey then picks up that object in a similar way. Other actions do not elicit the same neural activity.

Another phenomenon, “binocular rivalry,” likewise provoked both Griffin’s and philosophers’ interests. Numerous experiments have been done with humans, then monkeys. Basically, two different stimuli are shown, a different one to each eye. In such circumstances, the human either stays with and reports the image presented to one eye, or, more usually, alternates back and forth between consciously “seeing” one image and then the other. By measuring brain activity in various regions of the brain in humans, researchers have found responses that correlate with the human “seeing” an image; similar activity is reported in monkey brain scans. Elaborate procedures were developed: In some instances, images were presented, flickering at different frequencies for each eye, so that accompanying brain waves would reflect those frequencies and provide evidence as to which image was being processed in that region. Tantalizing findings for Griffin and many others: neurophysiological findings supporting both conscious awareness in the monkey subjects as well as differentiation between states of such awareness and their absence.³⁷

In yet other recent neurological studies, these conducted with two carrion crows (Corvus corone), single neuron responses were shown to correlate with the bird’s perception about stimulus presence or absence. The neuronal activity showed a two-stage process in which the first phase mainly reflected stimulus intensity and the later phase predicted the crow’s perceptual report, i.e. did the crow perceive the stimulus (a light) or not? In a well-controlled study, but too complex to report in detail here, the crows were initially rewarded for indicating correctly the presence or absence of a light stimulus whose intensity was then modified as the training proceeded. The bird brain does not have a layered cortex such as those of humans and other mammals; the palliative end brain appears to be capable of these and other abilities previously considered to require a cortex. Might the late-phase neuronal response possibly be a marker for consciousness, applicable broadly and across species? The researchers believe so.³⁸

Since Griffin’s writings, although a hard core of resistance remains as positivistic traditions maintain their hold on scientific thinking, more scientists are accepting of the likelihood of animals’ conscious thinking. In 2012, a group of eminent cognitive scientists signed “The Cambridge
Declaration on Consciousness." Essentially, the document states that humans are not unique in possessing the neurological substrates that generate consciousness. Further, it specifically declares that "non-human animals, including all mammals and birds and many other creatures, including octopuses," also possess these neurological substrates. The document, though influential, of course, does not exemplify the view of all cognitive scientists.

2) Versatility in Meeting Novel Challenges ("Versatile Adaptability of Behavior to Changing Circumstances and Challenges"

Animals confront novel challenges and adapt to those challenges in versatile ways. This issue was more Don’s focus in his books written after QAA, although, in QAA, he did have a chapter entitled the “Versatility of Animal Communication.” In QAA, Don raised the matter of an animal’s awareness of its ongoing actions. Although a human or an animal can be aware of its own innate, even stereotyped behavior, as it is performing that behavior, the argument gains greater strength when the organism is behaving variably and spontaneously. In QAA, Griffin concentrated on examples from the honeybee. For example, honeybees react variably to the waggle dance. Not all attending the dance do fly off to the food, and some that leave the hive wander about, sampling here and there, before arriving at the designated food source. Simply put, the response is not a "given."

Likewise, the bowerbird’s construction of bowers piqued Griffin’s interest in QAA and subsequent writings. The ingenious bowers and the often gloriously beautiful male bowerbirds similarly attract the public’s attention, as evidenced by films on “Nature” and other wildlife documentaries. Bowerbirds’ creation of bowers, some short, some several feet tall, some simple and some intricate and elaborate, raised, for Griffin, the possibility of a bird being aware of what it was creating. The bowers were structures that served simply to lure a female for mating; it was not a nest for either parents or offspring. The males decorated the bowers, using, if available, various items especially favored by that species. If not accessible, the birds improvised! Typically, they chose conspicuous objects, perhaps bright blue berries if around, or, if not, perhaps bright blue bottle caps, or even coins or keys the researcher accidentally dropped. The birds tidily cleaned their displaying area of debris. They removed wilting decorative flowers they’d previously placed on the bower.

Don raised the possibilities of birds very much aware of their choices, despite the vigorous arguments of several. A. Jock Marshall was most adamant; he had studied the birds. Critics claimed that the bowerbirds’ activities were all innate behavior patterns of a bird under the influence of its changing sexual hormones. Griffin printed the opposing reasoning of Marshall in detail. Marshall argued that the birds behave without conscious awareness of the results of their behavior. Don concludes his discussion with “…it may be a serious limitation in our thinking to assume a priori that no such awareness can possibly exist.”

By the time of Griffin’s later books, more field experiments had been conducted, for example, by G. Borgia and his colleagues studying individually marked bowerbirds and by Jared Diamond, exploring choices made by different bowerbird species. Diamond extensively studied the Vogelkop gardener bowerbird (Amblyornis inornatus). (Note how this work was a long-range impact of the 1969 expedition to New Guinea. The trip had not seemed particularly successful scientifically, but had inspired a lifetime of research in New Guinea for Diamond.) The bowers built by the birds vary between different populations of the same species. By ingenious experiments, Diamond found that the birds within a given population had distinct preferences both for colors
and placement of the chosen decorations and colors, and individuals within the group often exhibited their own preferences within the general group's design. Diamond purposively shifted some of the decorations to atypical locations and the birds usually set them back to where they "belonged." The colors of the decorative bits and pieces were displayed in a particular order. For the Wandamen population, black and orange fungi were set downhill from the bower, while colored flowers, red leaves, and red, orange or green fruits were just outside the bower. Inside were the rarest finds, which might include butterflies, beetleheads, acorns, etc. When Diamond placed bright poker chips of seven different colors, near or in the bowers, the birds used them decoratively; they chose colors in a hierarchy of preferences, blue typically being the favored. The order of preferences was likewise reflected in the colors the birds tended to choose first, select from the wild, and steal most frequently from the bowers of other birds. Individual birds had different hierarchies. In another population, the Kuwama, where the males did not typically use color in their decorations, most birds simply removed the chip "debris" from their area. As Diamond described his results, he considered them likely to be learned manifestations of "culturally transmitted styles, like human art styles."

In the revised edition of QAA and particularly in his later books, Griffin offered myriad examples of animals' versatile adaptive behaviors, which he proposed were strongly suggestive of conscious thinking. He reasoned that in adaptive complex behavior sequences, each step is modified according to the results of the previous actions. The steps often vary, so one is hard-put to postulate a rigid program of such steps. Consider the nests built by birds, perhaps disturbed midway by wind, or observe the "magnificent webs" of spiders, which likewise endure rips or partial dislodging.

In his post-QAA writing, Griffin's many, many examples of versatile adaptive behaviors were organized into broad topics. I include here the list in Animal Minds: Beyond Cognition to Consciousness (AM- Conscious or AM-C): Finding Food, Predation, Construction of Artifacts, Tools and Special Devices, Ability to Learn Concepts, Deception and Manipulation. He also discussed the Physiological Indices of Thinking (including NCC) and Communication, encompassing both natural animal communication and the Artificial Language Projects. Both such communication topics were included as well in the original QAA. Animals as "Natural Psychologists," gained prominence in Animal Thinking, where he developed the likely need for conscious thoughts for socially living animals to interact successfully with the group. Thus, they could correctly anticipate the intentions of the group members. In his final paper, co-authored with Gayle Speck, he discussed findings from lab-based animal learning experiments that suggested awareness; I will discuss some of these matters in a later chapter. (Griffin was much more constrained in this last paper, the result, presumably, of several required stringent revisions by the editor(s) and critiques by reviewers before publication.) The philosophical significance of animal thinking continued to engage him. And in his last book, he had a chapter that included, in its title, "Ethical Significance of Animal Consciousness." Griffin was fully aware that this topic (to be discussed later), though highly pertinent to both conservation and scientific research with animals, was fraught with even more controversy than was his academic discussions of animal consciousness. All these additions were likely the result both of his continued thinking about the problem of animal awareness and his reactions to continuing criticisms. I cannot do justice to all his examples, so I shall mention only a few.
A. Finding Food

To find food encompasses food-gathering techniques, strategies and decision-making employed by species as diverse as earthworms, whales, birds and insects, among others. The “bubble nets” made by humpback whales are particularly intriguing. The enormous whales slowly exhale, creating a stream of fine bubbles. Not just one whale, but a group, can participate in this process, whereby they create a giant circular net of the bubbles, with small fish and invertebrates inside. The net contracts; the prey is trapped, for they are reluctant to swim through the bubbles. The whales then gulp the tiny fish and plankton, forcing them through their baleen sieves, an effective swallow of concentrated prey. Coordinated swimming and exhalation are required for this endeavor, and strongly suggest conscious awareness of the technique and goal achieved by cooperation. 53

B. Predation

Griffin emphasized the purposeful nature of predatory behaviors and the often complex strategies employed by organisms from spiders to lionesses54 as well as evidence for planning such strategies. The previously mentioned jumping spiders (especially the salticid spiders of the genus Portia) can feed on web-building spiders by invading their webs. But Portia must not be detected by its prey, for the web spiders are larger and venomous; Portia can become prey. So, Portia varies its mode of approach. This can include taking detours and long circuitous routes around large obstacles, often losing direct visual sight of their goal. Once at the web, Portia vibrates the threads, modifying their vibrations to attract the web spider to the edge where it is easier prey. Some of the vibrations are even those used by the web spider during courtship. The web spider may communicate back with vibrations and Portia may respond.55,56 As Griffin reports, the researchers Wilcox and Jackson conclude that their investigations “bring us closer than we initially expected to something like the cognitive implications of verbal language.”57

Among Griffin’s many other examples is that of predatory birds drowning prey that are too large or too active to be carried off in a beak or talons. Pelicans and even hawks have been observed to do so, some remaining even 10 or more minutes to complete the task.58

C. Constructing Artifacts

In AM-C, Griffin delved into such examples of insect constructions as the cases and nests built by Caddis fly larvae59 and nests built by insects.60 He was likewise concerned with birds’ making and repairing their nests,61 the previously described bowers of bowerbirds,62 and “Beaver Engineering,” namely beavers’ extensive creations of lodges and dams.63 Don’s interest in beavers was long-standing, dating from his youth, with various, usually unsuccessful, attempts along the way to persuade his students to study beavers.64 Finally, when he “retired” to Cape Cod, he undertook his own beaver research.65

D. Tools and Devices

Looking to the Future

Tool use and especially tool-making merit special attention, for, as Griffin noted in Animal Thinking,66 it would seem to require an animal’s thinking about the final goal or product, as it is also thinking about the process of making a tool, often requiring several steps. Even simple tool use
would seemingly require an understanding of what the tool is to be used for and so involves thinking about at least a short-term “future.” By the 2004 paper, after multiple revisions required by the reviewers, he and Gayle Speck said succinctly, using the term “planning,” but also employing non-mentalistic words like “adaptation.”

*The use and especially the making of tools require at least short-term planning and adaptation of behavior to specific and often unpredictable situations.*

When Don wrote QAA, tool use was no longer the Rubicon separating man’s abilities from those of the “lowly” non-humans. Some animal tool use was already widely known by scientists and the public: the ingenious techniques of the Galapagos Darwin’s finches (specifically, Woodpecker finches, *Camarhynchus pallidus*) using cactus spines or twigs to pluck insects from crevices; sea otters (*Enhydra lutris*) using a rock to open shells both while floating on their back and when an anchored rock serves as an anvil along the shore; *some chimpanzees* (*Pan troglodytes*) tool-making that includes “fishing” for termites with a twig they have selected and then pruned. Griffin cited these and later added more. There had been several extensive reviews of animals’ tool use and tool-making after QAA, e.g., those by Beck and Hansell but the authors generally avoided the topic of conscious animal thought or dismissed it.

**Heron Fishermen**

Griffin described herons’ (family *Ardeidae*) fishing techniques in some detail, for he thought the bird was a good candidate for conscious thought. Herons catch small fish and other prey, dealing with parallax as light bends entering and leaving the water, but managing to stab the prey with their long, thin sharp bills Sometimes herons even snatch flying insects.

Many herons just stand in the water, waiting for a passing minnow to eat. But some herons also occasionally manufacture simple tools, namely bait, with individuals seeming to prefer different techniques: One green-backed heron (*Ardeola striata*) broke a twig and carried small bits to a pond, dropped it in, stood in the water, watched, and “fished” it back when the twig floated away. The hapless minnow who investigated became lunch. Another heron in the population perched on a branch overhanging the water, dropped in some material, perhaps a feather, bread crumb, berry or almost anything around, and flew down to grab approaching minnows. Adult herons are more effective at bait-fishing than juveniles, and within a population, only a few birds have been observed doing so. However, once a heron has become so sophisticated, it engages in the technique repeatedly.

**Brilliant Corvids**

New Caledonian Crows (*Corvus moneduloides*) have made it to newspaper stories by their own ingenious set of tricks: In the wild, they used or fashioned twigs to become hooks and, in a lab setting, did the same with wires to reach food otherwise not obtainable. They had not observed wires being bent until doing so themselves, though they had used pre-prepared hooks to gather food in the lab.

Ravens (*Corvus corax*) are another corvid with extraordinary cognitive abilities. Bernd Heinrich spent decades studying them. Among Heinrich’s experiments that so impressed Don were those in which ravens, using tools, seemed to comprehend causal relations. In one set, an adult captive raven with no experience with strings, after several hours, pulled up meat dangling on
a meter-long piece of string. Four other ravens were nearby and after a few days, three learned the task. They pulled up sections, stood on the captured string, and repeated this motion until reaching the meat. The ravens, however, each accomplished this feat in slightly different ways, e.g., some standing sideways, others not. If startled, ordinary ravens fly off with any food in their mouth. In this experiment, the one unsuccessful raven did try to fly away with meat on a string, but those who had pulled up the meat did not do so, even on the first startle.\textsuperscript{80,81}

Bernd Heinrich and Don often communicated with each other; Bernd sent Don drafts of papers about his experiments or book chapters. Likely, Griffin did the same. These manuscripts and the latest raven "news" were highly welcome in the Griffin lab, a soulmate successfully exploring the advanced cognitive abilities of "thinking" animals. Once, in September 1993, over dinner with Don and Jocelyn in a Lexington, Massachusetts restaurant, Don showed me letters to and from Bernd detailing his frustrated attempts to publish the raven research just described. Because Heinrich had mentioned consciousness, reviewers had commented that the work was "not scientific," that his research was "Griffinish."\textsuperscript{82} The work was finally published, two years later, in 1995 in The Auk, an unlikely journal for a cognitive paper.

In later Heinrich experiments, crossed colored strings were used, one holding a rock and the other meat; some ravens immediately pulled on the correct string. Other ravens did so after a brief tug moved the unpalatable rock. In subsequent trials, these other "slower" ravens continued to ignore the string attached to a rock, even when the rock hung directly underneath. They pulled the differently colored string attached to food, even when it was set off to the side. Another task, with different ravens and a looped string that, "illogically," required pulling down on the string to move the meat up was never successfully solved and the ravens lost interest.\textsuperscript{83} We do not know all causal relations or "physics" the ravens understand, nor the "rules" they apply, but gradually, across species, we may be able to determine ecologically appropriate generalities. (This later "crossed-string" and "looped-string" work was conducted after Griffin's demise, and is not discussed by him.)

\section*{3) Ability to Learn Concepts}

A major concern by philosophers and experimental psychologists was specifying the nature of concepts that animals could learn. Thus, one distinguished between the representation of a concept e.g., "trees" and a sorting or categorizing ability. Sorting photos into trees and "non-trees" could be learned even by pigeons in discrimination experiments.\textsuperscript{84}

\section*{Categories}

Don, in AM-C, makes the sensible argument that animals must understand at least some classes or categories, simply to exist. The Thompson's gazelle must recognize its predators, lions, wherever it sees one, irrespective of the particular visual stimuli impinging on the gazelle's retina. Lions may be near or far, frontal or side or partially viewed.\textsuperscript{85} Likewise, for the many socially living animals, it behooves them to understand the social relations among their counterparts, who is allied with whom, who is a challenger. For example, a harem male bat may effectively perform his leadership role by nudging the inattentive mother, unresponsive to the distress calls of her baby fallen to the floor.\textsuperscript{86} His harem has many mothers and babies, so he has more than a few social relations to keep track of.
Expectations

Griffin recalls the long history of behavioral scientists trying to deal with animal “goals,” “wants” and “expectations” by their using non-mentalistic concepts and terms. Tolman in 1932 and 1937 introduced the concept of “purposive behaviorism,” avoiding any explicit suggestion that animals might be thinking consciously of what they were doing; Tolman was enmeshed in the “Zeitgeist” of the times. He did, however, distinguish between “movements” and “actions.” Actions are identified by their consequences, movements, by the particular muscular contractions that comprise the action. Tolman believed that animals learned “actions” needed to obtain a reward (the “consequence”), not movements, per se. (See experiments with humans by D. Wickens, 1938 and 1939 which confirmed these views of Tolman.) And Tolman used terms such as “expectation” and “internal representation” in his theorizing. Among his fellow behaviorists, he was considered an “apostate;” he was “notorious.” In 1959, after, as Griffin describes it, “a long and distinguished career, Tolman finally confessed to having been a “cryptophenomenologist.”

Griffin delights in recalling Tinkelpaugh’s 1928 experiment, whereby a monkey saw a highly desirable banana chunk being placed under 1 of 2 cups, waited, and then got to retrieve the food. In some trials, behind a barrier, the enticing banana was replaced by a lettuce leaf. The monkey lifted the proper cup but then looked everywhere. She did not eat the lettuce; she’s just not that hungry. On some occasions, encountering the lettuce, “she shrieked at them [the observers] in apparent anger.”

Griffin’s point is the same and straightforward: Despite themselves, psychologists and other scientists had been confronted with “animal thinking” and “expectations” and “wants” for decades, of course, for centuries, millennia. Wasn’t it time to consider the possibility of animal’s conscious thinking and feeling?

Cognitive Maps

Might animals have cognitive maps, i.e. mental representations entailing landmarks and both relative distance and geographic information? By geographic, I mean the orientation of objects or landmarks with respect to each other and/or a focal point. From the first edition of QAA, Don indicated his concern with animals’ “cognitive maps,” a term introduced by Tolman (1948) from his studies of rats learning about their locale, and described by Griffin as “capabilities for perceptual organization” required for complex orientation for navigation. Tolman’s work was largely ignored; “cognitive” was for humans. Kohler’s earlier work with chimpanzees and dogs was also ignored.

But exciting results had been accumulating, beginning with the classic experiments in the 1920s of the Nobel Prize Laureate Nicolaas Tinbergen. He had arranged distinctive markers around the nests of digger wasps to determine that the wasps were using such cues to zero in on their tiny nests; by removing and rearranging the cues, he could determine which landmarks the wasps relied upon and preferred. In the 1970s, David Olton experimented with rats allowed to move freely in mazes without any reinforcement, they recalled the layout and where they had been and reacted appropriately later to obtain food or to avoid a place where they had been shocked. A critical issue for that time was that the rats had not been rewarded for wandering through the maze; they did so as they wished and where they wished, receiving no reward or punishment anywhere during their meanderings. But they had learned and remembered locale information.
And then there were the honeybees. Their dances conveyed both the distance and direction of a food source or water or even a potential new hive site if the colony was preparing to move. These days (2020s), the evidence for such honeybee symbolic communication is very strong, though it had been controversial for decades. Could bees have yet more advanced abilities? Vertebrates have cognitive maps; could it be that the honeybee, an invertebrate, also had such mental representations?

I recall, as a member of the Griffin RU lab in the 80s, the excitement as various new animal cognition findings were emerging. We heard about the “Lake Experiments” being conducted in Gould’s lab at Princeton, for his student, Fred Dyer’s, Ph.D. thesis. Does a bee know where food “ought” to be located and where it shouldn’t? After attending the dance of a forager bee, indicating the presence of food, does a recruit have a mental image of that location before she leaves the hive to get the food she has just learned about? We are assuming the bees are well-acquainted with the surrounding area. So, with the distance and direction information, she could theoretically place the indicated spot on a mental map ... if she held one.

Dyer had trained forager bees to a feeder along a lake shore. He then used the bees to “lie” about the location, i.e. to “misdirect” the recruits attending the foragers’ dance. This was accomplished as the foragers danced with reference to gravity, while the recruits interpreted the dances with respect to an artificial sun, a bright light inside the hive. (The foragers’ ocelli were covered, so the lamp was not sufficiently bright to them to function as the sun.) The foragers danced about a lake shore location, while their dances indicated a location in the middle of the lake to the recruits. Recruits did not go to the mid-lake position, but fruitlessly searched along the shore, with a few “happening” onto the lakeshore feeding station.

In further experiments, a feeding station was set on a boat on the lake and in subsequent trials, moved nearer and nearer the opposite shore; another station was set on the far shore. No misdirection was entailed; the foragers’ dances indicated a location correctly interpreted by the recruits as on the lake. The recruits did not arrive at the mid-lake boat positions though some arrived at the lake positions close to the opposite shore; many arrived at the shore station on the far shore. But a simple interpretation of hydrophobia does not suffice. To reach the far shore would have entailed a 1000-meter flight around the lake to avoid a 200-meter flight across. As the Goulds concluded, “Almost certainly they flew over.”

Jim Gould continued related experiments with additional controls and individually marked bees to verify the identity of the recruited bees and confirm that they were not mere passersby. He specifically uses terms like bees’ “mental maps” and “locale maps” to interpret the results. Others, including Fred Dyer, who also continued experimenting, became more convinced that a “route-based memory,” perhaps entailing eidetic images, was a preferred interpretation.

Besides the locale or route learning, the bees were attending to and remembering other details, but in distinctive and surprising ways: On their approach to the flower, they learned its color, odor and shape and, on their departure, the surrounding landscape, including location relative to the sun. Innovative experiments teased apart these characteristics and determined their relative significance for the honeybee. Another “window” into their minds?
Numerical Competence\textsuperscript{105}

Numbers, too, became recognized as the province of animals. As Griffin describes, ravens had exhibited “wordless thinking,” Otto Köhler’s term for their achievements in experiments he’d conducted in the 1950s and 1960s. The ravens could be trained to select boxes with a certain number of spots, spots that varied in size, shape and position. Ravens could accomplish this up to about seven spots.\textsuperscript{106} Later, several species, including chimpanzees, pigeons, parrots, rats, monkeys, dogs, and pigs, among others, exhibited a similar ability, namely learning to associate an Arabic numeral with that number of items. This capability often took an extraordinary number of trials to accomplish, in at least some of the species. The ability was termed “numerosity,” distinguishing it from the human ability to “count.” In learning to count, a human child learns that the next number is one more than the previous and that numbers later in the counting sequence refer to larger numbers. Finally, as Griffin notes, at least certain aspects of “simple” counting were achieved by chimpanzees.\textsuperscript{107} Griffin, at this point, is less concerned about delineating the animals’ specific abilities, than with the strong implication from the research, that, in his view, the animals are thinking about numbers of objects. Field experiments entailing acoustic playbacks of animal vocalizations bolster his beliefs. In this work, lionesses reacting to the recorded calls of other lionesses decide to repel the invading group, only if the resident lioness’ pride exceeds the invaders’ by at least two.\textsuperscript{108,109} Numbers do matter. And Griffin’s arguments that animals are conscious become more persuasive with his chosen examples and emphases.

Inside the Skinner Box\textsuperscript{110}

This was the last major topic Griffin took on in AM-C. He presents a convincing picture of an experimental subject’s odd experience inside the confines of a Skinner cage; the subject is usually a rat or pigeon. All external stimuli are removed, insofar as possible. The animal is hungry or thirsty, often very much so at 80% or even 70% of body weight. Thus, it is more likely that the animal will be motivated to perform the experimenter’s task to gain a food or water reward. There is little it can do inside this highly constrained environment other than a rat pressing the available lever or a pigeon pecking the button. Results of experiments are typically interpreted in strictly behaviorist, not mentalistic terms. Griffin sets himself the task of re-interpreting some experiments and their results in terms of possible thoughts entertained by the animal subjects. I urge the interested reader to refer to his book for specifics.

4) Deception and Manipulation

Deceiving Beasts

In Griffin’s first and short Cognitive Ethology book, QAA (1976), he mentions only one example of animal deception, while discussing the versatility of animal communication. A captive chimpanzee was led to food or an object and then put back into a cage. Then other chimps in his group, who had not seen the food/object were let out and, somehow, the caged chimp was able to direct the others to the food. But not always. A few times, the chimpanzee seemed not to want the others to find the food and appeared to attempt to prevent them from locating it. The investigators surmised that the chimp’s gestures and “expressive movements” were guiding the others when successful.\textsuperscript{111,112,113} (At this time, Griffin did not mention vocalizations as a possibility.)
In later books, Griffin greatly expanded the examples of deception, including a full chapter in both editions of *Animal Mind*. He draws some examples\textsuperscript{114} from the 1986 symposium on Deception organized by Mitchell and Thompson.\textsuperscript{115} Griffin is careful to contrast his approach with that of Dawkins and Krebs (1978),\textsuperscript{116} who stress the evolutionary significance of most animal communication to manipulate others for the benefit of the sender. Thus, in their view, much animal communication, has evolved to be dishonest signaling (e.g., in a "threat" posture, raised feathers or fur makes the individual look larger and more threatening.) Many ethologists accept this "dishonest signaling" interpretation, but many also consider animal communication to be mutually beneficial. Dawkins’ and Krebs’ approach is used to reject the possibility that communicating animals might *consciously* "mean what they say." However, those researchers did later modify their analyses\textsuperscript{117} by adding "mind-reading" to their views of communication (defined as an animal’s ability to predict the behavior, etc. of others).

Griffin distinguishes conscious deception from the physiological or anatomical deception used by animals, e.g., deceptive coloring or shape. Some tasty butterflies have evolved to mimic the coloration of toxic, noxious ones. Moths flash eye spots on their wings when a predator is closing in, and so appear a larger, more fearsome creature. Griffin describes the color changes of squid in their social interactions and, with significant prescience, notes how they have not been studied adequately, possibly revealing conscious intention, rather than automatic autonomic reactions.\textsuperscript{118,119} Much evidence has since been gathered about the complex color and shape changes employed by various cephalopod species in communication and predator avoidance/escape. Likewise, examples of conscious intention have been observed, particularly in octopuses. They have, for example, figured out and conducted escapes for themselves and others, sagely waiting until experimenters (seemingly) leave the premises; such escapes have been observed repeatedly since reported in 1875.\textsuperscript{120}

**Manipulating Beasts – The “Injury-Feigning” Plovers**

From the very first QAA, and continuing in every subsequent book, Griffin noted “distraction displays,” performed by various species, particularly ground-nesting birds. Armstrong\textsuperscript{121} and other ornithologists and ethologists prefer the term “injury simulation” or “broken-wing display.” These labels avoid the suggestion of conscious intent and emphasize the view of innate displays as an appropriate description of the behavior. The actions function to distract a predator’s attention away from the vulnerable nest or young. Especially striking are those made by several species of plovers, which exhibit several distinctive behaviors in the presence of an intruder. The "broken-wing display aka “injury feigning” is the most intense, typically accompanied by loud, raucous squawking, and extended, flapping wings, one or both dragged along the ground as though the bird were injured. It is a compelling display and very effectively grabs and holds the attention of any observer. (Similar behaviors are exhibited to natural predators, such as raccoons, foxes or dogs, but it is rare to see a natural encounter.)
Piping Plover Making a Broken Wing Display
aka “Injury-Feigning”
(by Eric Strauss)
(Additional photos of the plover and its display may be found in Chapter 7122)

The behavior was “customarily” explained as a conflict between fleeing and attacking, with resultant almost convulsive actions, performed in random directions. Griffin mentions several reviews of the display, noting that none interpret the act as the plover “intending” to lure the predator away.

In his final books, AM123 and AM-C,124 Don describes in detail the field experiments conducted by Carolyn Ristau (myself) with the piping plover (Charadrius melodus), and my attempts to examine the possible conscious intent of the parent plover. I applied the “intentional stance” as discussed by Daniel Dennett125126 and others to the possible “intentions” of a parent plover protecting its nest or young from an intruder.127 The work reveals that the plover adapts its behavior depending on the responses of the intruder, which was a human. The birds were attentive to the direction a human was looking, reacting more strongly to one walking at a moderate distance away, but looking towards the nest and less to a human walking at the same distance but looking away from the nest.

In other experiments, the birds learned to distinguish between a potentially “dangerous intruder,” one who had walked close to the nest, hovering and searching near it, and the “safe” human. The latter had simply walked past the nest at a much further distance (12-32 meters). In tests conducted both before and after these trials, each intruder simply walked past the nest at a moderate distance. The plovers in most instances reacted more strongly in the post-tests to the previously “dangerously-behaving” intruder than to the “safe” one. The plovers’ reactions included standing up or leaving the nest, staying off the nest longer, and displaying.

And, finally, Ristau did spatial analyses of the behavior of a plover and a human in relation to the nest. Sometimes, the human approached the nest and then moved in other directions, either following the displaying bird or not. The bird would display so that in almost every case, if the human had followed the bird, she would have gotten further from the nest. The bird also adjusted its path, such that in only a very few instances, would the human, in following the displaying bird, ever pass any closer to the nest. Typically, the bird would run off its nest, not displaying, until the human’s trajectory, if it followed the bird, would be along a path that never got closer to the nest. And should the human not follow the bird at any point, the bird “re-approached” and began again to vocalize and/or display. Finally, when the human was far enough from the nest, the bird flew in a high, huge, long circle back to its nest/young. Both Ristau and Griffin conclude that there is strong suggestive evidence that the plover “wants” to lead an intruder away from its nest/young; its behavior appears to be intentional, using an innate behavior strategically. Neither claim “deception,” but rather “manipulation.”
More Likely Conscious Deception

Examples Griffin offers for likely conscious deception include the use of false alarm calls by mammals, birds and even ants; these are detailed in reviews by Munn. In particular, Munn, himself, observed that tanagers (Lanio versicolor) and shrikes (Thamnomanes scistogynus) produce false alarm calls. Such “false alarms” allowed them to access food, as a conspecific immediately flew away or even dropped an insect, then captured by the vocalizer. Griffin describes some interactions in detail, concluding with Munn’s suggestion that “some amount of thinking is involved both in sending and receiving the alarm call. … alarm calls are interpreted as meaning something more like “hawk” than “jump.” Other experimental work, conducted in the forest, showed capuchin monkeys (Cebus apella nigritus) vocalizing more alarm calls in circumstances when they were easily able to grab a piece of desirable food and when the monkey was subordinate to a dominant who could simply grab the food without reprisal.

And finally, Griffin describes the many, many examples of deception in chimpanzees. Many instances are by chimps and other primates in captivity, which permits more intensive study than do field observations. Frans de Waal’s long-term studies of captive chimps and the reviews of Andrew Whiten and Richard W. Byrne offer detailed examples.

One such, reported by de Waal, and all too easily remembered, involves the surreptitious liaisons of subordinate male chimpanzees with females … only when hidden from the dominant. A subordinate male, discovered close to the act itself, hastily hid his erect penis with his hands.

As Griffin notes, Whiten and Byrne’s 1988 review in The Behavioral and Brain Sciences was followed by extensive and diverse commentaries, some of which showed the strong hold of behaviorism that limited some scholars from accepting the circumstantial evidence that primates are capable of “tactical deception.” Somewhat similar restraint is shown by Whiten and Byrnes’ choice of that term. Similarly, their responses to the commentaries indicated that they were not concerned with the “phenomenal worlds” of their study animals. Yet, in later contributions to a book they edited, they were, in Griffin’s words, “more relaxed.”

5) Natural Psychologists

In his 1984 book, Animal Thinking, Griffin devoted an entire chapter to “Natural Psychologists.” His particular concern with social behavior draws upon the suggestion made by both Alison Jolly (1966) and Nicholas Humphrey (1978) that consciousness may have arisen in human evolution when social groups were of sufficient size, interdependence and complexity, that each member would do well to understand the moods and intentions of others.

Don suggests that the same could apply to animal groups, even to the honey bee, a social insect, perhaps even more interdependent than humans. Scientists simply had not looked to see whether the bees are interchangeable entities. It seems they are not readily substitutable, for there are at least different social categories. The youngest, for example, clean cells in the honeycomb, then, in a few days, progress to building the comb, etc. And, within a class, might there not be differences between individuals, recognized by the bees, though not by the scientists? For instance, when a bee (or weaver ant, another social insect) makes recruitment gestures, not all sisters immediately (or ever) respond, and, indeed, sometimes, recruiting bees pass another by without so
signaling. In short, wouldn’t individual recognition, even if only of social classes, be advantageous to a honey bee?\textsuperscript{138}

**Self-Recognition?**

And what of self-recognition? Is an animal aware of itself? Which animals? The long, long series of “Mirror-Self Recognition” (MSR) are discussed by Don, beginning in AT (1984). In the initial experiments (1970), a chimpanzee had a mirror in its cage and opportunities to look at it.\textsuperscript{139} They, like human children, come to recognize their image; in children, it is usually not until about 18 months. To do so requires several stages. At first, a chimpanzee, like many species, including a child, may react aggressively or fearfully as though the image were another individual. It may look behind the mirror, and later apparently “test” the correlation between its own movements and those of the mirror image.

The experimental test consisted of a mark placed on the chimpanzee’s face, where it could not be seen, except in the reflected image. Most chimpanzees, not all, specifically touched the mark when looking in the mirror and so “passed” the test. The testing continued after Griffin’s time. Some, not all, members of certain other species passed the test: bonobos (\textit{Pan paniscus}), orangutans, (\textit{Pongo abelii}), gorillas (\textit{Gorilla gorilla}), dolphins (\textit{Delphinus}) and common magpies (\textit{Pica pica}).\textsuperscript{140} Elephants, known for their high intelligence and complex social interactions, were unable to pass the test ... at first.\textsuperscript{141} But when the Asian elephants (\textit{Elephas maximus}) were given a sturdy, massive mirror and allowed to explore the mirror with their trunk, they passed the test.\textsuperscript{142} But the intelligent African gray parrots (\textit{Psittacus erithacus}), noted for their impressive proficiency with artificial languages (discussed later), and similarly intelligent corvids, including crows and ravens, did not pass; another corvid, a magpie, did pass.\textsuperscript{143} And chickens? They did not pass the MSR, but very recently a rooster’s behavior suggested that it recognized its image in a mirror. When a flying hawk was projected overhead, the rooster gave a warning alarm if other roosters were present, but not if its view of others was blocked by a screen or if was alone or with a mirror reflecting itself.\textsuperscript{144} And a wee tropical fish, the Bluestreak cleaner wrasse (\textit{Labroides dimidiatus}), also passed the test. It does have complex social relations, keeping track of other fish as it cleans some and avoids being eaten by others.\textsuperscript{145} Monkeys, also engaging in complex social relations, did not pass, and neither did our pet cats and dogs, and numerous other species. The array of results is most perplexing and probably dependent on abilities seemingly unrelated to notions of “self.”\textsuperscript{146} It may also be the case that scientists have not engaged sufficiently with the “Umwelt”\textsuperscript{147,148} of many species and so have not designed tests best suited for each species.

If an animal has some sense of self, what is its understanding of others? Considering the chimpanzee, Premack and Woodruff argue that conscious empathy, and with it, the attribution of beliefs and desires to others, is a more basic and simpler process than the objective assessment of another’s behavior. They summarize, “The ape could only be a mentalist. Unless we are badly mistaken, he is not intelligent enough to be a behaviorist.”\textsuperscript{149}

**Strategic Cooperation?**

Griffin himself witnessed a scene of complex maneuvering, this within a group of lionesses. The events seemed best explained by attributing mutual understanding that resulted in beneficial cooperative behavior. The situation occurred in Africa, while he was visiting Dorothy Cheney and
Robert Seyfarth’s project investigating possible semantic information contained within vervet monkeys (*Cercopithecus aethiops*) alarm calls.\(^{150}\)

On a side trip to Kenya’s Amboseli National Park, they saw wildebeests (*genus Connochaetes*) about 200 meters from their jeep. The wildebeests were in two groups, both feeding, one near woodland and the other on an open plain. Suddenly, five lionesses (*Panthera leo*) appeared; the wildebeests stopped feeding and watched. The humans could not see all the lionesses at all times. But shortly, two lionesses were sitting conspicuously on two mounds. Another was crawling low to the ground and slowly advancing to a position between the two groups of wildebeests. Suddenly, another lioness rushed out of the forest and frightened the wildebeests. They ran, leaping over the ditch with the slinking, hidden lioness. She grabbed one, her mouth over the wildebeest’s, and killed it. It struggled and kicked, and was finally still. The other lionesses then leisurely walked to the kill and began eating.

As Griffin specifically says, although one observation cannot be seen as conclusive proof of intentional cooperation, the lionesses’ behavior surely suggests it. It certainly appeared that the lionesses were hunting cooperatively, each well aware of the others’ behavior and each with a specific role to play.\(^{151}\) Being his usual curious and enthusiastic self, Griffin was extraordinarily excited and began to leap out of the jeep to get a closer look. He was immediately pulled back by Dorothy and Robert who reminded Don that lions munch on humans as well.\(^{152}\)

Very many examples exist of likely conscious social behavior in numerous animal species, particularly dolphins, primates, and elephants, as well as cats and dogs, both wild and our domesticated pets. So much has reached the popular literature and filmmaking, that I have chosen to mention just a few examples of both those most popular and those less known.

6) Animal Communication – Artificial and Natural

From the very first edition of *The Question of Animal Awareness* (QAA), Don Griffin was fascinated by the versatility of animal communication and by the possibility of two-way communication with animals; their communication, he thought, could provide a “window on animal minds.” By “two-way,” Griffin literally meant that we could learn to understand animals’ communication, and in that process, “talk” back to them, perhaps by using their own signals. Alternatively, we might attempt to create an artificial shared means of communicating.

We often use an animal’s own signals in acoustic “playback” experiments whereby recorded animal calls are “played back” to the animals and their reactions observed. On a personal level, we all have experienced at least a limited form of two-way communication with animals, particularly domesticated species. We usually recognize when a dog is likely to be aggressive or a cat wishes to be petted. Similarly, a dog is likely to understand when we are probably going to take him for a walk. We listen to their vocalizations and watch their postures and movements. Alternatively, we might also strive to create an artificial shared means of communicating.

In that first book, Don wrote about natural animal communication, encompassing species from Jocelyn Crane’s beloved fiddler crabs (*Uca*)\(^{153}\) to chimpanzees (*Pan troglodytes*), including, on the way, insects such as the fireflies (family *Lampyridae*), signaling with light flashes.\(^{154}\) Griffin was also most impressed by another mode of animal communication, the use of artificial signals that an animal learns to understand at some level, and, in some projects, can itself use to “talk” to the
human experimenters. The language-like ability of captive chimpanzees (*Pan troglodytes*), impressed him: those taught hand signs by the Gardners and Fouts, some utilizing plastic shapes in Premack’s project, and others taught computer-based geometric symbols in the Rumbaugh’s. He was extremely impressed by the possibilities and accomplishments of an African Gray parrot taught to “speak” English words, the project of Irene Pepperberg. By the time QAA is published, Griffin, himself, has already written a review of the “language-like communication,” as he carefully terms it. (I shall refer to the research as “animal language projects,” however, being mindful, as should the reader, about the controversies surrounding the term “language” for those endeavors.)

But the predominant part of that section in QAA is devoted to the *Tanzsprache* (literally “dance speech”) of the honeybees; almost ten and a half pages of the sixteen are a discussion of his view of the dance’s symbolic nature. Generally speaking, this is an excellent strategy. Griffin uses examples, particularly from insects, which we tend to demean in terms of potential cognitive abilities. Then he demonstrates the insects’ capacities quite convincingly or at least forces us to consider the possibilities. Thereby, he has opened the door to our accepting advanced abilities and consciousness in other, “higher” organisms, for we have seen all that the “lowlier” sorts can do, even if we are uncertain whether they may be conscious.

**A. Artificial Language Projects**

**Gruff Sounds and Hand Signs - The Pioneering Attempts**

Griffin’s early discussions in QAA of the animal language research began by noting several almost totally failed attempts to teach home-raised chimpanzees (*Pan troglodytes*) spoken language. One of the most successful results was merely a few gruff sounds recognizable as words, produced after years of prodigious training. The same chimpanzee could, however, recognize many words of human speech. Speaking is just about impossible for a chimpanzee to accomplish, because of the anatomy and physiology of the chimpanzee larynx.

Griffin then discusses the Gardners’ research using hand signs to teach a chimpanzee, Washoe, an artificial lexicon, American Sign Language. (CR: A simpler, “pidgin” version of ASL was actually used in the Gardners’ and others’ subsequent projects.) Don credited Jane Goodall’s field observations of chimpanzees as influencing the Gardners’ choice of hand signing for their project’s mode of artificial communication; chimps use gestures in their natural communication. This, too, is a general approach taken by Griffin, emphasizing the significance of behavioral observations in the wild on designing appropriate experimental methods in the laboratory.

The Gardners were the pioneers. They were successful and others followed suit. Some used sign language and others opted for the more readily and convincingly documented use of computerized symbols or plastic shapes as lexical items. In QAA, Griffin emphasized the number of signs/“words” each project had been able to teach its subject(s). (E.g., Washoe acquired 130 signs in four years of training and invented a few more.) The chimpanzees could use signs to label items or photos of items. Some signs functioned as commands, such as “Open,” which Washoe used in a variety of ways, originally for doors, then for the opening of boxes, drawers and even picture books.
**Chimps Understanding English?**

Likewise, at the time of QAA, there were some examples of the chimpanzees in the projects understanding spoken speech. In later work, this became a focal point of some projects. One of the most proficient apes has been a male bonobo, Kanzi, born at Yerkes Primate Laboratory and a part of Savage-Rumbaugh’s project. His mother had been trained extensively at a Yerkes computer keyboard utilizing geometric signs as lexical units. Without specific training, he “picked up” this learning, while she did very poorly and was finally dropped from the project. The trainers did use gestures and spoken English with Kanzi. He excelled, using gestures spontaneously and comparatively long combinations of key presses at the Yerkes board. He combined both signs and keypresses to communicate with the trainers and understood an array of spoken words as well.

“Clever Hans” Intrudes

The ape language research has both generated considerable scientific controversy and also captured the public imagination. During a widely publicized conference organized by Thomas Sebeok in 1980, he described the work of Oskar Pfungst conducted many years before (1911). Pfungst had critiqued the performances of Clever Hans, the famous, so-called “counting horse.” Pfungst had shown that the horse was not adding or multiplying or exhibiting many other supposed intelligent abilities but rather was responding to inadvertent nonverbal cues of its trainer or even of a volunteer recruited from the audience. Although the observations adequately explained the horse’s ability, they had little to do with the animal language work. Nevertheless, Sebeok and others discredited that research, claiming it all was due to “wish-fulfillment” and Clever Hans effects. But, especially after this conference and the ensuing brouhaha, great care was taken to ensure and demonstrate “blind” procedures. I would add that publicity about the Clever Hans effect, however, had a devastating impact, especially on funding for these projects. Yet, due to public interest and that of donors, many projects gained support from those donors and continued their work. Unfortunately, some projects today, still have to rely on popular public support, rather than scientific institutional grants. An example is Irene Pepperberg’s continuing research on parrots’ cognitive abilities. She relies predominantly on funding from voluntary contributions to The Alex Foundation, Alex being the name of her first parrot subject.

“Rubicon” Crossing?

To show evidence of any abilities similar to those of human language was quite a jolt to the scientific community, especially to linguists. Language was one of the last “Rubicons” separating man from the “Beast.” Animals had already been shown to use tools, to imitate, even to cooperate. The main concerns voiced by the linguists were that the apes’ use of signs should show evidence of a grammar, i.e., rules for combining the signs and novel communication created by the application of those rules. Sign usage should also entail “displacement,” the ability to communicate about events remote in space or time. Hockett (1958) had already created a list of criteria by which human language could be qualitatively distinguished from animal communication; others later modified the list to encompass the variety observed in animal communication. In the revised edition, QAA-2 (1981), Griffin went to great pains to exhibit how many of the 16 criteria were indeed present in animal communication if one looked in detail at animals’ usage.
Additional projects later began and Griffin described them in his subsequent books. Among them was Irene Pepperberg’s continuing work with an African Grey Parrot (*Psittacus erithacus*), Alex, and, much later, two additional parrots. She laboriously taught Alex to speak precisely enough so that it was understood, even on first meeting, by strangers. Other researchers working with imitative birds, such as Michael Dalton with a Blue and Gold Macaw (*Ara ararauna*), claimed to have found numerous impressive linguistic abilities and complex utterances. His methodology was very different, purposefully relying on a more casual and interactive approach. The Macaw’s vocalizations were recorded and analyzed, including contextual information, but the bird’s speech was not at all easily decipherable by others and the work got almost no notice. Though knowing of the research, Griffin did not write anywhere about it. Clearly, the great effort invested by Pepperberg into training her parrot to speak very distinctively was intrinsic to the success of the work. As she explored the capabilities of her parrots, her techniques were decidedly more controlled and more amenable to the methodologies familiar to experimental psychologists. That’s not to say that Dalton’s Macaw didn’t achieve exemplary successes; we simply can’t properly evaluate the work.

Herbert Terrace studied a chimpanzee, “Nim Chimpsky,” so named after the famous linguist and harsh critic of the animal language projects, Noam Chomsky. When Terrace began, he expected to be able to train significant linguistic-like abilities in Nim, particularly, the use of at least a simple grammar. However, when analyzing his videotapes, he believed he saw a great deal of imitation, not any rule-based order of combinations (i.e., no grammar), and not spontaneous and novel use of the signs in new combinations. Such would be expected of a “linguistic” ape. He did agree that the apes can learn a hundred or more lexical items that can be used to refer to actions or to obtain items of interest to them. But Terrace became deeply suspicious of positive results from the other projects. As Griffin notices, it is hardly surprising that Nim imitated signs, since he was taught specifically to do so in his training.

