Chapter 1
Concepts for Wildlife Science: Theory

1.1 Introduction

We conduct wildlife studies in the pursuit of knowledge. Therefore, an understanding of what knowledge is and how it is acquired is foundational to wildlife science. Adequately addressing this topic is a daunting challenge for a single text because wildlife science is a synthetic discipline that encompasses aspects of a vast array of other academic disciplines. For example, many vibrant wildlife science programs include faculty who study molecular biology, animal physiology, biometrics, systems analysis, plant ecology, animal ecology, conservation biology, and environmental sociology, humanities, education, economics, policy, and law. The primary emphasis of this text is the design of wildlife-related field studies. Those addressing other aspects of wildlife science should find the text useful, but will undoubtedly require additional sources on design. For example, those interested in learning how to design quantitative or qualitative studies of how humans perceive wildlife-related issues will find the excellent texts by Dillman (2007) and Denzin and Lincoln (2005) useful.

The process of designing, conducting, and drawing conclusions from wildlife field studies draws from several disciplines. This process begins and ends with expert biological knowledge that comes from familiarity with the natural history of the system being studied. This familiarity should inspire meaningful questions about the system that are worth pursuing for management purposes or for the sake of knowledge alone. During the design and implementation of studies, this familiarity helps the researcher identify what is feasible with respect to practicality and budget. When the study is completed and the results are analyzed, this familiarity provides the researcher with perspective in drawing conclusions. Familiarity can, however, lead to tunnel vision when viewing the system and thus misses alternative explanations for observed phenomena. Therefore, to conduct wildlife science as objectively as possible, it is usually necessary to temper expert knowledge that comes from familiarity with principles drawn from other academic disciplines. We incorporate these concepts in later chapters that discuss sampling and specific study designs.

In this chapter, we begin by discussing philosophical issues as they relate to science. After all, it makes little sense to begin collecting data before clearly

M.L. Morrison et al., *Wildlife Study Design.*

understanding the nature of the entity being studied (*ontology*), what constitutes knowledge and how it is acquired (*epistemology*), and why one thinks the research question is valuable, the approach ethical, and the results important (*axiology*). Moreover, the philosophy of science provides a logical framework for generating meaningful and well-defined questions based on existing theory and the results of previous studies. It provides also a framework for combining the results of one's study into the larger body of knowledge about wildlife and for generating new questions, thus completing the feedback loop that characterizes science. For these reasons, we outline how scientific methodology helps us acquire valuable knowledge both in general and in specific regarding wildlife. We end the chapter with a brief discussion of terminology relevant to the remaining chapters.

1.2 Philosophy and Science

1.2.1 The Science Wars

In 1987, physicists Theo Theocharis and Michael Psimopoulos (1987) published an essay in *Nature,* where they referred to the preeminent philosophers of science Karl R. Popper, Thomas Kuhn, Imre Lakatos, and Paul Feyerabend as "betrayers of the truth" and Feyerabend as "currently the worst enemy of science" (pp. 596–597). According to Theocharis and Psimopoulos, by admitting an unavoidable social dimension to science, and that human perceptions of reality are to some degree social constructions, these and other philosophers of science working in the 1960s had enabled an avalanche of "erroneous and harmful … epistemological antitheses" of science (p. 595). They argued

> The problem is that although the epistemological antitheses are demonstrably untenable, inherently obscurantist and possibly dangerous, they have become alarmingly popular with the public, and even worse, with the communities of professional philosophers and scientists. (p. 598)

The result, Theocharis and Psimopoulos feared, was that "having lost their monopoly in the production of knowledge, scientists have also lost their privileged status in society" and the governmental largess to which they had become accustomed (p. 597).

Since the 1960s, entire academic subdisciplines devoted to critiquing science, and refereed journals associated with these endeavors, have become increasingly common and influential. The more radical members of this group often are called *postmodernists*. It is probably fair to say, however, that most scientists either were blissfully unaware of these critiques, or dismissed them as so much leftwing academic nonsense.

By the 1990s, however, other scientists began to join Theocharis and Psimopoulos with concerns about what they perceived to be attacks on the validity and value of science. Paul R. Gross and Norman Levitt (1994), with *Higher Superstition: The Academic Left and Its Quarrels with Science*, opened a frontal attack on critical studies of science. They argued that scholars in critical science studies knew little about science and used sloppy scholarship to grind political axes. Both the academic and mainstream press gave *Higher Superstition* substantial coverage, and “the science wars” were on.