Research efforts greatly expanded in training animals to use artificial lexicons. Gorillas, seals, dolphins, dogs, pigs and others have been subjects, pigs being especially able students, as are dolphins and gorillas. More than training an ability to label objects has been undertaken and achieved. Some animals can use and produce simple grammatical utterances, as illustrated by the animal’s understanding different sign orders and even creating different orders of signs to incorporate proper verb and preposition usage. To test the animals’ comprehension convincingly, peculiar commands were given, e.g., “put ball on hat.”

**Metacognition and … Consciousness?**

Some animals, after training, have demonstrated an understanding of numerosity and higher-level cognitive abilities. For example, Irene Pepperberg’s work demonstrated Alex’s “metacognitive” abilities. Alex learned, not merely labels for colors, but usually correctly responded when asked “What’s same? or “What’s different?” when presented with two familiar or unfamiliar objects. The items would be the same or differ in “color” (7 types), “shape” (5) or “matter” (4). “Matter” designated “material” and might be paper, wood, cork or rawhide. (Alex pronounced it “mah-mah.”) If asked “What’s same” when presented with very different items, he would say “None.” Alternatively, from an array of items, Alex would have to answer questions such as “What shape is X?” Overall accuracy in all these tasks was about 80%.
Griffin considered these findings "... a truly revolutionary advance in our understanding of animal mentality ...". Pepperberg, however, tended to avoid discussions of animals’ conscious experiences, cognizant of the harsh, rejecting attitudes by animal experimental psychologists towards such views. But, finally, as noted by Griffin and Speck, in 2001, Pepperberg and Lynn wrote a paper recognizing that the parrots’ communication, expressing simple thoughts and answering "moderately complex questions," is "evidence of perceptual consciousness." Years later, Irene Pepperberg was also one of the signatories to the Cambridge Declaration of Consciousness (2012), described earlier in this chapter. However, by Griffin’s and most others’ definitions, “perceptual consciousness” is a very limited claim, and not at all the extent of consciousness that Griffin is usually attempting to exhibit in his animal examples. What scientists dare publish is often a far cry from their motivating beliefs.

There are many captivating aspects of the ape language and cognition projects that reflect upon issues such as novel and metaphorical usage and the possibility of the apes referring to and reporting past events via their artificial lexicon. For example, the chimpanzee Washoe was reported to sign “metaphorically,” “Dirty Roger” after Roger Fouts did not grant her request. Another project chimp, Lucy, also was reported to use dirty similarly. Penny Patterson’s female gorilla Koko, who seemed jealous of Mike, a young male gorilla, signed “...Koko know Mike toilet.” Patterson even designed tests for metaphorical understanding with results for Koko similar to those of human 7-year-olds. These and other examples are discussed in detail in Ristau’s and Robbins’ (1982) extensive review of the animal artificial language projects. I cannot, however, find any discussion of these various aspects in any of Griffin’s writings. I suspect that, “hard-nosed” scientist that he was, he could dare to propose animal consciousness, but strove to limit his examples to those more clear-cut, striking and less subject to controversy. And importantly, in his articles and even his books, he had space limitations. He was covering the world of potentially conscious animal behavior and he had to be highly selective in his examples. Other reviewers’ more extensive analyses dealt with a minute portion of Griffin’s purview.

**Whistling Dolphins**

Griffin also discusses the notable work with dolphins, who naturally exhibit much spontaneous versatility both in the wild and in captivity. They produce a variety of sounds, notably click trains used for echolocation (e.g., in navigation and hunting). They also make longer sounds, mostly of lower frequency, but also some higher frequency whistle-like ones. The non-clicks feature in an extensive system of natural communication. Furthermore, captive dolphins can be trained to imitate both human actions and some whistle-like sounds. Most intriguing, the natural whistles seem to include a "signature" whistle that appears to identify a specific dolphin. How to gather relevant evidence? Given the high transmissibility of underwater sound and the intensity of multiple reflections, it is most difficult to determine which dolphin is emitting vocalizations. One could isolate an animal for better recording, but communication is a social phenomenon and better studied within a social group. Peter Tyack, another energetic, persistent and inventive former Griffin student, managed to develop a harmless suction cup device with a microphone; it recorded most strongly the vocalizations of the dolphin to which it was attached. (Recall that Peter also worked extensively with Roger and Katy Payne on whales and then continued cetacean research.) Using two captive dolphins, long-term pool mates, Tyack found that the apparent “signature”
whistle predominated in their interchanges. Each dolphin most often (about ¾ of the time) produced his own rather than the other "signature" whistle.\textsuperscript{187} Was this a "name?" Were they occasionally "calling" each other? Others' work also makes similar strong suggestions.\textsuperscript{188} Yet, despite the tempting conclusions, Griffin remains the ever-critical scientist. He warns that "much additional evidence would be needed to demonstrate that such use of whistles as the equivalent of names was actually occurring."\textsuperscript{189}

The dolphins have also been trained in various cognitive and communicative tasks, again exhibiting much versatility, essential to any "language-like" communication. Karen Pryor's dolphin learned to invent a "new" action for every trial.\textsuperscript{190,191,192} Dolphins also exhibit spontaneous observational learning and both they\textsuperscript{193} and sea-lions\textsuperscript{194} have been used in artificial language projects, trained to associate human gestures with commands and objects. These studies concentrated on comprehension, including the ability to understand different gestural orders, i.e., a simple "grammar" as interpreted by Griffin. The experimenters are much more constrained than Don in their view of the dolphins' and sea lions' "grammatical" accomplishments.\textsuperscript{195}

**Griffin's Conclusions About the Animal Artificial Language Projects**

Griffin concludes his discussion of the animal language projects by affirming that the Gardners' were quite correct, even in their early claims about Washoe and other signing apes' capabilities. As Don notes, considerable improvements had been made to the original experiments and more careful blind testing and other procedures had been implemented, so many earlier concerns were no longer relevant. Griffin's conclusion, unpalatable to so many behavioral scientists, is that "apes can use the equivalent of words to communicate a rich array of thoughts and feelings."\textsuperscript{196}

**B. Natural Animal Communication**

**Symbol-Using Bees?**

As noted, Griffin spent most of the discussion of animal communication in QAA about the honey bees' waggle dance. Recall that Griffin is enormously impressed by the "brilliantly pioneering experiments and insights" of von Frisch,\textsuperscript{197} a level of praise seldom expressed by this reserved man. As described, he credits that work with opening his thinking about what else animals might be able to do that we never imagined.\textsuperscript{198}

Griffin emphasizes both the accuracy of the bees' waggle dances and the versatility of use: the dance can signal the location of different foods or some other need of the hive, e.g., water, plant resinous materials or even a potential new hive site. When food is plentiful, the returning foragers often do not dance at all. Though the bees typically dance in the dark on a vertical surface, if need be, they will orient to the sun or artificial light and dance on a horizontal plane or the surface of a bee swarm (when bees are preparing to find a new site for the hive). In the case of a potential hive site, individual bees may change their preference after a visit to a location suggested by another's dance; they may now dance to the new site. Griffin also notes that bees do other "dance-like" motions, e.g., the *Schwirrlauf*, or "buzzing run" which seems to have a "Let's go!" communicative function.

All this is preparatory to raising the issue of the dance as "symbolic." He expects many to reject the idea, but quotes the meaning of symbol from the Random House *Dictionary of the English*
Language, “Something used for or regarded as representing something else.” although griffin realizes he may not convince all in his audience, he emphasizes the compelling case the versatility of dance use has raised against psychology’s behaviorism and biology’s reductionism.

The Monkeys’ Alarm and Other Semantic Communication

In the greatly enlarged edition, QAA-2, he expanded his insect communication section, describing in some detail the gestural communication of ants, particularly of highly social species which often use gestures to supplement odor productions and odor trails they lay. He further developed the topic of “Animal Semantics” (his term). Various animals exhibit “referential specificity” to the extent of distinguishing in their alarm calls between aerial and ground predators; listening conspecifics respond appropriately in their evasive actions. Such species include ground squirrels (Spermophilus spp.), chickens (Gallus gallus domesticus), and suricates (Suricata suricatta) among others.

But some species produce calls with greater specificity: they make distinctive vocalizations in the presence of different predator species/classes, again eliciting differential responses. Thomas Struhsaker was the first to observe and report such. The distinctive calls were made by vervet monkeys (Cercopithecus aethiops) to a leopard (Panthera pardus), martial eagle (Polemaetus bellicosus), or a dangerous snake, such as a python (family Pythonidae). Subsequent field experiments by Seyfarth, Cheney and Marler, using recorded playbacks of alarm calls and videotaped responses, provided further confirmation of the differential usage. Observing different, but appropriate, responses dependent on the monkey’s location offered evidence that the vocalization did not simply elicit stereotyped responses, but did seem to function to identify the type of predator. Even the mistakes made by young monkeys exhibited errors within a category, e.g., they vocalized “eagle” alarms to a small bird or even a falling leaf.

In his later writings, Griffin with Speck described new results from other researchers who had found additional animals that use specific alarm calls to identify predators ... and with interesting between-species differences. Klaus Zuberbühler identifies two Cercopithecus monkey species, the Diana (C. diana) and Campbell’s (C. campbelli) which have specific alarm calls for two different major predators, leopards and crowned eagles (Stephanoaetus coronatus). Besides responding appropriately to their own species’ alarm calls, Diana monkeys also respond correctly to Campbells monkey calls. More recent work by Zuberbühler is even more surprising. He has shown that the ten different primate species living in the area correctly respond to the Campbell’s distinctive alarm calls; those ten species have alarm calls, but most do not have predator-specific calls. Yet further, the resident hornbills (Black-casqued hornbills, Ceratogymna atrata) are also privy to the significance of the Campbell’s and Diana monkeys’ alarms. They vocalize loudly with hornbill alarms to the monkeys’ eagle alarm calls, eagles being a serious predator to the hornbill. They are not very vulnerable to leopards and are not particularly responsive to the monkey’s leopard alarm calls, both vocalizations described as acoustically “similar” by the researchers. The hornbill, a long-lived, large-brained bird, is presumed to have learned the distinctions. Klaus Zuberbühler has also learned the calls. Once, late in the day, walking back alone to camp, still 15-20 km. away through the tropical forest, he was warned of a leopard. How? ... by recognizing the Diana monkeys’ distinctive alarms.
Some other primates have one or more predator-specific calls (e.g., raptor alarm calls by lemurs and sifakas), but use other calls more broadly, alerting for both mammalian predators and frightening situations with the same vocalization.\textsuperscript{211} Still other species, the prairie dogs (\textit{Cynomys spp}).\textsuperscript{212} and meerkats (\textit{Suricata suricina}).\textsuperscript{213} seem to have developed more elaborated call types. They are both highly social, colonial species living in fairly permanent underground dwellings and have calls identifying specific classes of predators. Con Slobodchikoff, using model and real predators and human intruders, recorded the prairie dogs' alarm calls and later played them back to the prairie dogs. Different, appropriate responses were made to each call, as had likewise occurred during the initial trials with the models or predators/intruders. He concluded that "A call can identify the category of predator, such as coyote [\textit{Canis latrans}], domestic dog [\textit{Canis familiaris}] or red-tailed hawk [\textit{Buteo jamaicensis}] ..."\textsuperscript{214} Griffin and Speck cautiously summarize, "If confirmed, this level of semantic communication appears comparable to alarm calling by vervet monkeys and calls for further investigation."\textsuperscript{215} Again, I would surmise that at least some of the expressed caution derives from the reviewers' repeated stringent behaviorist critiques before the manuscript was accepted for publication.

Slobodchikoff and his students continued experiments and extensive analysis of the video and audio recordings, discovering yet more intriguing facets of the prairie dogs' communication system. Additional evidence further supports an interpretation of the alarm calls as providing information about the predator species, rather than eliciting a specific response. For example, the Gunnison's prairie dogs give distinctive alarm calls and different, but colony-wide, responses to humans (running and diving into burrows), coyotes (run to burrow and stand outside) and dogs (alert). The responses are appropriate to the danger and hunting strategies of each predator/intruder. The response to the prairie dog's red-tailed hawk call does not elicit a similar response among colony members. Only those prairie dogs within the direct, swooping flight path of the hawk run into their burrows, for, once begun, the hawk cannot alter its dive.\textsuperscript{216}

The calls made to humans are both distinctive from those made to other species, but quite variable; they differ depending upon apparel color and even the human's shape. Distinctive acoustic elements exist in calls associated with blue vs yellow apparel and blue vs green, but not for yellow vs. green which prairie dogs cannot perceptually distinguish.\textsuperscript{217} Certain geometric shapes also elicit differentiating calls, e.g., a circle vs triangle, but not circle vs square, though the prairie dogs did have different acoustically structured calls for a large and smaller square.\textsuperscript{218}

To summarize, many aspects of referential/semantic meaning do remain controversial. But clearly, we have vastly underestimated the sophistication of animal communication.

In the least, the simplistic "Groan of Pain" interpretation\textsuperscript{219} that all animal calls simply indicate a state of arousal, aggression, fear, etc., has had to be abandoned. Griffin saw in both the natural and artificial language studies a most promising "window on animal minds."

\textbf{Griffin's Reflections on Cognitive Ethology and Experimental Psychology}

He once said to me (1980), "I wonder sometimes if I am just part of a spreading revolution." He didn't seem to think so. He felt that he had quite independently created his revolutionary Cognitive Ethology, though he didn't apply the term "revolution." He did not have an extensive background in psychology. Thus, most probably, he would not have been aware of many of the experiments underlying the evolving "cognitive" views of animal behavior, at least as they were emerging in some centers, such as the University of Pennsylvania. Griffin's background was biology,
though he read widely. Recall that he took one Introductory course in psychology when he was a Harvard undergraduate and a few more psychology courses with the insistence of Karl Lashley, his Harvard graduate advisor. Once Griffin began to write further about his ideas and gathered whatever pertinent evidence he could find, he did specifically read psychology journals. The task was daunting; the literature enormous. Since my graduate school background included Learning and Motivation as a subfield and I had recently taught an undergraduate course in Learning, he asked me to give him a brief tutorial of several sessions, with selected related reading in Experimental/ Learning Psychology, which I did. In later publications, particularly the last, Griffin and Speck, 2004, he did write extensively about more current experimental psychological research. For the joint paper, Gayle Speck helped review that literature.

They sought, in the experimental psychology literature, findings that supported animal cognition and consciousness. A particular set of results, brought to his attention by Gayle, dealt with classical aka Pavlovian conditioning. As previously discussed, the work of Robert Rescorla had demonstrated that classical conditioning was not the simple, automatic association of stimuli as previously understood. It was a cognitive event; an animal learned that one stimulus predicted another. More generally, in Rescorla’s words, which Griffin quoted, “... the organism is better seen as an information seeker using logical and perceptual relations among events along with its own preconception, to form a sophisticated representation of the world.”

Gayle alerted Don to an extensive review by Lovibond and Shanks (2002) that examined numerous types of classical conditioning, including autonomic responses such as skin conductance changes, eyelink conditioning, conditioning under anesthesia, etc. She noted the reviewers’ conclusions, “there is little convincing evidence for Pavlovian conditioning without awareness” in human subjects. She suggested to Don that since animals can be classically conditioned, could not the same be true of animals? i.e., animals that can be classically conditioned are conscious, at least during that process. (See the Philosophy Chapter for a related current philosophical theory, the “Unlimited Associative Learning (UAL) Framework” developed by Simona Ginsburg and Eva Jablonka.) In their paper, Griffin and Speck conservatively write, “this conclusion has obvious implication for the question of animal awareness.” Lovibond and Shanks do note that, in some studies, inadequate measures of awareness have been used, and they suggest strategies for improving the sensitivity and validity of the criteria. Nevertheless, Lovibond and Shanks, and most contemporary psychologists do maintain their conclusions about the need for awareness in all, or almost all, instances of classical conditioning. In the view of Lovibond and Shanks, the few cases suggesting possible conditioning without awareness are considered only “worthy of further investigation.”

Comparisons of delay and trace eyelink conditioning also support interpretations of consciousness. In both situations, an organism, a human or a rabbit, etc. is presented with a CS, e.g., a tone, followed by the US, e.g., an air puff to the eyes. Soon, the organism learns to shut its eyes before the air puff. In “delay” conditioning the CS occurs, and the air puff occurs a bit later, but while the CS is still present, while the tone is still playing. In “trace,” there is a time interval between the end of the CS and the beginning of the US. Thus, the organism must remember the CS; it must keep a representation of the CS in working memory. That, hypothesize several researchers, requires awareness. If conditioning can sometimes occur without awareness, it would be for simple
sorts of delay conditioning, not trace. Griffin and Speck do note that the evidence is still somewhat controversial.

**Griffin, the Scientific Communities and Animal Consciousness**

Don was deeply disappointed by the strong resistance among his colleagues to accepting animal consciousness. Evidence of such consciousness in various species was slowly accumulating. Other scientists, in particular, chemists and physicists, seemed receptive. Don would remain perplexed to the end of his days as to why so many of his fellow behavioral scientists and biologists could not be convinced. He truly suffered hostility from a sizeable segment of his community. After the publication of *Animal Thinking*, an experimental psychologist publicly proclaimed that the book should instead be titled *The Satanic Verses of Animal Behavior.* As shall be discussed briefly, further evidence after Griffin’s time continues to support animal consciousness.

**Conferences: Spreading “Heresy” and Meeting Resistance**

During Griffin’s lifetime, numerous conferences dealt, at least in part, either directly with Cognitive Ethology or closely related research. I have selected a few particularly significant to the field and listed them in the table below. Some are discussed further in the text.

<table>
<thead>
<tr>
<th>Year</th>
<th>Conference Details</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>1975</td>
<td>International Ethology Conference</td>
<td>Griffin gave a paper on subjective feelings, a highly significant break from the behavioristic discipline of ethology which “restricted their considerations to observable behavior.”</td>
</tr>
<tr>
<td>1979</td>
<td>Symposium on Consciousness in Contemporary Psychology</td>
<td>Talks included animal consciousness. C. A. Ristau spoke about the ape language and cognition projects. Lynn Nadel, then on the Dalhousie faculty, presented bold new ideas about cognitive maps. During the general discussion, the question was raised as to whether fish feel pain. [See text.]</td>
</tr>
<tr>
<td>1981</td>
<td>Dahlem Workshop</td>
<td>“Animal Mind-Human Mind” – March 22-27, Berlin, FRG, Germany. The workshops are limited to 48 invited attendees, across diverse disciplines. A book was published of the papers and summaries of discussions in each sub-group. (D. R. Griffin, ed.), 1982)</td>
</tr>
<tr>
<td>1983</td>
<td>Animal Behavior Society (ABS)</td>
<td>June, Lewisburg, Pennsylvania. C. A. Ristau made her first presentation about the possibility of purposeful, strategic behavior when plovers make an innate broken wing display, luring predators away from the nest. The evidence seemed to persuade at least some of likely intentional aspects of the behavior, rather than only stereotyped, random movements.</td>
</tr>
<tr>
<td>1987</td>
<td>Animal Behavior Society</td>
<td>“Animal Cognition Symposium” – June 21-26, Williamstown, Massachusetts, USA. The day-long symposium and the book produced from the presentations were dedicated to Donald Griffin, “Cognitive Ethology: The Minds of Other Animals: Essays in Honor of Donald R. Griffin.” Carolyn A. Ristau was the symposium organizer and book editor (1991a). The papers represented diverse points of view, some at variance with Griffin’s views. Griffin’s contribution</td>
</tr>
</tbody>
</table>
was titled, “Progress toward a Cognitive Ethology.” The symposium was unusual in dealing with animal cognition at an ABS meeting, which was a stronghold of behaviorism.

**1987 - International Ethological Congress, Special Lunch Session on Animal Behavior and Cognition** – August 7-16, 1987, Madison, Wisconsin. The 1987 ABS Symposium (above) had provoked so much controversy that this special last-minute lunch session was convened to attempt to rebut a mentalistic approach to the study of animal behavior and to the emerging field of Cognitive Ethology.

**1989 – Animal Behavior Society- Symposium -Lexington, Kentucky.** Griffin notes that the 1987 ABS animal cognition symposium was “viewed by many as so misguided” that this counter symposium was organized in 1989. A book based on this symposium is entitled, “Anthropomorphism, Anecdotes and Animals” (R. W. Mitchell et al, (eds.), 1997).

**Various meetings and symposia** gradually grew more accepting of animal cognition and communication research, though typically still avoiding the issue of consciousness.

**2003 – International Conference on Acoustic Communication by Animals (subsection of Acoustical Society of America) - 1st Meeting** – July 27-30, College Park, Maryland. Although Griffin primarily discussed bat echolocation, he also urged his audience to consider the likelihood that animals can communicate their thoughts and feelings, not only to each other but to us. Acoustic communication is especially suited for this because wide bandwidths can convey detailed information. He further suggested that we investigate animal communication with neither anthropomorphism nor “mechanomorphism,” but a broad, balanced “zoomorphism.”

**2005 – Animal Behavior Society- Symposium in Honor of Donald Griffin (posthumous) – August 9-12, Snowbird, Utah.** The emphasis was on contemporary developments from Griffin’s lifetime of research, including his research on echolocating bats and migrating birds, but especially his work in Cognitive Ethology. Organized By Marc Bekoff, biologist, and Colin Allen, philosopher, the ten speakers included themselves and biologists, Gordon Burghardt, James Gould, Ronald Larkin, and James Simmons, and psychologists, Roger Fouts, Benjamin and Lynette Hart and Irene Pepperberg. Some of the talks did approach the issue of consciousness, while others avoided it. Though not a panel member, Carolyn Ristau made available copies of her biography of Griffin from the Proceedings of the American Philosophical Society (2005).

**Various meetings and symposia** gradually grew more accepting of animal cognition and communication research, though typically still avoiding the issue of consciousness.

Many of the conferences provoked both immediate reactions and lasting influences, as hinted in the Table. It was daring of Griffin to speak of animals’ subjective experiences at an International Ethological Congress (1975), particularly during one held in Europe where the emphasis on innate and mechanistic behavior was even stronger than in the US. The ethologist Marian Dawkins recalls Don’s “wonderful lecture as a sort of preview” of The Question of Animal Awareness published the following year.

As she recalls, the conference was unfortunately distinguished in other ways:

[One Russian delegate had an apparently fatal] heart attack on stage. A French delegate sent his wife postcards from the conference despite having murdered her and buried her in the garden before coming to Italy. ... He was arrested by the police when he got home and spent the next 10 years in a French prison from which he wrote book reviews for Animal Behaviour. ... Despite all these other excitements, the highlight of the conference was Don’s talk. It
changed so much, and I think much of the impact came from his having been up until that point in his career at the ‘hard’ end of biology. ... They don’t make conferences like that anymore!238

At the 1979 Dalhousie Conference on “Consciousness in Contemporary Psychology,” Lynn Nadel presented ideas from the 1978 book by John O’Keefe and himself, The Hippocampus as a Cognitive Map. They had found evidence for “place cells” in a rat’s hippocampus! They proposed that these neurons literally represented locations in physical space and were connected to new events and old memories. Routes traveled by a rat were integrated to create maps of the environment. Though the cells had been found in rats they are presumed to exist in many other species, including humans. The cognitive maps espoused in the ‘30s by E. C. Tolman and promoted by Griffin now had a neural basis.

During the general discussion, the issue was raised as to whether fish experience pain. This was not only theoretical but, for this particular audience, it was personal. Many conference participants were from Dalhousie University and other Nova Scotian institutions. Fishing is a common sport in the area and smoking one’s catch, a not uncommon hobby. The respondents were divided almost exactly into fishermen who said, “No,” and non-fishermen who replied that fish do feel pain. That specific concern has later been presented as a target paper in Stevan Harnad’s journal, Animal Sentience,239 with heated debate continuing in a discussion thread through many journal issues. Most concluded, based on neuroanatomical and behavioral evidence, that fish do feel pain.

In 1981 a prestigious Dahlem Workshop was organized by Donald Griffin, at the invitation of the Dahlem Konferenzen. With her usual, charming “mothering,” the inimitable Director, Silke Bernhard, implemented the gatherings and “smoothed” the way. The philosopher Colin Allen, himself deeply interested in Cognitive Ethology, termed the 1981 meeting as “discipline building.”

A distinctive model had been developed for the Dahlem Workshops, founded in 1974 and continuing through 2012, always held in Berlin. The Dahlem Konferenzen meetings are interdisciplinary and limited to 48 persons, selected by a program advisory committee, with major input, in this case, by Donald Griffin. As described by the Dahlem Konferenzen, that selection is determined by international scientific reputation, independent of national consideration. They do attempt a balance between Europeans and Americans, and about 10% of the places are reserved for young German scientists.

Before the conference, some participants are asked to write a review of their field, not a report of their own work. That paper is distributed with the request that it be read and questions formulated before the meeting. (Monitoring and applying social pressure to the requested behavior, the questions, too, are to be sent in before the meeting and distributed to the attendees.) No lectures are given; the questions become the basis of each group’s discussion. Four such interdisciplinary groups meet, each with 12 members. Rapporteur(s) summarize each discussion and the suggestions offered for future research. In the plenum session, the rapporteurs present the summary paper to all workshop participants for discussion. Finally, the background papers and the rapporteurs’ “State of the Art Reports” of the discussions are published.240 The book, edited by Griffin, was titled simply, Animal Mind-Human Mind.

For this conference, the four discussion groups were: “Neuropsychological Approaches,” “Evolutionary Biology of Thinking,” “Comparative Approaches to Animal Cognition” (Griffin was a
member of this group), and "Communication as Evidence of Thinking" (I, with D. Robbins, wrote the background paper for this group, "Cognitive Aspects of Ape Language Experiments.") (See photos of the four groups with participants’ names.)

It was a superb conference. The meeting's small size was an essential aspect of its success, especially the continued gatherings of the 12-person groups over several days. Some, but extremely little time, was allotted for independent exploring of Berlin and its enticements. All was effectively pre-arranged: lodging, restaurants, entertainment. One did have a choice of opera or orchestral classical music, but the seating was selected most judiciously by the “powers that be,” to encourage “pertinent” scholarly discussion among others with overlapping interests. Collegial behavior was encouraged. A senior person who had been rather harsh, perhaps correctly, but harsh, in his criticism of another's comments, was taken aside and “guided” as to appropriate conference behavior. Since these were still the days of the “Iron Curtain,” at least one participant had arrived early with books and “crossed over” to East Germany. There was nothing seditionary about the literature; they were professional volumes that scholars were unable to access on “the other side.” He described his severe apprehension at crossing the border, but his “stash” was undetected and delivered safely. There was little other obvious impact of the East-West divisions on most of us. Some few did go to the superb Pergamon Art Museum in East Berlin and meander, each with his/her own border-crossing story. It was different for the few East German scientists in attendance; they were closely monitored and had to return each night to East Berlin. Presumably, this was to prevent their defection; no one defected.

The meetings led to reinvigorated collegial relationships and new ones. Primarily as a consequence of that meeting and interactions there between Daniel C. Dennett and RU Animal Behavior Group members, he was invited to speak at the Rockefeller campus. From that talk and discussions during his visit, further fruitful interactions about experimental and theoretical approaches developed between him and Don Griffin, Dorothy Cheney and Robert Seyfarth, and myself.

Enthusiasm about the ideas and the conference seemed to me to be widespread among participants, but, yes, there was a range of interactions with the concepts of Cognitive Ethology. Gordon Burghardt, though not an attendee, reported hearing negative comments about “subjectivity” expressed at the meetings. Not a new reaction and one that has been discussed amply in other places in this book.

Later, back in the USA, the Animal Behavior Society accepted a proposal for Animal Cognition papers and discussions in honor of Donald Griffin for their 1987 ABS meetings. A day-long symposium, it was organized by myself and, initially, with Peter Marler. He dropped out. I guessed then, and still think, that he did so because the subject matter of “Cognitive Ethology” was just too risky.

The symposium was funded by ABS to a limited extent, namely $1,000, which, even in those days, didn't go very far. The small stipends precluded participation by one or more of the invited presenters. But the symposium was very well attended. “It was an extremely lively session, with long periods given to interchange between the audience, discussants and presenters of papers.” Not all lecturers praised Griffin's ideas; some speakers were specifically chosen who held views at variance from his and the scientists associated with him.
DAHLEM CONFERENCE (1981) (4 Photos)

Neuropsychological Approaches

Standing (Left to Right): Rudolf Cohen, Steve Hillyard, Bill Hodos, Floyd Bloom, Ted Bullock, Andreas Elepfandt, Gary Lynch

Seated: Helen Neville, Jerre Levy, Patricia Churchland Smith, Pat Goldman-Rakic

(from D. R. Griffin (Ed.), 1982, p. 332)

Evolutionary Ecology of Thinking


Seated: Jürg Lamprecht, Bill Mason, Marian Dawkins, Norbert Bischof, Hans Kummer

(from D. R. Griffin (Ed.), 1982, p. 354)
Comparative Approaches to Animal Cognition

Standing (L to R): John Cerella, Friedrich Wilhelm Hesse, Rüdiger Wehner, Henry Gleitman, Richard Brown, Jerry Fodor, John Crook, Gerd Lüer, Paul Rozin

Seated: Walter Kintsch, Douglas Gillam, Friedhart Klix, Don Griffin, Rainer Kluwe

(From D. R. Griffin (Ed.), 1981, p. 374)

Communication as Evidence of Thinking

Standing (L to R): Jim Gould, Bob Solomon, Herbert Terrace, Martin Lindauer, Colin Beer, Dan Dennett

Seated: Bob Seyfarth, Sue Savage-Rumbaugh, Carolyn Ristau, Peter Marler

(From D. R. Griffin (Ed.), 1981, p. 390)
ANIMAL BEHAVIOR SOCIETY – 1987
Attendees at Dinner Honoring Donald R. Griffin. ABS Meetings, Williamstown, Massachusetts

1. ?
2. Donald Kennedy
3. Janet Williams
4. Roy Horst
5. Jocelyn Crane Griffin
6. Donald R. Griffin
7. Robert Seyfarth
8. Carolyn Ristau
9. Irene Pepperberg
10. Colin Beer
11. Anne Marie Smith
12. Kenneth Able
13. Ronald Larkin
14. Anne Rawson
15. Doty Dunning
16. Kenneth Rawson
17. Alison Jolly
18. Timothy C. Williams
18a. ?
19. John Teal
20. Christopher Clark
21. Charles Walcott
22. George Michel
22a. Dorothy Cheney
23. Alan Kamil
24. Jack W. Bradbury
25. Alvin Novick
26. Roderick Suthers
27. Peter Marler
28. Peter Tyack
29. Carl Hopkins
30. Doug Quine
31. ?
32. Jeremy Hatch
33. Haven Wiley
34. W. John Smith
The event was even covered by a local radio station, perhaps TV as well. But the crew’s behavior, walking across the stage and generally being a disturbance, unfortunately only exacerbated the reactions of some to the heretical ideas being presented. And, though some audience members were highly displeased with this proposed new direction for the field of ethology and animal behavior, many others were receptive. As the 1987 symposium was described by me, “in the true, intellectual, critical and scientific spirit ... [the papers] presented ideas and experiments that have been stimulated and provoked by Donald Griffin’s work.” Subsequent years brought yet more presentations at ABS meetings dealing with animal cognition and issues of communication “not often tackled by researchers.”

The day ended with a surprise dinner in honor of Griffin. The celebration had managed to remain a surprise. He had wondered, though, why, before the date, he was suddenly receiving so many letters of greeting from former students and colleagues. And quite bizarrely, his wife Jocelyn, usually quite amenable to new suggestions for a restaurant, got quite feisty and upset when he proposed a change to the evening’s dinner plans. As he entered the “approved” dining area and suddenly saw Donald Kennedy (the evening’s Master of Ceremonies), he was astounded. Kennedy, then President of Stanford University, had flown in from meetings with congressional members in Washington, D.C. Later, Roger Payne appeared, en route from international whale meetings in England to Russia, detouring to Williamstown, Massachusetts to attend Don’s commemorative celebration. A most amiable evening ensued, with tales told of and “on” Don by the many friends and colleagues gathered together. (Some anecdotes are available in the “Reminiscences” section of the book produced from the symposium papers, as is a photograph of the dinner attendees. The group photo and individuals’ names are also reproduced in this biography.)

But the conference created “pushback.” A special impromptu lunch seminar was organized at the International Ethological Congress at the University of Wisconsin, Madison to combat some of the “dangerous” ideas presented at the June 1987 ABS Animal Cognition symposium. There was more. By the 1989 ABS meetings, a full symposium had been organized, with a book later published of the papers. As Griffin describes it, “a symposium exploring the prospect for a cognitive ethology (Ristau 1991a) was viewed by many as so misguided that a counter symposium was organized.” The tenor of the contributions is revealed by the book’s title, *Anthropomorphism, Anecdotes, and Animals: The Emperor’s New Clothes?*

There was, and continues to be, interest among philosophers in issues of animal cognition and sentience, particularly by philosophers of science and mind. At least initially, many philosophers seemed far more attentive to the research and writings in Cognitive Ethology than were animal behavioral scientists or even psychologists. Some Cognitive Ethologists, including myself and Donald Griffin, were invited to present papers at philosophical conferences, including the Philosophy of Science Association (PSA) and, in particular, the *Society for Philosophy and Psychology (SPP)*. Soon after the 1976 publication of QAA, Griffin was invited to lecture at the 1979 SPP New York University meetings in a symposium rather unusual for the society: “Animal Cognition and Communication.” In 1986 I presented my recent research to SPP, “‘Injury feigning’ by birds: A purposive behavior?” I was invited to become a member of the SPP Executive Committee (1987-89 and 1998-2002) and during those times worked with philosophers to create symposiums, entire conference programs and organize local arrangements (2000-Barnard College and Columbia University). In the 1999 SPP meetings at Stanford University, a symposium considered “Theory of
Mind: Infants, Primates & Pinnipeds” with contributions from psychologists Alison Gopnik (infants) and Ronald Schusterman (sea lions), and the primatologist Daniel Povinelli (chimpanzees). I chaired the session and the philosopher Colin Allen was the discussant. C. Allen was and continues to be an active member of the Society, specifically dealing with issues in Cognitive Ethology among other concerns. The cognitive ethologist Marc Bekoff has co-published with C. Allen and another philosopher of broad interests, Dale Jamieson.254

We tried to introduce and promulgate the approaches of CE to broader, more resistant audiences: For the 1995 Winter Conference on Animal Learning in Winter Park, Colorado, I organized a symposium on Animal Cognition. Unfortunately, I cannot find a list of the speakers, though I do recall that both Marc Bekoff and I gave presentations. I most likely spoke about my field research investigating the potentially “purposeful” behavior of “injury-feigning” plovers255 and Marc talked about animal play and promoted animal welfare. The audience was composed of staunch experimental psychologists, in that era, typically strong behaviorists. Most used caged animals in laboratory experiments, in which food or water deprivation was a frequent technique as were other uncomfortable procedures. (Audience attention could be a problem. Since the meetings began in the late afternoon, after a long exhilarating (and exhausting) day of skiing, wakefulness was encouraged by restricting speakers to a single slide, after which bright auditorium lights snapped back on and dozers hopefully reawakened.) The awake audience was resistant to the ideas presented.

Cognitive Ethology and presentations about animal cognition and communication were introduced with decidedly non-behavioristic interpretations at other conferences and gradually increased in number. I can mention a few examples; perhaps someone will more precisely trace the history of the spread of such ideas via conference presentations. The Animal Behavior Society slowly increased the number of related papers. The 1985 International Ethological Congress in Toulouse, France hosted an “Animal Cognition” symposium and other related topics during later meetings. The 1985 speakers included Irene Pepperberg who discussed her cognitive studies with the parrot Alex, while I presented aspects of my plover research. Almost all others, though dealing with cognition, avoided mention of the animals’ thinking; their terminology could have easily applied to non-sentient computer programs. The symposium was one of 41 during a nine-day conference. The only other one likely to include issues in Cognitive Ethology was a plenary event, “Ethology and Human Psychology.” The main speaker, however, carefully distinguished between “ethology” concerned with observations of spontaneous animal behavior in the natural environment and “psychology,” which could include such observations of humans but also often involved the psyche.

Even the International Ornithological Congress (IOC) came to include symposia related to issues in Cognitive Ethology. In the 1990 meetings, a symposium examined “Pain and Stress in Birds.” The symposium organizer asked me to discuss “Avian Intelligence and Suffering.” In my paper I noted the philosophical problem of “Other Minds” and indicated some limits to our understanding of avian emotional experiences. I agreed that “Though not without interpretive difficulties, there is evidence that birds do experience fear and have the cognitive capacities to do so.” I did also state, “We can know little about the actual subjective state experienced by the birds.” This was not the robust advocacy for Cognitive Ethology’s impact on animal welfare that he had apparently expected. So, I presented my talk, but my paper is not printed in the Congress
Proceedings for he would not accept it. Again, a reflection of the tensions between research examining the scientific basis for sentience and the desire to promote animal welfare. Later meetings of the IOC also included relevant discussions: For example, the 2018 IOC in Vancouver, Canada held a symposium on “Avian Cognition” which conveyed attitudes generally accepting of avian mental experiences.

The conferences noted are a sampling of the many presentations relevant to Cognitive Ethology. Some more contemporary research will be cited in a later chapter. I have already indicated relevant journals, but some merit further attention, particularly Animal Sentience which deals centrally with the topic of consciousness. Even if not often termed “Cognitive Ethology,” investigations into animal cognitive and conscious capacities have exploded, significantly instigated and promoted by Donald R. Griffin.

ENDNOTES

1 Donald R. Griffin (DRG), 1958a, Listening in the Dark, p. 69. [Pagination is the same for Arabic numerals in the 1974 reprint.]
2 Charles Darwin, 1859.
3 Alfred Russel Wallace, August 1858, “On the Tendency of Species to Depart Indefinitely from the Original Type.” This was the paper on natural selection sent by Wallace to Darwin in February 1858. It was read to the Linnean Society on July 1, 1858, along with materials establishing Darwin’s primacy concerning evolutionary ideas. The materials included Darwin’s 1839 manuscript, his 1857 letter to the biologist Asa Gray repeating his views, and Wallace’s paper. The three documents were published in August 1858 in The Journal of the Proceedings of the Linnean Society: Zoology.
6 Charles, Darwin, 1872.
7 DRG, 1976, QAA, p. 4-5.
8 DRG, 1976, QAA, p. 105.
9 DRG, 1976, QAA, p. 5.
10 DRG, 1976, QAA, p. 5.
11 DRG, 1976, QAA, p. 5.
14 DRG, 1976, p. 58.
15 Eileen Crist, 2008.
17 Carolyn A. Ristau and Eric Strauss, 2023, in press.
19 Griffin and Speck, 2004, p. 5.
23 Cited in DRG, 1976, QAA, p. 52.
24 DRG, 1984, AT, p. 44.
30 Carolyn A. Ristau and Eric Strauss, 2023, in press.
31 The open-access, online journal *Animal Sentience* is a good resource for current evidence about animal minds.


33 DRG, 1984, AT, p. 159-160


36 Daniel C. Dennett, 1991b.

37 Discussed in Donald R. Griffin and Gayle B. Speck, 2004, p. 7, 8, 13. Much of the research has been conducted by Tononi and Edelman. Griffin notes various publications, e.g., Edelman and Tononi, 2000, being the latest pertinent work for the review.


39 P. Low et al., 2012.


42 DRG, 1976, QAA, p. 76-77.


44 DRG, 1976, QAA, p. 76.

45 For example, DRG, 2001, AM-C, p. 93-99. This fairly long section is a discussion of the bowerbird research and its implications for consciousness.


47 Volume Two – Chapter 15, “Before the Revolution ...,” Part 3, The Trip to the Middle of Nowhere.”


50 DRG, 2001, AT-C, p. 93-99. In this section, Griffin discusses various researchers’ studies of bowerbirds’ displays and constructions with particular emphasis on Jared Diamond’s work.

51 DRG, 1984, AT, p. 35.


53 DRG, 1984, AT, p. 50-51.

54 Griffin once observed cooperative hunting by a group of lionesses, which is described in this chapter in the section, “Strategic Cooperation?”


59 DRG, 2001, AM-C, p. 82-85.


65 Volume Three - Chapter 18, Part 4, “Beavers ...”


68 DRG, 1976, QAA, p. 54.

69 Beck, 1980.

70 Beck, 1982.

71 Hansell, 1984.


73 H. Higuchi, 1986.


75 G. R. Hunt, 1996.

76 A. A. S. Weir et al., 2002.


80 Bernd Heinrich, 1995.
81 Bernd Heinrich, 1999.
83 Bernd Heinrich, 2009.
87 Edward C. Tolman, 1932.
88 Edward C. Tolman, 1937.
93 Edward C. Tolman 1928, p.224, AM-C, p. 130.
105 DRG, 2001, discusses numerical competence in AM-C-p. 131-134.
112 Emil W. Menzel, Jr., 1974.
120 Chris Plante, April 13, 2016.
121 E. A. Armstrong, 1949.
122 The plover photographs are in Volume Two – Chapter Fifteen, Part 1, section, “Preparing for Field Work – The Cognitive Plover Example.”
125 Daniel C. Dennett, 1983.
127 Carolyn A. Ristau, 1983.
132 Example proffered by Frans de Waal; described in DRG, 2001, AM-C, p. 226.
137 Reviewed by Martin Lindauer, 1971, as described by DRG, 1984, AT, p. 188.
139 Gordon G. Gallup, Jr., 1970.
140 H. Prior et al., 2008.
142 Plotnik et al., 2006
143 H. Prior et al., 2008.
145 M. Kohda et al., 2019.
146 One may also raise the issue of what aspect of “self” is being recognized. The MSR experiments appear to be related to an understanding of “self” as “body.” That is quite different from a sense of self as an entity with a mind, with beliefs and desires, and with continuity from the past and into the future. For further discussion—see Carolyn A. Ristau (2013).
147 Jakob von Uexküll (1909).
150 Their research is described later in this chapter (Volume Three – Chapter 16, Part 3) in the section on “Natural Animal Communication.”
152 Dorothy Cheney and Robert Seyfarth, personal communication.
155 In DRG, 1976, QAA, the animal artificial “language” projects are discussed primarily on p. 16-19.
160 Duane M. Rumbaugh et al., 1974.
162 DRG, 1976, QAA, p. 18.
163 This particular example is the work by the Hayes who, after six years of intensive training managed to teach their home-raised chimpanzee Vikki to pronounce, in a most raspy way, just four utterances, mama, papa, up and cup. Griffin doesn’t go into such detail, but this work may be found in C. Hayes, 1951 and K. Hayes and C. Hayes, 1951.
164 DRG, 1976, QAA, p.16. For further description of the early attempts at teaching great apes to speak, see the review in Carolyn A Ristau and Donald Robbins, 1982, p. 143-144.
165 Carolyn A. Ristau and Donald Robbins, 1982, p. 144.
166 Noted in Griffin, 1976, QAA, p.17.
167 Emily Sue Savage-Rumbaugh, 1986. Her work with Kanzi is described in DRG, 2001, AM-C, p. 248-251.
16-78  A Revolutionary: Donald R. Griffin - EndNotes

171 For example, The Alex Foundation, dependent on voluntary contributions, is a major source of funding for Irene Pepperberg's continuing research on parrots' cognitive abilities.
176 Both Griffin and I had independent correspondence with Michael Dalton about his work, while Griffin was still at RU, and long before Dalton's book was written. I had listened to audio recordings of the macaw's vocalizing. Perhaps, just as doting parents can understand a toddler's poor pronunciation, Dalton could, but I couldn't, understand the macaws.
177 Herbert Terrace, 1981.
178 DRG, 1984, AT, p. 198-199 is Griffin's discussion of Terrace's work.
179 Ristau and Robbins, 1982, p.167-171 is a discussion of the Nim project. On page 170 is a detailed description of the training whereby Terrace or another of the many trainers taught Nim signs by repeating an object's name and asking "What this?" thus teaching through direct imitation. Pages 173-178 are a comparison of the signing projects' methods, results, interpretations and critiques.
180 Francine G. Patterson, 1978. Her work was first described in DRG, 1981, QAA-2, p. 66, 67 and 97. Francine G. Patterson and Eugene Linden, 1981 is in DRG, 1984, AT, p. 97, 98 and in subsequent Griffin books as well.
181 DRG, 2001, AM-C, p. 183. Irene Pepperberg's work is discussed in p. 182-186
182 Irene M. Pepperberg, 1999.
184 Volume Three- Chapter 16, Part 3, "Evidence Supporting Animal Consciousness (Griffin's And Later)," section, "1) Possible Neural Correlates of Consciousness."
197 DRG, 1976, QAA, p. 19
198 Fulvio Bardossi and Judith N. Schwartz, 1986.
199 Griffin likewise offers the philosopher Charles W. Morris' definition, even more applicable to the bee dance Charles Morris, 1946, cited in DRG, 1976, QAA-1, p. 24.
204 Robert M. Seyfarth, Dorothy L. Cheney, and Peter Marler, 1980b.
205 This research, described in the following paragraphs, is cited in Griffin and Speck, 2004, p.13.
206 Klaus Zuberbühler, 2000.
210 Klaus Zuberbühler, September 4, 2021.
211 C. Fichtel and P. M. Kappeler, 2002.
212 Con N. Slobodchikoff, 2002.
216 Judith Kiriazis and Con N. Slobodchikoff, 2006
217 Con N. Slobodchikoff et al, 2009.
218 Con N. Slobodchikoff et al, 2012.
220 Griffin and Speck, 2004, p. 9-10. The authors discuss classical conditioning in this section; Rescorla is quoted on p. 9.
222 Gayle B. Speck, personal communication, March 16, 2021. Telephone interview. Gayle Speck’s contributions about this point concerning classical conditioning and awareness are derived from this conversation.
228 P. F. Lovibond and D. R. Shanks, 2002, p. 3.
230 Volume Three – Chapter 18, Part 7, “Consciousness and Ethical Concerns,” section, “Post-Griffin: Contemporary Evidence Supporting Animal Consciousness.”
235 Robert W. Mitchell et al. (Eds.), 1997.
238 Marian Stamp Dawkins, personal communication, June 12, 2022, E-mail.
239 Brian Key, 2016.
244 Carolyn A. Ristau, 1991e, Preface, p. xiii.
245 Carolyn A. Ristau, 1991e, Preface, p. xii.
248 Robert W. Mitchell et al. (Eds.), 1997.
250 DRG, 2003? (date uncertain), KNOWANIMALS [11EB03], unpublished ms.
251 See a brief description of the views of selected philosophers in Volume Three – Chapter 17, “A Brief Foray Into Philosophical Issues Concerning Cognitive Ethology.”
252 Carolyn A. Ristau, 1992. I cannot find any references for Griffin’s presentations.
253 Carolyn A. Ristau, June, 1986. Unfortunately, I cannot find the dates or titles of Griffin’s presentations.
CHAPTER SEVENTEEN

A BRIEF FORAY INTO PHILOSOPHICAL ISSUES CONCERNING COGNITIVE ETHOLOGY


PART ONE

INTRODUCTION

Overview

Philosophers’ interest in Cognitive Ethology and their potential assistance. Griffin emphasizes that Cognitive Ethology derives from biology.

1. A Brief View of Philosophical Interests in Animal Cognition

Philosophical issues surrounding Cognitive Ethology are closely related to many of the issues raised earlier both during the history of studying animal behavior and by scientists critical of Griffin’s ideas. Especially pertinent is behaviorism’s avoidance of mental terms, which, nevertheless, entailed almost surreptitious use of mental-like terms. Recall how Tolman’s “purposive behaviorism” was particularly suspect and he was considered a renegade by other behaviorists. “Philosophical behaviorism” developed to deal with some of these issues. (That field was also termed “analytic behaviorism” and “logical behaviorism.”) In its strongest form, it is “the view that psychological terms can be translated without loss of meaning into behavioral terms.”

The previous discussion in “Psychology in Revolution” indicates some of the difficulties encountered with this approach.

Broader philosophical interest in animal minds has been growing. As described by the philosopher Irina Mikhalevich, the concerns

break down into several (often overlapping) areas, including those interested in animal cognition, those interested in consciousness, with animal consciousness as a subset, those interested in the epistemology of animal cognition/consciousness science, and empirically-informed animal ethicists.

Mikhalevich adds that there are other areas of philosophical interest as well, noting the philosophy of mind and biology. Says Irina, “These days, the philosophy of biology and theoretical biology can blend seamlessly together.”

I cannot review the entire area, but I can indicate some principal issues as well as some philosophical publications, particularly those written as Griffin was making his case. I can also offer
examples of more contemporary developments. However, I am not a philosopher, so I must leave the intense discussions to the philosophers.

Issues dominating the discussions include: How to establish a science of Cognitive Ethology, of the study of animal minds? Note that some contemporary philosophers question the need for a single overarching scientific theory.\(^6\,^7\) If we are attempting to study thinking, experiencing, conscious animals, we need to define consciousness, recognizing that it is likely to differ from ours and differ among species. Or, in lieu of a definition, “some philosophers might argue that ... we could identify several markers of consciousness (whatever it may be).” Potential kinds as well as levels of consciousness are of concern. And rather than “levels”, Andrew Lee, for example, speaks in terms of “degrees,” emphasizing the continuing gradation; likewise, he and others speak more broadly of “sentience” rather than “consciousness.”\(^6\,^9\) Some, for example, Stevan Harnad, use the terms as synonyms.\(^10\)

Some philosophers, such as Stephen Stich, consider that consciousness depends on language;\(^11\) but animals, though able to communicate with each other, do not have language, or do they? Do some species’ natural communication systems share critical characteristics of human language?\(^12\) Humans, like animals, engage in non-verbal communication via postures, gestures, and sounds; couldn’t that be conscious? Griffin, though few philosophers, was centrally concerned with the possibility of human communication directly with an animal. i.e., two-way communication.\(^13\)

If an animal is conscious, what is the content of that consciousness? If an animal has a mental state, can it also be aware that it does? Does it attribute mental experiences, not just to itself, but to others? The evidentiary basis of our commitment that, like ourselves, other humans have minds, is the philosophical problem of “Other Minds.” How much more challenging when the “other” is an animal,\(^14\) and when animals are attributing mindedness to another.

Does an animal have a Theory of Mind (ToM), i.e., can it use the presumed mental state of another to predict the other’s behavior? Does an animal have “beliefs” and “desires” (philosophical terms, not our common usages). Mikhalevich raises the tantalizing concern: whether this metacognition or ToM is necessarily conscious. If so, is it thereby a possible source of evidence of consciousness?\(^15\)

Finally, concerns with methodology: How do we study animal minds? How do we do the work of a Cognitive Ethologist?

The biologists’ concerns with the new field of Cognitive Ethology were largely still those of scientists immersed in behaviorist traditions. Their interests, if any, were the biological basis of animal minds, the possible evolutionary history, and the neurological mechanisms that might give rise to consciousness and thinking. Unfortunately, many biologists simply could not be bothered about this new enterprise, and, as we have seen, considered the whole undertaking a setback to the scientific study of animal behavior. My own experiences in studying the possibly purposeful aspects of plovers’ injury-feigning behavior,\(^16\) and using the “intentional stance” as an approach to help design my experiments, attracted far more attention from philosophers of science and mind than from biologists or psychologists.\(^17\)
2. Cognitive Ethology Derives from Biology

We must be reminded that Griffin had proposed a Cognitive Ethology, i.e., a science that developed from the field of biology, specifically ethology. He had expanded ethology in a paradigm-changing, literally, revolutionary, way. Ethology had been defined by Niko Tinbergen, one of the giants of that field and a Nobel Laureate. Per his definition, ethology was the biological study of behavior, characterized by observable phenomena and by the scientific methods of inductive reasoning. In his view, ethology needed to address four problems. He quoted Julian Huxley, an evolutionary biologist and grandson of Thomas Henry Huxley, the "other discoverer" of "Darwinian" evolution. Julian Huxley had already distinguished three problems: that of causation, of survival value (or adaptive function), and of evolution. Tinbergen added a fourth, that of ontogeny (development). Proclaimed Tinbergen, "... a comprehensive, coherent science of Ethology has to give equal attention to each of them and to their integration." To these "Four Questions," which have guided ethological research, Griffin, in effect, added a fifth, "conscious awareness." For Griffin, such consciousness has causative properties: an animal can consciously want something and then arrange its behavior so as to achieve that goal and fulfill that desire. Such behavior would likely be most clearly observed in any evidence of planning, or when adapting to/finding a solution to a new challenge.

However philosophers may opt to construe the epistemological problems besetting the study of Cognitive Ethology, the researchers conducting fieldwork must emphasize careful and extensive observations of behavior. The questions guiding the study help the scientists select the particular aspects of behavior on which to focus. Griffin's/Cognitive Ethology's concern with an animal's mental experiences, its goals, its purposes, the sensory world in which it lives, and its feelings, helps determine the most salient features of an animal's behavior to be noted. Concern with those mental experiences can guide the experimenter both in the lab and in the field to design the appropriate experiments to help reveal the animal's goals and other aspects of its mental life.

Griffin's mission was aptly described by the ethologist Colin G. Beer:

...there were ... differences in our approaches - Griffin was being much bolder than I was. Where he contemplated the question of animal awareness as an empirical matter, open to experimental investigation, I took the more cautious stance of regarding it as a heuristic matter affecting what one looks for and notices about animal communication.

Philosophers can aid the science by helping reveal unrecognized assumptions made by scientists, biases in observations, and, perhaps, hopefully, by helping formulate a/some theoretical basis/bases for the studies.

3. Griffin's Strategies for Gathering Evidence of Animals' Mental Experiences

Donald Griffin envisioned the science of Cognitive Ethology as the study of mental experiences of animals particularly as they live in their everyday world. The first challenge, then, is to gather evidence that animals are conscious, that they do have mental experiences. Griffin took that concern as his main focus and suggested at least three main strategies which, in fact, have already been discussed. These were: 1) evolutionary continuity and the similarity of neural structures and processes in humans and non-human animals, 2) the versatility shown by animals in
meeting novel challenges and 3) the window into animal minds as revealed by their own natural communication and artificial language studies.

Griffin did suggest the possible content of an animal’s mental experiences but did not initially concentrate on attempting to specify precisely such content or proposing means of gathering such information. I believe he recognized that the field did not yet have the techniques to do so. The important exception is his interest in animal communication as a window into animal minds, potentially a “two-way window.” Others, particularly philosophers, were most concerned about the content of animal minds: the precise nature of the content and how to determine such.

Griffin did gradually change his focus and, during his late days, became more concerned with the likely content of an animal’s mind during its activities. Don also wished to concentrate on what it is to be a particular animal; what is life like for that animal? He suggested means to explore these issues. As we shall see, contemporary research has created some remarkable techniques that begin to reveal an organism’s consciousness, at least of a perceptual mind state.²⁰ Of course, there is controversy about the interpretations.
PART TWO

CONSCIOUSNESS: THE NATURE OF ANIMAL MENTAL EXPERIENCES

Overview

Griffin initially focused on convincing scientists to accept the likelihood of animal consciousness, while philosophers wished to probe the content of consciousness, a later focus for Griffin.

Some approaches to the nature of animal experience are included: von Uexküll’s concept of an animal’s “Umwelt” or worldview, Nagel’s belief in the impossibility of knowing an animal’s conscious content, and Griffin’s Cognitive Ethology.

1. Defining Consciousness

If animals are conscious, we must accept that their consciousness and mental experiences are not only likely to be different from ours but will also differ among species, presumably encompassing different types and levels. There has been much confusion about definitions of “consciousness” and “awareness” among the various users of the terms. As previously noted, Griffin settled on “Consciousness is the subjective state of feeling or thinking about objects and events.”

Likewise, the levels of consciousness have been described in several ways. Griffin largely accepted those described by the philosopher Thomas Natsoulas, whose views he mentioned first in the second edition of The Question of Animal Awareness (QAA2) (1981), and developed in considerably more detail in the first and second editions of Animal Minds (1992, 2001). Natsoulas discusses several kinds of consciousnesses, as defined in the Oxford English Dictionary (OED) which Natsoulas specifies not as higher or lower, but rather as different from each other. Griffin concentrates especially on Consciousness 3, “the state or faculty of being mentally conscious or aware of anything” and notes that Natsoulas considers this “our most basic concept of consciousness, for it is implicated in all the other senses. One’s being conscious, whatever more it might mean, must include one’s being aware of something.” Griffin often terms Consciousness 3, “perceptual awareness.”

Consciousness 4 from the OED, is “the recognition by the thinking subject of its own acts or affections,” what Griffin usually terms “awareness of oneself.” Griffin continues: Consciousness 4, by this definition entails

*a conscious awareness that one is thinking or feeling in a certain way. This is conveniently called reflective consciousness, meaning that one has immediate awareness of one’s own thoughts as distinguished from the objects or activities about which one is thinking.*

Griffin emphasizes this point because he notes confusion among scientists distinguishing between perceptual and reflective consciousness when discussing animal consciousness. Many philosophers and behavioral scientists are willing to accord animals perceptual, but not reflective consciousness. Even ethologists, including Cheney and Seyfarth, whom Griffin considers are “most inclined to ascribe conscious mental experience to animals” share the view that animals may know certain things but not know that they know. Griffin recognizes that it would be difficult to detect reflective consciousness in animals if it does occur. He notes that most of the suggestive evidence in his discussions points towards perceptual, rather than reflective consciousness.
2. **Jakob Von Uexküll’s “Umwelt”**

The insights of Jakob von Uexküll (1909)\(^3\) should be considered in examining the possible contents of an animal's mind. He used the term "Umwelt" to describe the environment as an organism perceives and interacts with it. He stressed that, although various species might live in the “same” physical environment, each lives in a different world. Each species has different sensing apparatus. Each species emphasizes different aspects of its sensing and, depending on its abilities and needs, interacts differently with the environment. He, too, agreed that consciousness is likely to differ from species to species. Furthermore, we should not assume that because we perceive and use our language to talk about a world of objects, that animals are categorizing their world in a similar way. Von Uexküll speaks instead of the "functional tone" of an object, the object's use or function for that particular organism. Thus, he suggests, a chair may have no particular significance to a dog until it jumps up on the chair, lies on it, and discovers pillows and other items to lie upon. I.e., the dog understands an object in terms of the action he does with it.\(^3\) This is quite similar to Piaget's description of the stage of “concrete thinking” by young children, for example, preschoolers, who tend to define a word in terms of how they interact with it. Thus, “a hole is to dig.”

3. **Thomas Nagel's view of animal consciousness**

Whatever the content of animal consciousness, we shall never know it. These are the views of the philosopher Thomas Nagel, expressed in his influential essay, “What is it Like to be a Bat?”\(^3\) He agrees that animals are conscious, but we, third-person observers, can never access the subjective, personal experience of an animal. Neither can we access another human's state, but we can make far better guesses than for a non-human. Many have argued against his views and, as we've seen, bat research has revealed many specific aspects of the environment that bats are able to detect and use as they navigate and forage for insects or other food. But at least to some, it may always be a mystery as to whether there is anything in the “bat's belfry,” or whether it is “a blooming buzzing confusion.”\(^3\)”
PART THREE
TOWARD A THEORY OF ANIMAL MIND

Overview

Several attempts to create a theory of “animal mind” during Griffin’s time included: functionalism, neuroscience and “Folk Psychology.” The latter is still a hotbed of controversy as noted in some philosophers’ critical views. However, some concepts, such as the “intentional stance,” have had heuristic applications in field experiments. Examples are Ristau’s work with plovers’ “injury feigning” and Seyfarth’s and Cheney’s analysis of vervet monkeys’ symbolic use of alarm calls. Philosophers such as Daniel Dennett, John Searle, Kristin Andrews and Irina Mikhalevich have weighed in on related issues.

To explore the issue of animal consciousness and cognition more thoroughly, let us accept Griffin’s suggestion and, indeed, admonition. Let us accept that animals have mental experiences or at least may have them. Let us understand the task as Don Griffin has suggested: assume the probability of awareness (pA) equals 0.5 and our task is to gather evidence that would either increase or decrease our acceptance of a greater or lower probability. Thus, pA = 0.5 becomes the null hypothesis. In comparative cognition research, the “simpler,” more mechanistic view that an animal has no consciousness is the usual assumption for the null hypothesis.

The concept of a “simple” hypothesis has engaged philosophers. As Irina Mikhalevich observes, some philosophers would consider that the mechanistic view expressed above is not “simpler” at all. To others, like herself, “simple” is the wrong metric to use for cognition or consciousness, and further, “simplicity as a value in science is itself suspect.”

The “null hypothesis” has likewise engendered philosophical interest. As Griffin and some philosophers have noted, that mechanistic assumption is a biased, non-objective stance to adopt in conducting the research. It can, for example, lead us to ignore certain behavioral details and sequences of interrelated activities, possibly quite long strings, as a “conscious” animal plans and executes behaviors aimed at achieving a goal.

But what is it we are searching for? What abilities might an animal have and to what extent are animals’ mental states like ours or unlike ours and similarly like or unlike those of other species? In short, we are searching for a theory of mind, a conceptual foundation from which to study animals’ mental capacities.

1. Functionalism and Neuroscience

Functionalism is a significant philosophical approach to mind; it encompasses several variants. Functionalists aver that a mental event is considered to be a brain event. However, a type or kind of mental state, such as a belief, does not map onto specific types or kinds of brain events. I can have the same belief today and tomorrow, yet different configurations of neuronal events in my brain may be producing the same belief. In more philosophical terms, “mental states are multiply realizable. And if the same mental states can be realized by different physical systems, then it can’t be the same thing as any one of those systems.” Thus, a theory of mind cannot be reduced to neuroscience. “Functionalism rejects the equation of mental states with brain states, including with specific neurons or their activation patterns.”
Instead, Functionalism stresses the causal relationship that mental states have to each other, “how mental states are related to each other, to perceptual inputs and to behavioral outputs.” Mental states are to be understood in terms of their functional roles.

This was a dominant point of discussion among philosophers of mind, from the 1960s. They speculated that, perhaps, “words like belief, desire, thought and intention could be defined in the way a physicist might simultaneously define mass, force and energy in terms of each other and in relation to other terms.” Another analogy looked to a description involving the workings of an engine: “a carburetor in an internal-combustion engine can be defined in terms of how it regulates the flow of gasoline and oxygen into the cylinders where the mixture is ignited, causing the piston to move.” Thus, a functionalist believes, “A mental state of a particular type does not depend on its internal constitution [e.g. the specific neurons firing], but rather on the way it functions, or the role it plays, in the system of which it is a part [how that mind state might readily lead to certain behaviors].”

Functionalism stood in opposition to neuroscience as a potential basis for a theory of mind, “though it is compatible with neuroscience in many respects. [In philosophical terms], functionalism was a response to and rejection of various attempts at providing a mind-brain identity theory.” Neuroscientific advances have permitted us to understand the contributions of different brain parts to sensory experiences and to more complex thinking. Neurons have been identified which “detect” lines of a certain slant or colors or certain frequencies or human faces. Yet each such neuron is part of an entire system, a very complex system, in which any one neuron may have as many as 10,000 connections to other neurons. The functionalist recognizes these facts, i.e. that in this particular animal, certain particular nociceptors and their associated circuitry cause pain. The functionalist, however, “rejects the claim that all pain states are nothing over and above those neural realizers.” Damage to various brain parts produces abnormalities in behavior, cognition, and affect, yet sometimes, other brain parts can take over at least some of said functions. Neuroscience has not produced a theory of how the mind works. As the functionalists proclaimed, we can have the same belief today and tomorrow, but the same configuration of neurons is not necessarily involved.

2. Folk Psychology

Folk Psychology is another attempt at a theory of the animal and human mind. It is our everyday means of understanding others. More precisely, Folk Psychology is understood to be the basis of our ability to interpret and predict each other by attributing mental states to each other, e.g., beliefs, desires, hopes, feelings and so forth.

In Beer’s address of 1987, as part of a symposium to honor Donald R. Griffin, Beer notes how, for the most part, the theory of folk psychology has been taken for granted in Cognitive Ethology.

... when Donald Griffin ... elected to “concentrate on images, intentions, and awareness of objects and relationships in the outside world [in] attempting to come to grips with the question of possible mental experiences in animals,” he was clearly assuming the categories of folk psychology.

But if folk psychology is to have the role of a scientific theory, it is reasonable to examine it from the point of view of philosophy and to reflect upon the doubts raised about folk psychology.
as a theory. In several papers, Colin Beer outlines the philosophical issues pertinent to a science of Cognitive Ethology. Though a self-stated “non-philosopher,” Beer has an extensive philosophical background and endeavors to provide a bridge between the efforts of cognitive ethologists’ and philosophers’ concerns and propositions. In effect, he has served as the “in-house philosopher” for Cognitive Ethology, though always emphasizing that he was merely raising issues, not giving the answers. I will offer his discussion, adding considerations from others as appropriate.

A. Thought and Language

To begin, Beer acknowledges that in living our daily lives, we work with an everyday psychology, a kind of “folk psychology theory.” Some philosophers, such as Jerry Fodor, consider that’s just about the only one we have to use in studying animals’ mental experiences. He postulates a “language of thought,” universal to cognitive beings. Into this and from this, sentences of spoken language can be translated IF the being has a language. Thus, pre-linguistic children and animals can engage in such “language of thought.” Others object, both to folk psychological theory and to the “language of thought,” though without offering a viable alternative.

Some philosophers wonder whether consciousness, especially thought, depends on language, by which they mean human language. Many philosophical theories of mind do rely heavily on the linguistic content of thought, though that reliance appears to be lessening. Yet, even humans have language-less thought, including music, art and dance, although linguistic thought can be part of it. Even the act of planning could conceivably be done via an array of visual images. The example of Temple Grandin, the autistic animal scientist, is a case in point, though she certainly has excellent linguistic abilities. To have language also implies a set of rules or grammar that permits the arrangements and rearrangements of linguistic units to make totally new communications. Whether such rules are innate and unique to humans is part of the controversy. The possibility of semanticity is another essential issue; can animals communicate using units that “represent” some object? Hence, the interest by Griffin and many in the detailed study of both animals’ natural communication and the language and cognition studies conducted with the great apes and several other species.

Stephen Stich is one of the most adamant critics of folk psychological “notions,” believing they cannot be fitted into a coherent and comprehensive cognitive theory.

B. Theory-Theory and Simulationism

Later philosophical renditions of folk psychology (not discussed by Beer) have considered at least two different approaches: “Theory-Theory” and “Simulationism.” These approaches are seen to constitute a major part of the continuing extensive debate about folk psychology. Theory-Theory is a scientific theory about humans developing an understanding of the outside world, i.e., about people’s development and use of folk psychology. It is the study of ‘how ordinary people come to their understanding of how minds work and how the world operates (e.g., gravity). One might say it is a scientific theory of the theory a human develops to understand the world she is part of. We are able to give explanations, predictions and interpretations of another’s intentional behavior because we have “tacit knowledge of an internally represented theory of commonsense psychology,” i.e., an underlying theory of such “commonsense psychology.”
Simulation theory challenges this view, claiming there is no evidence to support the idea that we have such knowledge. We have no such innate theory, modified by our experiences. Simulationism instead posits that we interpret and predict others by simulating their thought processes in our own reasoning mechanisms. We imagine the other in a situation or ourselves in the future and use a “disengaged” practical reasoning mechanism to predict the other’s feelings, thoughts and actions or our own in a future circumstance. Peter Godfrey-Smith proposes a model incorporating both views.

C. Philosophical Intentionality Applied to Cognitive Ethology

1) The philosophical term “Intentionality”

In philosophers’ attempts to provide a firm conceptual foundation for Cognitive Ethology, some have translated folk psychology into philosophical terms of intentionality, e.g., Bennett (1976, 1991), Dennett (1983, 1987) and Searle (1983). These ideas have been described by Beer (1991) and also by Ristau, among others, which include more recent discussions.

As described by Ristau (1991),

*The philosophers’ use of “intentional” has a much broader meaning than simply “on purpose” or “acting with intent,” ... terms such as “believing that,” “wanting it to be the case that” and “knowing that” are included among intentional terms. Intentionality in the philosophical sense concerns “aboutness” and makes reference to the content of statements or beliefs.*

As an example, if one believes, one is believing something; that something is the content or object of the belief. Beer (1991) notes, “You cannot just believe in the way that you can just sleep.”

One of the criteria for intentionality is rationality; the extent of animal rationality can be explored by relevant experiments with animals. Another criterion is termed “referential opacity.” In intentional statements, there can be a change in the truth value of a proposition when another term is substituted for the person or object in the original proposition. Daniel Dennett offers a simple example. Note how the reference of a belief, an intentional statement, is changed in the following, “George IV wondered whether Scott was the author of Waverly’ versus ‘George IV wondered whether Scott was Scott.’ Those are both intentional statements; they are “referentially opaque.” If cognitive ethologists wish to apply an intentional stance to the behavior of animals, an immediate challenge is how to translate such language-based criteria into non-linguistic terms, if this is possible.

2) Applying the Intentional Stance: Searle vs. Dennett

It should be noted that Daniel Dennett uses the intentional stance in a manner quite different from that of Donald Griffin and most other cognitive ethologists; the philosopher John Searle’s views are much closer to theirs. Searle assumes that animals are conscious and have mental states.

*I do not infer that my dog is conscious, any more than, when I came into this room, I inferred that the people present are conscious. I simply respond to them as is appropriate to conscious beings. I just treat them as conscious beings and that is that.*
As Colin Allen describes, "Searle (1998) argues that our intuitive, commonsense attributions of intentional and emotional states to dogs carry more epistemic weight than philosophically motivated skeptical concerns." For Dennett, however, the intentional stance and terms such as "belief" and "desire" imply nothing about the mental experience of the system to which the intentional stance is applied. Thus, applied to a computer, a chess-playing computer program may want to get its Queen out early or, for an animal, a plover may want to lead an intruder away from the bird’s nest. The intentional stance, Dennett sees as a heuristic approach in both cases, including the latter in which it is used to study the plover’s behavior. Colin Beer likewise considers that the intentional stance has at least heuristic value for Cognitive Ethologists and for others examining animal behavior who choose not to be termed “Cognitive Ethologists.” Among the studies highly relevant to the issues implied by the intentional stance, Beer cites those of James Gould with the honey bees’ waggle dance, the team of Dorothy Cheney and Robert Seyfarth studying semantic information in vervet monkeys’ predator alarm calls, Peter Marler and his students examining the “audience effect” in animal communication and Carolyn A. Ristau’s studies of the plovers’ strategic use of their “injury feigning” as well as her other investigations of their cognitive abilities. (All these projects are discussed in this biography.) In Beer’s view (and mine), those studies have managed to apply to animals an intentional theory devoid of linguistic forms.

3) Intention and Function

Whatever the fate of Folk Psychology or some variant in a formalized theory, certain matters must be dealt with in any theory. The problem of assigning intention is apparent in any description of behavior, whether human or animal (See discussion by Charles Taylor, 1964, and also recall Tolman’s distinction between a “movement” and an “action”). Beer offers the following examples: “I saluted” implies conforming to a ritual and the intention to do so. To say instead, “My arm went up,” is simply a statement that nerves fired and muscles contracted. How does this contrast apply to animals? The absence of language with which an animal could communicate to us makes the problem formidable. For the reasons described earlier, in particular, the Umwelt of each creature, even animals with some kind of language would not imply the absence of barriers to understanding. Recall Wittgenstein’s apt statement, “If a lion could talk, we could not understand him.” The lion’s experiences and the referents of its words would be so extraordinarily different from ours, so outside the realm of our understanding, that such interaction would be incomprehensible, according to Wittgenstein’s view.

As described by Beer, the philosopher Ruth Millikan offers analyses highly pertinent to the problems in describing animal behavior. She applies her general theory of function to body organs, behaviors, customs, and both inner and outer representations. Her term “proper function” accounts for the actions of such bodily organs as a heart or the kidney in terms of their teleonomic or evolutionary function, i.e., a heart pumps blood; a kidney cleanses the blood. By extension, a belief maps onto the world a description of a state of affairs, or presumably true statements, which can be the basis for other beliefs and also intentions. She also argues that the intentionality of language can be described without reference to the speaker’s intention. She further holds that an understanding of the intentionality of thought should be separated from the problem of understanding consciousness. Millikan understands the honey bees’ waggle dance to be a case of
intentionality; it’s about a state of affairs in the world. She considers it to be “referentially opaque,” that is, we can tell what the bees’ dance is about, the existence of nectar at a certain specified location, but she believes the bee cannot. In her view, the bees, both the dancer and recipients, simply react.

This was the crux of Don Griffin’s question, years before, when he asked the prospective grad student, Jim Gould, “Do the bees know what they’re doing?” Unlike Millikan, Griffin thinks it’s highly likely that the bees do know. The dance is not rigidly attached to one specific situation; it does not always result in a single stereotyped response. The dance can be about any of several matters, availability of nectar, or pollen, or water, or a prospective location for a new hive.

The Gould lab “lake experiments” were designed to deal with the very matter of bees’ knowledge and rationality. Would they still fly to a location indicated by the dance, if they knew no flowers could be there, i.e., if the food source was located in the middle of a lake? (These experiments were conducted with bees who had foraged in the area before and presumably knew the environs.) The results seemed to indicate bees’ hesitancy to fly in such circumstances, although some have criticized aspects of the methodology and interpretation.

Don Griffin continued to be intrigued by the problem. Later, he conducted experiments in the Great Marsh in Barnstable, Massachusetts. The honeybees presumably knew that no flowers grew in the marsh, yet returning foragers would dance to locations in the marsh. (The experimenters had created floating stations with sugar water containers.) Would the dance-observing bees fly to the stations indicated? Unfortunately, we have no published data about the results.

Though not as convinced as Griffin, Beer surmises that there may be more intentionality about the bees’ dance than Millikan accedes to.

D. Other Philosophers’ Views of “Folk Psychology”

1) How Aware is a Toad? / Levels of Animal Consciousness

Various philosophers have voiced their doubts about folk psychology and I refer you to the discussions in Colin Beer’s writings and others, for I can give only short shrift to their ideas in this book. Some philosophers, e.g. Kristin Andrews, are far more positive about using folk psychology as a resource in comparative cognition and have written extensively about the matter. Among the philosophers’ hesitancies were those by Daniel Lloyd, concerned that consciousness was a “mixed bag.” “… as wielded by ordinary folk and spruced up by philosophers, [it] seems to include four disparate phenomena: sensation, perception, reflection and introspection.” Some have wondered whether even sensory receptiveness necessarily entails consciousness. The experimental work with blindsight, already discussed, is relevant and suggests a negative answer.

While Lloyd was persuaded that a toad is perceptually aware, he considered that the toad would be incapable of reflecting on its experience, i.e. of being introspective about it. Like the philosopher Arthur Danto, Lloyd “… envisions animal consciousness as bound to the moment of experience, the ‘present and particular’ and lacking in the ability to ‘metarepresent’ such experience as happening in the past or future.” (This, recall, was Griffin’s view of the beliefs held by many philosophers about animal sentience.)
More recent experimental work questions that lack of metarepresentation. See Chapter 18 for a description of present foraging by cuttlefish entailing future planning, all based on past learning. Also relevant is the extensive work by Nicola Clayton and her lab, particularly with corvids, examining “episodic memory and foresight,” or, as it is conservatively termed by most comparative psychologists, “episodic-like.” The psychologist Tulving had claimed episodic memory was uniquely human, the capacity to recognize that it is oneself in the past episode and in the present.

In Clayton’s work, scrub jays (Aphelocoma coerulescens) buried larvae, a preferred food, and, separately, peanuts, the less preferred. The birds had learned that larvae decay, becoming inedible after five days, while peanuts do not. Given the choice, after five days vs. sooner, the jays chose appropriately.

Whereas the scrub jay seems to be able to imagine itself at least in the past and present, Lloyd thinks differently about the toad. He states that insofar as the toad would be incapable of maintaining a continuing image of past and future events with itself participating in them, the toad would not be able to sustain a continuing image of itself and thus would not have a sense of self. Hence, I note, the great interest in experiments purporting to determine which, if any, animal species do have a sense of self; the “mirror experiments” (MSR) and the controversies concerning their interpretation are a special case in point.

2) Philosophical Behaviorism

In his various writings, Beer deals with the issue of Philosophical Behaviorism. We have already seen the limitations of psychological behaviorism in dealing with cognitive and mental issues, and similar difficulties befall philosophical behaviorism. Philosophical behaviorism and logical positivism had their roots in the writings of philosophers such as Wittgenstein, A. J. Ayer, and, in particular, Gilbert Ryle. Ryle strove to define words such as beliefs and desires in terms of “dispositions to act in a certain way,” but that was simply not feasible.

3) The “Eliminativists”

A completely different approach is taken by Patricia and Paul Churchland (so-called “Eliminativists”), namely that folk psychology has been around, unchanged since at least the time of Aristotle and has not developed as a theory. It cannot account for a great deal of what such a psychological theory should do, i.e., deal with creative imagination, memory, mental illness, etc. In their view, we need something else. Yet they acknowledge, as do many, including philosophers, that folk psychology is used by people to understand and predict others’ behavior. In that sense, it is a fully developed, but non-formalized theory of human behavior. But if it is to be considered a scientific theory, it must be evaluated, as scientific theories are, in terms of its predictive power and explanatory success in mind/brain research.

Yet another, more recent view is that the “folk” who use folk psychology in their daily lives, actually do not do a very good job of explaining and predicting another’s behavior with it; yet folk psychology persists. Rather than examining folk psychology as a quasi-scientific enterprise, a better approach, suggests Roxborough, is to examine its use empirically “in situ.” Results suggest a normative function, i.e., social, cultural, and moral purposes.
The debate concerning folk psychology continues still. I particularly recommend the interdisciplinary set of commentaries surrounding Alvin L. Goldman’s target article in *Behavioral and Brain Sciences* (1993) as well as Beer’s writings and later books and articles by others.
Overview

Research in Cognitive Ethology continues. Some biases are noted in the study of animal behavior. Some contemporary approaches to studying animal mind are described, lauding efforts to use a “bottom-up”/evolutionary approach.

Finally, speculation about “Now and Next?”

Notwithstanding philosophers’ concerns, Donald Griffin realized that, even without very clearly specified concepts, science can advance and has in other fields until more precise definitions can be achieved. Cognitive ethology has, therefore, typically used folk psychology in dealing with questions of animals’ mental experiences. Claims of anthromorphizing are countered by Griffin and others emphasizing their use of “critical anthropomorphism”111 and what Griffin came to term “zoomorphism.”

1. Biases in Studying Animal Behavior

There has been a growing examination of some common “biases” in studying animal behavior. Some bias-controlling strategies are considered by Kristin Andrews, a philosopher who has spent decades working alongside scientists studying animal cognition in the lab and in the field. In a most useful and positive review of Andrews’ book, How to Study Animal Mind,112 Irina Mikhalevich describes the three biases presented by Andrews:

- the proscription against wantonly attributing human characteristics to animals (anti-anthropomorphism), preference for cognitively simpler explanations of animal behavior (Morgan’s canon) and the injunction against taking human beings to be the measure of all things (anti-anthropocentrism).113

As both Andrews and her reviewer Mikhalevich agree, only the last is well supported. Philosophers have “roundly criticized” anti-anthropomorphism and Morgan’s cannon, and as I have noted, so has Griffin, many, many times. Again, as Griffin has endeavored to illustrate, from insect behavior to that of apes and elephants, those proscriptions have resulted in

... a double standard in how evidence of cognitive traits is interpreted in the human and animal cases. Different terms are used to describe the same behavior in humans and in other animals.114

For example, as earlier noted, humans have “episodic memory” (what-where-when first-person memory) while animals may be attributed only “episodic-like” memory when they meet similar criteria.

Andrews115 and others, make the case, like Griffin, that there is a middle ground between the extremes of anti-anthropomorphism ostensibly seeking to avoid bias vs. an anthropomorphism that assumes animals are just like us vs anthropocentrism, in which human mentality is the “gold standard” against which all others are to be measured. John A. Fisher had earlier (1991) proposed a taxonomy of anthropomorphism.116 He argued that anthropomorphism, as typically used in applications to animals, does not deserve the critical abuse it receives, unless one can defend the
statement that “ascriptions of mental states to nonhuman animals is a ‘categorical fallacy.’” In less technical (and less precise) terms, unless one can prove that animals do not have mental states, ascribing such possible states in the pursuit of exploring and understanding their behavior is a reasonable procedure. Griffin, acknowledging Fisher’s work, cites the biases arising from denying animals conscious states.

Furthermore, if researchers tend to regard their animal subjects as insentient, they are likely to ignore how critical aspects of the environment affect the animal, influencing its behavior and even its performance in experimental tasks. Andrews advocates that researchers “premise consciousness,” i.e. presume it to be the case, thereby compelling “researchers to consider how the animals’ perspectives factor into their behavior.” Mikhalevich cautions that philosophers might consider under what conditions such assumptions should be made. I would agree that assuming likely consciousness encourages broader, more pertinent observations, and contributes to improved design of field observations and experiments in the lab and field. Assumed insentience can affect the scientists’ actual observations, for any observation is a conscious or unconscious selection of all that is occurring.

I strongly recommend both the Mikhalevich review and Andrew’s book for philosophical insights into these essential topics, written to be accessible to non-philosophers.

2. **Further Developments in Understanding Animal Mind**

   **A. Advances in Neuroscience, but ...**

   In the decades that have followed Griffin’s initial work in Cognitive Ethology, there have been no adequate replacements for folk psychological ideas. There have been enormous advances in neuroscience. Via the use of MRI and other techniques of monitoring brain activity, correlations have been observed between likely neuronal activity and certain presumed mental activity. Examples include a person looking at a moving or colored image or listening to music, etc. Some MRI and related imaging have been conducted with both humans and animals.

   But even the most direct experience remains inaccessible to neuroscience: the redness of red, the felt pain, the “elementary sensations,” “raw feels” or “qualia” as termed by philosophers. Phenomenological experience makes it impossible, according to many, to reduce the mental to the physical. We may find neurological correlates of certain mental experiences, but we will never be able to find neurological activity that is the mental experience. Electrical activity in neurons is not the qualia, the experience of our pain or of the red we are seeing. Neuroscience cannot truly understand what it is like to have a sensation, for that requires having the sensation.

   Neuroscience has become the “big draw” in psychology and a major recipient of funding from governmental and philanthropic institutions; researchers of animal behavior are the “poor relations.” Yet basic truths remain: correlated neural activity is not the experience, does not provide an integrated theory of understanding “folk psychology;” neuroscience is not a “theory of mind.”

   **B. Attempts to Determine the Origins of Consciousness and Cognition, but ...**

   Efforts have continued to specify philosophically, then empirically, the criteria for consciousness and the delineation of potential animal cognitive capacities. I can offer a few examples of such attempts. Over the past two decades, Ginsburg and Jablonka have created a model of an evolutionary transition marker to indicate the change from non-conscious to conscious
Chapter 17 – Philosophical Issues-Part 4: Cognitive Ethology Proceeds

life, the Unlimited Associative Learning (UAL) framework. The UAL is an expanded type of associative learning, encompassing such features as novel compound conditioned stimuli and “trace conditioning.” “Trace conditioning” describes a situation in which the stimulus to be conditioned (CS) ends before the unconditioned stimulus (UCS) begins, e.g., a tone (CS) ends and then a delay occurs before a shock or some other unconditioned stimulus (UCS) occurs.\(^\text{125}\) (As we shall see, in his last paper, with Gayle Speck, Griffin too, proposed that certain kinds of classical conditioning, trace conditioning among them, were likely to be possible only if a subject were conscious, and were, thereby, potential markers of consciousness.\(^\text{126}\)

Stressing the evolutionary basis of consciousness and emphasizing it as a biological phenomenon, best examined from the “bottom-up,” is also the approach taken by the philosopher Walter Veit.\(^\text{127}\) He proposes a five-dimensional framework for consciousness to aid philosophical thinking and to guide and assist scientists in the design and interpretation of their experiments. Included in his five dimensions, very similar to those offered by Birch et al. (2020),\(^\text{128}\) are “self-consciousness, evaluative experience [hedonic], sensory experience, the integration of experience at a time [the binding problem] and the integration of experience across time.”\(^\text{129}\) Theory of Mind (ToM), the attribution of mental states to others and the concomitant ability to use that information in one’s interaction with others seems to be included as part of Veit’s concept of “self-consciousness” or at least as providing evidence for self-consciousness.

Others, such as Arthur Reber,\(^\text{130, 131, 132}\) make a case for cellular-based sentience arising from the very first living, single-celled organism, the prokaryotes, while Lynn Margulis\(^\text{133}\) has proposed a similar theory of a “microbial mind.” At first, Reber considers that only animals (being mobile entities) have “minds,” limited though they be, but he later expands his view to propose with Baluška that the early one-celled “plants” might also be conscious.\(^\text{134}\)

Speaking more specifically of cognition, Linson and Calvo argue that while animal cognition is often taken for granted, cognition is considered absent in plants. They note criteria met by plants and stress a difference in degree, not kind, with respect to plants and animals. Our scientific reluctance to accept the evidence provided by plants is traced back to the strong influence of Aristotle on our science, who treated locomotion as a marker of cognitive agency.\(^\text{135}\)

As is evident, there is growing interest in the simplest systems, from both scientists and philosophers. It is laudable that such attention is being directed to simple systems and possible evolutionary developments thereafter, a “bottom-up” (or “bottom-sideways”? ) approach rather than “top-down.”\(^\text{136}\) There is, likewise, much controversy associated with these endeavors.

C. Attempts to Bridge the Romantic/Killjoy Divide, but …

There have been attempts to mediate the contentious romantic/killjoy interpretations of animal behavior, terms first used by the philosopher Daniel Dennett in 1983.\(^\text{137}\) Among those attempting to create a bridge between the starkly different viewpoints are, for example, Hanus (2016)\(^\text{138}\) and Starzak and Gray (2021).\(^\text{139}\) Hanus notes how Associative Learning researchers (killjoys) “readily implement cognitive constructs like memory, attention and representation in their terminology.” Starzak (philosopher) and Gray (psychologist) have created a three-dimensional model of “causal cognition,” wisely expanding the concept of “causal reasoning” which is usually interpreted in human terms. By so doing, they are attempting to lay a framework for research suitable for killjoys or romanticists. The vectors of their 3-D model are 1) an organism’s
ability to integrate information received, including means to gather and flexibly use the information, 2) a dimension of implicitness vs. explicitness and finally 3) the ability to use different sources of causal information (e.g., by an animal's manipulating cause and effect itself, by observing others do so, or by observing natural co-variation of events.) Each vector is, in turn, a composite of its own 3-D vector, which produces the “value” to be situated on the final 3-D grid. Since, in their view, “the behavioral criteria for consciousness are notoriously unclear,” the dimension of “explicitness,” defined as “what an animal can do with causal information,” is intended to capture at least some aspects of consciousness. That is clearly a very limited view of consciousness; in fact, as Mikhalevich remarks, does it track consciousness at all?240

There are significant other criticisms. Firstly, their model is very much a “top-down” one, and may not map onto real-world abilities, particularly of young children and non-humans. How much more preferable to attempt to create a model from simpler organisms and expand “up” or rather “out.” Secondly, the model requires a metric that they do not have. I cannot imagine how to place any species or individual on their grid. Nevertheless, their efforts to create a “workspace” for all are laudable.

These attempts at exploring the continuity between human and animal minds typically yield either a contemplation of animals in human terms or humans in animal terms. As Donald Broadbent once commented, “You could either humanize the brutes or brutalize humans.”

3. Now and Next?

It does seem likely that from the realm of folk psychology and heuristic applications of the “intentional stance,” and from attempts to bridge the difference between associative and cognitive interpretations of animal behavior, there are some core concepts that can apply in similar, not identical, fashion to human and non-human animals. A construct similar to or overlapping in critical ways with “intention,” “purpose” or “goal” is one such. Specifying the differences between species and between circumstances will be part of the problem in using such a term. But such framings seem to lose the concern with consciousness. Mikhalevich avers that any such future directions need also to strive to identify “the relationships among cognition, affect, motivations and phenomenal consciousness.”242 The “folk psychology” debate continues, as do potential future developments in constructing theories more widely satisfying. Nonetheless, we continue to advance our understanding of animals’ behavior and their mental capacities and experiences.

END NOTES
ENDNOTES FOR PART ONE BEGIN HERE

1 These issues are discussed in Volume Three – Chapter 16, “Revolutions,” primarily in Part 2, “Revolution in Ethology and The Birth of Cognitive Ethology,” in the section, “Critics’ Arguments and Griffin's Responses.”
3 The section, “Psychology in Revolution” can be found in Volume Three - Chapter 16, “Revolutions,” Part 1.
4 Irina Mikhalevich, personal communication, April 15, 2021, E-mail.
5 Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section.
8 Irina Mikhalevich, personal communication, January 22, 2022, E-mail comments on this section.

Stevan Harnad, personal communication, May 21, 2022, E-mail. (Harnad has also presumably indicated his use of consciousness and sentience as synonyms in other published accounts.)

Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on the Philosophy section. She notes the difference between defining consciousness, e.g., as a “felt experience” and identifying its dependence relations.

Comparisons between human language and natural animal communication were discussed in Volume Three - Chapter 16, Part One, section “Revolutions in Many Fields and Universities,” in the subsection “Revolutions in Psychology.”

Griffin’s interest in two-way communication was discussed primarily in Volume Three - Chapter 16, “Revolutions,” Part 3, in the section “Communication - Natural and Artificial,” primarily in the subsection, “A. Artificial Language Projects.” He was also interested in the possibilities of communicating with bees and controlling their foraging site via a robotic honeybee; this is discussed in Volume Three – Chapter 18, Part 4, “Bees,” section “3. Finally, a Robotic Bee.”

Kristin Andrews, 2020, Chapter 1. In this chapter, Andrews discusses the problem of “Other Minds” applied to animals.

Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section.


As an example, I was invited to give presentations at meetings of The Society of Philosophy and Psychology, but much more rarely invited to speak at biological, ethological or psychological conferences. I did speak at such, but I applied to do so. The role of conferences in “spreading the word” about Cognitive Ethology is discussed in Volume Three – Chapter 16, Part 3, in the section, “Conferences: Spreading “Heresy” and Meeting Resistance.”


See Volume Three - Chapter 16, “Revolutions,” Part 3 and Volume Three - Chapter 18, particularly Part 6, “Conscious Elephants... and Other Ruminations” and Part 7, “Consciousness and Ethical Concerns” for discussions of these issues and both Griffin’s and more contemporary experimental approaches.

ENDNOTES FOR PART TWO BEGIN HERE


25 Thomas Natsoulas, 1978c.

26 Thomas Natsoulas, 1978c, p. 910-911


31 DRG, 1992, p. 11.

32 “Perceptual consciousness” is briefly discussed in Volume Three - Chapter 16, “Revolutions,” Part Three, subsection, “Metacognition and ... Consciousness?” Griffin notes there that Irene Pepperberg and her colleague S. K. Lynn write that their research parrots are capable of “perceptual consciousness.” Pepperberg had publicly avoided the issue of animal consciousness prior to that publication.