In 1995, the New York Academy of Sciences hosted a conference entitled “The Flight from Science and Reason” (see Gross et al. (1997) for proceedings). These authors, in general, were also highly critical of what they perceived to be outrageous, politically motivated postmodern attacks on science. *Social Text*, a critical theory journal, prepared a special 1996 issue titled “Science Wars” in response to these criticisms. Although several articles made interesting points, if the essay by physicist Alan D. Sokal had not been included, most scientists and the mainstream media probably would have paid little attention. Sokal (1996b) purportedly argued that quantum physics supported trendy postmodern critiques of scientific objectivity. He simultaneously revealed elsewhere that his article was a parody perpetrated to see whether the journal editors would “publish an article liberally salted with nonsense if (a) it sounded good and (b) it flattered the editors’ ideological preconceptions” (Sokal 1996a, p. 62). The “Sokal affair,” as the hoax and its aftermath came to be known, brought the science wars to the attention of most scientists and humanists in academia through flurries of essays and letters to editors of academic publications. A number of books soon followed that addressed the Sokal affair and the science wars from various perspectives and with various degrees of acrimony (e.g., Sokal and Bricmont 1998; Koertge 1998; Hacking 1999; Ashman and Barringer 2001). At the same time, the public, no doubt already somewhat cynical about academic humanists and scientists alike, read their fill about the science wars in the mainstream media. Like all wars, there were probably no winners. A more relevant question is the degree to which all combatants lost.

A student of wildlife science might well ask, “How can Karl Popper be one of the more notorious enemies of science” and “If science is an objective, rational enterprise addressing material realities, how can there be any argument about the nature of scientific knowledge, let alone the sometimes vicious attacks seen in the science wars?” These are fair questions. Our discussion of ontology, epistemology, and axiology in science, making up the remainder of Sect. 1.2, should help answer these and related questions and simultaneously serve as a brief philosophical foundation for the rest of the book.

1.2.2 The Nature of Reality

If asked to define reality, most contemporary scientists would probably find the question somewhat silly. After all, is not reality the state of the material universe around us? In philosophy, *ontology* is the study of the nature of reality, being, or existence. Since Aristotle (384–322 B.C.), the *empiricist* tradition of philosophy has held that material reality was indeed largely independent of human thought and best understood through experience. Science is still informed to a large degree through this empiricist perception. *Rationalism*, however, has an equally long tradition in philosophy. Rationalists such as Pythagoras (ca. 582–507 B.C.), Socrates (ca. 470–399 B.C.), and Plato (427/428–348 B.C.) argued that the ideal, grounded in reason, was in many ways more "real" than the material. From this perspective, the criterion for reality was not sensory experience, but instead was intellectual and deductive. Certain aspects of this perspective are still an integral part of modern science. For many contemporary philosophers, social scientists, and humanists, however, reality is ultimately a social construction (Berger and Luckmann 1966). That is, reality is to some degree contingent upon human perceptions and social interactions (Lincoln and Guba 1985; Jasinoff et al. 1995). While philosophers voiced arguments consistent with *social constructionism* as far back as the writing of Heraclitus (ca. 535–475 B.C.), this perspective toward the nature of being became well established during the mid-twentieth century.

Whatever the precise nature of reality, *knowledge* is society's accepted portrayal of it. Over the centuries, societies have – mistakenly or not – accessed knowledge through a variety of methods, including experience, astrology, experimentation, religion, science, and mysticism (Rosenberg 2000; Kitcher 2001). Because the quest for knowledge is fundamental to wildlife science, we now flesh out the permutations of knowing and knowledge acquisition.

1.2.3 Knowledge

What is knowledge, how is knowledge acquired, and what is it that we know? These are the questions central to *epistemology*, the branch of Western philosophy that studies the nature and scope of knowledge. The type of knowledge typically discussed in epistemology is propositional, or "knowing-that" as opposed to "knowing-how," knowledge. For example, in mathematics, one "knows that" 2 + 2 = 4, but "knows how" to add.