33 DRG, 1992, AM, p. 11

34 Jakob von Uexküll, 1909.


37 Nagel’s views and the interactions between Griffin and Nagel are discussed in Volume Three - Chapter 16, “Revolutions” Part 1, in the subsection entitled, “Philosopher Thomas Nagel asks, ‘What is it Like to Be a Bat?’”

38 William James, 1890, Vol. 1, p. 488.

ENDNOTES FOR PART THREE BEGIN HERE
39 Irina Mikhalevich, personal communication, January 22, 2022. E-mail.
40 Kristin Andrews and Brian Huss, 2014. See their work for a more thorough discussion of the null hypothesis and related issues of anthropomorphism and anthropectomy in animal cognition research.
41 Irina Mikhalevich, 2015.
43 Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section.
44 Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section.
45 Ned Block, 1996. The general Functionalist concepts were expressed by Ned Block, with additional comments/clarifications expressed in the quotations from personal communications to CR from Irina Mikhalevich, January 22, 2022.
46 Janet Levin, 2004, revised, 2018. The quotations in this section are from the 2018 entry.
47 Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section.
48 Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section.
49 Definition adapted from Peter Godfrey-Smith, 2005.
56 Patricia S. Churchland, 1986, cited in Colin Beer, 1991, p. 27. Patricia and Paul Churchland are among the philosophers objecting to Fodor’s “language of thought.” Daniel Dennett (1983) does as well, though he does offer his conception of an “intentional stance,” a heuristic means to study animal behavior. His views are discussed briefly later in this section.
57 Irina Mikhalevich, personal communication, January 22, 2022. E-mail comments on this section. Mikhailovich suggested there seemed to be a decreasing emphasis by philosophers on language in their theories of mind,
59 Carolyn A. Ristau and Donald Robbins, 1982.
60 Mark S. Seidenberg and Lauren Ann Petito, 1979.
61 The ape language and cognition studies have been discussed in a separate section of Volume Three - Chapter 16, “Revolutions,” Part 3, section “6) Communication – Artificial and Natural,” subsection “A. Artificial Language Projects.”
66 Peter Godfrey-Smith, 2005.
69 Daniel C. Dennett, 1983.
71 John Searle, 1983.
77 Daniel C. Dennett, 1983.
78 The quotations about King George used by Dennett are those of Bertrand Russell (1905, 1956).
80 Colin Allen, 2016.
86 James L. Gould, 1986. The “lake experiments” were discussed in Volume Three - Chapter 16, Part 3, “Evidence Supporting Animal Consciousness (Griffin’s And Later),” section “Cognitive Maps.”
88 Griffin’s experiments with honey bees in the Great Marsh are discussed more fully in Volume Three - Chapter 16, Part 2, “Bees,” section, “At the ‘Sugar Cube.’”
100 Endel Tulving, 2005.
103 The issue of self-recognition and the Mirror Self-Recognition Experiments (MSR) are discussed in Volume Three - Chapter 16, in the section on “Natural Psychologists,” subsection, “Self-Recognition?”
107 Craig L. Roxborough, 2016.

ENDNOTES FOR PART FOUR BEGIN HERE
111 Gordon M. Burghardt, 1991, p. 73-75.
113 Irina Mikhalevich, 2021, p. 2.
114 Irina Mikhalevich, 2021, p. 3. Mikhalevich is describing K. Andrews’ point of view with which she agrees.
117 John Andrew Fisher, 1991, p. 84.
118 DRG, 2001, AM-C, p. 28.
124 Simona Ginsburg and Eva Jablonka, 2007b.
129 Walter Veit, 2023, p. 38.
135 Adam Linson and Paco Calvo, 2020, p. 49.
137 Daniel C. Dennett, 1983.
138 Daniel Hanus, 2016, p. 245.
140 Irina Mikhailovich, personal communication, January 22, 2022. E-mail comments on this section. She added
the specific query, “I’m not sure how it tracks consciousness at all.”
142 Irina Mikhailovich, personal communication, January 22, 2022. E-mail comments on this section.
CHAPTER EIGHTEEN

THE RETURN: BEES, BEAVERS, BATS, ELEPHANTS ... AND CONSCIOUSNESS IN BARNSTABLE

[Chapter Eighteen has Seven Parts:
This file has Parts One to Four: Part One – Introduction; Part Two - Bees (Apis mellifera); Part Three - The Ponds in His Life; Part Four - Beavers (Castor canadensis).
The next file has Part Five - Back to Bats.
The last file of this chapter has two Parts: Part Six - Conscious Elephants ... And Other Ruminations; Part Seven - Consciousness & Ethical Considerations]

PART ONE

INTRODUCTION

Overview

The Introduction briefly reviews Don’s and Jocelyn’s moves after his retirement from Rockefeller and the species he next studies. A new interest is elephant communication, focusing on what it is like to be an elephant and the potential application to conservation.

After Griffin’s retirement from Rockefeller University (1986), and then his stay at Princeton, Griffin moved back "home," to Barnstable (1987). He joined the Harvard Museum of Comparative Zoology as a Research Associate, with space and lab/workshop at its Concord Field Station in Bedford, Massachusetts.

During Don and Jocelyn’s visits to the Cape, before moving there, they typically stayed at the Lamb and Lion Inn on Route 6A. Their lodging overlooked the Great Marsh with a grand view of it. (The inn still exists.) After a while, they rented an “idyllic” house on Scudder Lane in Barnstable. Recognizing the likely limitations of their increasing age, Jocelyn was persuasive in urging them to live in a senior residence in Lexington, Massachusetts. There, many of their needs could be attended to, including some medical assistance, should it be required. Being located fairly close to both Boston and Cambridge, many residents were academics from those cities. Edward O. Wilson, the famed Harvard evolutionary biologist, was an occupant and became a frequent dinner partner. Though he realized the residency's benefits, Don would often uncharitably refer to himself and the other dwellers as the “inmates,” isolated in rather luxurious surroundings from the rest of the world. Jocelyn passed away there in 1998, as did Don in 2003. But after Jocelyn’s death, Don purchased a home on Powder Mill Road in Barnstable, beside a large wooded area. Within it was ample room for visitors, including his grandchildren, and for his boat, constructions, and other “tinkering.”

While living on/near the Cape, Don was constantly involved with his research and data analysis. Many of the activities he pursued in retirement were extensions of topics or projects that he had begun at earlier points in his life. To better understand his endeavors, I have briefly summarized the prior work that was the springboard for his later undertakings. And in a few cases,
I have mentioned research conducted after his death which had been inspired by his earlier work or questions.

During his retirement times, he focused on bees, beavers, and bats and even had plans to work with Katy Payne on elephants’ communication and social behavior. They had daring hopes: to elucidate “what it is like” to be an elephant and to use that knowledge to inform improved conservation efforts. This chapter will deal with these various efforts.
PART TWO – BEES (*Apis mellifera*)

**Overview**

Working with local high school students, Griffin explored honeybees’ ability to find food that was not only moving but located where food should not be. With Jim Gould’s Princeton lab members, Don studied another potential source of communication, honeybees’ nearfield acoustics. His plans to retire to Princeton were stymied.

If only scientists could create a robotic bee for their investigations, totally under their control. Finally, there was a model bee, but …

1. **At the “Sugar Cube”**

   The “Sugar Cube”: that was the meeting place for … the bees, the students, the researchers. It was in the middle of a marsh, “The Great Marsh” aka the Barnstable Marsh in Cape Cod. At high tide, this big, white 10-foot square building, the “Sugar Cube,” was floated out, complete with a hive of honeybees. Using this construction, Griffin, with, of course, the help of many in his “Tom Sawyer” entourage, was planning to tweak the honeybees’ behavior in their quests to find food. In a marsh, there are no flowers, hence no distractions from other food sources, no tantalizing floral odors leading bees astray, or at least out of the experimental protocol.

   But how did the “Sugar Cube” get there? That was another tale, involving the many abilities of a favorite, local Barnstable High School teacher, Dr. Peter J. Auger, and a multitude of others. These others included Pete Auger’s brother, Greg, Dr. Eric Strauss, a long-time Barnstable resident, then on the faculty of the University of Massachusetts Boston (UMB), a score of townsfolk and Pete Auger’s high school students. Pete, an ecologist, was also a part-time faculty member at Boston College. Greg Auger was an IBM retiree and an avid nature photographer and videographer. It was via the “Sugar Cube” that Greg first met Don Griffin, and soon became an enthusiastic volunteer, later with some pay that Don managed to find for him. He joined Don with the bees and Griffin’s subsequent projects with beavers, and became an almost constant companion in the later bat studies.

   Eric Strauss had worked with Pete “forever,” beginning as one of Pete’s students in high school in 1976. Their relationship continued through much of their lives, through grad school together and even in California when Eric joined the Loyola Marymount University faculty and also became the Founding Executive Director of the Center for Urban Resilience (2010). Pete joined him to help with ecological programs. Even Eric’s long-term work studying piping plovers (*Charadrius melodus*) began while working with Pete.

   Don Griffin was a significant mentor to Eric who was having a successful career in wildlife and urban ecology. He was even succeeding at the challenging task of obtaining grants, first at UMB, then at Boston College. Don warned Eric against getting too involved with animal consciousness as a prominent professional interest. By his own self-assessment, Eric preferred the “big picture” and Don provided an important counterpoint by emphasizing scientific precision. Don also advised Eric against accepting a Harvard position. As Don knew well from his own experience, the environment was “too competitive.” Yes, Don was the idealistic scientist, but also the practical New Englander.

   Pete was a high school teacher who loved teaching, working with students … and fieldwork. Along the way, he was accepted into medical school, and drafted by the Yankees baseball team, but
enthusiastically returned to teaching high school and becoming a well-loved high school baseball coach. When Pete heard of Don's interest in a bee project in the marsh, he announced, "Let's build you a beehive platform in the marsh" ... so, the “Sugar Cube” was born. Even the town's maintenance staff joined the effort; donated lumber appeared; the high school woodshop built the floating “lab;” students volunteered to participate. Once the “Sugar Cube” was floating on the marsh, one needed a boat to reach it. Don often navigated, with Pete and his wife, Eric and one or more high school students along, depending on the task at hand. The “Sugar Cube” became a point of Barnstable town pride.

Guiding many of the experiments was the intriguing question: Could the bees extrapolate the location of the next food source from having found food at the previous ones? Prior work from von Frisch, Jim Gould's lab and other researchers had indicated such a possibility. In the experiments, bees that had found sugar water on successive trials, with each source being a set distance away from the previous, would then extrapolate the next position. Most peculiar. Where in nature does one find ambulatory food sources that suddenly appear at specified, regular distances? Don speculates that changing food source distances could occur near a steep mountain slope where the sun reaches broader areas as it rises. Thus flowers in areas further away will open, making their nectar available in different locations. Griffin was determined to investigate this matter as carefully as possible.

Seated inside the “Sugar Cube” was not only the hive but the cameraman, usually Greg, videotaping the honey bees dancing inside the hive. With that data, Don and Peter Auger could extract information provided by the bees indicating food location. Presumably, the bees would dance to specify where they had just been, or would they? Might they be signaling the next food site of the series?

Multiple small, movable feeding stations were established. For each experiment, only one held food. Next to the feeding station, a student marked the individual bees that arrived. To prevent that student from becoming a marker for the bees, indicating food, a student was also situated at each of the other non-food sites. Great pains were taken so that they looked as similar as possible, wearing identical T-shirts, for example.

Eric noted Don’s pleasure at working in a context where “publish or perish” was not the emphasis. Only then could one freely explore ideas and experiments, recognizing that no one’s Ph.D. thesis or academic tenure depended on the outcome of the efforts. But Don did repeatedly emphasize to Pete the need to publish work he conducted with his students in many areas of ecology/animal behavior. Since Pete Auger had a Ph.D., with more publications, he could likely obtain a full-time university position. But publishing was not Pete’s forte, although he often co-authored with Eric on topics ranging from diamondback terrapins (Malaclemys terrapin) to coyotes (Canis latrans).

Unfortunately, I cannot find information concerning the outcome of these bee experiments. In many ways, they were successful: The giant “Sugar Cube” aroused local interest in bees and the bee project. The students had a “hands-on” opportunity to study animal behavior through experimental field research, led by a “master” and his very, very able colleagues.
2. To Bees in Princeton, but ...  

Before the “Sugar Cube” days, Griffin had spent a few years of “post Rockefeller retirement” at Princeton University. His former student and then long-time friend and colleague, Jim Gould, was a Princeton Professor. Don had thought Princeton and his associations there would provide an intellectually stimulating environment for his retirement research, and the area an amenable site to live. He had been spending time at Princeton working on bee projects in Gould’s lab while still officially on the Rockefeller University faculty.

After his “RU retirement,” Griffin’s old friend, the Princeton biologist, John Bonner, had proposed to the faculty that Griffin be formally appointed as a “Lecturer” (without stipend) for the academic year 1987-1988. Bonner noted in a letter to the Dean of the Faculty that Don had already participated in teaching at Princeton. The faculty unanimously passed the motion at one of their monthly meetings. Don taught a couple of courses, including some graduate school instruction, helped advise students, and, as shall be described, studied honeybees’ near-field acoustic productions, a potential additional method of communication. He and Jocelyn purchased a home and had a delightful glass addition constructed to house her brightly colored, luxurious tropical plants.

Griffin also was given an office at Princeton. A problem. Space was tight in the department. Irritations arose. By the next year, in June 1988, while Griffin was away in England for several weeks, John Bonner again proposed the same appointment for 1988-1989. This time, things were different. The Dean objected because Don Griffin was being given benefits and privileges that they were not granting their own retired 70-year-old faculty. But it was late in the academic year, so an appointment was given for 1988-89. But the following Fall term 1989-1990 was provided as a “Final Appointment.”

A slippery slope. Allison Jolly, of Rockefeller University, had also been appointed as a lecturer for 1987-88 but was being paid to teach one course for the Women’s Studies department. The “casual nature” of these arrangements was troubling to the Dean.

Griffin moved on. The next phase of his “retirement” was as a Research Associate at Harvard’s Field Station in Concord, Massachusetts. His projects there will be the subjects of subsequent sections. But now, back to the Princeton bees.

3. Bees Buzz Quietly (Near-field Acoustics)

How Does the Honeybee Get Information from Another’s Dance?

During the mid-1980s, before Griffin’s “retirement,” he had been especially intrigued with the possibility of bees using another channel of communication to convey location information in their waggle dances. Although most scientists accepted that the dances contained such information and that honey bees were using it to find food, a few skeptics remained. Those claimed that odor and other cues were the regulating stimuli. But, as a noted entomologist asserted, “From a scientific point of view, the debate should have ended with [Jim] Gould’s review (1976) ...”

Yet questions lingered. How can the bees following the forager’s dance observe that dance in the dark hive? “... which of the many components of the dance do the follower bees perceive as signals (i.e., which components are transmitting the information?)” There were many troubling aspects to the problem: Though the bees are very close to each other, their bodies do not actually touch. The “follower bees” can orient themselves at various angles to a dancing bee.
Griffin studying the near-field acoustics of the honeybee waggle dance.
In Princeton Laboratory of James Gould, Summer, 1984. (by Raymond A. Mendez).
From RU, Winter 1985/1986, p. 6

Wiggling Air Particles
Griffin suspected that air motion produced during a bee’s waggle dance might provide a critical part of the message. In the summer of 1984, while still on the RU faculty, he was working at Princeton with an academic “grandson.” That was William Towne, a student of Jim Gould. They were engaged in studying “near-field” acoustics, using both Griffin’s RU equipment and that of Gould’s at Princeton.

The dancers create faint “sounds,” some at about 250 Hz, i.e., very, very low-frequency acoustic signals. But, in Griffin’s view (March 1987), “…since bees are virtually deaf to airborne sounds, the significance of these dance sounds has been unclear.”15 Maybe the bees were not sensing “sounds,” but air particle movements, as occurs during near-field acoustics.

Around the same time, William Towne had trained free-flying honeybees to respond differentially to airborne vibrations of 14 and 265 Hz, if each predicted different events. Specifically, one frequency predicted a mild electric shock while the bee was drinking sugar water at a feeding station, while the other did not. Those particular low frequencies were chosen because the bee produces them during her waggle dance.16 Scientists believe that bees are sensitive to air particle movements rather than pressure oscillations (i.e., ordinary “far-field” sound waves). The researchers argue that the wavelengths of such low-frequency auditory stimuli would be more than a meter long. Thus, the pressure would not vary appreciably over the short distances sensed by
either a stationary, feeding honeybee or a recipient attending a dancer. But, perhaps, the bees could detect the air particle movements. To summarize, however they are doing so, honeybees can detect the frequencies produced during dances.\textsuperscript{17}

**Hairy Bees and Antennae**

Questions remained: \textit{how} were the bees detecting air movements and what possible sense organs were involved? Griffin suggested, “hairlike sense organs that are strongly stimulated by air motion.” Further, he proposed that near-field displacement signals served both to attract other bees to the dancers and to convey dance information, particularly, “desirability.”\textsuperscript{18}

As far as I can determine, Griffin did not specify the likely sense organs. In fact, there seem to be at least two. Note that air pressure produced by the dancers drops off strongly with distance from the dancer, specifically, with the cube root of that distance. Bees might use their hair receptors to sense the small, more distant, weak, pulsed pressure changes, all of several centimeters away. The vibrating “fuzz” might serve to “entice” a bee to come closer and gather dance information. But what part of a bee senses the near-field air particle movements, the presumed source of the “dance information?” Perhaps the antennae and its Johnston’s organ were the “ears,” a possibility favored decades before by von Frisch.\textsuperscript{19} Griffin would have been aware of both possibilities since he knew von Frisch’s work thoroughly and was an ardent promoter of his research. Prior research in 1976 had, in fact, indicated the likely importance of antennae in attending the dance: Bees with partly amputated antennae were less likely to appear at the indicated food site; those with severely amputated antennae simply did not follow the dance as well as normal bees.\textsuperscript{20}

**“Rube Griffinberg” Apparatus and Then Denmark**

Don and Will Towne worked on several bee experiments together and individually, which will be discussed in the context of Will’s continuing work in Denmark. There, Will collaborated with several researchers from Denmark and Germany, all combining their diverse talents and, critically, their equipment, to gather data on “the acoustic near-field of a dancing honeybee.”\textsuperscript{21} Towne used Griffin’s sound recordings from Princeton; these were the wings’ near-field very soft buzzing, i.e., air movement very close to a dancing bee. The recording had involved yet another “Rube Griffinberg” contraption, this time a gooseneck lamp contrivance holding two small microphones, all attached to a wheeled, movable cart. Inside an observation hive, the bees danced, with microphones only 2 mm above them, and a video camera recording all positions and activities. The “sun” shone, being a lightbulb to which the bees oriented their dances. In these experiments, Griffin had trained marked bees to visit a feeder and then recorded their dances with the gooseneck/video/mike apparatus.

In Odense, Denmark, a similar experiment was performed, but the tiny microphones were handheld at bee level rather than in a movable, but fixed position, that the gooseneck provided. The experimenter could thus manually adjust the microphones’ position to the rapid and irregular movements of the dancers and pressure gradients could be measured. A high-fidelity tape recorder (Nagra IV-SJ) and a video camera provided audio recordings, with a synchronizing signal on the videotape and a third audio Nagra track. To determine the positions of follower bees relative to the dancers, the Würzburg, Germany colleagues looked to digital copies of original 35 mm, 60 frames/second films produced by von Frisch and Jander in 1957!
How to ascertain that wing vibrations, and not some other body part, created the soft buzzing? In the Princeton experiments, Griffin and Towne had clipped some bees’ wings. The dance sounds of those bees were compared to the sounds of normally-winged bees. The result? No sound was recorded from the clipped-wing bees, though the dances’ vigor seemed the same. But perhaps a “clipped-wing” bee is simply not motivated to produce sounds, rather than being mechanically unable to do so. Another experiment controlled for that possibility. This time, only two wings were cut off, not all four, and sounds were, nevertheless, produced, though with asymmetrical pressure. The wings were creating the buzzing. (All this detail is just to impress the reader with the difficulties and precision involved in the research, and the care taken to eliminate alternative explanations.)

The Danish/German experiments also revealed a very complex spatial distribution of the air pressure (and, thus, of the air particle movements). But that complexity itself might provide information for the bee. Where did the follower bees position themselves to receive such information? About 80% did so in a semi-circle near the dancing bees’ abdomen. Actual bodily contact with the dancing bee via the followers’ antennae could be done only about 25% of the time, e.g., only a quarter of the time during the informative “waggle run.” Otherwise, the followers were too far from the dancer, i.e., an average of 7.5 mm, while a bee’s antennae are only 3 mm. As determined from photographs, the bees appeared to be exceptionally precise about their head placement. The bees held their heads, and thus their antennae, near the dancer’s abdomen, the region of maximum air particle movement.

But the researchers warn that their suggestions were, at that point, merely hypotheses, and all needed further investigation. A final part of the research report contained a most complex mathematical analysis of air particle movements; I won’t even attempt to describe that.

A Dream: A Model Bee

If only, if only, the researchers had a model bee, a robot that they could control as wished, whose characteristics they could modify at will, whose dancing would be followed by real bees.

Not just these scientists, but Griffin (1985), too, had mused,

... my long-range dream is to make a model bee and exchange messages with real bees. I’d like to talk to the bees. So far we’ve only been eavesdropping.

It is somewhat amusing that a couple of years later, in 1988, another statement appeared that reached a wider audience than did the Rockefeller University Research Profiles publication quoted above. RU staff had written both pieces, but the earlier writing was drawn from a longer essay based on an extensive interview with Don Griffin, with many quotes by him. The later, 1988 rendition, when Don was an Emeritus Professor, is much more conservative, deleting “I’d like to talk to the bees”:

His long-range dream is to make a model bee that could exchange messages with real bees, ‘So far,’ he says, ‘we’ve only been eavesdropping.’

But Don did indeed want to talk to the bees, as well as have them talk to each other. Similarly, he was intrigued with the ape language work and the possibilities of “two-way” communication between human and ape.

Many other bee researchers held a similar dream. Even in 1927, the biologist and evolutionary theorist J. B. S. Haldane had written in The Future of Biology:
We may be able to tell our bees to fertilize those apple trees five minutes to the south-east. To do this we should presumably need a model bee to make the right movements, and perhaps the right noise and smell. It would probably not be a paying proposition, but there is no reason to regard it as an impossible one.24

A Lone Endeavor (Griffin and Bees at the Harvard Field Station)

But let’s leave those dreams for a while, and concentrate on Griffin’s further efforts with honeybee near-field acoustics. Don continued those interests during his “retirement” to Harvard. Although he had suggested that bees might communicate “desirability,”25 I have found no published data by him on that deriving from near-field acoustics. Others did obtain relevant information that will be discussed later.

Out at the Harvard Field Station in Concord, Massachusetts, Griffin had his lab trailer. Not the Princeton or Rockefeller University level equipment, but once again, inventive “Rube Griffinberg” creations. These were “jerry-rigged bits of common Radio Shack and other uncommon gadgetry (even ‘spyware’)”26 to videotape the bees and record air pressure changes close to the dancers.

He conducted his work with the most minimal funding: some support from Harvard University, supplemented with out-of-pocket monies. Griffin was, as usual, undaunted, but these were the years when on occasion we would hear him refer to himself and his various research endeavors as those of “an unfunded has-been.” Yet he continued to love his work, and continued to refer to the later enterprises as “recreational science.”

Don had set himself a most formidable task during his retirement with his lone approach to honeybee near-field acoustics. I’m sure this solitary endeavor was not his choice, for he had planned to retire to Princeton University, where his good friend and former student Jim Gould was a faculty member. Griffin was there only a couple of years, however.27 Even William Towne’s follow-up to their joint efforts had involved the collaboration of several people, with different skills, in fact in different countries, even different continents. The mathematics involved was more than formidable, and though Griffin was considered more facile than most other behavioral scientists in his knowledge of physics and associated math,28 the physics of near-field pressure dynamics was another matter. I know of no published work by Don on honeybee near-field acoustics, though he had contributed to the previously noted enterprise with Will Towne and collaborators. I can only assume that Don asked not to be listed as an author, thinking the others had been involved more deeply.

Science Advances

In the larger world of research, science progressed. Most suggestions offered by William Towne and his colleagues in their 1987 journal article were, indeed, accurate. Among other findings, the Johnston organ was determined to be the sensing mechanism for air particle movements. Specifically, thousands of sensory cells in that organ sense the movement of the antenna responding to near-field air movements.29 Furthermore, Kirchner and Dreller (1993) showed that the bees could extract information about both distance and direction using the air currents accompanying the dance.30 Specifically, the duration of each train of air pulses increased proportionally to the distance of the food source.31 These air pulse durations are likewise correlated with the physical length and duration of the waggle part of the dance. Since von Frisch’s
time, these dance characteristics have been hypothesized, then shown, to provide information about the distance to food. (The air pulse information can also be described as a negative correlation between the distance and the “carrier frequency” of the dance sounds and pulse repetition rate.\textsuperscript{32,33})

Direction to the food is available as a cue to the honeybee since the wagging part of the dance moves in a specific direction, which is oriented with respect to the sun. During the wagging, the soft sounds are produced.\textsuperscript{34} Note that in far-field acoustics, the sound has no directionality, so a single sensor cannot indicate the location of the sound source. Near-field sound can have a directional component for the air is moving back and forth in a given direction relative to the sound source. Several researchers have found that the “profitability” of the food source is reflected in the probability of sound production by a dancing bee.\textsuperscript{35,36} The distance to the food influences that probability as well, with longer distances resulting in lower probabilities of sound creation. Some studies reported very low probabilities; they ranged from $p = 0.1$ to $p = 0.6$, depending on sugar water concentration.\textsuperscript{37} It may well be that the intensity of the wagging (thus the likelihood of sounds?) and/or the persistence of the bee in performing her dance are communicating desirability. But how is information being transmitted non-acoustically, if it is; if not, why dance? More mysteries, perhaps solved by the time you are reading this.

4. Finally, a Robotic Bee

And finally, in 2018, after failed attempts by many, there came to be a robotic bee. (Recall that even Jim Gould had tried.\textsuperscript{38}) “RoboBee” was created by scientists at the Free University of Berlin.\textsuperscript{39,40} The invention of RoboBee and subsequent experiments required the diverse talents of nine faculty from that University, including members of the mathematics, robotics, biology and neurobiology departments.

Reviewing all stimuli associated with the bee dance, the RoboBee creators noted that vibrations of both the thorax and the wings produce air particle oscillations during the waggle run. The researchers cited many stimuli emanating from the bees: environmental cues from odors clinging to the bee, modulated electric fields and dance-specific semio-chemicals,\textsuperscript{41} among other stimuli.\textsuperscript{42} Yet, they comment, after decades of research, “it still remains unknown which of the associated cues play an essential role in attracting, motivating, and instructing the future recruit.” Which are optional, which by-products of the dance? The RoboBee can permit controlled experiments to determine the essential stimuli ... if the honeybees accept RoboBee as “one of their own” and follow its “dance.” In their 2018 paper, the authors present the “first report on successful dance-following behavior of bees with a robotic bee.”\textsuperscript{43} They not only have videotapes of the following but tracked the flight of recruits via “harmonic radar” and collected highly detailed descriptions of the bees’ behaviors. Although the numbers were small in the data set, some bees were able to extract the relevant information from RoboBee dances and arrive at the experimental food source. (Data were from only 4 bees with 8 tracked flights.) The researchers surmise that some additional cues were missing from “RoboBee,” perhaps “correct” body temperature or odor, which could have inspired more bees to participate.

Those and other factors are to be explored in subsequent research. The next version of RoboBee will (hopefully) be well-integrated into the hive and so become chemically indistinguishable from the hive. Don Griffin would have been fascinated and elated by the work.
PART THREE

THE PONDS IN HIS LIFE

Overview

We encounter several ponds, each with its own tale. Each has its own role in Griffin’s youth, his research and/or the surrounding community.

There were many ponds in Don’s Cape Cod life; the glaciers had done a fine job of creation. To relieve some of the possible confusion, I will review a few of those most significant to Don.

Hinckley Pond (aka Coggin’s Pond) (A Site with History and a Young Boy’s Explorations)

(Photograph in Chapter 4 – “Wandering in the Marsh”)

In Barnstable, Hinckley Pond played a role in the village’s early history: Next to the pond, Barnstable’s founder, Lathrop, had built his first house. His home had likewise served as the first Meeting House for his Puritan parish (aka “Independents”), until the official Meeting House was constructed, also beside Hinckley Pond. The first home/Meeting House later became the oldest part of the renowned Sturgis Library.

The pond likewise figured enjoyably in Don’s childhood; some of Griffin’s youthful times there are described in the chapter, “Wandering in the Marsh.” Recall that he searched for muskrat and other creatures at the pond, with some success and delight. He fished, at his father’s urging and company, with little or no success or delight. He also “spent many happy hours ‘messing about’ in small boats on Hinckley Pond,” including sailing there. After his Aunt Heloise bought the “official Meeting House” and adjoining property with Hinckley Pond, Don was able to store a few boats by the pond as well, a helpful convenience. The fields and shoreline became sites of imaginary wars, even including naval battles with sailors cast from pieces of lead and shrapnel bits. However, that pond did not become a site for either his bat or beaver studies.

Mink Pond (A Tale of Conflict and Beavers)

(Photograph in Chapter 18 - Part 4 – “Beavers”)

Mink Pond, or “Stump Pond” as the local children called it, was in Concord, part of Estabrook Woods, Harvard land. Among the various pond dwellers was a colony of beavers who had constructed both a dam and their lodge. They were Don’s study subjects in the 1990s, during his “retirement.”

The area has a long history. In the 1800s, Thoreau, Emerson and other friends of the private family owners had strolled in the Estabrook Woods, for “nature observation.” The area also had a long history of preservation and endless, heated, long-lasting conflict over that. The controversy was first highlighted in 1932 when the owner families closed the Estabrook Trail to the public. To shorten the rest of the story, the preservationists temporarily achieved a victory in the 1960s by the owners making large land transfers. Harvard was a major beneficiary, acquiring 682 acres or 40% of the preserve. Later, the nearby towns also managed to purchase land.

It was valuable land, very, very valuable land, a thickly wooded area not all that far from Cambridge (17 miles) or even Boston (22 miles).
To be sure, it was rumored that some on Harvard’s oversight boards and high in the administration, were not quite very fond of beavers or poking about in the woods. They saw the land sparkling as extra funds for Harvard’s endowment. Developers saw potential very lucrative housing developments; the local Middlesex school, which owned 110 acres, saw needed extra funds for their programs; the towns saw parks and playgrounds; dog owners saw their pets happily romping through the woods, occasionally chasing whatever little creatures might scurry along or fly by. Not so the environmentalists and conservationists.

Even when, in the 1990s, Harvard joined a coalition with the powerful “Trustees of Reservations,” and others, the conservation issue was not completely solved. Care was taken to indicate the research and educational use of Harvard’s preserved land. Griffin’s host, the Concord Field Station of Harvard’s Museum of Zoology together with Harvard’s Department of Organismic and Evolutionary Biology conducted research and field courses. The internationally famed Harvard scientist Ernst Mayr entered the fray. He wrote to the Middlesex School, saying that if they preserved their portion of the woods and if Estabrook Woods itself could be preserved, he would work with them to establish a student curriculum using the woods as an ecological classroom. I would guess, knowing the passion Mayr held for the woods and the potential research there, that Mayr did help create such a course, even though the school did sell off some holdings.

And Griffin was doing his part. Although Cape Cod has a vast number of freshwater kettle ponds, including many with beavers, Don did choose to work on the Harvard property. (He began about 1991, with special concentration during 1996 -1998). His beaver studies are discussed later in this Chapter -Part 4, “Beavers …” Convenience was likely to be among the factors in selecting this site, for Mink Pond was only 3 miles from the Field Center in Bedford and 9 miles from his Lexington home at the time. His research helped make the case for preserving Estabrook Woods. And a Middlesex student did find his way to Mink Pond and worked with Don Griffin and the beavers, later conducting post-graduate beaver research in Colorado.51 Other students, including those from Boston University and Harvard, also joined Don.

But by 2017, the town of Concord brought a suit against Harvard and four property owners, wanting at least unlimited public access, dogs off leash and more parking. The opposition fought back. Thoreau and the other 19th-century meanderers were somehow cited as “evidence” ... for both sides.

By 2019, the town dropped its case against Harvard and a private family owner. Once an agreed “no dogs off leash” rule was actually enforced, public usage and parking needs plummeted. Estabrook Woods sighed; the beavers continued their constructions undisturbed, not even by Don who had passed away years before (2003).

**Trout Pond (Sewage Attracts Mosquitoes Attracts Bats Attracts Scientists)**
(Photos in Chapter 18 - Part 5 - Bats)

**Trout Pond** in Mashpee and the nearby Mashpee Church figured substantially in Don’s life, though not in a religious manner. Set in a bit of hardwood forest, and becoming a beloved site, the pond, like Thoreau’s famed Walden Pond, was quite ordinary. Trout Pond was small, only about 300 meters long and about 50 meters wide, near a road with a sewage treatment facility at the far end. Many years before, an earthen dam had formed the pond, the dam then becoming a dirt road. Underneath the road was a culvert draining the pond into a small overgrown brook.
Photos from 1934-37, when Don was a Harvard undergraduate (BA 1938) reveal him climbing a ladder set against the church, sometimes an empty jar in hand, or large cans set under the eaves of the church, or a car laden with nets and lumber. Another shows a girl in the distance beside a tunnel net, standing in front of the church.\(^5^2\) (Photo in Chapter 6 – “Young Men and Bats”) All this, including the young lady’s help, was for catching bats, (the Little Brown Bat, *Myotis lucifugus*) who chose to live under the church eaves. They were, presumably, among the 16,000 plus bats that Don and his ever-changing “Tom Sawyer” band of helpers managed to band over his young years.\(^5^3\)

During his retirement at the Harvard field station, Don, with Greg Auger, spent many, many nights together with the bats at Trout Pond. Aside from occasional expeditions to more distant locations, it was the principal site for Griffin’s bat studies at the time. The site was most alluring to bats. Though Griffin states that the pond and brook didn’t smell of sewage, the effluents from the sewage installation “doubtless contributed to the nutrients for aquatic insect larvae.”\(^5^4\) Hence, the adult flying insects, hence, the bats, hence, the researchers.

They strongly suspected that the bats feeding at the pond were from the church roof colony, only about two hundred yards away. To establish that as fact, however, required countless hours of watching, banding and chasing.\(^5^5\) And recall another “interconnected” matter: Trout Pond was one of the sites saved in the environmental battles led by Jean Thomas.\(^5^6,5^7\) Much more about those bats and Don and Greg and visiting volunteers and researchers in this chapter, Part 5, “Back to Bats.”
PART FOUR

BEAVERS (*Castor canadensis*)

Overview

Beavers had fascinated Griffin for decades: their abilities to construct and repair beaver dams and lodges and their elusive communication. In the freedom of his “retirement” to Harvard’s Concord Field Station, Griffin and others study the beavers’ astute behaviors. Clambering over the beaver lodges, he observes the beavers via video probes into their dark chambers. Don readily counters arguments asserting the stimulus-bound, behavioristic explanations of beavers’ activities.

Beavers in the Night

Softly paddling in the chilly night air, Don Griffin sat in a canoe piled high with electronic gear. An instrument screen emitted an eerie green glow as he floated in Mink Pond, a small New England pond in Estabrook Woods. Or, yes, in his 80s, he might be clambering (quietly) over a beavers’ lodge or poking a hole in their dam. His way was lit by a battery-operated headlamp, very different from his young days of dripping carbide lamp illumination. Greg Auger, Griffin’s almost constant companion in the beaver explorations, once came upon Don atop a beaver lodge with a black felt bag over his head. It was a weird apparition, moving, twisting, and sometimes talking to...
himself. Don was videotaping into the beavers’ dwelling, using the bag to reduce any ambient light, while dictating his observations. Occasionally, a walker, merely strolling in the woods, would encounter the same sight. The locals were not at all enamored of beavers; some inhabitants were finding their land flooded by beavers’ activities. This creepy "bagged" man just added to their disdain.

Griffin’s beaver investigations at Mink Pond began about 1989 or so, and most intensely during 1996-1998, all this during Griffin’s retirement to Harvard’s Field Station. Since beavers are primarily nocturnal creatures, Don was usually out at night. Much to his wife Jocelyn’s concern, he was conducting his explorations and studies into the very late-night hours. She insisted that he go with another person and so he did, but, sometimes, he was out on his own. Hers was a sensible precaution, for previously, out during the wintertime by himself, he’d slipped on the icy ground and injured his leg. With difficulty, he had managed to reach his car and drive home.

Beavers were enjoying a resurgence after practically being decimated by the late 1800s through hunting. Long beaver coats and warm beaver hats had been desirable fashion statements for a considerable while. Finally, the beavers were protected. Reintroduction efforts were a significant part of the solution as was explicit legislation protecting the beaver.

Beavers are a “keystone species.” Intrinsic to that designation is their dam building, so fascinating to Griffin and so irritating to nearby homeowners who might find their gardens or basements flooded. To be a “keystone species” means the animals are central to an entire ecosystem; their presence helps create an environment in which many species flourish. Without the keystone species, drastic environmental changes occur, and numerous species can be lost from that environment. Beavers build canals to move water from larger water sources to their feeding and lodging areas; other species use the created waterway as well. Beavers maintain ponds and wetlands that increase biodiversity, purify water, and prevent large-scale flooding, though local annoyances can occur.

“Beloved Beavers”

Presumably, Griffin had seen the beavers or their lodges or dams as a young boy, when roaming the marshes and ponds near his childhood home in Barnstable, Cape Cod. In 1940, while working on his Ph.D. studies of avian homing and still engaged with bats as well, a biologist friend wrote to him. The friend, Fred Packard, seemed dubious about Don’s bat echolocation claims but extolled the virtues of beavers.

I recommend that you read up on beavers. They are grand little conservationists, and although they do some flooding of areas, they impound waters to the great benefit of our civilization. Some jackass ranchers blew up five miles of beaver ponds near here [Rocky Mountain National Park, Colorado] several years ago – and that whole valley has been practically deserted by man and stock due to the lack of water the beavers made available at just the right time.

Don needed little persuasion to be directed towards his “beloved beavers,” as Jim Gould has described Don’s relationship with them. For a while, during his time at Cornell, Don and another Biology Dept faculty member, Charlie Walcott, worked together on beavers. Charlie became a life-long friend, though their more lasting mutual research interest was avian orientation and
navigation. Walcott did note how Don would repeatedly try to entice students and colleagues into studying beavers.65

Jack Bradbury also recalls the scene at Rockefeller University:

*DRG was always trying to get someone to work on beavers. He suggested it to me for a thesis, and again later as a postdoc. And I heard from other students that he was trying to get them to work on beavers.*  

And Don was always ready to combine pleasure with business, or vice-versa. He delighted in entertaining the Gould’s young children by taking them to New Jersey’s Pine Barrens to search for beavers, The children grew up knowing the Griffins and kept Don’s gift of a toy stuffed beaver for many years.67 My own pre-teenage son had the fun of a canoe jaunt with Don on Mink Pond, hoping to sight the beavers that lived there.

Sometime over the years, Don had attempted, without success, to constrain the beavers for study in a swimming pool. They left.68

**“Retiring” with Beavers**

So, once again, the 80+ year-old Don was studying beavers. This time, his site was in the Harvard-owned Estabrook Woods, an area under “siege” by locals and even by certain Harvard financial interests. The latter worried about the Endowment and expenses; Estabrook was prime, untouched acres of real estate, worth a small fortune. So there were strong reasons for Don to work at Mink Pond, in those woods, rather than at any other New England beaver pond. His studies helped provide the necessary evidence that the land was, indeed, being used for research, the “raison d’etre” for Harvard’s continuing to own the site.69

In his 1984 book, *Animal Thinking (AT)*, Don had made the case for the conscious beaver, stressing the beavers’ extraordinary “engineering” abilities70 He produced an even more thorough and convincing description of their likely “conscious” behaviors in his final book (2001), *Animal Minds, Beyond Cognition to Consciousness (AM-C)*.71 Among the beavers’ activities were those described in studies of their general behavior,72 and “thoughtfully analyzed” in research by Wilsson.73,74

In AT, Griffin had ruminated about the beavers’ dam building. A primary function of a dam is to create a pond deep enough for beavers to swim through in winter, under the ice; this behavior likely evolved from times of their serious predation by wolves and others. Collecting branches, mud, stones, and other small objects is unlike that of any other non-human animal’s “environment-altering” activities. As beavers create a dam for future benefit from an adequately deep pond, Don wondered about their thoughts. “Are they possibly thinking about that future use, that the dam will create a deep pool?”75

Beavers are particularly sensitive to the sounds of running water. Wilsson76 and Richard77,78 have tested the hypothesis that the sound of running water is a stimulus for placing materials for dam construction. From the experiments, Wilsson claimed evidence of rigid, stereotyped, stimulus-response actions. The relevant behavior was the beavers’ piling of mud and material near a speaker playing broadband sounds, including that of turbulent water.79 But, as previously noted,80 that is a good “fail-safe” mechanism, not uncommon in animals and even humans. The sounds are *one* stimulus for dam building/repair. Yet those are not the only conditions under which a beaver repairs a dam. Neither does a beaver carry repair objects to locations where there is a noisy stream.
But the sound of turbulent water does attract a beaver’s attention. Yet, as Richard noticed, that sound and/or a lowered water level resulted in the beavers inspecting the dam. It then places materials where needed to plug the leak, not necessarily at the sound of rushing water, if there is such.

As Richard also observed, sometimes, humans place pipes through a beaver dam, to avoid flooding roads or basements. Initially, the beaver may place obstructions near the noisy outflow of the pipe. However, in many cases, the beaver discovered where water was entering the pipe, and, much more effectively, plugged that opening. And, Griffin remarks, beavers repair their dams with appropriately sized items, not just any stone or branch.81

When beavers live in lakes or streams deep enough to permit traveling under the ice in wintertime, they don’t build dams. Instead, they create a dry, above-ground lodge with an underwater entrance. Don queries, if an experimental ethologist added material to a dam the beavers were already building, would they stop when an appropriate pond had been produced?82 Good questions.

Meeting an Emergency

Hope Ryden, an American documentary producer and a wildlife author and photographer, had an avid interest in beavers. Based on her observations, she published a book in 1989, Lily Pond: Four Years with a Family of Beavers.83 Griffin relates an emergency dam building that Hope and her companion had witnessed at Lily Pond.84 Each evening, the male beaver, who had conducted most of the dam maintenance, left the family lodge and came to the dam. He inspected it, often adding bits of mud. One day, human vandals tore a large hole in the dam and a torrent of water began rushing out, threatening to empty the pond in mere hours. Ryden and her companion began piling large stones upstream, hoping to reduce the water flow and allow the beavers to make repairs. At the usual hour, the male appeared and “immediately responded to this emergency with drastically altered behavior.”85 He cut a few small branches and towed them to the dam, managing to pin some under the new pile of stones, though others cascaded downstream. There were few dead branches to use, for humans picnicking nearby had used them for firewood. Instead, he was using green vegetation from lilies, which the beaver family usually ate. That evening, three other beavers, seldom observed at the dam, attempted to help, but their piling branches atop the dam was ineffective in the rushing waters. Then a change in tactics. The beavers dove to the bottom of the pond and brought up mud and more vegetation, now used to plug underwater gaps between the rocks. Usually stick piling is done and, afterwards, holes are plugged with mud and debris. Note that the sound of rushing water came from the top, not underwater, where the repairs were being made.

Ryden watched through most of the night, as the beavers kept working. She returned the next afternoon, at a time before the male beaver’s usual “inspection tour.” But again, most unusual behavior from that beaver. Upon leaving the lodge, before going to the dam, he removed a large stick from the lodge itself and dragged it to the dam. When the other beavers emerged from the lodge, they, too, removed sticks and brought them to the dam. The water level was somewhat stabilized but at a much lower level.

With the continuing (slower) leakage, the previously underwater entrances to the lodge, which sequestered the kits, gradually became exposed. Within a few days, the beavers had reconstructed the entrances so that, once again, they were more safely underwater. And very
slowly, over days, with more dam building, the beavers managed to raise the water level of the pond.

To Griffin, to myself also, a most compelling case for conscious, thinking beavers: Recalling the previous day’s inefficient modes of dam restoration, they changed their behavior to remove parts of their own lodging to use for repairs. And beavers don’t simply pile mud and objects by white noise sources. They evaluate the situation and its changes. Recall their recognizing the greater efficacy of plugging the inlet of a pipe through their dam when obstructions at the noisy outlet didn’t suffice. Griffin (1991) compares their stick-piling behavior to that of humans’ walking or grasping with a hand, all with a strong genetic and evolutionary basis. “But at least in our species, conscious thinking and choice sometimes guide when, where, and to what purpose such actions are used.”

**Griffin and His Band**

With his inimitable gentle, persuasive abilities, Don conducted his beaver explorations with a band of helpers. They were mostly volunteers, some students wanting field experience, or undergraduate or potential graduate degree work. I’ve discovered several involved in the work.

One such was **Nancy Williams**, an undergraduate or graduate student at Boston University (BU), about 19 miles away. She had commented on an article in a BU Alumni magazine on beavers’ “sex lives,” the research of a BU Professor, Peter Busher. Beavers are among only 3% of mammals that are “socially monogamous,” meaning a pair stays together to raise a brood. In her comment, Nancy recalled,

> Years ago, I studied beavers at Estabrook Woods with Dr. Donald Griffin. Based on my one year of spending hours observing them until the early morning hours, I would say that beavers would quickly find a new mate if one should disappear. They act as a family, taking care of the young and the dam and lodge with the one-year-olds staying around to assist with the newborn. They are marvelous creatures!

At some point in Don’s “retirement” beaver investigations, **Ann Graybiel** received a letter. Many years before, as a Harvard undergraduate, she had worked in Griffin’s lab and gone with him one summer to Trinidad to help him and his then grad student Rod Suthers with studies of the fishing bat (*Noctilio leporinus*).<sup>88</sup> When the letter came, she was (and still is) a very highly regarded neurobiologist at MIT. She recalls, in an email to me, having occasional interactions with the Griffins over the many years.

> I was entranced by him and by Jocelyn—what extraordinary people! The two of them together ... what a dream. ... They were unique and I cared about them very much.

> My husband and I took them out to dinner on what I think was her [Jocelyn’s] 80th birthday [1989]. (We arranged for Greek music; you may know her romantic love for this). I rarely saw either one for years though had a “the world-is-upside-down experience,” when I was called by a homeowner in Lincoln, MA, where we live, asking for me to give a recommendation of Dr. Donald Griffin as a prospective renter of a barn in which to store his gear for his project on beavers! Can you imagine!! I revered him.<sup>89</sup>
Another of the “Band” was **Gayle Speck**, retired, and a Radcliffe College graduate, and, afterwards, a Harvard Business School student. As a graduate, she could audit Harvard courses; “The History of Science” had been one choice. There, she’d read about Don Griffin and those objecting to his ideas on animal cognition and awareness. Fascinated, and realizing he was nearby at the Concord Field Station, she called, said she’d love to meet him, did, and began volunteering for him (around 1993 or 1994). As she recalled, “He set her to work, the usual situation for anyone who contacts him.” Her main contribution was library work, reviewing journal articles relevant to animal consciousness. In particular, she read and analyzed experimental psychology research.\(^90\) She also became his teaching assistant for a Harvard graduate course on animal awareness.

On a couple of occasions, she did join Don in fieldwork with the beavers at Estabrook Woods. As she recalls, at this point, Griffin was especially interested in beavers’ dam building, specifically intending to observe their behavior if he poked a hole in their dam. She prepared well for her first beaver expedition … she thought. She was dressed very warmly for the chilly night she was expecting, so warmly, that Griffin laughed and asked if she were going on an Arctic expedition. At any rate, she decided that instead of fieldwork, she’d stick to the library work and analysis.

And then there was a Hippo man, **William (Bill) Barklow**. For about 13 years, Barklow had studied hippopotamus (*Hippopotamus amphibius*) communication, initially in African rivers and then in zoo pools. As with beavers, the hippopotamuses are a “keystone species.” The channels they forge through vegetation-dense rivers and wetlands are used by many animals; their feces are significant fertilizers. Like other species, the hippopotamus encounters diffraction problems when vocalizing, due to different densities of the air and water. Whales’ loud sounds encounter different water densities as they travel in the ocean. The initial hypothesis that fishing bats could somehow echolocate fish through the water likewise entailed diffraction difficulties.

The work of Bill Barklow (and others) showed that hippopotamuses can communicate vocally both above and under the water, can do both simultaneously, and can hear in both air and water simultaneously. In fact, although hippos seem a noisy bunch to human observers above the water, most hippo communication is underwater. And sounds made in either medium are almost completely reflected at the water’s surface.\(^91\) Beavers, though semi-aquatic, vocalize only in the air, both above the water and within their dry lodge chambers. Their loud tail slaps against the water are a form of communication, functioning as danger alerts or apparently, sometimes in play; they are heard both above and below the water.

Back to hippos: Bill Barklow managed to get “close” to his subject, known for killing more humans than any other large African animal, i.e. far more than lions, leopards, etc. put together. In fact, mosquitoes kill the most, about one million Africans each year, via disease vectors.\(^92\) At the Toledo Zoo, Bill knew the hippos so well that one would open its mouth widely so that Bill could massage its gums. The same animal liked its molars to be sprayed with a hose, not so different from behavior in the wild, where they sometimes open their mouths wide under a waterfall.\(^93\)

One can easily imagine that Don and Bill shared numerous “techy” concerns as well as mutual fascination with animal behavior. But Barklow was focused on the mechanisms to produce and receive the sounds. Griffin’s interest, especially at this point of his scientific journey, was in the **meaning** of animal communications.

They also lived in overlapping personal and scientific worlds. Bill Barklow recalls,
... when I was in graduate school at Tufts, I worked in the Payne’s lab at their home in Lincoln MA ... (1974) ... They, along with Cornell, had developed one of the first real-time spectrograms; a Rube Goldberg contraption that printed the transform on 35mm paper film strips. This was when Katy was pasting together those strips to reveal an entire humpback song.94

Of Griffin and the beavers, Bill adds,

I spent many hours in the field with Don late in his career. His wife insisted that he always have someone with him. His curiosity was boundless. He was interested in what goes on inside a beaver lodge. and managed to get a microphone (no video at the time) in the lodge, and recorded some sounds. But without video, he was never sure it was a beaver and not a muskrat.95

Hope Ryden, the previously mentioned author of Lily Pond, also came to visit and to observe the Mink Pond beavers, at least once, perhaps more often. During her visit on July 18, 1997, they encountered quite an exciting time within the beaver lodge. A week before, Don had his first video of the first baby beaver,96 though the baby’s age is not known. Brief notes of two hours of video indicate much grooming between an adult and baby, both even falling over in the process. When Hope Ryden visited, the baby seemed to be engaging in cecotrophy, i.e., the baby reached its paw into its cloaca and ate something from it. Later the baby tugged at a root in the lodge. Those were all the “main events.” Don gave Hope copies of about 3 hours of video from their observations inside the beaver lodge on that day and his of the previous day. However, no publication I can find by either of them refers to observations of the Estabrook beavers.

And the longest “band member” was Gregory Auger who helped Don with the beavers for several years. Greg’s companionship and skills acquired from his video and photography hobby/business were particularly valuable. He was as well a most useful resource for Don for audio recording and electronics assistance.

What’s Happening Inside a Beaver Lodge?

As described above, Griffin’s interests expanded beyond a concern with the stimuli provoking dam repair to social behavior and communication within the dark beaver lodge. Initially, he was able to gather only acoustic recordings. Using Greg’s help and constant adjustments and “fiddling,” he was able to make both simultaneous video and good-quality acoustic recordings ... with difficulty.

They placed an infrared light (IR) within the lodge which the beavers could not see; beavers’ vision is quite poor even in ordinary light. But again, difficulties; at least once, the baby seemed to get too close to the hot IR source. The small monitor Don was using in the field did not afford a very clear view of the camera’s achievements, and, all too often, the camera was not aimed at the “action.” But that frequently could not be determined til back in the lab with a bigger, better monitor. He changed IR filters several times, attempting to improve the video. And then... wind noise interferes with good quality acoustic recording. Don also invented a “whirley probe” to feel whether the camera end was in a cavity and not merely inside a small space between sticks.
His few summary notes available to me, again reveal a very persistent researcher, but beavers not ready to divulge too many secrets of any mental machinations. They slept a great deal and they groomed. To Griffin’s surprise, there was much co-habitation. Griffin’s videotapes repeatedly captured muskrats (*Ondatra zibethicus*), even their attempts at copulating! These semiaquatic rodents will typically dig a burrow into a bank in marshes, swamps and wetlands; sometimes they even construct a lodge. Like the beaver, they make an underwater entrance, for safety from predators, while their living area is higher and dry. But why bother building your own house, if you can just camp out in one already constructed, indeed, by someone else, a beaver? Often, voles (the “meadow vole,” *Microtus pennsylvanicus*) appeared too, as did mice (*Peromyscus*) and flying insects. Don made “attempts at playback,” I presume, including beaver vocalizations, but he did not notice any distinctive reactions.97

In late 2000 (October 31), Griffin began notes for what he terms “a sort of commonplace book, ruminations, reading notes and thoughts about what I’m reading.” “... first some continuations, but I really want to branch out beyond this “upwithfinishings.” Those “continuations” include work with beavers: “I want to try tests of acoustic, and maybe other, stimuli for dam repair. But I keep being stymied and don’t really get any significant data. Still hoping, however.”98

Then, on July 10, 2003, he was composing a letter to Dietland Muller-Schwarze, a scientist studying beavers. Beyond writing popular books and articles about beavers, Muller-Schwarze was considering the production of a beaver documentary and, therefore, interested in Griffin’s videos. Since Don’s videotapes were research films, Don thought that they were not of documentary-level quality and that there was little of interest in many of the 58 hours of Hi8 cassette tape.

*I often left the camcorder running continuously waiting for something to happen. Thus the interesting fraction (maybe 10%) is widely scattered. There is also one bad gap of some of the best which I loaned to an interested lady who was to copy it, but it somehow got.*
mislaid. For that, I have only a VHS copy of selections showing parents “dunking” babies and moving them to some lower chamber in the lodge.

There is lots of grooming, yawning, and a little feeding, but I’m not sure how much shows sharing. My main interest was in beaver sounds and I had a reasonably good mike in a separate probe. I tried playbacks of various kinds of sounds I thought beaver might respond to, but they showed so few responses that this never seemed significant. ...

Some of the most interesting sequences of other animals in the beaver lodge, including two muskrats at least starting to copulate, were very early, before I had improved the probes. Things might often intrude between the camera and behavior. Scanning was often lopsided, rather than horizontal because the video probe was only approximately vertical. But again, specialized “Griffinberg inventions” to deal with the difficulties:

I had systems to raise and lower the aim, and these were gradually improved, so that the later video could cover a wider range.

To find and select segments for a film, even one simply for research and teaching, was a task he considered daunting. He did have notes on the playbacks, but “these notes are mixed in with many, many pages of other notes.”

...as you can imagine, trying to make a selection is tedious and I have become overinvolved now in other enterprises.99

As typical, in his private, reserved manner, he didn’t describe the “other enterprises.” Unfortunately, aside from his fierce determination to finish a manuscript with Jim Simmons about bats and another extensive review paper about consciousness with Gayle Speck, his central “enterprise” was attempting to survive against progressing thyroid cancer.

END NOTES
ENDNOTES FOR PART ONE BEGIN HERE
1 Eric G. Strauss, personal communications, September 16, October 29, December 21, 2021. Zoom interviews. These were supplemented by my own [CR] memories.

ENDNOTES FOR PART TWO BEGIN HERE
2 Gregory Auger, personal communication, November 4 and 7, 2016. In-person interviews in Mashpee, Massachusetts. Most of the information in the section about the "Sugar Cube" derives from these interviews with Greg Auger and others with Eric Strauss.
3 Eric G. Strauss, personal communication, September 16, October 29, December 21, 2021. Zoom interviews provided much information in this section.
4 DRG, 1984, AT, p. 140-141; 2001, 1992, AM, p. 140. In both references, Griffin describes the general nature of the experiments and the possible adaptive value.
7 DRG, 1992, AM, p. 140-141.
8 John Bonner to Dean Aaron Lemonick, 1987, June 8. From Griffin, Donald R.; Faculty and Professional Staff files #90, Subgroup 2: B, AC107.02, Princeton University Archives, Department of Rare Books and Special Collections, Princeton University Library.
9 Aaron Lemonic, Dean of Faculty to Prof. Arnie J. Levine, 1988, June 13. From Griffin, Donald R.; Faculty and Professional Staff files #90, Subgroup 2: B, AC107.02, Princeton University Archives, Department of Rare Books and Special Collections, Princeton University Library.
Chapter 18 – The Return – EndNotes

10 Aaron Lemonic, Dean of Faculty to Prof. Arnie J. Levine, 1988, June 13. op cit.
13 Gould’s critical experiments are discussed in Volume Two - Chapter 12, Part 5, section, “James (Jim) L. Gould (And Bees That Lie).”
17 William F. Towne and Wolfgang H. Kirchner, 1989.
22 Fulvio Bardossi and Judith N. Schwartz, 1985, p. 6.
23 Rockefeller University, 1988, p. 4.
26 Carolyn A. Ristau, 2005, p. 408.
27 Griffin’s post-RU retirement time at Princeton is discussed in Volume Three - Chapter 18, Part 2, “Bees,” section “To Bees in Princeton, but ...”
28 Ronald Larkin, personal communication, June 14, 2019. Telephone interview.
30 Wolfgang H. Kirchner and Claudia Dreller, 1993.
31 Aknksha Singla, 2020, p. 46-47. This paper is a review of the “codes” used in the honeybee dance, including the near-field acoustic information.
33 Wolfgang H. Kirchner and Claudia Dreller, 1993, p. 69.
34 Wolfgang H. Kirchner and Claudia Dreller, 1993, p. 69.
36 Wolfgang H. Kirchner and Claudia Dreller, 1993. Other studies conducted by Kirchner and his colleagues found similar results in other honeybee species.
37 Wolfgang H. Kirchner and Claudia Dreller, 1993.
38 James L. Gould’s attempt to invent a model bee is mentioned in Volume Two - Chapter 12, Part 5, section, “James (Jim) L. Gould (And Bees That Lie),” subsection, “More About Bees at Millbrook.”
39 An earlier version had been created by Michelsen and colleagues, but no videotape evidence existed of honeybees actually following the dance of the mechanical bee
40 Alex Michelsen et al, 1992. The earlier mechanical dancer had been invented and used by Michelsen and colleagues in 1992. No videotapes existed of the real bees’ reactions to the model, nor was it known whether the recipient bees actually followed the dancing itself. However, some bees had arrived at the designated food station. “RoboBee” was a more advanced creation.
41 A “semio-chemical” is a pheromone or other chemical that conveys a signal from one organism to another, modifying the behavior of the recipient organism.
43 Tim Landgraf et al, 2018, p. 3.

ENDNOTES FOR PART THREE BEGIN HERE
45 See further historical discussion about Barnstable in Volume One – Appendix: Chapter 3, “Home”: A Reserved Village in Cape Cod.” [This is the complete chapter]
46 Later history about the pond and the young teenage Griffin’s use of it may be found in Volume One - Chapter 4, “Wandering in the Marsh,” particularly in the section, “The Lure of Hinckley Pond.”
47 DRG, 1987, BOOK 6A.
48 Further description in Volume One - Chapter 4, “Wandering in the Marsh,” section on “Imaginary Countries and Businesses.”


51 Unknown author, n.d. (after 1998 and before 2005). A popular article about Estabrook Woods and Donald Griffin’s work at Mink Pond. Also includes Mayr’s offer to establish a curriculum with Middlesex School.

52 Photographs are from the collection of Margaret Griffin, one of Griffin’s daughters.


56 Wendy Williams, personal communication, March 9, 2005. Interview in Mashpee, Massachusetts plus various other conversations during our social visits with each other and her husband Gregory Auger.

ENDNOTES FOR PART FOUR BEGIN HERE

58 Gregory Auger, November 7, 2016, personal communication. In-person interview in Mashpee, Massachusetts.


61 Dietland Muller-Schwarze, 2011.


63 Fred Packard to DRG, 10 April 1940, FA# 164, Box 9, Folder 90, 450 G-875, Donald Redfield Griffin Papers, Rockefeller University Archives, RAC.


66 Jack W. Bradbury, personal communication, September 19, 2020, Email.


69 The controversies over Mink Pond have been discussed in Volume 3 - Chapter 18 – Part 3, “The Ponds in His Life,” section, “Mink Pond (A Tale of Conflict and Beavers).”

70 DRG, 1984, AT, p. 128-132.

71 DRG, 2001, AM-C, p. 99-112. Beavers’ behaviors are described in this section, particularly emphasizing the likely conscious aspects of their activities.


75 DRG, 1984, AT, p. 128-130.


80 Beavers’ innate, “failsafe” response to turbulent water was previously noted in “Volume Three - Chapter 16- Part 2, “Revolution in Ethology and The Birth of Cognitive Ethology,” in subsection, “(Mis)understanding Innate Behaviors.”


82 DRG, 1984, AT, p. 131.


87 Nancy Williams, 2017.
88 For a more complete description of Ann Graybiel’s time with the Griffin team in Trinidad, see Volume 2 -
Chapter 12, Part 2, “Trinidad - A Research Station in Paradise ... Muddy Roads, Many Bats.”
89 Ann Graybiel, personal communication, February 22, 2021, E-mail.
90 Analysis by Gayle Speck and Donald Griffin of selected experimental psychology research relevant to
conscious behavior may be found in Volume 3 - Chapter 16, “Revolutions,” Part 3, “Griffin’s Reflections on
Cognitive Ethology and Experimental Psychology.
92 Jessica Learish, 2016.
93 William E. Barklow, personal communication, December 4, 2015. In-person interview in Natick, MA.
94 William E. Barklow, personal communication, October 22, 2014, E-mail
95 William E. Barklow, personal communication, October 22, 2014, E-mail.
96 DRG, July 7, 2003, BVRVIDEOLISTA, unpublished descriptive list. The list I have has a hiatus between May 4
and July 11, 1997, so either no videos were taken during that time or this list is supplemental to another.
98 DRG, October 31 to December 30, 2000, COMMPLACE, unpublished manuscript.
99 DRG, DM-S10JUL03, Jul 10, 2003. Letter to Dietland Muller-Schwarz. In the section about Griffin’s letter to
D. Muller-Schwarz, all quotations are taken from this letter.
Overview
While studying beaver activity with Greg Auger, Don Griffin recognizes the potential use of Greg’s infrared light-sensitive nightscope for bat studies. They return to bat studies at Trout Pond, Jim Simmons sometimes joining them, bringing along more sensitive military equipment.

Don and Greg spend much effort inventively upgrading their own apparatus and experimental control: artificial insects, fake echoes, fishing poles?

Surprises emerge: Were bats sometimes capturing prey but not producing the significant "terminal buzz" that permitted precise localization of the prey? Or was the buzz simply very, very faint?

Descriptions and photos from some of the last bat conferences Don Griffin was able to attend.

A brief update on some research issues.

The Return
On yet another night, Don was floating in a canoe on that small pond in Harvard's Estabrook Woods. Once again, he was intending to spy on beavers. But this night was different. Greg Auger, his assistant and colleague, had just shown him the power of a nightscope; Don was astonished by how much more he could see with it. Greg’s version was a military-grade nightscope that fit onto a camera as a lens, the lens (light filter) was highly sensitive to infrared light, not heat. A $5,000 piece of equipment! (Greg was a serious amateur photographer, providing many photographs used in books authored by his wife, Wendy Williams.)

That nightscope was a welcome addition for their work in the dark night. Otherwise, their vision was aided only by the Griffinberg-modified infrared lights. Now objects could be seen that were further away and in greater detail. Back to bats! Not that Don forsook the beavers entirely, but his old passion was re-ignited.

Other bat researchers were delighted that Griffin was, in their view, returning to “the fold.” In the field, night after night, Greg was a steady and enthusiastic companion, pursuing the bats with Don. At bat conferences, some would even thank Greg for aiding Griffin’s “return.” One such meeting was the Bat Conservation International Meeting of April 2002 in Austin, Texas, one of Don’s last conferences; he also presented a paper there. (See photos later in this chapter.) The book of papers from that meeting was dedicated to Griffin. Most bat researchers believed that Griffin’s excursion into animal consciousness was just that, a detour away from the serious business
of the science Don did so well. In fact, as I have learned during my interviews, few had even read his writings on animal minds.

**Into the Night with Bats**

The choice of field site was a familiar one, Trout Pond near Mashpee Church in Mashpee, Cape Cod.\(^7\)

The pond is convenient. It’s located in the town where Greg lives, is not too far from Griffin’s Barnstable home, and is close to a local Friendly’s. That is a favorite, modest restaurant chain, where Griffin might meet the volunteers joining him for the evening. And, most importantly and fairly reliably, the pond has many hungry bats, little brown bats (*M. lucifugus*), or “lucis” as Don termed them.

On a typical night, Greg and Don would have eaten their dinners before meeting. Sometimes, instead, Don joined Wendy and Greg at their home for a meal.

Don insisted on carrying all his equipment by himself, setting up before Greg arrived. Once, Greg happened to be present while Don was lugging one of the many batteries required for the work. This was a particularly hefty car battery, so Greg offered to carry it. Don did not accept the offer well. He was irritated, gruffly stating that he could do it himself. That determined independence is very much characteristic of the usually reserved, most polite man.

Having arranged the equipment, Don would settle down to his usual peanut butter and jelly sandwich, ready for Greg’s appearance. Depending on the season and time of dusk, the evening might begin as early as 5:30 or 6 p.m. or so, and so did the mosquitoes. The bats, mosquitoes and researchers remained until about 9 or 9:30 pm, varying throughout the season. Jim Simmons joined them, perhaps every couple of weeks. Jim’s enthusiastic and insightful presence and his superb, expensive equipment would have been extremely helpful, but Jim led a busy life. He was a professor at Brown University, in Providence, Rhode Island, about 1 ½ hours’ drive away. With a full laboratory research program, mentoring graduate students, teaching courses, and family/children responsibilities, his hours were full. So, his visits, though greatly appreciated, were infrequent.

Griffin and Greg appear to have begun the bat work in 2001. In April 2003, Don was again writing to request permission from the preservation consortium to use the land. He briefly described the state of their research and emphasized the importance of Trout Pond:

*We have searched diligently for other places for our scientific research on this subject without finding any other area where there is nearly as much insect catching by bats at low altitude where we can record their ultrasonic sounds and obtain infrared video showing both the bats and the insects they catch.*
... and he stressed the significance of their research:

... because bats achieve a degree of speed and acuity in their interceptions of moving targets that is difficult to match with military sonar.\(^8\)

...and he proudly highlighted his very accomplished collaborating scientist, Professor James Simmons of Brown University.

Trout Pond did indeed offer extremely practical advantages: During much of the season, from about June to mid-September, the bats did intense hunting in a very confined volume in the woods, 6 X 6 X15 meters above the shallow overgrown brook (the “glade”). Thus, the researchers could “locate camcorders, infrared illumination and microphones reasonably close to the scene of insect pursuit and capture.”\(^9\)

The Terminal Buzz

What became the core of Don’s fascination during his “return to bats?” The intense studies began in the 2001 field season,\(^10\) though Greg and Don may have conducted preliminary work in 2000. They were still continuing the research in Don’s last year (2003).

From his first field notes, Griffin indicates an avid interest in the final phases of a bat’s echolocation as it pursues an insect. These are the calls made after the search phase, as the bat approaches and then makes its terminal buzz before capturing or “missing” the flying insect prey.

It was also this rapid, final series of pulses that had so excited Don back in the summer of 1950. With his massive pile of unwieldy apparatus, he had first heard a terminal buzz from big brown bats (\textit{Eptesicus fuscus}) hunting in the field. Finally, he, personally, had evidence from the “wilderness,” that bats \textit{used} that buzz in their insect pursuits and captures. Recall that it took another decade before he could gather adequate simultaneous film and acoustic evidence to convince others thoroughly.

As Don depicts the 1950 field event:

... there was a marked increase in pulse repetition rate, from 5 or 10 per second during straight flight to bursts of several pulses at interpulse intervals as short as 5 milliseconds. This contrasted markedly with the increase of about twofold when bats dodge wires in laboratory experiments ... \(^11,12,13\) [Those were, for example, the lab experiments that the young graduate students, Don and Robert Galambos, had conducted.\(^14\)]

... and [the field work] immediately suggested that echolocation was used to guide the split-second maneuvers needed to intercept flying insects\(^15,16\) ... \textit{Myotis lucifugus} showed similar increases in pulse repetition rate ...\(^17\)


\textbf{2001\(^18\)}

Don’s 2003 letter to the preservation consortium described the advanced state of their research and apparatus. But it took innumerable trials and tribulations to get there. In 2001, Griffin and Greg were already in the field around April 9th, searching for bats, continuing their efforts that season all the way into October.\(^19\) (In subsequent years, they opted mostly for a shortened “prime time” field season, which also afforded fewer miserable chilly, damp nights.) To offer the reader a close view of the realities of field research, and to provide evidence of the incredible persistence of
Don Griffin in his approach to science, I shall describe in some detail the work he and Greg conducted with the bats. Greg was present on most nights and contributed considerably to creating some apparatus they used.\textsuperscript{20,21} I hope that my depictions do not dissuade future would-be researchers from such a career.

In the scenario, on that first night out in April 2001, Greg does see at least one bat, though not the lucis they knew were most populous. Later that week, it is some high-flying bats that Griffin sees at the pond, but only rarely; he records no terminal buzzes. Likewise, stationed at the church that night, Greg sees no bats. On the weekend, they are joined by Betsy and Pete. Presumably, these are Betsy Weaver, who is a good friend of Don’s and Peter Auger, Greg’s brother who had worked with Don in the marshes with the bees and the “Sugar Cube.” Pete is a high school teacher, also teaching part-time at Boston College and often brings students along to join the bat work.

Watching at the church, the group does see a few bats, but Griffin concludes that, somewhat surprisingly, bats are active at the pond before they emerge from the church. Hmmm… they had thought, convenient for their studies, that it was the population living under the church eaves that feed at the pond. Later, they confirmed that this is usually the case, though it required much effort to verify the source of the feeding bats.

By the next day, Griffin is already dissatisfied with his videocorder and is trying out a new one. And so it continues, eager scientists, but recalcitrant bats, and, most especially, equipment not up to the task. Not being able to record their buzzes. Continuing and continuing, usually, no buzzes are recorded. Sometimes just one buzz from an evening’s work. Don takes infrared (IR) video now but still does not see catches. By the end of April, and after many nights of work, there are more bats. But this is daily work, with few exceptions, By April 27, “bats at Trout Pond … some insects but no useful looking video.”

Then, on the weekend, Jim Simmons arrives with his gear, the “Simmonsonian kilobuckage” apparatus, as Don labels it. Jim had a very sizeable research grant, and the equipment to go with it. Jim had also been able to borrow from the military, a thermal imaging camcorder. As I remember it, that camera was so very heat sensitive that it could detect a mosquito way across the pond. I had the good fortune to be able to join an evening’s bat “soiree” during my visit arranged to coincide with Jim Simmons and his “kilobuck” equipment. Apparently, it was the very first evening Jim had brought his thermal imaging videocorder and the first time I had met Greg as well. Pete Auger and some students were also present for the event.\textsuperscript{22} An astonishing experience to be able to see the bats at night, all bright green on the camcorder screen, and even the insects they were pursuing. A quiet evening, since no one wished to disturb the bats or audio recording and observations, but, nevertheless, the individual excitement and enthusiasm were shared amongst us all.]

That April 27 night’s work made Griffin realize he could find more buzzing if he tuned his bat detector (the heterodyne) differently, in fact, to a higher frequency of 35 kHz.\textsuperscript{23} The nights of work continue, sometimes with other visitors and volunteers, such as Griffin’s former Grad student Peter Tyack, now a faculty member at WHOI on Cape Cod, bringing along another WHOI cetacean scientist, Patrick Miller, both interested in communication. Griffin fusses more with the heterodyne tuning, and many of his reports include “fussing with apparatus,” “not good recordings,” “not good video.” Daytimes are also spent “fussing with apparatus” and printing sound spectra of the bats’ vocalizing. Fortunately, he usually does not have to contend with the slow, smoky carbon-producing Kay Electric Sonagraph used by the Paynes in their whale song project and by other early acoustic
communications researchers. By this point, Griffin has bioacoustics computer analysis software, aptly titled "Canary," for it was developed initially for bird song analysis. In fact, the Cornell Laboratory of Ornithology is the creator, aided and abetted by the talents of Chris Clark, the former Marler grad student. Greg used a different sound spectrum analyzer, "BatSound."

In May, they are seeing more bats than in April. Sometimes the bats are best over the pond, sometimes more at the "glade," the area at the overgrown brook. Sometimes Greg and Don see a few moths. These are welcome creatures, as potential prey for the bats and much more readily visible by humans and videotaped than are the tiny mosquitoes and other minute insects. And along the way, the scientists realize they have to give up their headlamps because that and other ordinary lighting seem to disrupt the bats' insect hunting. They fiddle with the placement of the additional IR lights they've acquired. They find that they shouldn't aim the IR light directly at the bat, for it illuminates the background too much, better to aim from the side, so the bat is more distinctive in the videotapes. And extraneous noise! After a good rain, the pond can become high, and water noise grows worse as the pond drains through the culvert. The nuisance noise includes very high-frequency components that may overlap with the bat's sonar productions - drastic impacts on the clarity of recorded bat sounds.

In May, Greg's IR image intensifier seems to be producing good videos, and they produce many. But ... worries about the audio quality achieved. In one instance, the printed sound spectra appear to show three buzzes within 1 second. Is that just one bat, Griffin wonders, or do they also face a problem with multiple bats' signals being recorded?

Griffin also notices some extremely puzzling phenomena: He occasionally records buzzes without clear indications of either search or approach phase signals. Other times, many buzzes, but no catches. Still other occasions, bats, but no buzzes.

One night in June, he tries using the old "Pierce horn" from William Pierce. This was most likely the very same horn used by the undergraduates, Don and Robert Galambos, for their bat ultrasonic studies. Too few bats that night. Finally, another evening, with their usual apparatus, he gets two, maybe three "classic" catches, but the only good signal-to-noise ratio recording does not have the bat within the video frame.

The next day and intermittently afterwards, he adds to their activities, recording bats' social communication sounds at the church. These are not ultrasonic, and many can be heard by humans. He doesn't give up on the echolocation sounds at the pond, but he is clearly discouraged by their lack of progress, and so expands their endeavors. One evening in July, they are joined by Pete Auger and his students who contribute to the efforts ... and confusion. They all count the bats emerging from the church .... Hmmm ... a lot of different numbers. Compromise! About 280. Among those flying bats are feeble flyers; could they be young bats, making early attempts at flying? Could their communication and echolocating sounds differ from the adults'? Pete's high-fidelity Marantz recorder records "lots of complex signals, but no way to tell which types of bats made which signals." [CR: Very recent work with captive big brown bats (Epistecus fuscus) studied from infancy showed that they in fact develop the sounds used in echolocation, in particular, the terminal buzzes, considerably before they gain full control of their wings. Sounds are developed by about 17 days and proper wing control by 24. Some of the bats' early attempts at flying are indeed feeble; they can make at least some of the proper sounds but may misjudge distances for landing. The young remain sensibly cautious until their sound and locomotion coordination improve.]
Likewise, in recent studies conducted with a different species of bat, the greater sac-winged bat \textit{(Saccopteryx bilineata)}, the infant bats were \textit{babbling}. These were not the very high-frequency sounds used for echolocating, but the lower-range sounds used for social communication. Like humans, the infant bats “babble” almost constantly, not needing a particular stimulus to initiate any session; they repeat syllables, tend to be rhythmic, and include both variable and adult communicative components. Eventually, in the male, the sounds develop into the territorial and courtship vocalizations used by adults. Once again, Griffin was posing intriguing questions.

That night in June, Griffin notices several bats producing overlapping signals; “are they talking????” \[^{28}\] [CR: We still don’t seem to know the full answer to that one. We do know, as previously discussed, that a bat seems to be able to “shut up” its echolocating calls when flying close to a familiar bat. Thereby, it is helping \textit{not} to jam the other’s signaling and, perhaps, the researchers suggest, benefitting from the other’s echolocation information. \[^{29}\] And we also know that mother and infant bats communicate vocally with each other, with “social sounds,” aiding the process of re-uniting in a dark cave field with thousands, maybe a million or so, bats. At least some bat species have territorial and courtship social sounds. And likewise, we have learned that even the \textit{(Myotis myotis)} bats’ ultra-high frequency echolocating calls provide individual identification. This information, the researchers suggest, might facilitate many of the social behaviors observed in bats, such as maintaining their group as they fly so very fast and furiously in the night to a favorite foraging site. \[^{30}\]

Don and Greg continue their efforts at the church. To improve the acoustic recording, Don soon creates a “Rube Griffinberg pole” (his term) to get the audio recorder way up to the church attic’s triangular window. Bat sounds seem to be originating from there.

Back at Trout Pond, Don continues adapting his apparatus and, one night in August, Jim Simmons “made recordings with his rig.” As Greg recalls, Jim was particularly intrigued by the observations of bats successfully capturing insects, apparently \textit{without buzzing}. Could such bats possibly be using echoes from calls made by other bats? Such questions led to Jim’s (and others’) great interest in swarming whereby tens, hundreds, even thousands, of bats fly together in tight groups. How did the bats manage not to jam each other’s signals? Might they be sharing echolocation information? The next development became a “batting” trip to Utah, an expedition joined by Don, Greg, Jim and Alan Grinnell among a few others. \[^{31}\] They did not find jamming or establish concrete evidence for sharing echolocating information, though the researchers suspected that such was occurring. Producing the paper, \[^{32}\] published with Jim as lead author, did entail a few glitches. In Don’s view, Jim Simmons was something of a perfectionist, and frankly, in my view, not unlike Don himself. Jim always wanted to get that next bit of data to \textit{really} clinch an argument. Don declared, “Jim is going to publish that paper if it’s the last thing I do.” \[^{33}\] Indeed, among Don’s last projects, in his very, very last days, was work on a bat paper with Jim Simmons; I believe this was the paper, published after Don’s death.

That season ends. They didn’t capture any insects, so their identity is not known. “All video was made well after dark when we could not see the bats or insects.” After describing the acoustic, lighting and video instrumentation used, Griffin notes, “None of these arrangements was optimal, and we will be trying to obtain much better-controlled observations in 2002.” \[^{34}\]
On a more personal note, this industrious scientist did take some time off. No field data exist for June 13 - June 24, 2001, and again not for June 26 - July 3, and this is “prime” bat field season. Maybe a conference, maybe a visit to friends, family or colleagues, but I do not know.

In December 2001, further reviewing the summer’s data, he decides that all ...

... these May videos aren’t good enough to spend more time on them. Mikes weren’t as close to bats as late summer experience showed was necessary to detect faint buzz signals.

Many, many hours of work! For naught? No, they further refined their experimental protocol and apparatus and determined more clearly the capacities and limitations of it all for the questions they were asking. And, and ...

... this is his intrigue and reason for continuing this line of research. “… the videos do indicate how the relative intensities of search and buzz signals vary during insect pursuit.”

Often, he has even observed a sharp decrease in the intensity of the sounds as the bat begins the approach phase signals or even the “feeding” or terminal buzz, and even what appears to be “buzzless” insect catching. Why, why, why? The bat should be wanting to be very accurate as it attempts to close in on the tiny insect, which, being so small, will not reflect very much of the echolocating sounds that reach it. And the bats at Trout Pond should have been very hungry, for the Mashpee Church bats were a nursery colony. Thus, the summertime feeding bats were predominantly pregnant and lactating females.

I shall not describe in detail two more intensive seasons of work but highlight some obstacles and achievements.

2002

Ever Modifying the Experimental Apparatus

Don had improved the auditory recording yet more with borrowed “Racals” from Alan Grinnell and Jim Simmons. A Racal was the instrument made by the Racal company and used in the early days of recording ultrasound with a tape recorder; it was not digital.” As Greg recalls, they “weighed a ton.” The Racals were providing recordings with better signal-to-noise ratios, i.e., the bat’s vocalizing was not lost in background noise, such as that from wind, or rushing water, or even a microphone’s own internal noise.

On May 1, 2002, Don writes, pleased with Greg’s video which “looked fine, many buzzes and catches.” Then in capital letters, “A GOOD NIGHT, AT LAST.” They are sometimes doing flash photography as well. The season’s notes are interspersed with various “GOOD” and “GOOD CATCH,” “buzzless catch,” “faint buzz noise,” “silent catch,” and “buzz” seems to come AFTER catch.” Even as an outside reader of the columns of dates, figures and terse notes, I feel happy for these earnest researchers.

Jim Simmons is sometimes present with his fine equipment. And they constantly modify to improve recordings; a black felt background is added during one of Jim’s visits to reduce both ultrasonic acoustic and thermal reflection from leafy backgrounds.

In his basement workshop in his Barnstable home, Don is constantly fiddling, trying out new variations to the apparatus, constructing devices. Both he and Greg are also analyzing data, and indexing the raw data for future review. Don hopes to entice Greg to do some of his putterings in the “lab room” that Don has established for bat detector and radio microphone development enterprises. He considers Greg a fine “gadgeteer.”
A Revolutionary: Donald R. Griffin

Don at Trout Pond
(Photography by Gregory Auger)

Top: At Trout Pond, Mashpee, Massachusetts, May 2003 with apparatus for bat echolocation studies

Right: 2002 or 2003

Bottom: May 2002
In a letter to a colleague, he writes, "... one of Pete Auger's Boston College students ... is supposed to be helping me with the tedious job of analyzing the recordings of bat detector signals with an eye on the possibility of 'buzzless' insect catching." After she left our meeting, "I did 3 minutes more of "BatTime"..., and it took me nearly an hour to do so." I'm curious to see how far she progresses.\[41\] [CR: Although, to me, the situation seems extremely frustrating, Greg recalls that Don didn't show much frustration.] "We would just get down there and enjoy what we were doing," Greg remarked.\[42\]

This is also the year a fishing pole enters the story. Greg recalls that Don had overheard some fishermen complaining that bats attacked their fishing lures at dusk. In Greg's words, Don is a "sponge," he absorbed everything and eventually something would reappear in his inventions or his writing. Thus, in 2002, they begin hanging lures from a fishing pole. One evening, a small sinker from fishing paraphernalia hung as enticement, and "The bat flew past lure as though inspecting it, and buzzed but did not hit it."\[43\] The dry fly lures used by the fishermen best attract the bats. Don cuts off the barbs, so as not to injure the bats. They also tether moths to the line. Unfortunately, even though bats normally happily catch free-flying moths, and strongly prefer them to flies, the tethered moth just doesn't do it. They tether insects as lures. Their efforts continue the following year.

During the winter (2002-2003), Don is reviewing 2002 data and planning the next year's work. As Don writes in a letter to Jack Bradbury (January 19, 2003), a friend, his former RU grad student and later RU faculty member, now at Cornell:

... we managed to get some recordings, mostly of M. lucifugus attacking tethered insects, but some spontaneous catch-like maneuvers, although only once in four evenings did we get video of both the bat and the insect it caught.\[44\] [Bolding by CR]

**Presenting Their Research to NASBR**

In 2002, Don and Greg attend the North American Symposium on Bat Research (NASBR) in Burlington, Vermont (November 6-9) and Griffin presents their findings from Trout Pond. (A summary of the instrumentation used may be found in the EndNotes.\[45\]) Their essential messages are:

[We] ... often detected no approach phase or buzz on warm May or June evenings near a large nursery colony when we could see sharp turns and other catch-like maneuvers. ... Many catches were preceded by the typical search-approach-buzz sequence, but others showed only search phase signals. ... [When the microphone was close enough] ... we always detected at least a minimal buzz, but often no approach phase. ...

Why should bats vary the intensity of approach and buzz phase signals? Our videos almost never showed evasive maneuvers by the insect [which would have suggested that the insect had detected the bat's ultrasound.] Withholding information from other hungry bats seems unlikely because so many insects were available. ...\[46\]

[And his final statement]: We might learn more with more sensitive microphones.\[47\]

I do find that last sentence particularly distressing. Here is a dedicated scientist, who has created the field of bat sonar and worked with enormous diligence and persistence throughout his life. He believes he has a fascinating problem, but, though the necessary, proper microphones have been invented, he can't have them. No wonder we hear him mumbling sometimes in these last
years, "an unfunded 'has-been'." But at the same time, he heartily enjoys what he terms his "recreational science."

And I do wonder a bit about the significance of the Conference schedule. Griffin speaks on Saturday, the last day of the meetings, during a "concurrent session" at 8 am in the morning after the conference's only evening reception. Not ideal. Griffin also receives daunting news during the meetings: his medical test results are completed. His thyroid cancer has returned. I don't know how Griffin reacted, but Greg's wife told me how devastated Greg was to hear this from Don.48

Nonetheless, Griffin is Griffin. He continues to make detailed research plans for 2003.

"What is Worth Doing in 2003?"49 [Don's title for his ponderings]

Don penned several sets of notes about plans for 2003. Some of these thoughts concentrated on bat research, with his particular interest still on the bats' terminal buzzes and their process of insect catching.

"Buzzless" Bats?

Why, why does the bat sometimes choose to make either no buzzes or, as was seeming more likely, such a huge intensity reduction? It does not happen consistently.

To even begin attempting to answer that question, one needed yet more accurate, high-fidelity recordings, acoustic and visual, of repeated encounters of bats in natural circumstances, capturing flying insects. How to do that?

In January 2003, recalling that both the bat's echolocating sounds and the mike are largely directional, Don ponders:

Ways in which we might get directional beam pattern from bat under natural conditions at Trout Pond glade: Suppose we arranged a bracket to hold more than one mike in a sort of perimeter around a tethered insect. Rather than dangling from a thread, suppose lure was attached to a vertical wire so that it would always be in one position. Ideally, we would like excellent broad band mikes and multichannel recording, but suppose for practical reasons we used D-10 heterodyne detectors.50

He continues his ruminations, but adds ruefully and with considerable irritation:

The advantage of using these heterodyne bat detectors is that we have three and they aren't that expensive. But I suppose Jim [Simmons], Cynthia Moss and others with MUCH better equipment should be doing this, rather than we with inadequate equipment. Grr!51

[Bolding by CR.]

And on another occasion, he asks,

What would it take to record ultrasonic frequencies right on the same digital video tape? Probably only Simonsonian kilobuckage. [Bolding by CR.]

[CR: It is a challenge, a nuisance and time-consuming, for the researchers to review the data from the different instruments, making certain that they are exactly synced, to, I believe, millisecond precision.]

Griffin also described some 2003 plans in an April 19, 2003 letter he wrote to a member of the "elfs," the elephant team in Dzanga-Sangha. Dzanga is a protected forest elephant reserve in the Central African Republic; Andrea Turkalo headed the "elf" project. [Griffin also held an especial interest in elephant communication and social behavior, as will be discussed
Griffin thanks them for elephant vocalization recordings he shall use for his plenary address at a 2003 midsummer Acoustics Society meeting in Maryland. He then tries to arouse their interest in his own bat recording.

You might be interested in what I am trying to prepare to do this year re bats’ insect catching by echolocation. I think we pretty well settled that they do always make buzzes, but the intensity is sometimes way down certainly 20 maybe 30 dB below what it is at other times. Really accurate measurement would be very difficult because recorded intensity varies greatly with distance and also to a smaller degree with angle re axes of both mike’s sensitivity and bat’s emission pattern – and if that isn’t enough trouble, both, indeed everything probably, varies with frequency within the octave sweep.\(^52\)

In fact, the final evidence that “they do always make buzzes” derived from synchronized, very high-quality, high-frequency recordings and two videos, Greg’s from the side and one from below. The images revealed that the bat and microphone were often not in the same plane, as suggested from the image made by a single camera. Thus, the actual distances between them were sometimes larger than that suggested by a single image. Furthermore, importantly, the highly directional bat vocalizing and the highly directional microphone were often not aligned so as to be maximally effective for recording. When they were, at least faint buzzes were always detected. Likewise, many of Jim Simmons’ high-frequency recordings, up to 40kHZ, showed the elusive buzz.\(^53\)

**To Make an Insect**

Don next turns his thoughts to creating an insect lure to be placed at just the ideal spot for recording. Not just any sort of insect was needed, but one that would attract a bat, specifically the *Myotis lucifugus*, at Trout Pond.

Then it is April 19, 2003, and Don is hopeful (and impatient to get going). As expressed in the same letter to the “elves,” Don had, in 2002, made some video of bats attacking artificial insects. *They will attack anything roughly the size of insect prey; but in the lab they learned over a few days that some of the stuff we were throwing up was edible (mealworms) and the rest was not (ball bearings of various sizes, plastic disks, etc.). What I hope to do is get real, live insects tethered lightly enough that the bat will get them off and get a meal. Maybe come back for more. The trick will be to get an attachment that won’t let the insect escape yet flimsy enough that the bat will get it loose without too much trouble.\(^54\)*

He continues with his usual enthusiasm:

*I can’t wait for it to warm up enough to catch some insects and experiment with attachment methods. Here the lovely ocean is still ca 40F, so Spring comes more slowly than inland. Maybe tomorrow.\(^55\)*

In 2003, they waited at least til April 23 to tromp off to Trout Pond, though Greg had been hearing bats buzzing since the middle of April. But plentiful insects are needed for good recordings of bat catches and finally by the 23rd, it “warmed up?” to all of 47°F by 5:30 pm, though 42°F by 8:30 pm. All Don and Greg saw were a few small insects over the pond early on, which then became fewer as the evening progressed.

Remember, it is so dark that they usually cannot see the bats, much less the insects. They are often aware of the bats’ close presence from the bat acoustic detectors or the video with IR light
or Greg’s image intensifier. They do attempt to use bright halogen spotlights adapted with IR filters, but the batteries seem to last only about an hour. And Don remarks that at least one night, he never actually saw a bat, even with a white spotlight; that “did however cause bat to go away.” Hence their hesitancy in using white light. They commandeered an ordinary, blue kitchen bowl, intended as an opaque baffle for an LED (a light). Sadly, their resultant poor video reveals that it’s not truly opaque.

Another night, a crucial battery for the heterodyne bat detector isn’t charged, and among the multitude of electronic bits and pieces, Don has forgotten the necessary piece to connect a 12V supply instead of the battery. That leads to a “GGGGrrrrrr” in the notes, a rarely expressed emotion for this reserved man.

More fishing poles are part of the endeavor this year as Don attempts to lure bats into an optimal acoustic and video recording location with an insect tethered to the pole. “Poles” plural, for he attempts to attain just the right sort of jiggle from the right sort of pole to entice the bat. And what sort of rig to hold the pole on its own? But the bats are having none of it. Even, one night with “lots of bats … All out over pond but they didn’t come close to a pseudo-insect lure (a dried bit of oak leaf).”56

By July 2003, well into the fieldwork, his plans express some of the disappointments and difficulties,

This year’s attempts to get the “lucis” to take tethered insects got nowhere. Lures with or without live, moving, wing-flapping insects were hit, and occasionally a bat would grab and hold for a second or less. But no insect was actually removed or eaten.

Maybe my insects were too large. We know lucis take Drosophila [fruit flies, D. melanogaster] but some of the insects Greg’s video showed being taken were certainly fairly large. Hmmm. Could I tether smaller insects? It is difficult enough with those I have tried. But there is that U. Mass student friend of Natasha’s who is said to have induced fruit flies glued by the feet to a light thread to fly. [Natasha Korobov is a student working with Don and Greg on the bats.]

Maybe flying was the critical element, not just flapping their wings in place, but actually flying.
Natasha did bring her friend with the free-flying fly tethered to a fishing line. Greg remembers how the fly would often keep flying out of the video frame and the rather hilarious efforts to keep the camera on their “flighty” subject. They had about 6 feet of line, so the fly could travel a good distance. And it was almost impossible to entice a bat to capture the fly while the researchers simultaneously captured the episode on video.\textsuperscript{57}

**Or “Experimental Echoes?”**\textsuperscript{58}

In considering “experimental echoes,” (July 28, 2003), Griffin was musing about a device (to be created) that would emit experimentally controlled echoes emulating those of various species of insects. He considers two approaches:

1. **An actual, physical, “pseudo-insect” equipped to vary its echoes.**
2. **Rebroadcast of the bat’s search phase signal from a speaker to produce a phantom echo, the properties of which could be manipulated.**

He develops these possibilities with great specificity, outlining the likely difficulties he can imagine. For those who would like to consider these details, his writing has been copied into the Endnotes.\textsuperscript{59} In brief, among the significant issues is the extraordinarily tiny time intervals involved in the bats’ prey detection: “Bats appear to detect an insect with one or at most a very few search phase signals. With lucis these last only 2 or 3 msec.” Likewise, small insect wingbeats can be very rapid too, maybe 500 Hz or 500 times a second, a mean period of about 2 msec. How exactly a bat can detect this information and what aspect(s) is/are being detected is not at all clear. [CR: Yet a bat is choosing which insects it prefers, and wing beat pattern could provide a clue.]

Then, how to create a device to be a “pseudo insect?” Here an essential criterion is something that can vibrate very rapidly, simulating rapidly beating insect wings. “Jim Simmons suggested using or adapting something called a ‘bimorph’ used in studies of vibration sensitivity.” Griffin’s not at all sure what such a contraption might be, but will begin his search through some catalogs, maybe some for experimental psychology. He muses further ... 

**maybe “the devices used in some guidance devices for the blind, supplying information as tactile signals to the skin. ... Dissect a small loudspeaker? Or small earphone?**

After his disheartening analyses of any potential attempts to use “Pseudo Echoes” (details in Endnotes\textsuperscript{60}), he returns again to the possible creation of a “pseudo insect.”

**Could a hydraulic inflation/deflation device simulate size fluctuations at several hundred Hz?... Could a reed relay be adapted to simulate a moving insect wing?**

**2003 - The End of Field Seasons/ Last Conferences**

Come August 2003, Don is still concerned with the problem of tethering a flying insect as Natasha and her friend have done. Sadly, (August 21), he learns that Natasha will not be available to provide help in the near future and probably not in the fall. Don is thinking “Maybe worth trying her tethered house flies in April and May [2004] when bats presumably hungrier.”

During 2003, Don is also preparing paper presentations about their work. One, rather wide-ranging, is intended for his plenary address to a 2003 midsummer meeting at the University of Maryland (July 27-30). It is a conference of a new subsection of the Acoustical Society of America meeting, titled “International Conference on Acoustic Communication by Animals.” The other paper, by himself and Greg Auger, he intends to present to the Acoustical Society Meetings in
Austin, Texas, November 10-14, 2003. The title is simply "Variability of feeding buzzes in little brown bats (Myotis lucifugus)." The abstract, but not the completed paper, is available in the society's journal (JASA).61

“Bat Conservation International Meetings” - Austin, Texas (April 2002)

Griffin had also attended a somewhat earlier meeting about bats shortly before in 2002. As noted, the volume of papers from the symposium on echolocation at the meeting was dedicated to Donald R. Griffin, recognized as creating the field of echolocation.62 Don also presented a paper at the gathering, “The Past and Future History of Bat Detectors.”63 Fortunately, Greg Auger took several photos at the conference (this chapter).


First, the “Animal Acoustic Communication” meeting in Maryland. It was an ambitious gathering, the first of this group. The organizers saw a need to bring together researchers, particularly both the new, young ones and the well-established scientists, with limited chance previously for such interactions. And there were many international attendees and speakers. Among the organizers were some familiar names: Christopher W. Clark (Cornell) and Robert J. Dooling (U. Maryland), both previously from the Marler lab in the Animal Behavior group at Rockefeller University. The bat researcher Cynthia F. Moss (U. Maryland) was another of that organizing team.

The hope was to help advance the field of animal bioacoustics through greater integration across the biological sciences. The organizers saw the field of bioacoustics as focusing on animal sound descriptions and the mechanisms of the sounds’ production and perception. They wished to promote broader approaches, including issues of evolutionary and ecological adaptation, ontogeny, and genetic basis, all within an integrated context of multi-modal animal communication. There were three Keynote Speakers: Besides Don, Jack Bradbury (Cornell) and Darlene Ketten (WHOI) spoke.

Don had begun his preparations early, conversing with Jack Bradbury on one of his frequent visits to Cornell and its people. “I would like to accept Jack’s suggestion to at least include ‘communication as a window’ theme. Just how?” he is wondering in notes to himself in mid-February, 2003. Recall that he had also gathered recordings of the very low-pitched elephant “rumblings” from the “elfs” in Andrea Turkalo’s Dzanga project. Despite this advance planning, he must have sloughed off a bit, for continuing on in these same preparation notes, the next entry is dated July 2, 2003: “Time to get really into this!” He jots down many ideas, replete with many typos. He notes tantalizing research that needs to be further developed, including Con Slobchikoff’s work with prairie dog communication.64 He mentions again the “whispering bats,” wondering again why they seem sometimes to lower their intensity level during the final approach and terminal phases.

“Every time microphone sensitivity improves we learn more.” [CR: I’ll correct the rest of the typos. His two-finger “hunt ‘n’ peck” technique achieved in his youth was happily curtailed most of his life by oral dictation, hand-written compositions and very competent secretaries. Those fine helpers were not so readily available in his retirement years.]
Bat Conservation International – Echolocation Symposium Attendees - Austin, Texas – April 2002

Identified Persons:

(Photo by Gregory Auger)
... And at the BCI 2002 Conference:

Top (Left to Right):  
**Don Griffin and Alan Grinnell**

Bottom (Left to Right)  
**Don Griffin and Elizabeth Kalko**

(Photos by Gregory Auger)
And again he admonishes in his notes, “SOME seemingly outrageous speculations have proven to be correct. ... but ...[they can be] STARTING POINTS FOR CRITICAL INVESTIGATION.” [Any capitalized words are Griffin’s.] Among his examples, he mentions the “outrageous” suggestion that bats have sonar and that they use it to catch tiny insects.

He continues again to the topic of consciousness. In his February 15, 2003 notes, he writes, Information conveyed is a central concept. But information about what? I will emphasize intentions, conscious experiences. ... Acoustic communication IS EVIDENCE, once we open our minds to this fact of life.

And on July 2, 2003, Communication, acoustic and other channels as well may well be a source of information about the subjective feelings and thoughts of animals. ...

... with our conspecifics we use acoustic and other communication to infer what our human companions feel and think, and here is no reason why this same basic approach cannot provide significant data about the conscious experiences of other animals.

He does label his continual “pushing” on the subject a “Cracked record.”

I cannot locate a copy of his final presentation, but a copy of the text of his PowerPoint slides indicates that he gives a broad overview of animal communication, concentrating on bats’ echolocation, including a video. He urges further study of the potential human capacity for echolocation. And he speculates about the possible significance of the wide bandwidths available in acoustic communication; that broad bandwidth could convey detailed information. Finally, he urges recognition that, like humans, animals can convey subjective thoughts and feelings in their communication.65

He recognizes that such considerations ... may seem to many a radical idea, primarily because we have been trained to avoid anthropomorphism ... but in doing so we tend to slip into a “mechanomorphism” which may be equally misleading. We need a balanced “zoomorphism.” And a broadminded investigation of animal communication can facilitate this significant development.66

The impact? Jack Bradbury recalled a very good presentation by Don.67 The meeting was deemed a big success, and similar convocations followed every few years. Attendees had a “fun” time, meeting other researchers across the globe interested in similar problems; new collaborations were initiated.68 The journal Current Science published an overview of some highlights of this meeting in North America and its “paired” meeting in South America.69 In Brazil, Peter Marler was a Keynote Speaker. His talk had a more “acceptable” tone than Griffin’s. Marler spoke of the relevance of avian vocalization to Tinbergen’s Four Questions: ontogeny, causation, function and evolution. The reviewer noted that these are the topics “in which ethologists are mostly interested.”70 As to the Maryland conference, the reviewer concentrated on technological advances and didn’t even mention Griffin’s topics or the fact that he was a Keynote Speaker. Once again, Don was facing resistance from scientists just to consider animal sentience.

Not all agree with this assessment. Again, Jack Bradbury, a conference participant and Keynote speaker, believes that most of the persons at the conference were already converted to Griffin’s point of view. Jack’s beliefs seemed close to Griffin’s. But perhaps, the participants’ views are closer to an attitude that animals might be conscious. Their starting point is likely closer to pA
=0.0, rather than pA= 0.5, Griffin’s null hypothesis, where pA is the probability of Awareness or Consciousness.”

“Acoustical Society of America (ASA) Meeting” - Austin, Texas (November 2003)

Onto Don’s preparations for the ASA Meeting in Austin, Texas November, 10-14, 2003. A draft of a paper exists which I believe is the one intended for that meeting, but I cannot verify that. The draft (July 12, 2003) is titled, “The Importance of Faint Sounds in Echolocation and Communication.”\textsuperscript{71,72} In it, Don briefly reviews the history of the work conducted at Trout Pond, primarily by himself and Greg Auger, along with their occasional colleague, Jim Simmons. Initially, they believed buzzless insect captures might occur. Recall that only with greatly improved acoustic and video approaches, could they determine that buzzes were always made during capture sequences. They reached this conclusion with the aid of two video cameras that permitted them to verify when auditory recording was occurring with close, directly aligned bat and microphone. Specifically,

\textit{All of these recordings with two camcorders did show buzzes, provided that (1) the bat was within two or three meters, (2) it was headed within about 90 degrees of the bearing of the bat detector from the bat’s position, and (3) its position was within about 90 degrees of the axis of maximum microphone sensitivity.}

But, nevertheless, some approach phases and some buzzes are very low intensity. Griffin raises the possibility of a “Whisper World” of communication. He notes that several species, such as shrews, rats, and mole rats, do use low level echolocation to detect nearby objects in their typically dark environments. The evidence, however, is primarily from the lab, not the field. What of the bats? Don recalls his early attempts in the 1950s to record the tropical bat, \textit{Carollia perspicilata}. Initially, no sounds could be recorded, for the bats were using orientation sounds with lower intensities and higher frequencies than could be detected by the microphones then available. Later, with much more sensitive microphones held close to the “whispering bat,” low-intensity vocalizing was detected.\textsuperscript{73,74} Again, in 1969, at a field site in Papua, New Guinea, a “river of bats” flew out of a cave each evening, but Griffin and the other researchers could detect no orientation sounds. Finally, the sounds were determined to be very high-frequency vocalizations, greater than 100kHz, and quite low intensity.

Griffin wonders if those other species and the bats could be using such low intensities in a “whisper world” of both echolocation and social communication. Soft vocalizing might help avoid a predator’s attention, prevent competing males from hearing a courting, vocalizing male, or better ensure that only the nearby, familiar companion was privy to the information. [CR: Recall the work of Chen and Moss, suggesting nearby, familiar bats might be sharing the echolocating information produced by the one vocalizing bat, the other remaining silent.\textsuperscript{75} The research by Yossi Yovel et al.\textsuperscript{76} indicates that ID information is available within echolocating calls. Thus a bat could potentially identify incoming signals as those of the very nearby neighbor, which would be more accurate for itself than any vocalizing it might overhear from a more distant neighbor. Just as the bat’s brain can select and identify its own vocalizing from that of others, could a bat similarly privilege a specific other individual’s? Fascinating questions.]

Griffin surmises that low-intensity echolocation could be particularly useful for animals that live underground, move through leaf litter, or shelter in subnivean spaces (i.e. within the
snowpack). The species likely include shrews, mice, rats and voles. The animals would be primarily concerned with objects at very close range. They need to determine if a tunnel continues or is blocked and so not a very good refuge or pathway. To gather recordings would be challenging, due to ambient noise, the internal noise of many microphones and the difficulties of filtering out interference if the animals are using broadband signals (e.g. sniffs). But, but, notes Don, some of these spaces often have very low ambient noise levels. We would need better recording equipment. He concludes that “... exploring a potentially significant unknown acoustic territory might repay the effort.”

I have included all these details to further illustrate the versatility and breadth of Griffin’s thinking and his undaunted persistence. During these later years of intense bat research (approximately 2001-2003), Don was also involved in much more abstract thought about animal consciousness. His particular intrigue focused on elephant consciousness and he communicated some of his ideas both to the ongoing elephant project, the “elfs” in Dzanga, as well as to Katy Payne. Katy had been studying elephant communication and social behavior for decades, most recently with the “elf” group until her retirement. More on elephants in another section of this chapter. He was also at work with Gayle Speck writing his last detailed review and analysis of the most contemporary research pertinent to animal consciousness. And he was working with Jim Simmons on that final bat paper noted earlier.

Reflecting

Clearly, Don does not yet realize the seriousness of his medical condition or refuses to accept it. He is revising the ASA paper draft on July 12, 2003. Even on August 23, he is planning for spring 2004, the following year. He never attends those November 10-14 meetings in Austin, Texas; he passes away on November 7, 2003. I don’t believe that any of his plans for the potential “pseudo insects” or “experimental echoes” materialized. He had only those few months remaining, and chemotherapy takes its toll. Recall his letter of July 10, 2003, where he states, but does not clarify, “I have become overinvolved now in other enterprises.”

Bat Research Continues

Since Griffin and Auger’s work, the field has greatly advanced, both technologically and in research accomplishments. Jim Simmons comments that bats can be seen via their own body heat with a small hand-held instrument. A videocorder can be kept in one’s pocket and connected to a camera. High-speed digital video recordings now also exist, allowing precision not previously obtainable. The advances in research are too extensive to discuss in this book, beyond my descriptions in Chapter 11 and a few comments here. Let me note that terminal buzzes emitted before capture have been found in other very different species of echolocators, the toothed whales (Odontoceti), including 50-ton sperm whales (Physeter macrocephalus) and porpoises (Phocoenidae).

Furthermore, these researchers found evidence supporting Griffin’s thinking that a bat’s echolocating involved conscious, decision-making processes. In addition to automatic aspects, these authors found evidence for insectivorous bats’ (Myotis daubentoniid) use of the terminal buzz in a very rapid decision-making process. By removing prey just before capture, they determined that the terminal buzz is not inflexible, but adaptable. “...neither the buzz nor capture movements are...
stereotypical, but dynamically adapted based on sensory feedback.” The bat does not simply continue a series of “catch” actions, but very, very rapidly stops trying to catch the missing prey and modifies its behavior and vocalizing.

Validating Don’s intense interest in “terminal buzzes,” these same authors state quite simply, “The function of the terminal buzz is still not understood.” They note several possible functions suggested by various other researchers. Perhaps the buzzes help track evasive targets. [CR: That possibility seems relevant to thoughts, many decades before, by Griffin and his then Harvard grad student Alan Grinnell. It appeared to them that the bat seemed able to predict future positions of the target insect.] Mariana Melcón, Annette Denzinger, and Uli Schnitzler suggest that a buzz might provide post hoc information. It might help “bats assess the cause of unsuccessful capture attempts and eventually react adequately.” [CR: Recall Don’s surprise at a bat apparently buzzing after a capture (observation of May 31, 2002) and the many buzzes made without successful capture. Could the buzzings provide fruitful use of post-hoc information?]

A review by Lasse Jacobsen et al. (2013) discusses how the integration and interaction of intensity, directionality and time information in bat echolocation signals creates the bat’s perception of its surroundings. Intensities can range from “whispers” to very loud, each contributing to the bat’s overall view of its acoustic scene. For further information about recent research on these many topics, I refer the reader to an update of the history of bat echolocation research by Yossi Yovel.

Now to larger, non-flying creatures, the elephants.

ENDNOTES
1 Gregory Auger, personal communication, July 13, 2021. Email. Description of the night scope.
2 Gregory Auger, personal communication, November 4, 2016, In-person interview in Mashpee, Massachusetts. Many aspects of Griffin and Auger’s work together on bats at Trout Pond derive from the interview.
3 Gregory Auger, personal communication, May 8, 2018. Telephone conversation. Gregory described Don’s reaction to the night scope and the gratitude of the other bat researchers in Griffin’s “returning” to the fold.
5 Photographs from the BCI bat conference may be found titled “Bat Conservation International – Echolocation Symposium Attendees - Austin, Texas – April 2002” and “… And at the BCI 2002 Conference” in this chapter, Volume Three – Chapter 18, Part 5, “Back to Bats,” in section, “2003 - The End of Field Seasons/Last Conferences.”
7 Trout Pond is described in more detail in this chapter (Volume Three – Chapter 18, Part 3, “The Ponds in His Life.”).
8 DRG, April 12, 2003, letter to Trustees of the Reservation and to Cornish Associates. Computer file TROUTPOND03.
10 Both Griffin’s notes and published papers indicate intensive studies originating in 2001.
11 DRG, 1953.
12 DRG, July 12, 2003, LOWINTENSITY-WHISPER DRAF-July 12 2003, unpublished manuscript. The quoted section is from this manuscript.
The Griffin and Galambos experiments are described primarily in Volume One - Chapter 6, “Young Men and Flying Bats,” subsection, “The Ears of Bats,” the section, “Pierce and His Incredible Parabolic Horn” and the section, “What Was Accomplished? Its Significance.”

DRG, 1953.

DRG, 1958a.

DRG, July 12, 2003, LOWINTENSITY-WHISPER DRAF-July 12 2003, unpublished manuscript. The entire quoted excerpt is from this manuscript.


Gregory Auger, personal communication, November 7, 2016. In-person interview in Mashpee, Massachusetts.


Gregory Auger, personal communication, November 4, 2016. In-person interview in Mashpee, MA.

The “980 te” is his most frequently used detector, which first stores the bat’s ultrasonics digitally, thus preserving the fidelity very well, and then replays at a slower speed.

James L. Simmons, personal communication, April 20, 2019. Telephone Interview.

This work is described in Volume One - Chapter 6, “Young Men and Flying Bats,” subsection, “The Ears of Bats,” the section, “Pierce and His Incredible Parabolic Horn” and the section, “What Was Accomplished? Its Significance.”

Kathryn Knight, 2019. This is a brief summary of the 2019 journal article by H. W. Mayberry et al., 2019.


Chen Chiu and Cynthia F. Moss, 2008.


James A. Simmons, et al., 2004.


DRG, December 22, 2002, 09SEP02 from BATWIN03, unpublished document.

DRG, December 22, 2002, 21SEP02 from BATWIN03, unpublished document.


Gregory Auger, personal communication, April 14, 2018. Telephone Interview.

DRG, December 22, 2002, 21SEP02 from BATWIN03, unpublished document.

DRG, January 19, 2003, letter to Jack Bradbury, computer file JACKB19JAN03.


Pettersson D-100 bat detectors were used to detect bat ultrasonic echolocating calls. Bright white light reduced insect catching so we used a Canon XL1 camcorder with ITT Pocketscope model F6010B image intensifier and near infrared lateral illumination to keep light off the leafy background. Near IR illumination came from three 50- or 75-watt 12-volt halogen bulbs with glass filters (Schott RG 695, 715 or 780) cooled by small blowers.

Gregory Auger and Wendy Williams, personal communication, November 4, 2016. In-person conversation in Mashpee, Massachusetts.

The quotations in this section, unless otherwise indicated, are taken from DRG, July 18, 2003, unpublished document, file 18JUL03rebats.
ith one or at most a very few searches’ Alarm and Other Semantic Communication.” Primarily in Part 3, section “Natural Animal Communication,” subsection “The Monks’ Alarm and Other Semantic Communication.”

52 DRG, April 19, 2003, Letter to Eric of Andrea Turkalo’s Dzanga Elephant Project. Computer file: Dear Eric-Thnx Elephant ...
53 Gregory Auger, personal communication, November 7, 2016. In-person interview in Mashpee, MA.
54 DRG, April 19, 2003, Letter to Eric. Computer file: Dear Eric-Thnx Elephant ...
55 DRG, April 19, 2003, Letter to Eric. Computer file: Dear Eric-Thnx Elephant ... 56 DRG, April 3, 2003, BATSAPR03, unpublished document. Unless otherwise indicated, the preceding several paragraphs of descriptions of the difficulties during the fieldwork are taken from BATSAPR03.
57 Gregory Auger, personal communication, April 14, 2018. Telephone Interview. Unless otherwise indicated, the descriptions of tethering are derived on this conversation.
58 DRG, July 18, 2003, 18JUL03rebats, unpublished document. Griffin’s ideas about creating a “pseudo-insect” and using “experimental echoes” are quoted and adapted from this document.

**PSEUDO-INSECT:** This might be a physical copy with some mechanism to move the wings simulating flight. The simplest way might be a wing with an electro-magnet or other way by which an applied electrical signal would move the wing at appropriate frequencies IF bats classify insect echoes simply by frequency of echo modulation, this might be feasible. But if they respond selectively to variations in frequency and amplitude, as would occur from changes in body orientation, the simulating signal might have to be complex – and unknown at the start.

General issue with this approach: Bats appear to detect an insect with one or at most a very few search phase signals. With lucis these last only 2 or 3 msec. IF the bat classifies single or very small numbers of echoes by amplitude modulation of some sort, changes in echo amplitude must occur within 2 or 3 msec. small insect wingbeats can be several hundred Hz. 500 Hz means period of 2 msec, so the echo would result from one cycle of amplitude fluctuation, assuming the modulation was periodic – that is, without silent intervals. Thus, it is conceivable that the bat’s brain could distinguish between targets by amplitude modulation at approximately the reciprocal of pulse duration. Better perhaps to think in terms of periods of wingbeat AM. If these periods are much longer than the bat’s pulse duration, the amplitude change within one pulse or echo will be small. If the period of the echo AM is less than pulse duration there will be time for more than one cycle.

An alternative to a moving wing-like element might be a sphere that could be inflated and deflated following the waveform of an applied electric signal. The frequencies of wingbeats in small insects run up to several hundred Hz, so an oscillating airflow might be a challenge to arrange.

**CONTROLLED REBROADCASTS:** A basic problem is that the position of the bat when it emits the first search phase signal cannot be controlled. Calling bat-mike distance Dm and bat-speaker distance Dsp, the apparent range of the pseudo-echo is \((Dm + Dsp)/2\). The direction re bat will be the same for speaker and pseudo-echo. Assuming zero delay in the electronics, if \(Dm > Dsp\), Rec, the apparent range of the pseudo-echo target, will be. Dsp – so that the pseudo-target would appear to be beyond the playback speaker. This might not be too bad, except that it might complicate the bat’s processing if the pseudo-target was always directly behind a real physical echo-producing object. For this reason, it would probably be best to keep \(Dm < Dsp\), and that in turn would probably require Dsp to be 2 or 3 meters [?].

Suppose as a start one tried to keep Dm as small as possible, by having mike on pole and starting simulation only when Dm perhaps a meter or less. The speaker might be on another pole say 2 m from the mike. A further problem is that the mike would receive the signal broadcast in a different direction from that of the speaker. IF the signal varies with direction, that might produce an unnatural echo pattern.

What happens after the first pseudo-echo? The bat will have moved, so that a second pseudo echo will have a different Rec – probably resulting in rapid apparent motion of the pseudo-target. It’s difficult to see how to overcome this problem.


64 Con Slobodchikoff’s research with prairie dogs is discussed in Volume Three - Chapter 16, “Revolutions,” primarily in Part 3, section “Natural Animal Communication,” subsection “The Monkeys’ Alarm and Other Semantic Communication.”


67 Jack W. Bradbury, personal communication, October 28, 2021, E-mail.

68 Jack W. Bradbury, personal communication, October 9 and 28, 2021, E-mail.

69 Anil Kumar, 2003.

70 Anil Kumar, 2003, p. 1400.


72 D. R. Griffin and G. J. Auger, 2004. The title of the paper scheduled for presentation at the November 10-14, 2003 meetings of the Acoustical Society in Austin, Texas was “Variability of feeding buzzes in little brown bats (Myotis lucifugus).” Griffin had passed away on November 3 and Greg Auger does not believe he made the presentation instead. The citation was published in the 2004 JASA, hence the 2004 date.

73 D. R. Griffin and Alvin Novick, 1955.

74 DRG, 1958a.

75 Chen Chiu and Cynthia F. Moss, 2008.

76 Yovel Yossi et al, 2009.

77 DRG, July 12, 2003, LOWINTENSITY- WHISPER DRAF-July 12 2003, unpublished manuscript.


79 James A. Simmons et al, 2004. I believe, but am not certain, that this is the paper they were working on.

80 DRG, Jul 10, 2003, DM-S10JUL03. Letter to Dietland Muller-Schwarz.

81 James A. Simmons, personal communication, April 20, 2019. Telephone interview.

82 Cornelia Gerberl et al, 2015.

83 See particularly Volume 1 – Chapter 11, Part 2, “Echolocation Research by Griffin and Others” and Part 4, “The Present And Future For Bat Echolocation Research (And Devices).”

84 Peter Teglberg Madsen and Annemarie Sørløkke, 2013.

85 Peter Teglberg Madsen and Annemarie Sørløkke, 2014.

86 Cornelia Geberl et al, 2015.

87 Cornelia Geberl et al, 2015, p. 4122 has the quotation.

88 J. M. Ratcliffe et al, 2013. These authors express a similar view.


CHAPTER NINETEEN

A LIFE WELL-LOVED: A BROADER VIEW


"I wondered whether animals might not be doing a lot of things we never imagined they could do." (p. 2)

Overview

Views of Donald R. Griffin are expressed by the “Many Voices:” his colleagues, family and friends as well as those from the media, including memorials and obituaries after his death. Some are about Don as a person, others as a scientist and some concern the significance of his contributions. Numerous anecdotes from 40 interviews are included.

Don’s curiosity, his originality, his brilliance, his intellectual skepticism, his modesty ... these were often cited attributes. And to quote Roger Payne, his student and colleague, “Our affection and admiration for Don are quite immoderate.”
I have, through many words, attempted to create a sense of Donald R. Griffin, the scientist, the human being. I have tried to elucidate some of his thought processes and tried to understand what made him the extraordinarily creative person that he was. He studied things that “weren’t there:” the unknown sense of the bat; the unknown ability of birds to “home,” to migrate using some unknown cues; the unknown, but he believed knowable, conscious content of an animal mind.

Now to other views. Many, many of Don’s colleagues, friends and family, most generously spoke to me at length about Don Griffin. Happily, this seemed to be a positive experience; they recalled times with Don with good pleasure. I inquired about their own research and collaborations with Don as well as general social interactions. In this part, I will include many of their memories. You have encountered some of these recollections already sprinkled throughout the book, but I thought it good to collect some here as we finish this book together.

The Many Voices

Following are some of their thoughts and memories as well as those given to Katy Payne for a potential obituary (unpublished) that she shared with me;

1. CR: I asked my interviewees, “What sort of man was Don Griffin? Are there a few adjectives that might describe him?” Many responded, but that question did rankle at least some of the “Many Voices.”

Many Voices: “Don is not describable by a few adjectives,” Jim Simmons replied with some irritation and exasperation at my simplistic question. He continued: Don was both “the immovable object and the irresistible force.” I shall let the reader, by now, fairly well acquainted with Don Griffin, muse about Jim’s meaning. I surmise that, in Jim’s view, Don could be immovable concerning the veracity of his firmly held beliefs such as the consciousness of animals, or even about a technical matter, while Don’s unique intellect and enthusiasm were “the irresistible attractive force.”

One particular adjective was applied by almost everyone in answer to one or another of my questions: He was curious, “innately curious.” “He wanted to know how everything worked.” “Curious about nature, curious as a scientific interest.” More about Griffin’s curiosity soon.

“He loved intellectual endeavors.”

“Reserved, very reserved.” “A private person; I knew him through a discrete window, nothing about the rest of his life,” said Greg Auger. “I knew him only through working with him with bats at Trout Pond [Cape Cod, Massachusetts].” Greg and his wife Wendy Williams did, however, occasionally dine with Don at local restaurants and in their home. Others noted how they did not speak personally with Don, yet, as a student, one felt a mutual sense of respect. Carl Hopkins recalled, how, as a graduate student, Don Griffin was “a little bit intimidating, a bit terrifying. … That was my inexperience. He was so helpful and encouraging to me, it was exciting to me that I had discoveries [about electric fish communication] that Dr. Griffin was particularly excited about.”

That title of “Doctor” Griffin was used by many of those who knew him first as a student or postdoc. For a very long time, even at Rockefeller, Ron Larkin recalls Griffin answering the phone as “This is Dr. Griffin.” In contrast, RU Professor Theodosius Dobzhansky, the famous geneticist, insisted that everyone, faculty, students, staff, address him as “Dobie.” Like Griffin, most RU faculty were far more formal than “Dobie.” Over time, Don Griffin became less formal, even changing his
phone greeting. When Larkin was a Research Associate, then later a junior faculty member with Griffin, Don, at one point, asked Ron to call him "Don." Ron felt most awkward doing so, particularly since others in the lab at that time were mostly grad students addressing Griffin as "Dr. Griffin." For a considerable while, Ron avoided the matter as much as possible and used "Dr. Griffin" as necessary. While away, Griffin's letters to Ron and the lab members were typically signed "DRG." Many, many of the "Many Voices" attest to using "Doctor Griffin" even today, when referring to him. This includes the noted neurologist Dr. Ann Graybiel, who continued to interact with Don over the years, occasionally having dinner together with Don and Jocelyn during his "retirement" at Harvard's Concord Field Station. Tim and Janet Williams who'd shared many long hours with Don in the field in Trinidad likewise felt uncomfortable calling him "Don," as Griffin had suggested. Yet, after so much informality, "Dr. Griffin" seemed uncomfortable too. They settled on DRG, pronounced "Derg," which Don graciously accepted.3

Yet others described Don Griffin quite differently. Katy Payne described him as "a very sociable fellow. ... He loved companionship, and talking and speculating and worrying about how to proceed [in the field of Cognitive Ethology]." Don had many visitors while at RU and the Concord Field Station; the visitors were interested in the ongoing science and conversations with Griffin about his related ideas. That’s not to say many were not also friends; they were, the bond initially established through shared scientific interests. Don also wanted to confer with those who disagreed with him, hoping to persuade them to his views.

His sociality was most genuine. Katy Payne’s daughter Holly recalls a time Don visited during one of his trips to Cornell (early 2000s). Holly’s one-and-a-half-year-old toddler ran to greet him as he entered. Suddenly, Don was “on the floor on his belly to be at Sophie’s level for a real greeting.”4

And yet, a good friend also described him as "the cat that walked alone."5 “He was a modest man,” this a comment by many. “He was humble about his accomplishments.” “Not pretentious.” Said Jack Bradbury, “I never saw him swagger.”

“He was not pushy,” Jack continued, adding, “He did not interfere, and was more sensitive politically than one might think. He didn’t threaten, but was quite effective.”

“He was a genuinely kind person.” “He was sympathetic, caring for his graduate students.” “I’ve never heard him say unkind things about other people.” He was a "lovely mixture of fierceness and gentleness."6 “Don did not belittle people, but he was really stimulated when others knew enough to bounce ideas about with him.” “He was generous and helped others do their work.” And he was truly interested in what you had to say.

“You must talk about his enthusiasm.” “… his infectious enthusiasm.” “He loved what he was doing.” Brock Fenton recalls an occasion at the Millbrook field station. Don was in the radar shack, working with the apparatus (the “Witch”) used to study migrating birds. Some RU grad students were visiting, a part of their RU educational program to explore the ongoing research work of many labs. In Fenton’s words, they were a "pretty tense" lot. One student remarked, somewhat astonished, “Gee, you look like you’re having fun!”

“How inspirational he was, loving animals, getting things done. That lasts a whole career when you see someone that engaged.” Those remarks were by Ron Larkin, whose former research
endeavors had been lab work conducted with rats. As we know, inspired by Griffin, Ron turned to studies of avian and insect navigation/migration.

Griffin’s enthusiasm, coupled with his boundless curiosity, was also potentially deadly: recall his leaping out of a jeep in Kenya to get a better look at a lioness’s kill. He was safely restrained by Dorothy Cheney and Robert Seyfarth, reminding him that lions can devour humans too.

“Don’t forget the “twinkle in his eye; that was pretty important.” Such was Brock Fenton’s advice to me. He had such a dry sense of humor,” remarked some. In later years, such humor led to his bearing the many criticisms of his work, whether it be echolocation or bird migration, but particularly Cognitive Ethology. Jack Bradbury remarked that “Don was never arrogant, but he was incapable of intimidation. He expected his students to be equally fearless.”

He developed acronyms and short phrases that were both a convenient shorthand for relevant scientific terms, but also often expressed his opinion of the critiques offered. Among them were GOP or the “Groans of Pain” interpretations of animal communication. “Paralytic Perfectionism” seemed to be required by those who demanded precise definitions of concepts like “awareness” and the kinds of evidence they deemed necessary.

“He liked to tease,” some recalled. One sort, likewise demonstrating Griffin’s excellent linguistic abilities, occurred during a conference, most likely the 1979 NATO conference on the Isle of Jersey. Don readily consumed journal articles written in German, oversaw the translation of some of his writings into German, and even wrote some in German. But this time it was French. As Julia Chase recalls the event, Don was giving the opening address.

[He] made reference to the fact that everyone was being expected to use English. A bit parochial of us? he mused. So, he proceeded to give his opening address in French much to everyone’s delight.

Another sort of tease: Recall Don as a young boy delightedly pinning down, with a thumbtack, the hair of a girl sitting at the desk in front of him. And recall his complimenting a girl, a young teenager like himself, on her red hair, “just like the fur of a vole [a red-backed vole].” Don claims that his remark was intended as a sincere comment. He “pushed the envelope” of social ways in somewhat later years, too. Despite his awkward youthful behaviors, he did manage to have female acquaintances and girlfriends. In his more mature years, women found him “gracious” and much the “gentleman” of an earlier era.

Griffin encountered the lack of expertise that seemed common in a subset of associates with a certain sense of bemusement. The deficiencies pertained to tool use and “electronics” and associated nomenclature. The subset was typically young women, both students and postdocs. With a wry smile, he would patiently and gently teach the acolyte, perhaps while murmuring a comment about “tool use genes not on the X chromosome.”

Overall, most “Many Voices” agreed, “a great sense of humor.”

And a great storyteller! Such was especially the comment of those who spent long hours in the field with him or lingered over dinner together. Timothy Williams recalls the tales Don told him and Janet, Tim’s wife and fellow researcher, as they all spent hours bumping along the Trinidadian roads, searching for bats. A good story always helped.
2. CR: I asked my interviewees, “What motivated Don Griffin? What were his goals?”
   Many Voices: Curiosity! That was the overwhelming response I received. “A wide-ranging intellectual curiosity.”
   “. . . I think he was a curious person and wanted to understand things.”
   “Donald Griffin was one of the most incredibly alive persons I ever knew . . . his curiosity and thirst for knowledge, his wide-ranging interests and grasp of facts and concepts were phenomenal.” Those, the thoughts of Irene Pepperberg.
   “Though Don had the combined wisdom of a brilliant scientist, a philosopher and a man of the world who had seen and experienced much, when it came to his curiosity of nature, he was a child at heart . . .”, the words of Hope Ryden.
   Griffin’s fascination with animal minds was another source of motivating interest. Beverly Greenspan mused that he wanted to perceive the world the way other species did.
   “Don does not love dogs. He wanted to understand how they work... he was so profoundly kind and interested in how exciting and complex their world might be. He walked past dogs; I sit on the floor and pet them.”
   He wasn’t driven by fame; he was curious. “He tolerated his fame and status,” thought Timothy Williams. Don disliked the hierarchical scene at Harvard; the RU scene with no hierarchies suited him far better. [CR: I would disagree about the lack of hierarchy at RU, though, perhaps it was much reduced as compared to Harvard.]
   In Don’s family background, however, there was certainly a sense of the patrician, of the elite. Recall a grandfather trying to maintain the “style” even as family fortunes had plummeted. The sailing, bird watching, even living in Barnstable or just summering on “the Cape” was also the lot of the privileged. Don’s father had his annual new Buick as long as that was the least bit affordable, though Don was largely content with an old, practical station wagon. (Retired in Barnstable, he did have another modest second vehicle for “going out.”) Don attended an elite boarding school and the family (the men) went on to Harvard. Jocelyn Crane’s mother was a socialite, and also very supportive of Jocelyn’s desire for a career in science. Most attractive to Don was Jocelyn’s superb success in her own right as a marine scientist. In those days, that was much more difficult to achieve as a woman than today. His first wife had been a Radcliffe graduate student when they met.
   Don moved among the intellectual elite, because of his own intellect and interests. To an extent, he also moved sometimes among the very wealthy. They were often attracted by his sense of adventure and the fascinating research he conducted; he charmed them (and many others) with his fine talent for storytelling. He managed to help persuade several to donate land and large sums when establishing the RU Millbrook field station and, at various times, to support his own research. One respondent described Don as “always living at the edge of elite society.” Although the “Many Voices” did not believe Griffin was motivated by fame, one family member who considered the matter thought Don’s position and status were, indeed, most actively sought.
   Similar to the other “Many Voices” was Peter Tyack’s view, “I don’t think he was driven by ego, but by scientific interest.” Don was not one to “rest on his laurels,” reflected Brock Fenton. “He was always asking what to do next. And he had a very, very broad field of view. Sometimes, such people can be intimidating,” Brock continued.
“He wasn’t like Watson, who sought fame,” reflected Jack Bradbury. “Don wanted to add the bricks needed for building the science. And he simply loved puzzles. He’s my kind of scientist; we just loved the science.” There could be irritating aspects to Don’s curiosity. Jack recalled how, while a grad student with Griffin, he would carefully prepare before Don visited Jack’s apparatus. Jack prudently drew a precise diagram of the connectors that ran his automated equipment, for Don delighted in seeing how things worked. That curiosity might involve his taking apart and “replacing” said connectors, but ... unfortunately, not always back to the same place.19

“He was genuinely interested in science and its questions, and likewise dissatisfied with academic politics. He wanted to and did simply go off into the field, get filthy, and find something interesting,” recalls Timothy Williams.

“He wasn’t the kind of guy to write to ‘piss off’ colleagues or to be ‘profound’ or inflammatory,” remarked Marc Bekoff. He was deeply interested in what he was doing and wrote about his thoughts and his work and observations. “I’m also doing work in cognitive ethology, but I’m more radical and focused on promoting animal welfare.”

Several persons said they did not really know his motivations. Jim Simmons simply said he had “no clue” as to Don’s motivations. Don was just fascinated by finding out about stuff.”20

3. **CR: “What were his attributes as a scientist?”** “Many of you have stressed his curiosity. What else would you suggest?”

**Don Griffin, “musing,” October 1999** (by Gregory Auger)

**Many voices:** “He was very intelligent.” “He was brilliant,” “a giant.”
He had "great originality and independence of thought, recognizing interesting problems that others would have ignored. He was innovative, very interested in the capacities of animals." Those were the thoughts of the long-time student, colleague and friend, Alan Grinnell.

Don Griffin was "creative and imaginative; he pushed the envelope." He was a great observer of nature, a great experimenter, and an excellent writer...”, the thoughts of Peter Tyack and expressed by others as well.

“He saw so many sides of a question.” “His thinking was not linear.” “He was open to so many possibilities, so many ideas.”

Ron Larkin, Griffin’s collaborator on the bird migration radar studies, noted how “Don knew more about the physics and technology than did others in the field. He was very insightful and found the truth in something that others might not find. He came down hard on getting good data. And he certainly knew how to put an argument together. When he had seen something and was convinced that he was right, he was adamant. He was very persuasive.” Ron recalled an afternoon with Don in his RU office: Don, leaning back in his office chair and musing, “You know, I coulda been a lawyer.”

Uli Schnitzler recalls how Don didn’t like too much speculation; he wanted to see the data. Once, as Uli was ruminating about some possibilities, Griffin told him a favorite tale about someone observing white sheep in a meadow: “white on that side,” was Don’s admonition. Others too heard that warning from Don when they exceeded Don’s decidedly cautious approach to the adequacy of data.

Yet, we also recall the quote from Don’s 1958 book on bat echorolocation, Listening in the Dark: "Excessive caution can sometimes lead one as far astray as rash enthusiasm.” In that case, he was referring to the “over conservative conclusion” made by Pierce and himself ... “that flying bats only occasionally emitted their inaudible sounds...” That was due, in fact, to the highly directional nature of both the bats’ vocalizing stream and the parabolic horn with a microphone.

Throughout his scientific life, Don entertained what were, to many others, fanciful ideas, be they about echorolocation, avian migration or animal mind. But Griffin immediately strove to determine means to gather evidence for his “farfetched” ideas, perhaps plans for necessary field observations and/or experimental designs. Recall that he chose to title one of his memoir essays, “Reflections of an Experimental Naturalist.”

Schnitzer also remarked that Don knew what he knew and what he didn’t. He was very critical, most especially about his own ideas and findings. He imagined all possible objections and counterarguments. Others note he was “very critical of fuzzy thinking.”

Brock Fenton summarized Don’s review of a paper he’d asked Griffin to look over: “True to form, he was critical, engaging, challenging and supportive.”

Eric Strauss had an interesting slant. He had worked on the “Sugar Cube” bees with Don, Greg, Peter Auger and a slew of Peter’s high school students. In his view, Don Griffin was “eager to work with people not on a critical track for publication. In that way, they could explore ideas that might likely not be fruitful. Such was the situation with the high schoolers, who thoroughly enjoyed the camaraderie, the outings, being part of “real” science, and learning technical matters, experimental design, innovation, and persistence, all fortes of Griffin. Several students did go on to earn Ph.D.s in various biological areas.
In the general field of science, "he knew everyone," according to a couple of the "Many Voices." But those scientists he knew were particularly in biology and ethology, not very many psychologists. Don was most attracted to a comparative approach to bat echolocation and to animal thinking and consciousness as well. Such concerns derived from his interest in the adaptive value of a trait, its relation to ecology and its evolutionary origins. As Jack Bradbury notes, “Don was concerned with finding general principles, why things are as they are. In Jack’s view, scientists were generally focused on one of three questions: ‘what, how, and why.’ “The ‘what’ scientists loved to classify things; they might be taxonomists. Asking ‘how’ was answered by mechanisms, very reductionistic. The ‘why’ scientist is asking why the animal does it the way it does. That is often answered by comparative studies, looking at other species that do it differently. What is causing the difference?

Later, Griffin grew more and more cognizant of areas in psychology most relevant to cognition and consciousness. With Gayle Speck, they reviewed and analyzed the experimental and comparative psychology literature, re-interpreting many of the findings in less behavioristic terms. Conferring with Katy Payne about her fieldwork on elephant social behavior, he considered observational and experimental techniques from various fields that might help reveal an animal’s state of mind. For such work, others’ research on communication and psychology was especially relevant.

Many attested to the ease, indeed the pleasure, of working with Don in the field. But as Brock Fenton specifically noted, “Don did not suffer fools gladly. ... He could be testy.”

And lastly, a view via a daughter, when she was a young woman, almost college age. A one-time suitor of the young Nancy Griffin knew Don initially through her. “She was not so interested in science but was quite a magical person. She thought her father was ‘interesting and inexplicable.’ So, I figured Don must be doing interesting science.”

In Don’s view, as he reflected on his life, he expressed a certain amount of surprise at having been "paid to have fun.”

4. CR: “What of Don Griffin as a Teacher, as a Mentor?”

One of the “Many Voices” did not consider Don a model for teaching, except by example: how he worked and his enthusiasm. Griffin didn’t, for example, teach the RU grad students, budding academic professors, how to make a formal scientific presentation. He didn’t give guidance in writing a journal paper. Everyone agreed, however, that his critiques of one’s papers were sought and invaluable. Griffin’s own writing, as has been noted, was excellent. His dictated first drafts were typically more than equal to the state of our writing achieved after many drafts and revisions. Jim Gould, in his obituary, wrote of Don’s “hard-nosed papers” and “elegantly written books.”

Most persons specifically said he was a great teacher or “a wonderful” teacher. Fernando Nottebohm recalled that Don had many volunteers for his bird navigation projects at the RUCFR field station; some were RU students, some even Millbrook neighbors.

[They were] thrilled to be allowed to see how science was done. ... Don loved this opportunity to explain in simple terms phenomena that to the layperson were mysterious or just impossibly difficult. Don was a born teacher and he did it with warmth, humor and enormous satisfaction.

Don often exhibited a flair for the dramatic in his teaching, using live bats, for example, to demonstrate their echolocating. Francois Vuillemeier had been a TA for Don’s undergraduate
Animal Behavior course when he was a Harvard graduate student. He describes an engaging structural component of Griffin's approach to teaching. "He was a very good lecturer [who] developed his themes in a detective story-like manner. With his rather soft-voiced delivery, his auditors remained very quiet, expectant, simply waiting to see what would turn up next."27

Gayle Speck recalls an after-hours lecture to Harvard undergraduates. Who is going to be interested in the speech of this old man, she wondered. Within five minutes, they were rapt. Afterward, many came up to talk to him.

As a mentor, he was inspiring, said many. Jack Bradbury reminisced,

At my first Ethology Congress in Sweden, Griffin repeatedly dragged me over to be introduced to an exalted senior member of our field and then promptly left me there trying to find something intelligent to say to this person. He always expected you to do your homework and read the current literature. Therefore, he assumed you would have something to say. So we did it. He also demanded clear and clean writing and every thesis draft was given the same rigorous scrutiny by Don. When we finally graduated, we felt we had earned it by running the most exacting of gauntlets. And we loved him for it. He was a mentor of mentors in every respect, and I hope that I and my students have been able to keep that tradition alive.28

For Peter Tyack, Don has also served as a mentor and model for his own interactions with students. Don Griffin was “incredibly supportive of what I wanted to do, emotionally supportive, but intellectually very critical.” Alan Grinnell specifically mentioned Griffin's being “very supportive” of him, as did numerous others. Uli Schnitzler, coming as a postdoc to Don in the '60s, remarked how open and receptive Don was to Uli’s work. “Do what you would like to do,” was Griffin’s directive.

Irene Pepperberg had switched fields from her Ph.D. in theoretical chemistry to work on cognition with parrots. She said Don Griffin was the closest she had as a mentor in that field.

Recall Brock Fenton’s remark about Don, “True to form, he was critical, engaging, challenging and supportive.”

5. CR: I also asked if Don Griffin had been an influence on each person’s life, and if so, how?

Many related responses have appeared in the prior “Mentoring” question. Jack Bradbury notes Griffin as "an incredible model" for how to be a scientist and do science. Carl Hopkins, though electing not to continue as Griffin’s RU grad student, nevertheless considered him “an important person in my life.” For Eric Strauss, it was Griffin’s commitment to precision in science that was a significant part of Don’s “wonderful inspiration” for him.

Griffin, while supportive, was likewise sensitive to the realities and practicalities of everyday life. In Eric Strauss’s case, Eric was good at “getting gigs in urban ecology and grants,” though also very much interested in animal consciousness. “Approach that very carefully,” warned Don [CR: This was in the late 1980s.] “Go in the direction you want to be pulled. Think about tenure.” Eric recalled that Griffin had talked him out of taking a job at Harvard because it was such a competitive atmosphere.

Several had heard Griffin speak early in their academic careers, often with compelling videos or even live bats incorporated into his talk. They were sufficiently inspired by him and his topic to
persuade them to want to work in that field. Those talks were most often accounts of bats’ echolocation or animal communication and cognition/awareness.

Irene Pepperberg first met Don Griffin, with some trepidation, at an animal cognition conference. She recalled how gracious he had been to her, and how attentive and interested in her work. He invited her to be his guest at an evening reception, introducing her to all the scientists there. He later invited her to give a formal presentation at the Manhattan campus of RU. Since none of the field station scientists and students had traveled down to the city for the talk, she also presented informally at a lunch seminar at Millbrook. But … a hostile reception to her work from Peter Marler. When Don was President of the Harry Frank Guggenheim Foundation, she, among other animal cognition researchers, received a research grant from the institution. That “saved me completely,” she reported. As earlier mentioned, after the NYAS “Clever Hans” conference, funding for many “related” projects simply dried up for a while.

Others who were trying to deal with “suspect” topics also found support from Don’s published ideas. Marc Bekoff had organized a symposium at the American Zoological Society, at a time before QAA had been published. “Play” was the topic and the approaches varied from taxonomy to physiology, while Marc himself dared to speak about the emotional side, an animal’s enjoyment of play. Not very acceptable.

Gordon Burghardt considered that Don’s books and talks lent support to him to talk about the cognitive aspects of biology. Burghardt invented the term “critical anthropomorphism” to mediate between those who sought only reductionistic explanations of animal behavior and those who were trying to investigate broader mental processes. Burghardt later began studies of play behavior.

In short, Don’s influence on this group of persons associated with him was very personally and intellectually supportive, and, literally, “inspirational.”

6. CR: I queried the “Many Voices” as to their view of Griffin’s major contributions. Some demurred, saying he had made so many contributions; he was interested in and worked in so many fields. Most, however, answered “bat echolocation,” the revelation of a totally new sensory modality. Brock Fenton described echolocation as the “Magic Well,” a term used by Von Frisch about the marvels of the honey bee and her waggle dance and, likewise, the title of a paper by Griffin about bat echolocation. Every time one peers into that well, one finds something else. Brock added that “Whenever you think you’ve discovered something new, you find that Griffin’s already written about it.” [I (CR) might add that various persons have said this, particularly referring to his 1958 book, Listening in the Dark.] “Don urged people to look at animals, not in a cage,” Brock continued and then exclaimed, “Griffin was just a giant!” For Alan Grinnell, Griffin’s foremost accomplishment occurred when he was at Cornell University, discovering “the ability of bats to catch insects on the wing.”

A few people noted his work on bird navigation and migration, especially the radar studies. But the next most common response after bat sonar was Don’s work in animal consciousness. In Jack Bradbury’s words, in addition to bat sonar, it was Don’s “relentless pushing of animal consciousness.” But Jack didn’t like my question asking of the “most” significant accomplishments. Those who stressed Don’s significance in the area of animal sentience emphasized how he, using Marc Berkoff’s words, “opened the door for serious discussion of
consciousness and sentience ... and of the evolution of consciousness.” Don’s emphasis on the continuity of consciousness was noted by several. “He had such foresight.” Added Bekoff, “I can’t believe there’s anyone who does not see how his foresight [opened the way] and who doesn’t understand the importance of what he was doing when he wrote the three books [and their significantly expanded editions].”

“It’s hard to decide his major contribution,” mused Eric Strauss. “I think all his work leads up to the legitimacy and justification to ask what do animals think?”

7. CR: What, said I, to the “Many Voices,” do you think was Don Griffin’s view of his own accomplishments?

Generally, he was proud of the precision he had brought to the descriptive science of behavior, thought Eric Strauss. [I (CR) think, in addition to his scientific achievements, he was proud of his incredible persistence and took both pride and delight in his ingenuity in designing instrumentation when none existed or wasn’t affordable.]

“He was proud of his research on bats. And he considered animal cognition as a phenomenon that should be investigated. He saw himself as a trailblazer in that field,” reflected Timothy Williams. He was proud that he could take on “sacred cows,” mused Gordon Burghardt.

Grinnell remembered Don writing to him that he was most proud of the recordings and insight of that summer of 1950. Bringing a “ton” of precious recording equipment to the field, Don was finally able to demonstrate that bats, while flying, were able to detect, locate and catch flying insects by echolocating, all this in their natural settings. As we recall, final proof took a decade, achieved through collaborating with Fred Webster, using super high-speed photography. Alan Grinnell agrees with this assessment of Don’s achievements. That letter to Grinnell was apparently written before Don’s public statements about animal cognition and awareness and before the publication of QAA.

Don was proud of the students he had helped foster ... a comment voiced by several. Unlike many an academic, rather than competition, “Don took real pleasure in the successes of his students following through and creating new paths into research.” Katy Payne noted that “he had established the field,” [she was referring to animal consciousness] ”and had ceded it.” [CR: That statement, I think, applies equally well to the field of bat echolocation.]

Many made comments that Don was “pushing” animal consciousness so hard. Once, he commented to Irene Pepperberg, that maybe then “we can land in the middle and scientists will discuss” rather than spew vitriol.

Katy Payne reflected, with a mix of sadness and pride, “The field is burgeoning now; he would be so pleased.” And Gaye Speck mused, “I think he knew the value of his own accomplishments. I think he knew he had made minds change.”

8. CR: And then, I pushed my patient interviewees yet more, “What of Don’s frustrations and regrets? Do you have any sense of those?

Alan Grinnell, who knew Griffin since Alan’s Harvard undergraduate days working in Don’s lab, remembered that “Don was never quite satisfied with whatever data he could get. Not that he was a perfectionist who wouldn’t publish, but he was always ready to say there could be a caveat he hadn’t considered.”
Greg Auger, who had worked beside Don many nights over several years, pursuing bat studies at Trout Pond, Cape Cod, said Don didn’t show much frustration. He was always the “consummate leader,” keeping others motivated. Don was fascinated by constantly improving what they were doing. He was always constructing in his basement shop.

Others too mentioned his not exhibiting frustration; a few even suggested that he was not frustrated by anything. We do know his patience could be tried, as when photographers visiting the scientists at Simla, Trinidad, were eager to get their “story” and, in Griffin’s view, interfering with the group’s research. Some recall his quietly humming to himself while being “tried,” a clue to modify one’s behavior. Most of that irritation was directed at the limitations of his available equipment, their not being sensitive enough or their constrained directionality, or lighting problems and such. Alan Grinnell mentions the same exasperation, faced by all, as they suffered from the limits of technology to solve the mysteries they were pursuing. But, noted Alan, Don was “very good at jury-rigging equipment” and eager to learn techniques from others. And Jim Simmons, recalling Griffin’s long history of research, mentions Don’s “frustration at getting stuff done” ... i.e., at the pace and precision he wished. Jim noted how heavy the equipment was and difficult to work with. For most of Don’s career, he added, “it was the era of vacuum tubes,” easily broken, and problematic.

Studying bats at the end of his years, Griffin was aware of the superb technology that existed and was utilized by others in the field. As he was making do with his self-engineered gear and modified Radio Shack apparatus, he did sometimes despondently refer to himself as “an unfunded has-been.” His only academic funds at the time were a small “Emeritus” stipend he received from Rockefeller University and extremely modest financial support from Harvard. His Harvard Museum/Fieldstation “Research Associate” position was unpaid.

Others mentioned his aggravation with academic bureaucracy and with the institutional push for publicity which had always interfered with his ongoing research. We’ve seen some amusing, though frustrating incidents reported in other chapters. As noted, the academic political scene, particularly during his time at Harvard, was exasperating to him.

There were personal regrets, as known to a very few. His intense involvement in science had a cost. It is a cost borne by the families of many devoted scientists and other professionals, in almost any field. It is almost impossible to engage to the extent of one’s scientific wishes and the demands of the academic world and still participate fully in a family. Don’s first wife was not a scientist, so they did not work together on projects as some fortunate few academic couples have managed. They did not go off to live in the field together, as again, some very, very few academic families have. As revealed on a few occasions throughout this book, Don would often not be present at home when the four children were young. He did try to take them, indeed, used their help, on some local batting expeditions. He did not greet and inspect the young man coming to the house to woo his teenage daughter. One interviewee simply said, “he neglected his family for science.”

He regretted the hurt to his family caused by his marriage to Jocelyn, though he did not regret marrying her. He had helped his parents financially, but he regretted not having helped them even more. He remembered that they had used their savings to provide his tuition both at boarding school and as a Harvard undergraduate.

And at the very end, though many came to visit him during his final illness, it was not his
fellow scientists, but his family, specifically, a daughter, who took care of him. That, too, is the usual state of things. But it is also the case that Don, being a most private person, kept his condition a secret from almost everyone. Not until near the very, very end of his days were friends made aware.

As to his work in Cognitive Ethology, he continued to be frustrated that he could not persuade most other scientists to come around to those ideas. He did, however, have strong supporters. "He was very frustrated that the study of animal cognition was not mainstreamed as it is now." And of the various species to investigate, he was frustrated that he could not persuade anyone to do focused studies about beavers’ cognitive skills. A few students/colleagues joined him in such work for a while; Charlie Walcott was among them. We know that his own efforts during his days at the Concord Field Station, were a source of vexation for he was not obtaining the data he sought.

And, of course, "he would have loved to live long enough to see the pendulum swing as it has.” Yes, in my (CR) mind, it has swung, and there are many engaged in the enterprise of animal consciousness and cognition, but there is still much reluctance to deal with non-human mental states. Non-mentalistic interpretations of behavior still have strong adherents. After all Griffin’s struggles and all the hostility he withstood, all the criticisms, he is most frequently not even quoted in journal articles about research enabled by him and his ideas. Don Griffin’s tenacity in developing and promoting the field of Cognitive Ethology was instrumental in permitting scientists to research and write about animal cognition and sentience. As Gordon Burghardt noted, Don was instrumental in establishing the journal, Animal Cognition, and wrote the lead article in that journal in 1998. Yet he was not even mentioned in the 25th year celebration of that journal.

Throughout, Griffin bore the critics and their criticisms admirably and countered them with astute intelligence. "They didn’t seem to deter him.” He had established himself so very well as a highly reputable scientist that he could continue his efforts, even in this “less legitimate” field. Peter Tyack noted that he spoke of the criticisms with a sense of humor, but Peter thought they probably aggrieved him more than he showed.


Many have been sprinkled throughout the book, but some didn’t readily fit into the ongoing narrative.

**TV Personalities (The Bats)**

Charlie Walcott recalled an interview he conducted with Don Griffin on “Exploring Nature,” the public television show that he moderated. Don, then a faculty member at Harvard (1955), arrived for the live program with oscilloscopes, microphones, a barrier for his bats to fly through, and, of course, bats. Viewers saw and heard the bats fly through spaces in the barrier, using ultrasonic pulses, scaled down to audible frequencies. Great show! But ... an important detail was forgotten: It took time to re-capture the bats, now all flying about the studio. And suddenly the news program began, with bats swooping and diving around Louis Lyons, the newscaster. The news continued; the bats continued; and, at the program’s end, Don recaptured the bats and all returned to Harvard. As Charlie Walcott states, “Some old-timers at the station still remember the incident.” And he adds, “Don and I became very close friends.”
The Ghost
It happened after dusk in an Italian cemetery, on the bat-trip to Pisa, Italy with Jim Simmons and Jocelyn. Griffin had heard that bats tended to fly between a church belfry and the nearby tombs. So, he spent some evenings stalking about among the tombstones. One evening, his perambulations happened to coincide with the conclusion of a church service. An elderly lady, dressed elegantly in flowing scarves and a black coat, came down the church steps and peered toward the graveyard. Horrors! She crossed herself urgently and fled. Don realized she thought she’d seen a ghost.

10. CR: Evaluating Griffin’s Contributions to Cognitive Ethology
I asked, How do others engage with Don’s work in Cognitive Ethology? How do you think they evaluate his contributions in that general area of inquiry?

From the philosopher, Irina Mikhalevich, “The philosophy of animal cognition and consciousness owes a great deal to Donald Griffin’s work.” She also notes, “These days, philosophers of animal cognition regularly collaborate with scientists from a wide range of the animal cognition and behavior world. That, too, might be part of the legacy.”

“Griffin was a significant, influential scholar at a critical time, posited Arthur Reber, himself a cognitive psychologist. [Don Griffin was instrumental] “... during those decades when Behaviorism was (finally!) in decline and a more expansive and expressive cluster of biosocial sciences with cognition and mind in their crosshairs began to assert themselves.”

And Frans de Waal also spoke of Don Griffin. De Waal is the noted primatologist and author of several very popular books that reveal the surprising abilities of several species, particularly those of our close cousins, the chimpanzee. In a letter to me, he wrote, “I featured Don several times in my latest book to make clear how important he has been for the field of animal cognition.”

Significantly, Griffin and his work served as an impetus to enter his fields of study. Most particularly, that was bat echolocation, the field he created and fostered over many decades. Animal Cognition and Awareness was another daring enterprise, still meeting resistance in some quarters, while also inspiring many younger scientists to explore the possibilities. Older scientists, many, were inspired to modify their attitudes toward animal cognition and sentience.

Voices from the Media
When Don Griffin passed away, the media responded, in both newspapers and scientific journals. All cited his historic work with bat echolocation. Well before Don’s demise, a book had been published of papers from a 1979 conference on the Isle of Jersey. It was a highly significant meeting entitled “Animal Sonar Systems,” the first of a continuing series. The book’s Preface summarized well the consensus of the media and the conference attendees.

... [we] dedicate this book to Donald R. Griffin. Our colleagues unanimously rendered homage to the spiritual father, the inventor of echolocation.

And a volume was published of the papers from one of the last conferences that Griffin attended and gave a presentation, the April 2002 meeting of Bat Conservation International in Austin, Texas. The volume was dedicated to him, including these words from the Dedication authored by M. Brock Fenton,
Once again, Don took us to the magic well of echolocation. It is his well, but he always shared it – and he will ever be with those who go there.\textsuperscript{52}

And as we had seen, throughout this book, he was not just the originator, but the inspiration for many with him and after him to pursue research in this field, broadly re-imagined to include the many echolocating species. (Not to forget the enthusiastic contributions of Robert Galambos as these young students first explored bats’ “supersonic” abilities).

His creation of the field of Cognitive Ethology was also always noted, and its multi-faceted impacts, particularly on the science of animal behavior, and in promoting a broader acceptance of the possibility of animal consciousness.

“Don Griffin was a scientists’ scientist,” asserted Don Dewsbury, himself an animal behavior researcher who importantly also serves as a historian for that field. He expressed how Don had risked his reputation as “a rigorous researcher in animal behavior” by his suggestions of animal awareness and thinking as “amenable to scientific study.” But despite continued skepticism, Dewsbury considered that Griffin had successfully moved the animal cognition studies “in his direction.” Even the skeptics “showed great respect for this remarkable scientist.”\textsuperscript{53}

Others remarked that Griffin knew the risks he faced, but thought the inquiry was legitimate. He had an international reputation for his bat studies, and was a member of the National Academy of Sciences, among other achievements and accolades. Said Fernando Nottebohm in the LA Times, “He didn’t have to apologize.” "He was one of the great American scientists of the 20th century. ... He had a bold mind."\textsuperscript{54}

Said Jim Gould, in his eloquent obituary, the view that animals, too, are part of the evolutionary mental continuum "has moved from the lunatic fringe to what may be described as the default assumption: the burden of proof has shifted."\textsuperscript{55}

The philosopher and animal rights activist, Peter Singer opined, “The consequences have been significant, not only for our understanding of animals, but also in creating, among scientists themselves, a different atmosphere for the discussion of how animals should be treated – in experiments, in farming, and everywhere.” In zoos, animals’ lives are enriched with problem-solving and toys, pulling many creatures out of apparently psychotic behavior patterns.\textsuperscript{56}

In the New York Times, the ethologist, Marian Stamp Dawkins, stated simply, “He started a revolution in the way we see animals.” He said the study of animal mind, of animal consciousness, is "a legitimate question.”\textsuperscript{57}

A special issue of the journal, Consciousness and Cognition had as its theme the “The Neurobiology of Animal Consciousness.” It was “dedicated to the memory of Donald R. Griffin (1915-2003), whose thoughtfulness, courage, and integrity were essential to reawakening scientific interest in animal awareness.”\textsuperscript{58}

**The Future Lies Before Us**

With the discussion of each field of inquiry that Griffin pursued, I’ve related some suggestions for the work that might lie ahead. I’ll now remind the reader of a few. Grinnell offered an excellent description of many problems awaiting in bat echolocation research, that, by the time you read this, may or may not be partially solved.\textsuperscript{59} David Pye focuses on bats’ and other species’ “apparatus” for producing calls, e.g. the “leaf” of leaf-nosed bats, and the apparatus for detection. He sees much work to be done in that area, especially concerning the potential information in the bats’
echolocation. Many, particularly Uli Schnitzler, highlight the continuing need for comparative studies, noting how one simply keeps finding new things as one considers a different species. Simmons emphasizes determining the subset of the available information in an echo that the bat actually uses and the function of that information; i.e., what the bat does with it. Neurophysiology enters the picture, with the continuing work of Suga and others to determine the neural mechanisms that lie between the bats' signaling and what it is doing. The mysteries of bats' swarming continue to be unraveled yet still confound. Griffin's "Magic Well" of echolocation continues to astound. The field he began and ceded continues.

The realm of avian navigation and migration now even encompasses theories at the level of quantum physics. All agree there is no "magic bullet" of explanation.

The burgeoning interest in Cognitive Ethology is astonishing and, especially, for those of us who struggled during the birth of the field, gratifying. The relevant work goes by different names now; some are still wary of the original term. Recall the discussion of the development of several related fields. The conclusion was that, though studied within different fields of inquiry, the impact of Cognitive Ethology has been widespread. One sees research in "animal cognition," "comparative cognition," "cognitive ecology," "animal sentience," and all subsets.

Popular books and films revealing animals' social and mental abilities charm the public. Some simply charm, without much evidence for their claims, while others pose and sometimes answer tantalizing questions. It is now acceptable to undertake Ph.D. theses in philosophy on the subject of animal sentience. Recently, there were "Zoom" meetings mediated by Stevan Harnad exploring "Invertebrate Animal Sentience." The two virtual gatherings quickly garnered the 1,000 maximum reservations each, while unknown numbers of viewers were able to watch later on YouTube. "Animal Sentience," an open-access digital journal, edited by Harnad, maintains continuing lively discussions centered on focal papers published in the journal, comprising diverse fields of inquiry. Conferences abound that focus on animal cognition and even sentience, and many conservative international meetings finally have, not merely posters, but symposia on the subject.

Let me repeat the summary of the Animal Ethics team analyzing the development of several new scientific disciplines. They conclude that Cognitive Ethology, though a small field, "has permeated the work of scientists in other disciplines, which are now more open to attributing minds to nonhuman animals."

Would that Donald Griffin were with us to experience these changes. We all deeply respected and admired him. He profoundly impacted science. And, in the words of Roger Payne, "Our affection and admiration for Don are quite immoderate."

ENDNOTES
1 The "Many Voices" with whom I corresponded and/or interviewed by telephone or E-mail are listed in the Acknowledgements in the front of Volume One.
2 Donald R. Griffin, (DRG) accessed from various letters from Griffin during 1973 to early 1980 in the private collection of Ronald P. Larkin. In later letters, particularly those addressed solely to Ron Larkin when he had left Rockefeller, Griffin usually signed as "Don."
3 Timothy C. Williams, personal communication, March 19, 2022, E-mail.
4 Katherine B. Payne, personal communication, November 1, 2020, E-mail.
5 Betsy Weaver, March 10, 2005. Personal interview by CR, Barnstable, Massachusetts.
6 Betsy Weaver, March 8, 2005. Personal interview by CR, Barnstable, Massachusetts.
7 Ronald P. Larkin, personal communication, June 16, 2019. Telephone interview.
Griffin wrote a paper in English primarily about bats, titled, “A Scientist’s Speculations and Surprises.” It was intended as a popular-level article for a German magazine. Over several letters from May to July of 2003, he communicated/debated with the magazine editor, Dr. Henschel, about the proper translations of his thoughts as expressed in the German version of his article. His digital files for the letters (unpublished) and the paper are prefaced by “GEO.”

Griffin wrote a paper in English primarily about bats, titled, “A Scientist’s Speculations and Surprises.” It was intended as a popular-level article for a German magazine. Over several letters from May to July of 2003, he communicated/debated with the magazine editor, Dr. Henschel, about the proper translations of his thoughts as expressed in the German version of his article. His digital files for the letters (unpublished) and the paper are prefaced by “GEO.”

These incidents are described more fully in Volume One-Chapter 4, "Wandering in the Marsh," section, "Friends and Relations." The quoted phrase is taken from Griffin, 1987, BOOK 6A, unpublished notes.


Hope Ryden, personal communication, 2004. Remembrance sent to Katy Payne for a Griffin obituary that was finally not published.

Eric Strauss, personal communication, September 16, 2019, Telephone conversation.

Brock M. Fenton, personal communication, March 24, 2021, Zoom interview.

Jack C. Bradbury, personal communication, July 8, 2020, Telephone interview.

James C. Simmons, personal communication, April 20, 2019, Telephone interview.

DRG, 1958a, p. 69.

DRG, 1958a, p. 69.

DRG, 1985, autobiographical memoir in Dewsbury.


Irene Pepperberg’s presentation at Rockefeller University was on January 29, 1981.

Karl von Frisch, 1950.


M. Brock Fenton, personal communication, March 24, 2021, Zoom interview.

Alan D. Grinnell, personal communication, April 30, 2019. Telephone interview.

DRG et al, 1960. These efforts are described in Volume One - Chapter 10, Part 1 - “The Extraordinary Summer of 1950,” section titled “Flying Bats, Flying Insects - Griffin’s Spectacular Discovery!”

Katherine Boynton Payne, personal communication, November 1, 2020. Telephone interview

Eric G. Strauss, personal communication, June 10, 2022, ZOOM interview.

Described in Volume Three - Chapter 18, Part 5, “Back to Bats,” in the entry of Griffin, January 4, 2003, BATSWIN03, unpublished document. Another “Grr” is a reaction to his forgetting to bring a necessary piece of apparatus for the evening’s work, also in “Back to Bats”, but in the subsection titled “To make an Insect.”

The cited frustrating incidents concerning publicity were described in the following chapters: The event that occurred in Trinidad with photographers and reporters is in Volume Two - Chapter 12, Part 2, section, “Fun and Frustrations in Simla Life,” in subsection “Frustrations = Photographers.” Incidents at the Millbrook Field Station during preparations for “Open House” are described in the same chapter in Volume Two - Chapter 12 - Part 5, section, “The Broad and Narrow Views (Bureaucracy #&*%!),” in subsection, “Publicity (More #&*%@!).”

Jack Bradbury, personal communication, September 19, 2020, E-mail.

DRG, 1999b.

The research by Griffin and Jim Simmons is discussed briefly in Volume One - Chapter 11, Part 2, subsection, “Hans-Ulrich Schnitzler’s Work at RU.” This section primarily describes research by Schnitzler on horseshoe bats’ constant-frequency ultrasonic echolocating.

Katherine B. Payne, personal communication, November 1, 2020, E-mail. The story is adapted from a recollection of Katy’s daughter Holly.


Irina Mikhalevich, personal communication, April 15, 2021, E-mail.

Arthur Reber, personal communication, September 26, 2021, E-mail to Oxford University Press, copied to myself, Carolyn A Ristau.

Frans B. M. de Waal, 2016.

Frans B. M. de Waal, personal communication, October 10, 2016, E-mail.

René-Guy Busnel and James F. Fish, 1980. The meeting is discussed in Volume Three - Chapter 16 - Part 3, in the subsection titled "Conferences: Spreading "Heresy" and Meeting Resistance."

Alan C. Grinnell, 1980, p. x.


Peter Singer, quoted in James L. Gould, 2004, p. 3.

Carol Kaesuk Yoon, November 14, 2003.

Editorial Board of Consciousness and Cognition, 2005, p. 3.

See discussion in Volume One - Chapter 11 - Part 4 –“The Present and Future for Bat Echolocation Research (And Devices).”

Further discussion in Volume Three - Chapter 18, Part 7, “Consciousness and Ethical Concerns,” in the section titled, “Cognitive Ethology: A Science that Struggled, Ideas that Spread.”

Further discussion may be found in Volume Three - Chapter 18, Part 7 – “Consciousness and Ethical Concerns,” in the sub-section titled “Griffin, the Public and Animal Consciousness.”

Animal Ethics, 2020, p. 3.

HONORS: In 1952 Griffin was elected a fellow of the American Academy of Arts and Sciences. He was awarded the Daniel Giraud Elliot Medal from the U.S. National Academy of Sciences for his book *Listening in The Dark* (1958). He was also elected to the U.S. National Academy of Sciences and the American Philosophical Society, was a Fellow of the Animal Behavior Society, of the American Ornithologists Union (1980) and a Scientific Fellow of the New York Zoological Society. In December 1978, he became president of the Harry Frank Guggenheim Foundation and continued for 4 years. His professional organization affiliations also included the American Physiological Society and the American Society of Mammalogists.

**TIMELINE**

(The Rockefeller Archive Center in Sleepy Hollow, New York previously held letters and other documents from 1940-1984 pertinent to Donald R. Griffin. Much information in this Timeline derives from those records.)

* indicates continuing research project

**1915- August 3- Born** in Southampton, Long Island, New York

1917-1924- Family lives in and near Scarsdale, New York

1924- Family moves to Barnstable, Massachusetts

*1930s-1950s- Bowdoin Scientific Station, Kent Island, Bay of Fundy, New Brunswick-Griffin & colleagues: bird homing and navigation, especially petrels and gannets

*1930s-1950s- Penikese Island, Massachusetts - bird banding, primarily terns and herring gulls to study homing and obstacle avoidance. With Austin Ornithological Research Station.

1934- Harvard Freshman - First scientific publication (“Marking Bats” in *Journal of Mammalogy*).

*1935-1938-16,000 bats banded - gained information on longevity, hibernation, migration.

**1938- BS Harvard**

1938- Awarded prestigious 3-year Junior Fellowship with "generous research funds."

1939- Huyck Preserve, Rensselaerville, New York - summer fellowship - bat banding, published papers

1940- MA Harvard

**1941- Sept. 4- married Ruth Castle** - Home 10 Dana Street, Cambridge, Massachusetts
1942- spring- PhD Harvard about avian navigation. Griffin suspended his Junior Fellowship (Society of Fellows) at Harvard to work on the war effort.

1941-1944- With the onset of war, Griffin worked in several Harvard labs: war research at Harvard in SS Stevens psycho acoustic lab ("Smitty" = founder of modern psychophysics), 1942- Bat Bomb (Lytle Adams); late 1942- Harvard fatigue lab (includes research for very cold conditions); 1944- George Wald (later Nobel Laureate -infra-red night vision)

1943- May 2- 1st child born, Nancy

1945- Feb 26- 2nd child born, Janet


1946-1953- Summer 1946 appointed to Cornell Univ as Assistant Professor, beginning July 1. Various scientific studies of avian homing including Bonaventure Island, Quebec; Caribou, Maine; and Kent Island in Bay of Fundy, New Brunswick, Canada where he had also gone as a graduate student. 7 years at Cornell where studies focused on bird homing and migration patterns. Used radioisotopes in studying avian homing, but also studied bat echolocation.

1947-1950- Parts of Summers: Homing experiments re bird orientation and applied work in mammalian sensory physiological adaptation to cold in Pt. Barrow, Alaska; funding by ONR. Initially for human work, then totally for bird navigation, which Navy thought would be useful for military.

1948- Cornell University – promoted to Associate Professor, with tenure.

1948- June 16 – twins born: Margaret and John

1950- “The Extraordinary Summer of 1950”: In the field, Griffin gets first evidence that flying bats catch flying insects. Summer at WHOI in Woods Hole: recorded vocalizing of sea robbins (fish) and bats in lab with Kay Electric Sonagraph.

1950-51, 1951-52, 1952-53 -summers – ONR contract in Fairbanks, Alaska to study the human ability to survive in cold weather, but also conducted avian studies.

1952- Elected a Fellow of the American Academy of Arts and Sciences

1952- July-Cornell- promoted to Full Professor

*1953 - spring (2 months)- Panama. Recorded bats including fish-catching bats with Harvard student Prentice Bloedel ("Pen"). Harold Trapido, former DRG student, is contact. ONR = some funding. Also side trip to Venezuela to study acoustic orientation via echolocation of oil birds, first known case in birds. Was to be sabbatical year at Cornell; spring term ’53 to be full salary from Cornell, but taken away since he was moving to Harvard.


Mid-1950s -Jamming experiments with grad student Alan Grinnell using “white noise” at frequencies of bat echolocation signal. Result: jamming led to only slight increase in minimum size of wires avoided.
1958- *Listening in the Dark*, Yale University Press. For this book, Griffin was awarded the prestigious [Daniel Giraud Elliot Medal from the U.S. National Academy of Sciences](https://www.nasonline.org/history/past-awards/). The award is given every 3 to 5 years for meritorious work in zoology or paleontology during that time.


*1960- July - Griffin’s first trip to Simla, Trinidad* to Beebe Tropical Research Station. Jocelyn Crane (his future wife) is working on fiddler crab behavior, Griffin and his students on bat visual detection, echolocation and fish-catching bat abilities. Several trips til August 1969.


1962- June 4 – William Beebe dies


1965- Autumn- moved to New York City. **December- married Jocelyn Crane**

1965-1986- **Griffin at The Rockefeller University (RU)**

1965- **Griffin organizes & directs** the new Institute for Research in Animal Behavior (IRAB) - jointly sponsored by Rockefeller Univ and New York Zoological Society. **Superseded by RUCFR field station in Millbrook.** Joining by leading ethologist Peter Marler (bird song) & Fernando Nottebohm (bird song and neurogenesis). Director 1965-‘69; Peter Marler Co-Director some of those years, then becoming Director. RUCFR "became one of leading US centers for the study of animal behavior.” (from C. Gross, 2005)

1969- August – last trip to Simla, Trinidad.

1969- summer – Alpha Helix/RV project: 4-month trip to New Guinea to study bats and cave swiftlets, with UCLA, including wife Jocelyn Crane Griffin, UCLA: George Bartholomew, Jared Diamond, Alan Grinnell, etc.

1970- **Millbrook** -DRG’s unpublished memoir of 2003 indicates that the relationship between RU and Zoo (NYZS) shows signs of strain in 1970.

1972 (Approx)– Philosopher Thomas Nagel at RU for a year; discussions with Griffin about animal consciousness.

1972 and 1973- The two autumn voyages of the **WHOI ship, Atlantis II**. Griffin’s team studies avian migration over the Atlantic Ocean.


1978- Summer trip to Afghanistan, Soviet Turkistan, and Leningrad with Jocelyn for art studies; meet Russian bat biologists.

1978 (December)-1983- **President of the Harry Frank Guggenheim Foundation**, “and he used this position to encourage research in animal behavior.” (C. Gross, 2005, Griffin Obituary)
**1980- Summer to Chillagoe, Queensland** with colleagues. Group studies sensory physiology and perception in bats, bowerbirds and cave swiftlets. Griffin: recording echolocating calls of bats and detection capacities of swiftlets. Funding by National Science Foundation.


**1986- DRG officially retires from Rockefeller**


**1989-2003- Harvard:** Research Associate of Museum of Comparative Zoology, working at Concord Field Station, Lexington, Massachusetts. Occasionally teaches a Harvard undergraduate course. Work on bats, beavers, and bees, as well as animal consciousness, with elephants as a potential research subject.


1996 late-1998- Main video studies of beavers: put microphones, video cameras and Infrared light into beaver's lodges to study social behavior and communication.”

**1998- Jocelyn Crane Griffin (Griffin's 2nd wife) dies, Ruth Castle Griffin (Griffin's 1st wife) dies**

2000 (approx.)-2003- Griffin and Katherine Boynton Payne plan elephant communication and social behavior studies.


2001- 2003- Bat echolocation research at Trout Pond aka Mashpee Pond, Massachusetts with Gregory Auger. ”Terminal Buzz” = main focus. July 30, 2003 is the last session of available notes.


**2003- DRG dies November 7** (88th year- still doing field studies of the terminal buzzes of bats ("Whisper World"), near field communication of bees and beaver communication as well as studying elephant social behavior and animal consciousness)

**VOLUME THREE – APPENDIX #2**

**TIMELINE: DONALD R. GRIFFIN’S STUDENTS and ASSOCIATES  
(Cornell, Harvard and Rockefeller Universities)**

[NOTE: Volume One contains Chapters 1 – 11; Volume Two contains Chapters 12 – 15; Volume Three contains Chapters 16 – 19.]

<table>
<thead>
<tr>
<th>INSTITUTION</th>
<th>Research Work, primarily that related to person’s association with Griffin</th>
<th>Main Institutional Affiliation</th>
<th>Discussed in this book (primary site)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>DRG-HARVARD as Undergrad/Grad</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Donald R. Griffin</td>
<td>BS 1938; PhD 1942</td>
<td>Co-discoverer with DRG of bat sonar when both were Harvard undergraduates. Then neurophysiology of bat echolocation.</td>
<td>Univ California, San Diego (UCSD)</td>
</tr>
<tr>
<td>Robert Galambos</td>
<td>BS 1938; PhD 1942</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>CORNELL Faculty 1946-1953</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| Graduate Students | | | |
| Ann M. Rawson | did not complete bat studies | | |
| W. C. Curtiss | ? | vision in bats & owls | ? | Vol.2-Chap. 12, Part 2 |

App #2-201
**HARVARD Faculty**

*1953-1965*

*Name & institution in DRG letter to John R. Raper, Chair Harvard Biology, 1972 Dec. 12, Correspondence, Folder 107 Ra-Ri, Donald R Griffin Papers, ROCARCH

** I thank Alan Grinnell for his help in listing Harvard students, their work & institutions.

### Undergraduates

(partial list)

<table>
<thead>
<tr>
<th>Name</th>
<th>Year(s)</th>
<th>Degree(s)</th>
<th>Institution</th>
</tr>
</thead>
<tbody>
<tr>
<td>Charles Gross</td>
<td>1957</td>
<td>BA</td>
<td>Princeton University</td>
</tr>
<tr>
<td>Roger Payne</td>
<td>1957</td>
<td>BA</td>
<td>PhD Cornell 1961; Post Doc Tufts (Roeder)-neurophysiology: moth hearing; Asst. Prof Tufts University</td>
</tr>
<tr>
<td>Alan D. Grinnell</td>
<td>1958</td>
<td>BA</td>
<td>Univ California, Los Angeles (UCLA), Emeritus</td>
</tr>
<tr>
<td>Ann Graybiel</td>
<td>1964</td>
<td>BA</td>
<td>Massachusetts Inst Technology (MIT), Cambridge, MA.</td>
</tr>
</tbody>
</table>

### Graduate Students

<table>
<thead>
<tr>
<th>Name</th>
<th>Year(s)</th>
<th>Degree(s)</th>
<th>Details</th>
</tr>
</thead>
<tbody>
<tr>
<td>Donald Kennedy</td>
<td>1954, PhD</td>
<td>MA 1954, PhD 1956</td>
<td>BA 1952-before DRG; began PhD with DRG, switch to George Wald w DRG an advisor. Neurobiology. Stanford University: Professor &amp; President Chap. 11</td>
</tr>
<tr>
<td>Kenneth S. Rawson</td>
<td>PhD</td>
<td>PhD 1971</td>
<td>later, continued homing &amp; circadian rhythms in mice; many other research topics Swarthmore College, Pennsylvania Chap. 12, Part 1</td>
</tr>
<tr>
<td>Timothy Goldschmidt</td>
<td>PhD</td>
<td>PhD 1958</td>
<td>1955 paper w DRG on bird flight direction; PhD: insect visual systems-probably with George Wald Yale University; Emeritus Chap. 11, Part 1</td>
</tr>
<tr>
<td>Cam Gifford</td>
<td>PhD</td>
<td></td>
<td>finished PhD with Emlem? Indiana University, WHOI, Massachusetts Marine Academy Chap. 12, Part 1</td>
</tr>
<tr>
<td>Jon Steen</td>
<td>PhD</td>
<td></td>
<td>finished PhD with Per Scholander at USCD Indiana University, WHOI, Massachusetts Marine Academy Chap. 12, Part 1</td>
</tr>
<tr>
<td>*Robert D. Lisk</td>
<td>PhD</td>
<td>PhD 1960</td>
<td>olfaction &amp; mating in hamsters Bio Dept, Princeton Chap. 11, Part 1</td>
</tr>
<tr>
<td>Name</td>
<td>Field/Institution/Position</td>
<td>Contributions</td>
<td>Current Position/Location</td>
</tr>
<tr>
<td>---------------------</td>
<td>------------------------------------------------------------------------------------------</td>
<td>--------------------------------------------------------------------------------------------------</td>
<td>-------------------------------------------</td>
</tr>
<tr>
<td>Jean B. Harrison</td>
<td>PhD 1962</td>
<td>Effects of body temperature on bats' auditory responses</td>
<td>Bio Dept, Wellesley, then UCLA</td>
</tr>
<tr>
<td>Alan D. Grinnell</td>
<td>*PhD 1962</td>
<td>Neurophysiology: frog muscles, bat echolocation</td>
<td>Bio Dept, UCLA, Emeritus</td>
</tr>
<tr>
<td>Richard M. Wylie</td>
<td>PhD 1962</td>
<td></td>
<td>Walter Reed Army Med Center, Wash., DC-Neurophysiology</td>
</tr>
<tr>
<td>Alvin Novick</td>
<td>med degree</td>
<td>Medical resident &amp; worked w DRG. Comparative bat echolocation. Later AIDS activist.</td>
<td>Ch. 10, Part 2</td>
</tr>
<tr>
<td>Charles R. Michael</td>
<td>PhD 1965</td>
<td></td>
<td>Dept Physiology, Yale Medical School</td>
</tr>
<tr>
<td>Martin C. Michener</td>
<td>PhD 1965</td>
<td>Possibly beaver communication as grad student. Later, ecologist, specializing in Wetlands</td>
<td>Lincoln, Mass.</td>
</tr>
<tr>
<td>Timothy C. Williams</td>
<td>begin 1964; '66 MA, AB 1964-Swarthmore; Summer 1964: 1st trip to Trinidad, with wife Janet (she wrote book). Went to RU w DRG in 1965</td>
<td></td>
<td>Swarthmore College, Pennsylvania; Emeritus</td>
</tr>
<tr>
<td><strong>Post Docs</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alan D. Grinnell</td>
<td>1962</td>
<td>Briefly until UCLA position</td>
<td>Bio Dept., UCLA; Emeritus</td>
</tr>
<tr>
<td>Nobua Suga</td>
<td>1963-1965</td>
<td>*came when Grinnell left for Post Doc</td>
<td>Bio Dept, Washington Univ., St Louis, Mo.</td>
</tr>
</tbody>
</table>
### *Roderick A. Suthers (1937-2019)*

1964 briefly til U Indiana position  
Dept. Anatomy & Physiology, Indiana U  
Chap. 12, Part 2; Volume 3-Appendix #3

---

### Research Assistants

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Details</th>
<th>Institution</th>
<th>References</th>
</tr>
</thead>
<tbody>
<tr>
<td>Clarke R. Slater</td>
<td>1959-1962</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>SIMLA, Trinidad</strong></td>
<td></td>
<td>Griffin was a visiting scientist during his research trips. Then oversaw Simla station when IRAB was founded in 1965 and he was IRAB head. 1969 was DRG’s last trip to Simla. The persons listed are some scientists that DRG and his students seemed to have interacted with most.</td>
<td></td>
<td>Chap. 12, Part 2; Chap. 14</td>
</tr>
<tr>
<td><strong>(William Beebe Tropical Research Station of NY Zool Society)</strong> (partial list)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>John Alcock</td>
<td>1966, other dates?</td>
<td>working with advisor Brower on butterflies; then behavioral ecologist</td>
<td>Arizona State University</td>
<td></td>
</tr>
<tr>
<td>Lincoln Brower</td>
<td>1966, other dates?</td>
<td>Butterflies. Was not DRG student; lifelong study &amp; conservation of Monarch butterflies.</td>
<td>University of Florida</td>
<td></td>
</tr>
<tr>
<td>Julia Chase, now Brand</td>
<td>1967,’68, probably Summers?</td>
<td>was Rod Suthers Res Asst/grad student when he was on Indiana faculty. Then came independently when faculty.</td>
<td>Rutgers University, Newark, N.J</td>
<td>Chap. 12, Part 2; Chap. 15, Part 1</td>
</tr>
<tr>
<td>Charles Collins</td>
<td>1930-1970</td>
<td>1960=DRG first trip to Simla; first met Jocelyn</td>
<td>Simla; NY Zool. Society. Became Assoc Director of Simla, then Director at Beebe's death. IRAB.</td>
<td>Chap. 12, Part 2; Chap. 14</td>
</tr>
<tr>
<td>Jocelyn Crane</td>
<td></td>
<td>worked w Roeder (jt 1965 paper) &amp;DRG on moths, NOT clear if student. W. Virgina Univ.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dorothy Dunning</td>
<td>early 1960’s, other dates?</td>
<td>switched to UC San Diego w Bullock, maybe overlap w DRG; in Trinidad w transistors &amp; helping Tim Williams</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Peter H. Hartline</td>
<td>approx mid 1960’s</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
# Rockefeller University Faculty 1965-1986

ROCKEFELLER University does not have undergraduates, only graduate students, Post Docs, etc.

## DRG Graduate Students

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Education/PhD</th>
<th>Affiliations</th>
<th>Chapters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Timothy C. Williams</td>
<td>1965-PhD 1968</td>
<td></td>
<td>came w DRG from Harvard, bat studies in Trinidad. Afterwards behavioral biology of bats, mammals. Swarthmore College, Pennsylvania; Emeritus</td>
<td>Chap. 12, Parts 1, 2, 5; Chap. 16, Part 1</td>
</tr>
<tr>
<td>Barry S. Bean</td>
<td>1964-PhD 1970</td>
<td></td>
<td>BS-Tufts-1964; PhD w Griffin, Hotchkiss/Zinder; returned to RU as post doc 1971-73; cellular biology: male reproductive biology</td>
<td>Lehigh University, Pennsylvania</td>
</tr>
<tr>
<td>Beverly Greenspan</td>
<td>1969-PhD 1975</td>
<td></td>
<td>BA-Brown-heard DRG lecture. Worked w Jocelyn Crane on fiddler crabs, officially w DRG</td>
<td>Bowdoin College, Maine; then medical career</td>
</tr>
<tr>
<td>Jose Torre Bueno</td>
<td>1970-PhD 1975</td>
<td></td>
<td>wife Susi: radar, computer; Jose: bird metabolism while flying</td>
<td>Duke Univ: Post Doc, then faculty. Later formed company creating medical algorithms. Chap. 12, Parts 3 &amp; 5</td>
</tr>
<tr>
<td>James Gould</td>
<td>1970-PhD 1975</td>
<td></td>
<td>BS -Calif Inst Technology; bee dance info &amp; sensory aspects</td>
<td>Princeton University Chap. 12, Parts 3 &amp; 5; Chap. 18, Part 2</td>
</tr>
<tr>
<td>Michael L. Brines</td>
<td>1973-PhD 1978</td>
<td></td>
<td>BS U Notre Dame; bees, then med school. Neurodegenerative diseases as specialty. MD Yale Univ., then Yale faculty. Then founded a pharmaceutical company.</td>
<td>Chap. 12, Part 5</td>
</tr>
<tr>
<td>Marilyn L. Yodlowski</td>
<td>1975-PhD 1980</td>
<td></td>
<td>BA-Cornell; infrasound in pigeons; then med school</td>
<td>Faculty at several med schools, including Harvard &amp; Chap. 12, Part 5</td>
</tr>
</tbody>
</table>
### Appendix #2 – Timeline: Griffin’s Students & Associates

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Background/Role</th>
<th>Institution/Location</th>
<th>Chapters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Peter L. Tyack</td>
<td>1977-PhD 1982</td>
<td>BA-Harvard; joint student w DRG &amp; Roger Payne; whale social behavior &amp; communication</td>
<td>Woods Hole Oceanographic Institute</td>
<td>Chap. 12, Part 4; Chap. 16, Part 3</td>
</tr>
<tr>
<td><strong>Tufts</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>DRG Post Doc/Research Assoc/Guest Investigator</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hubert Markl</td>
<td>1965-1966</td>
<td>Post Doc, Communication by stridulation in leaf-cutting ants</td>
<td>Max Planck Inst, Germany</td>
<td></td>
</tr>
<tr>
<td>Jeremy J. Hatch</td>
<td>1967-1969</td>
<td>Post Doc, shorebirds</td>
<td>Univ. of Massachusetts</td>
<td></td>
</tr>
<tr>
<td>(1937-2015)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hans-Ulrich Schnitzler</td>
<td>1968 (Aug)-1969</td>
<td>Post Doc, bats-comparative study echolocation, Doppler Shift Compensation</td>
<td>Univ. of Tubingen, Germany</td>
<td>Chap. 11</td>
</tr>
<tr>
<td>Alan Lill</td>
<td>1967-1973</td>
<td>Griffin/Marler. Trinidadian manakins lek breeding</td>
<td>Monash Univ., Australia</td>
<td>Chap. 12, Part 2</td>
</tr>
<tr>
<td>Peter Hollander</td>
<td>1971-74</td>
<td>Visiting</td>
<td>?</td>
<td></td>
</tr>
<tr>
<td>Edward Buchler</td>
<td>1972-1975</td>
<td>bats-including ontogeny of echolocation Post Doc, then Research Assoc</td>
<td>Univ. Maryland, then business career</td>
<td>Chap. 12, Part 3</td>
</tr>
<tr>
<td>M. Brock Fenton</td>
<td>1976-77; 1980</td>
<td>Sabattical. Evolution of bats &amp; their echolocation, research in Millbrook. 1980: on Chillagoe, Australia expedition, as was DRG.</td>
<td>Canada: Carleton University, Ottawa then Univ. of Western Ontario, London, Ontario</td>
<td>Chap. 11; Chap. 12, Part 3; Chap. 15, Part 3</td>
</tr>
<tr>
<td><strong>Faculty (in Griffin’s</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
### Appendix #2 – Timeline: Griffin’s Students & Associates

#### Lab

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Role/Research Activities</th>
<th>Institutions</th>
<th>Timeline References</th>
</tr>
</thead>
<tbody>
<tr>
<td>Roger S. Payne</td>
<td>1966-1971,</td>
<td>Asst Prof-owl auditory, then whales &amp; whale song; gave RU Xmas lectures 1972; RU faculty</td>
<td>New York Zoological Society which became Wildlife Conservation Society in 1993.</td>
<td>Chap. 12, Part 3,</td>
</tr>
<tr>
<td>(1935-1993)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>studies; then Asst Prof with Griffin. Radar studies of avian migration.</td>
<td></td>
<td>Chap. 15</td>
</tr>
</tbody>
</table>

#### Others Associated with Griffin at RU

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Role/Research Activities</th>
<th>Institutions</th>
<th>Timeline References</th>
</tr>
</thead>
<tbody>
<tr>
<td>David Thompson</td>
<td>approx 1974?</td>
<td>Assisted DRG &amp; others with research; co-publish</td>
<td></td>
<td>Chap. 15, Part 3</td>
</tr>
<tr>
<td></td>
<td>to ?</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Jocelyn Crane Griffin</td>
<td>1930-1971</td>
<td>Fiddler crabs, etc. Wrote <em>Fiddler Crabs of the World</em></td>
<td>New York Zoological Society</td>
<td>Chap. 12, Part 2;</td>
</tr>
<tr>
<td>(1908-1998)</td>
<td></td>
<td></td>
<td></td>
<td>Chap. 14</td>
</tr>
<tr>
<td>Katherine Boynton Payne</td>
<td>not at RU,</td>
<td>Whales &amp; whale song; later elephant &quot;rumble&quot; communication</td>
<td>New York Zoological Society, then Cornell Lab of Ornithology</td>
<td>Chap. 12, Part 4;</td>
</tr>
<tr>
<td></td>
<td>but NY Zool.</td>
<td></td>
<td></td>
<td>Chap. 18, Part 6</td>
</tr>
<tr>
<td>Douglas Quine</td>
<td>in Trinidad?</td>
<td>Illinois Natural History Survey</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

#### RU: Marler PhD Students/Post Docs (partial list)

These students had interests that overlapped with Griffin’s (as did others not listed here). 

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Role/Research Activities</th>
<th>Institutions</th>
<th>Timeline References</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>primarily Chap. 12,</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Parts 3 &amp;4</td>
</tr>
</tbody>
</table>
### Appendix #2 – Timeline: Griffin’s Students & Associates

<table>
<thead>
<tr>
<th>Name</th>
<th>Years</th>
<th>Field/Research Focus</th>
<th>Affiliation</th>
<th>Chapters</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carl D. Hopkins</td>
<td>1966-PhD1972</td>
<td>electric fish communication; worked with DRG on sound transmission through atmosphere</td>
<td>Cornell University</td>
<td>Chap. 12, Parts 2, 4, 5</td>
</tr>
<tr>
<td>Steven Green</td>
<td>1964-78; PhD1973</td>
<td>primate communication</td>
<td>Univ. of Miami</td>
<td>Chap. 12, Part 4</td>
</tr>
<tr>
<td>R. Haven Wiley</td>
<td>1966-71; PhD1970</td>
<td>PhD: sage grouse lek behavior; then social behavior in birds &amp; other species</td>
<td>Univ. of North Carolina.</td>
<td>Chap. 12, Parts 4 &amp; 5</td>
</tr>
<tr>
<td>Robert Seyfarth</td>
<td>1976-1981</td>
<td>Vervet monkey symbolic communication</td>
<td>Univ. of Pennsylvania</td>
<td>Chap. 16, Part 3</td>
</tr>
<tr>
<td>Gregory Auger</td>
<td>Harv. Museum of Comparative Zoology, 1989-2003</td>
<td>work w DRG on beavers, bat echolocation</td>
<td>Mashpee, Massachusetts</td>
<td>Chap. 18, Part 5; also Chap. 11</td>
</tr>
<tr>
<td>Many volunteers and students</td>
<td></td>
<td>primarily bat echolocation, some beaver work</td>
<td></td>
<td>Chap. 18</td>
</tr>
</tbody>
</table>

With initial interests in history, Ann switched her major to chemistry, then biology. “I was lucky enough to go to Trinidad with Donald Griffin to conduct research on echolocation in bats,” also assisting Don’s Harvard grad student, Rod Suthers. She does note it was “scary” at night in the jungle and there were “lots of snakes.” But all was well. She also studied bird retinas, working with vision physiologist George Wald. Then, graduate school at MIT with Hans-Lukas Teuber and J. H. Nauta. Soon after her Ph.D., she joined the MIT faculty. As such, she was the only woman scientist in the building and was given an “office” in a small room where monkeys were kept. But she went on to a distinguished career in brain science, receiving numerous honors including the President’s National Medal of Science 2001.

(Further descriptions of Ann Graybiel and her work may be found in Volume Three - Chapter 16 - Part 1 and Chapter 18 - Part 4)

Donald Kennedy (1931-2020)  Harvard Ph. D. 1956

After receiving his Ph. D., Donald Kennedy’s first faculty position (1956) was at Syracuse University, New York, where he gained acclaim, particularly for his teaching. He was then recruited for that talent by Stanford University (1960). His neurophysiological research also was most significant. He had demonstrated the existence of crayfish “command” neurons, capable of producing complex, fixed action patterns of locomotor behavior, in particular, escape responses. For two years (1977-1979), in challenging circumstances, he served as Commissioner of the Food and Drug Administration (FDA) under President Jimmy Carter. Kennedy returned to academic life at Stanford, becoming President of Stanford University (1980-1992). Known always as a firmly principled human being, he forcefully advocated for policies in the public interest. Prime among them were his efforts in environmental regulations. As an ardent believer in public service, he thought persons should “give back” to society, instituting at Stanford, a voluntary public service program, in which 70% of students participated. Earning antagonism from some faculty, he stressed the importance of undergraduate teaching, in addition to faculty research efforts. During his tenure, he enormously enhanced Stanford’s role as a premier educational and research institution. Daring to confront a powerful Congressman over funding for the University, he lost, and stepped down as President, but remained a faculty member. He became Editor-in-Chief of the prestigious Science journal of the American Association for the Advancement of Science. There, he relished writing the often controversial Opinion pieces, advocating issues for the public/scientific good. In his obituary in Science, he is described as a Harvard-trained neurobiologist who wore his honors lightly and was “driven by an insatiable curiosity about the world and a wish to help make it a better place.” Kennedy passed away after contracting Covid-19.

(Further descriptions of Donald Kennedy and his work may be found in Volume One - Chapter 11 - Part 1 including a TEXT BOX)

(photos available in Suthers Obituary)

Born in Ohio, exploring near the family cabin in the Ontario woods, being fascinated by birds, waking early to go birding before school and becoming an Eagle Scout were among Rod Suthers’ early naturalistic activities. After secondary school, he spent a year traveling in Australia, further developing his passion for nature “in the rough.” Upon graduating from Ohio Wesleyan College, he joined Don Griffin’s lab at Harvard as a grad student, intent on studying bird navigation. But Boston’s weather was uncooperative, so he switched to research on bat echolocation. His main focus for his Ph.D. (1964) and through much of his early scientific life was the fascinating fishing bats (Noctilio leporinus) (Suthers, 1965) that had so intrigued Don Griffin. The “research paradise” at the William Beebe Tropical Research Center at Simla, Trinidad was the base for these and other bat studies, continuing through a year’s postdoc with Griffin and afterwards. The 1966 postdoc summer provided a happy mélange of scientists and their undertakings, many associated with Griffin and his associates. (Simla activities merit their special section and Rod’s research is described in Chapter 12 - Part 2.) Among Suthers’ research activities in Trinidad and later in continents around the world were investigations of the leaf-nosed bats (Phyllostomidae, particularly Carollia perspicillata), the little Vampire bat (Demostus rotundus) and the optomotor reflex (flicker-fusion threshold) of nine different bat species. The latter work warranted an article in the prestigious Science journal in 1966. This was a welcome boost to Rod’s academic career during his first year as a faculty member at Indiana University where he remained for his entire academic life. That life included multitudinous collaborative field expeditions around the world with colleagues and his graduate students.

In Suthers’ later studies of echolocation calls used by fishing bats, he found an unusual change in their vocalizations in some distinctive circumstances. If a bat was flying in a direction that would result in a collision with another, it lowered the frequency of its calls, producing “honking-like” noises, i.e. the modified echolocation call was now being used as a communication, a warning. The recipient bat would veer away. Since then, other bat species have been shown to modify calls similarly to function as alarms.

In Rods’ later work, he switched back to his long-standing interest in birds, initially with oilbirds (Steatornis caripensis), a species shown by Griffin to echolocate, though not as precisely as bats do. Suthers discovered that oilbirds produce their echolocating clicks using both sides of their syrinx independently. (The syrinx is the avian larynx.) Continuing his studies of the mechanisms of avian sound production with passerines, he worked particularly with Bengalese finches, which produce such complex songs that some scientists had hypothesized that they have “two voices.” And so they do, again using both sides of their syrinx. They are thereby capable of alternating sides and also of producing two different notes at the same time, an impressive way to woo a lady finch. The work has been described as “pioneering” in our understanding of the avian syrinx. To do this research, he created an ingenious apparatus, another “gadgeteering” trait he shared with Don Griffin.

Many note his boundless, high energy, his dry sense of humor, “always with a twinkle in his eye,” and his very precise and creative ways of designing experiments and inventing the apparatus required to solve scientific problems. He was likewise described as “unconventional” and also “driven, determined, careful and competent.” He “worked very long hours and expected his lab group members to do the same.” Happily, his family was often along on the expeditions, and his
wife, Barbara (Braford) Suthers, herself a biology graduate student in Indiana University when they met, was a co-author on some of his publications.\textsuperscript{12}

And a closing anecdote from a graduate student with him in Africa in the 1980s:
Unfortunately, the horseshoe bats they were studying chose to inhabit a hollow giant baobab tree, entered by crawling through a hole at the bottom. “Unfortunate,” for the base had fresh leopard tracks. Rod, “with that twinkle in his eye,” suggested the grad student enter first, since he was “all skin and bone and not worth the eating, unlike himself.” AK-47 at the ready, a park ranger went in first. All survived.\textsuperscript{13}

(Further descriptions of Roderick Suthers and his work may be found in Volume Two - Chapter 12 – Part 2)

ENDNOTES
\textsuperscript{1} These sketches were difficult to place within the body of the text.
\textsuperscript{2} Ann M. Graybiel, n. d. Retrieved from Gruber Foundation Yale web site:
www.gruber.yale.edu/
\textsuperscript{3} Marcia Barinaga, 1991.
\textsuperscript{4} Donald Kennedy, 1997.
\textsuperscript{5} Jeffrey Mervis, 2020.
\textsuperscript{6} Sue Dremann, 2020.
\textsuperscript{7} Roderick Atkins Suthers Obituary, 2019. Retrieved from
\textsuperscript{8} Roderick Atkins Suthers Obituary, 2019.
\textsuperscript{9} R. A. Suthers and Hector, 1983.
\textsuperscript{10} R. A. Suthers, 1997.
\textsuperscript{11} Jack W. Bradbury and Sandra L. Vehrencamp, 2011, p. 49.
\textsuperscript{12} R. A. Suthers, 2019, Obituary.
\textsuperscript{13} Dave Hartley, March 17, 2019, in R. A. Suthers Obituary, 2019.
Donald R. Griffin (Professor at RU 1966-1986. Director of IRAB 1966-1972; Director, then Co-Director with Marler of the Rockefeller University Field Research Center for Ethology and Ecology (RUCFR) in Millbrook-1972-)  
(The date of each Ph.D. or other position within the Griffin lab is indicated in parenthesis.)


1980s: Julia Chase Brand (’80-’84, Visiting), Roger Payne (Visiting), Carolyn A. Ristau (’80-’86, Res. Assoc.), Peter Tyack (’82), Marilyn L. Yodlowski (’80)

Peter Marler (Professor at RU 1967-1989. Co-Director of the RUCFR with Griffin, then Director from -1981)

The list includes Marler lab members as well as students working with Fernando Nottebohm when he was a member of the Marler Group and those with Thomas T. Struhsaker. Both began as junior faculty members in 1967. Later, Nottebohm was head of his own laboratory.


App #4-212
Fernando Nottebohm (RU Asst. Professor, 1967; Associate Prof. in 1971; full Professor in 1976. Director of RUCFR from 1981-2016)

1970s: Darcy B. Kelley, Kirk R. Manogue

GLOSSARY of “GRIFFIN TERMS”

[I include Griffin’s updated, unpublished thoughts about these terms, as available.]

**cognitive ethology** – In notes to himself in August 2003, Griffin was reconsidering two terms he had introduced and used over the years. One was “cognitive ethology,” a new sub-discipline he had created and the central interest in his first book of several books on the subject, *The Question of Animal Awareness* (1976). (The other term was “echolocation”; see this Glossary).

“Cognitive ethology” has been accepted and used by some, though others ... reject it or wish to redefine it to exclude subjective experiences. I meant, and mean, the term to include both conscious and nonconscious cognition with an emphasis on animals under natural conditions. Perhaps it is time to suggest a term or terms that clarify this. Unfortunately, I have difficulty thinking of brief and easily understood terms. “Ethology of consciousness”, --- “conscious ethology,” --- “subjective ethology,” --- (D. R. Griffin, 2003. TermsAUG03, unpublished notes.)

**critical anthropomorphism** - With this term, Griffin is emphasizing that he does not mean to imply that animal thinking and consciousness are identical to those processes in humans. As Eileen Crist (2008) notes, Griffin is attempting to counter the “fear of anthropomorphism” expressed by many behavioral scientists (Griffin, 1997). Griffin argues that empathy is useful, maybe necessary, in attempting to “discern the presence of mind in others.”

**echolocation** - Griffin used a broad term to denote the bats’ abilities, to point out the similarities to other phenomena, such as a blind person’s ability to navigate using acoustic cues and human’s use of radar. Griffin meant then to distinguish echolocation from an automatic physiological response (Griffin, 1944; see also Nash, 2016, p. 66 -70), and in 2003 considered that a term other than “echolocation” might emphasize that point more strongly. Nash makes the case that Griffin’s experience with the military use of radar and with Harvard’s wartime military work and open door exchanges between physicists and natural scientists as significant in developing Griffin’s thinking from bats’ mere “obstacle avoidance” abilities to considering their perceptual abilities via echolocation to detect, locate and hunt insects.

In his 2003 reconsideration of the term “echolocation,” Griffin muses

“Echolocation” means locating something by echoes. It was something of a struggle to get my note in Science in 1943 accepted [CR: publication occurred in 1944], because a referee said “echo-ranging” was sufficient. Of course location is more than ranging ... But students of bat echolocation later showed that they (and dolphins also) do more than locate a target, and they can determine something about it, distinguish it from other, often quite similar objects. (Griffin, Friend and Webster, 1965 and Simmons et al 1973 ?date?... [CR: the date is most likely 1974]

Thus a better term might be “echoception”, meaning perception based on echoes. But the meaning of that term is not as clear on first encounter with it as echolocation ... What alternatives are worth considering? Echo perception, Echo-classification, Echodiscrimination, What else and maybe better? (D. R. Griffin, 2003, TermsAUG03, unpublished notes)
“Excessive caution can sometimes lead one as far astray as rash enthusiasm.” - Griffin first used this phrase in reference to the ultrasonic vocalizations that bats make when echolocating. Initially, he, as an undergraduate, and Harvard Professor William Pierce had conservatively concluded that bats only occasionally make the sounds. Their conclusion was based on their being able to record the sounds only occasionally. Later, Griffin and his fellow student Robert Galambos discovered the misinterpretation. The extreme directionality of both the bat’s vocalizing and the parabolic horn that focused the sounds for recording meant that the sounds could be recorded only when the horn was placed directly in front of the bat’s mouth, not to the side. This was difficult to accomplish when the bats were flying and changing directions. (Described in D. R. Griffin, 1958. Listening … p. 69)

GOP - Groans of Pain. - Griffin characterized the behaviorist scientists as interpreting animal communications similarly to GOP, i.e. that animals were instinctively and reflexively responding to the signal of another as one did to a sudden injury. Such scientists were ignoring the possible likely voluntary and meaningful use of vocalizations, gestures, etc. in animals’ interactions with each other.

inclusive behaviorism - In Griffin's view, this was the “twentieth-century doctrine that entreated scientists to focus solely on observable behavior, without any reference to the mental experience...” These scientists were thereby considering animal sentience to be either nonexistent or epistemologically inaccessible. (E. Crist, 2008)

inmates - Griffin termed the residents, including himself and his wife Jocelyn Crane, as “inmates” at the well-appointed Lexington, Massachusetts retirement community where they lived during his later retirement. Among the “inmates” was the noted biologist, Edward O. Wilson, their friend and frequent dinner companion. Most inhabitants did not venture far and could eat all their meals, if they so chose, at the onsite dining hall. The Griffins did still venture far (J. Gould, personal communication, May 29, 2019. In-person interview, Princeton, New Jersey).

lantern slides - Don often referred to “PowerPoint” presentations as “lantern slides” according to Ron Larkin (personal communication, re Chap 12-Part 4-November 22, 2020, E-mail).

mechanomorphism – This is a view that considers animal behavior from a mechanistic approach with no reference to mental states. Griffin says, in 2003, in notes for a presentation at the International Conference on Acoustic Communication by Animals, July 27-30, 2003, Maryland, ... another important role for animal communication is its capability of informing us about their subjective thoughts and feelings...” He mentions the possibility that the wide bandwidth available in acoustic communication can convey detailed information. ... This seems to many a radical idea, primarily because we have been trained to avoid anthropomorphism ... but in doing so we tend to slip into a mechanomorphism which may be equally misleading. We need a balanced zoomorphism ... (Griffin, 2003, MDpowerpoint, unpublished notes).

mentophobia – Griffin regards “mentophobia” (avoidance of mind) as “a vestige of the long reign of behavioralism in the life sciences” (Crist, 2008). He specifically uses the term when referring to behavioral scientists rejecting his proposal for animal communications serving as a “window on animal minds” (Griffin, 1998a, in autobiography in Squire, p. 89; also 2001, AM-C, p. 34-36). Gayle
Speck, a colleague and co-author, notes that Griffin regarded “mentophobia” as the behaviorists’ “deep-seated fear of the loss of human uniqueness” (Speck, 2005, p. 4).

**mindful vs mindless** – As Eileen Crist notes (2008), Griffin often focused on contrasting construals of a behavior, and simply asked one to consider the possibility of a “mindful” interpretation.

**paralytic perfectionism** was a Griffin term developed from his earlier phrase “premature perfectionism.” He considers that the term describes the response of much of the scientific community as it demands “perfectionist” standards for what must of necessity be the beginning (indeed, pioneering), and certainly not yet perfected, analysis of animals’ consciousness and their subjective feelings. (C. A. Ristau, 2005, p. 407).

**philosopause** - Griffin used the word to describe the period later in a scientist’s career when they turn to more general questions instead of the highly detailed studies of earlier times. Griffin sees his own as his growing dissatisfaction with the rampant reductionism common among biologists and psychologists; they simply ignored the possibility of animal consciousness and feelings. His response was to establish the subfield of Cognitive Ethology (D. R. Griffin, 1998a, in Squire, p. 88).

Although some others, e.g. Nash (2016, p. 224) have allied the term with “menopause,” and a period when scientifically fertile days are past, I have always held quite a different interpretation. I see Griffin’s use of the term as suggesting a “pause” in one’s scientific work to consider the broader and deeper questions underlying one’s efforts, which is precisely what he did. Yet, given Griffin’s wry sense of humor, often self-deprecating, the allusion to menopause is certainly well-taken.

**pseudo-rigor** - As Brock Fenton, a fellow bat researcher, recalls a response Griffin sometimes made to critics about his work on animal consciousness was simply, “Their comments sound like ‘pseudo-rigor.’” Fenton continues, “I’ve never heard him say unkind things about other people.” (B. Fenton, personal communication, March 24, 2021, Telephone interview).

**recreational science** - In later years, Griffin used to refer to his continuing research, reading, and writing as “recreational science.” As many will attest, Griffin loved his work. He sometimes remarked that he had been paid throughout his life to have “fun.”

**Rube-Griffinberg inventions** - This term was used by Griffin’s friends and colleagues (and himself) to refer to the many, many contraptions and instruments he invented to solve some technical problems. He began creating such constructions in childhood days, one of the early ones being his teenage invention of a carriage return for his new typewriter.

In a letter to Brock Fenton of January 2003, he writes of the “dreaming and scheming” about research plans for the summer. They (he and Greg Auger who works most nights “batting” with Don) want to “learn what insects our bats are pursuing. One scheme is to borrow one of those leaf collecting vacuum systems (which must be idle during the summer) and attach a long flexible tube that we could extend up into our nice little insect catching “glade” and waffle in insects that look like those we see bats catching. Yet another ‘Rube Griffinberg’ or ‘Rube Augerberg!’” (Griffin, January 2003, BROCK03JAN, unpublished letter to Brock Fenton).
simplicity filters - Griffin’s continuing complaint was scientists’ and, initially, his own, use of “simplicity filters.” Researchers assumed that animals did not have complex abilities; they must be using simple cues and simple processes. He takes an example from the 1930s in which his elders, and he could not accept the possibility that birds might navigate by the sun or stars during homing or migratory flights. Griffin later changed his mind and subsequent research indicated that birds could make simple approximations of sun and star movements across the sky. He has further examples, including the initial highly conservative conclusions by Pierce and himself that bats produced only occasional ultrasonic vocalizing; this was, in fact, an artifact of the highly directional microphone they were using (D. R. Griffin, 1976, QAA1, p. 9-14).

species solipsism - The term refers to the belief, derived from the solipsistic philosophical position, that “no one can ever prove, with logical rigor, that another person is conscious. We can only make inferences.” Most persons do not hold such views. When applied to the presumed lack of consciousness in non-human animals and the lack of scientific inquiry that proceeds from the possibility of consciousness, DRG termed the view, “species solipsism.” (C. A. Ristau, 2005, p. 407)

Taboo - Griffin created a Table of twenty terms that behavioral scientists use when dealing with the possibility of animals’ mental experiences. They ranged from “OK” to “Taboo.” On the “OK” end were concepts such as “Pattern Recognition” and “Neural Template.” Towards the middle, we find “Concept” and “Understanding,” and at “Taboo” sit “Choice,” “Free Will” and “Consciousness.” Griffin was urging and arguing for the “Taboo” (D. R. Griffin, 1981, QAA2, p.113-116).

zoomorphism – See the entry “mechanomorphism” for Griffin (2003, MDpowerpoint, unpublished notes) contrasting the need for a balanced “zoomorphism” instead of either “anthropomorphism” or “mechanomorphism.”
DONALD REDFIELD GRIFFIN’S PUBLICATIONS

DONALD R. GRIFFIN’S BOOKS


---

<table>
<thead>
<tr>
<th>Year</th>
<th>Citation of Journal Article or Book Chapter [adapted from <a href="https://neurotree.org/neurotree/publications.php?pid=1366">https://neurotree.org/neurotree/publications.php?pid=1366</a> with additional citations]</th>
</tr>
</thead>
<tbody>
<tr>
<td>2009</td>
<td><strong>Griffin DR.</strong> Windows on nonhuman minds. In M Webster, A Weekes (Eds.), <em>Process approaches to consciousness in psychology, neuroscience and philosophy of mind</em>. 219-231.</td>
</tr>
</tbody>
</table>

App #6-218
<table>
<thead>
<tr>
<th>Year</th>
<th>Title</th>
<th>Journal/Book Details</th>
</tr>
</thead>
<tbody>
<tr>
<td>2001</td>
<td>Griffin DR. Animals know more than we used to think.</td>
<td><em>Proceedings of the National Academy of Sciences of the United States of America</em>. 98: 4833-4. PMID 11320232 DOI: 10.1073/pnas.091088198</td>
</tr>
<tr>
<td>1998</td>
<td>Griffin DR. From cognition to consciousness</td>
<td><em>Animal Cognition</em>. 1: 3-16. DOI: 10.1007/s1007100500002</td>
</tr>
<tr>
<td>1988</td>
<td>Griffin DR. How I managed to explore the 'magical' senses of bats.</td>
<td><em>Scientist</em>. 2(18), 11-12.</td>
</tr>
<tr>
<td>Year</td>
<td>Author</td>
<td>Title and Details</td>
</tr>
<tr>
<td>------</td>
<td>--------</td>
<td>------------------</td>
</tr>
<tr>
<td>1987</td>
<td>Griffin DR</td>
<td>Phylogenetically widespread &quot;facts-of-life.&quot; <em>Behavioral and Brain Sciences</em>. 10: 667-668. DOI: <a href="https://doi.org/10.1017/S0140525X00055126">10.1017/S0140525X00055126</a></td>
</tr>
<tr>
<td>1986</td>
<td>Griffin DR</td>
<td>Foreword to <em>Dolphin cognition and behavior, A comparative perspective</em>. In RJ Schusterman and FG Wood (Eds.), Lawrence Erlbaum Associates.</td>
</tr>
<tr>
<td>1986</td>
<td>Griffin DR</td>
<td>Foreword to <em>Red Fox, The catlike canine</em> by JD Henry. Smithsonian Institution Press.</td>
</tr>
<tr>
<td>1986</td>
<td>Griffin DR</td>
<td>Review of <em>Communication in the Chiroptera</em>. MB Fenton, J Umiker-Sebeok, TA Sebeok, (Eds.), <em>The Quarterly Review of Biology</em>. 61: 284-284. DOI: <a href="https://doi.org/10.1086/414990">10.1086/414990</a></td>
</tr>
<tr>
<td>1985</td>
<td>Griffin DR</td>
<td>The cognitive dimensions of animal communication. In B Holldobler and M Lindauer (Eds.) <em>Experimental behavioral ecology and sociobiology</em>. Springer. (This is also Vol. 31 of <em>Fortschritte der Zoologie</em>)</td>
</tr>
<tr>
<td>1983</td>
<td>Griffin DR</td>
<td>Thinking about animal thoughts. <em>Behavioral and Brain Sciences</em>. 6: 364. DOI: <a href="https://doi.org/10.1017/S0140525X00016484">10.1017/S0140525X00016484</a></td>
</tr>
<tr>
<td>Year</td>
<td>Author(s)</td>
<td>Title</td>
</tr>
<tr>
<td>------</td>
<td>-----------</td>
<td>-------</td>
</tr>
<tr>
<td>1980</td>
<td>Griffin DR</td>
<td>The early history of research on echolocation. In RG Busnel, JF Fish, (Eds.), <em>Animal sonar systems</em>. Springer.</td>
</tr>
<tr>
<td>1980</td>
<td>Griffin DR</td>
<td>What do animals think? <em>Behavioral and Brain Sciences</em>. 3: 618-620. DOI: 10.1017/S0140525X00007172</td>
</tr>
<tr>
<td>1978</td>
<td>Griffin DR</td>
<td>The sensory physiology of animal orientation. <em>Harvey Lectures</em>. 71: 133-72. PMID 568607</td>
</tr>
<tr>
<td>1978</td>
<td>Griffin DR</td>
<td>Echolocation of extended surfaces. In In K Schmidt-Koenig &amp; WT Keeton (Eds.), <em>Animal migration, navigation, and homing</em> (pp. 143-151). Springer-Verlag.</td>
</tr>
<tr>
<td>1978</td>
<td>Griffin DR</td>
<td>Prospects for a cognitive ethology <em>Behavioral and Brain Sciences</em>. 1: 527-538. DOI: 10.1017/S0140525X00076524</td>
</tr>
<tr>
<td>1977</td>
<td>Griffin DR</td>
<td>Expanding horizons in animal communication behavior. Chapter 2 in TA Sebeok (Ed.), <em>How animals communicate</em>. Indiana University Press,</td>
</tr>
<tr>
<td>1976</td>
<td>Griffin DR</td>
<td>The audibility of frog choruses to migrating birds <em>Animal Behaviour</em>. 24: 421-427. DOI: 10.1016/S0003-3472(76)80052-8</td>
</tr>
<tr>
<td>1976</td>
<td>Griffin DR</td>
<td>A possible window on the minds of animals <em>American Scientist</em>. 64: 530-535.</td>
</tr>
<tr>
<td>1974</td>
<td>Griffin DR, Simmons IA</td>
<td>Echolocation of insects by horseshoe bats <em>Nature</em>. 250: 731-732. DOI: 10.1038/250731a0</td>
</tr>
<tr>
<td>1974</td>
<td>Griffin DR, Hopkins CD</td>
<td>Sounds audible to migrating birds <em>Animal Behaviour</em>. 22: 672-678. DOI: 10.1016/S0003-3472(74)80015-1</td>
</tr>
<tr>
<td>Year</td>
<td>Author(s)</td>
<td>Title</td>
</tr>
<tr>
<td>------</td>
<td>-----------</td>
<td>-------</td>
</tr>
<tr>
<td>1967</td>
<td>Williams TC, Williams JM, Griffin DR.</td>
<td>The homing ability of the neotropical bat Phyllostomus hastatus, with evidence for visual orientation.</td>
</tr>
<tr>
<td>Year</td>
<td>Authors</td>
<td>Title</td>
</tr>
<tr>
<td>------</td>
<td>---------</td>
<td>-------</td>
</tr>
<tr>
<td>1962</td>
<td>Thorpe WH, Griffin DR</td>
<td>Ultrasonic frequencies in bird song</td>
</tr>
<tr>
<td>1962</td>
<td>Thorpe WH, Griffin DR</td>
<td>Lack of ultrasonic components in the flight noise of owls</td>
</tr>
<tr>
<td>1962</td>
<td>Griffin DR, Dunning DC, Cahlander DA, Webster FA</td>
<td>Correlated orientation sounds and ear movements of horseshoe bats</td>
</tr>
<tr>
<td>1962</td>
<td>Webster FA, Griffin DR</td>
<td>The role of the flight membranes in insect capture by bats</td>
</tr>
<tr>
<td>1961</td>
<td>Novick A, Griffin DR</td>
<td>Laryngeal mechanisms in bats for the production of orientation sounds</td>
</tr>
<tr>
<td>1960</td>
<td>Griffin DR, Webster FA, Michael CR</td>
<td>The echolocation of flying insects by bats</td>
</tr>
<tr>
<td>1959</td>
<td>Grinnell AD &amp; Griffin DR</td>
<td>The neurophysiology of audition in bats</td>
</tr>
<tr>
<td>1958</td>
<td>Griffin DR, Grinnell AD</td>
<td>Ability of bats to discriminate echoes from louder noise</td>
</tr>
<tr>
<td>1957</td>
<td>Griffin DR, Gross CG</td>
<td>The orientation of birds</td>
</tr>
<tr>
<td>1957</td>
<td>Hall, JS, Cloutier, RJ, Griffin DR</td>
<td>New longevity records and notes on tooth wear of bats</td>
</tr>
<tr>
<td>1956</td>
<td>Goldsmith T, Griffin DR</td>
<td>Further observations of homing terns</td>
</tr>
<tr>
<td>1953</td>
<td>Griffin DR</td>
<td>Sensory physiology and the orientation of animals</td>
</tr>
<tr>
<td>1953</td>
<td>Griffin DR</td>
<td>High frequency sounds of tropical bats</td>
</tr>
<tr>
<td>1953</td>
<td>Griffin DR</td>
<td>Hearing and acoustic orientation in marine animals</td>
</tr>
<tr>
<td>1953</td>
<td>Griffin DR</td>
<td>Acoustic orientation in the oil bird</td>
</tr>
<tr>
<td>Year</td>
<td>Author</td>
<td>Title</td>
</tr>
<tr>
<td>------</td>
<td>--------</td>
<td>-------</td>
</tr>
<tr>
<td>1951</td>
<td>Griffin DR</td>
<td>Audible and ultrasonic sounds of bats.</td>
</tr>
<tr>
<td>1951</td>
<td>Griffin DR</td>
<td>Acoustical location of insect prey by bats.</td>
</tr>
<tr>
<td>1950</td>
<td>Griffin DR</td>
<td>Foreword.</td>
</tr>
<tr>
<td>1950</td>
<td>Griffin DR</td>
<td>The navigation of bats.</td>
</tr>
<tr>
<td>1948</td>
<td>Griffin DR</td>
<td>Topographic orientation.</td>
</tr>
<tr>
<td>1946</td>
<td>Griffin DR, Wald G</td>
<td>Factors affecting the change in refractive power of the eye at high and low illuminations.</td>
</tr>
<tr>
<td>1946</td>
<td>Griffin DR</td>
<td>Supersonic cries of bats.</td>
</tr>
<tr>
<td>Year</td>
<td>Author(s)</td>
<td>Title</td>
</tr>
<tr>
<td>------</td>
<td>-----------</td>
<td>-------</td>
</tr>
<tr>
<td>1946</td>
<td>Griffin DR, Hubbard, Wald G</td>
<td>The sensitivity of human rod and cone vision to infra-red radiation.</td>
</tr>
<tr>
<td>1946</td>
<td>Griffin DR</td>
<td>Mystery mammals of the twilight.</td>
</tr>
<tr>
<td>1946</td>
<td>Griffin DR</td>
<td>The mechanism by which bats produce supersonic sounds.</td>
</tr>
<tr>
<td>1944</td>
<td>Griffin DR</td>
<td>Echolocation by blind men, bats and radar.</td>
</tr>
<tr>
<td>1944</td>
<td>Griffin DR</td>
<td>The sensory basis of bird navigation.</td>
</tr>
<tr>
<td>1943</td>
<td>Griffin DR</td>
<td>Homing experiments with herring gulls and common terns.</td>
</tr>
<tr>
<td>1940</td>
<td>Griffin DR</td>
<td>Homing experiments with Leach’s petrels.</td>
</tr>
<tr>
<td>1937</td>
<td>Griffin DR, Welsh, JH</td>
<td>Activity rhythms in bats under constant external conditions.</td>
</tr>
<tr>
<td>Year</td>
<td>Author</td>
<td>Title</td>
</tr>
<tr>
<td>------</td>
<td>--------</td>
<td>-------</td>
</tr>
</tbody>
</table>
VIDEO, PHOTOGRAPHIC AND MANUSCRIPT COLLECTIONS

CONCERNING GRIFFIN

1. Videos

**1940 - History of Bat Echolocation.** The film made by Robert Galambos and Donald Griffin, and narrated by Galambos, demonstrates their early experiments with bats avoiding obstacles. The obstacles were rows of thin wires through which the bats successfully navigated. It is in the archives of the Dept of Neuroscience, University of California San Diego. A link to the film on YouTube is Robert Galambos - History of Bat Echolocation.

**Circa 1992-1994?** - Gregory Auger made a video of Griffin's lecture on animal consciousness to an audience at the Sturgis Library in Barnstable, Massachusetts. The sound and visuals are not always as clear as he had hoped.

**2008 - Super Bat.** 48-minute documentary directed by David Korn-Brzoza from the Magelan TV Studios. With specialized photography, the film demonstrates how flying bats use echolocation to catch prey. Extensive footage and portrait of the older Don Griffin at work and talking about bats. Available from Amazon Prime Video for rent or purchase.

**2014 - Secrets and Mysteries of Bats.** Nature Documentary of 48 minutes available on YouTube. Using infra-red and high-speed cameras, and 3-D graphics, the film attempts a “bats-ear” view of its night-time journeys. The film also includes footage of bat scientists, in particular, Donald Griffin.

**2018 – Listening in the Dark** – 43-minute HD video by Maeve Brennan, produced by Laura Shacham for ReelNice. The film circles around the figure of Donald Griffin and his fascination with and discoveries about bats. It emphasizes humans’ intrusions into nature while also probing into some of nature’s mysteries and marvels, particularly that of bat echolocation. Commissioned for the Jerwood/FVU Awards 2018.

2. Photographic Collections

**Janet Abbott and Margaret Griffin (Weldon),** daughters of Donald Griffin, have the most extensive collection of photographs relevant to Don Griffin. They comprise personal and scientific photos deriving primarily from Don’s earlier life extending into graduate school. The photographs are not currently available for use by researchers or the public.

**Ronald Larkin** has a set of photographs taken by himself, relating to work with Don Griffin at Rockefeller University, primarily research into avian navigation using radar. He has generously shared his photos with me for this book.

**Gregory Auger** has photos and a few videos from the time he worked with Don while Griffin was affiliated with the Harvard Field Station in Concord, Massachusetts (post “retirement.”) These deal primarily with the bat studies they were conducting, including, as well, photos of visiting scientists and colleagues at conferences. Greg Auger is very amenable to sharing the photos for use by other researchers and has been most helpful in providing copies for this book.
The Rockefeller Archive Center has photos donated by Donald Griffin, mostly related to research. They were formerly housed in the facilities in Sleepy Hollow, New York, and were available for use by researchers after approval. The RU holdings have been withdrawn from the facility and future access is unknown.

The Rockefeller University has published photographs of Griffin and research by himself and his students in various publications, particularly The Rockefeller University News and Notes. Many are available in the online versions of the publications.

The Princeton Art Museum holds donations from Donald Griffin. These are primarily original photographs taken by Don's grandfather, the well-regarded photographer, Robert Stuart Redfield. The photos include family members and scenic views of Barnstable and Cape Cod, Massachusetts.

Other Colleagues have a few photos of their times with Griffin.

3. Letters and Documents written by Donald R Griffin

Many letters and manuscripts were donated by Donald R. Griffin to The Rockefeller Archives, but since his and other holdings have been removed by Rockefeller University, they are not available to researchers at this time. Ronald Larkin has letters from Griffin written to Ron; Ron has put them in the care of Carolyn A. Ristau who hopes to find a suitable repository where they will be available to researchers. Many other handwritten documents by Griffin are in the possession of Griffin’s daughters, Janet Abbott and Margaret Griffin (Weldon), but are not currently available to others.
SELECTED MEMOIRS ABOUT DONALD R. GRIFFIN - LIST


REFERENCES

for VOLUME THREE of THREE

PART FOUR – REVOLUTIONS AND AFTER

CHAPTER 16 - REVOLUTIONS

CHAPTER 16 - Part One - Cultural and Scientific Revolutions (1960s and ‘70s)


**CHAPTER 16 – Part 2 - Revolution in Ethology and the Birth of Cognitive Ethology**


CHAPTER 16 – Part 3 - Evidence for Animal Consciousness (Griffin’s and Later)


http://digitalcommons.rockefeller.edu/research_profiles/6.


### CHAPTER 17 – A BRIEF FORAY INTO PHILOSOPHICAL ISSUES CONCERNING COGNITIVE ETHOLOGY


Fletcher, G. J. (2013). The scientific credibility of folk psychology. Psychology Press.


Roxborough, C. L. 2016. Putting the folk back in Folk Psychology: The social, cultural, and moral character of Folk Psychology. [Doctoral dissertation, York University, Toronto, Canada]. https://yorkspace.library.yorku.ca/xmlui/bitstream/handle/10315/32284/Roxborough_Craig_L_2016_PhD.pdf?sequence=2&isAllowed=y


CHAPTER 18 - THE RETURN: BEES, BEAVERS, BATS, ELEPHANTS ... AND CONSCIOUSNESS IN BARNSTABLE


Learish, J. (2016). The 24 deadliest animals on Earth, ranked. CNET.


CHAPTER 18 – Part 5 – Back to Bats


Yovel, Y. (2023). Bat echolocation research - An updated history. [Book not yet available in English]


CHAPTER 18 - Part 6 - Conscious Elephants ... And Other Ruminations


**CHAPTER 18 – Part 7 – Consciousness and Ethical Concerns**


De Waal, F. (2016). *Are we smart enough to know how smart animals are?* W. W. Norton & Company.


CHAPTER 19 – A LIFE WELL-LOVED: A BROADER VIEW


**APPENDIX #3 - GRIFFIN’S HARVARD STUDENTS: A FEW BIOGRAPHICAL SKETCHES**


**APPENDIX #5 – GLOSSARY OF “GRIFFIN TERMS”**


**APPENDIX #6 – DONALD REDFIELD GRIFFIN’S PUBLICATIONS**


**APPENDIX #9 – C. A. RISTAU MEMOIR RE DONALD R. GRIFFIN (2005)**

THE AUTHOR:

Dr. Carolyn A. Ristau is a Cognitive Ethologist who had the privilege of working in Donald R. Griffin’s Rockefeller University lab. Those were the times when he was ardently working to develop the new field of Cognitive Ethology he had just created, while also enthusiastically continuing his studies of avian migration and bat echolocation. Carolyn Ristau was impressed by his ideas and dedication. She wanted others to know more of their development, hence this broad-based biography.

Among Dr. Ristau’s projects are extensive reviews of the ape and other species’ language and cognition research. She explored “injury-feigning” and other parental behaviors of plovers, designing field experiments to investigate the cognitive aspects of such activities, previously considered largely reflexive. Her field sites included the Eastern US shores, the grasslands of Colorado, the tundra of Churchill, Manitoba and the Arctic, as well as studies of chimpanzees in Africa. She brings her earlier education in physics to elucidate the technical aspects of echolocation and her later work in developmental and social behavior to inform the animal communication and behavioral studies. Her students at Barnard College of Columbia University and several other universities appreciated her enthusiastic and in-depth teaching in the fields of Psychology and Animal Behavior.

THE BOOK:

This book is more than a biography. It is a tale of a revolutionary scientist who faced not merely opposition, but outright hostility towards his radical ideas about the existence of bats’ echolocation, birds’ use of the sun and moon and other sights and sounds as cues in migration, and his proposition that animals are conscious, can think and experience emotions. One scientist was so incensed over Donald Griffin’s proposition of animal mentality that he publicly termed a Griffin book, the “Satanic Verses of Animal Behavior.” Griffin was repeatedly accused of setting science back, but, of course, he wasn’t; he was initiating new approaches to animal behavior studies.

In this book, we find insights and anecdotes about the animals Don Griffin studied and first-hand reports of field studies and their hardships, frustrations, and exhilarating accomplishments. Forty friends and scientists lend their remembrances and expertise to the narrative. We learn of the research conducted by others in Griffin’s “net” and update the studies. We read previously unpublished writings by Griffin, from his youthful naturalistic journals to his adult musings and research plans about animal consciousness. And we are privy to tales with which Don Griffin, the storyteller, entertained his associates.