In Plato's dialogue *Theaetetus* (Plato [ca. 369 B.C.] 1973), Socrates concluded that *knowledge* was justified true belief. Under this definition, for a person to know a proposition, it must be true and he or she must simultaneously believe the proposition and be able to provide a sound justification for it. For example, if your friend said she knew that a tornado would level her house in exactly 365 days, and the destruction indeed occurred precisely as predicted, she still would not have known of the event 12 months in advance because she could not have provided a rational justification for her belief despite the fact that it turned out later to be true. On the other hand, if she said she knew a tornado would level her house sometime within the next 20 years, and showed you 150 years of records indicating that houses in her neighborhood were severely damaged by tornadoes approximately every 20 years, her statement would count as knowledge and tornado preparedness might be in order. This definition of knowledge survived without serious challenge by philosophers for thousands of years. It is also consistent with how most scientists perceive knowledge today.

Knowledge as justified true belief became a less adequate definition in the 1960s. First, Edmund L. Gettier (1963), in a remarkably brief paper (less than 3 pages), provided what he maintained were examples of beliefs that were both true and justified, but that should not be considered knowledge. In his and similar examples, the justified true belief depended on either false premises or justified false beliefs the protagonist was unaware of (see Box 1.1). Philosophers have been wrestling with the "Gettier problem" since then, and are yet to agree on a single definition of knowledge. A second problem with Plato's definition relates to ontology. If reality is to any degree socially constructed, then truth regarding this reality is to the same degree a social construct, and so society's accepted portrayal of a proposition – whether justified true belief or not – becomes a more relevant definition. At any rate, wildlife scientists attempt to acquire knowledge about wild animals, wildlife populations, and ecological systems of interest, and apply that knowledge to management and conservation, and so they must understand the nature of knowledge and knowledge acquisition.

Box 1.1 The Gettier Problem

Gettier (1963, pp. 122–123) provided the following two examples to illustrate the insufficiency of justified true belief as the definition of knowledge (see Plato [ca. 369 B.C.]1973).

Case I:

Suppose that Smith and Jones have applied for a certain job. And suppose that Smith has strong evidence for the following conjunctive proposition:

> (d) Jones is the man who will get the job, and Jones has ten coins in his pocket.
>
> Smith's evidence for (d) might be that the president of the company assured him that Jones would in the end be selected, and that he, Smith, had counted the coins in Jones's pocket ten minutes ago. Proposition (d) entails:
>
> (e) The man who will g et the job has ten coins in his pocket.
>
> Let us suppose that Smith sees the entailment from (d) to (e), and accepts (e) on grounds of (d), for which he has strong evidence. In this case, Smith is clearly justified in believing that (e) is true.

But imagine, further, that unknown to Smith, he himself, not Jones, will get the job. And, also, unknown to Smith, he himself has ten coins in his pocket. Proposition (e) is then true, though proposition (d), from which Smith inferred (e), is false. In our example, then, all of the following are true: (*i*) (e) is true, (*ii*) Smith believes that (e) is true, and (*iii*) Smith is justified in believing that (e) is true. But it is equally clear that Smith does not *know* that (e) is true; for (e) is true in virtue of the number of coins in Smith's pocket, while Smith does not know how many coins are in Smith's pocket, and bases his belief in (e) on a count of the coins in Jones's pocket, whom he falsely believes to be the man who will get the job.

(continued)

Box 1.1 (continued)

Case II:
Let us suppose that Smith has strong evidence for the following proposition:

(f) Jones owns a Ford.

Smith's evidence might be that Jones has at all times in the past within Smith's memory owned a car, and always a Ford, and that Jones has just offered Smith a ride while driving a Ford. Let us imagine, now, that Smith has another friend, Brown, of whose whereabouts he is totally ignorant. Smith selects three place names quite at random, and constructs the following three propositions:

(g) Either Jones owns a Ford, or Brown is in Boston;

(h) Either Jones owns a Ford, or Brown is in Barcelona;

(i) Either Jones owns a Ford, or Brown is in Brest-Litovsk.

Each of these propositions is entailed by (f). Imagine that Smith realizes the entailment of each of these propositions he has constructed by (f), and proceeds to accept (g), (h), and (i) on the basis of (f). Smith has correctly inferred (g), (h), and (i) from a proposition for which he has strong evidence. Smith is therefore completely justified in believing each of these three propositions. Smith, of course, has no idea where Brown is.

But imagine now that two further conditions hold. First, Jones does *not* own a Ford, but is at present driving a rented car. And secondly, by the sheerest coincidence, and entirely unknown to Smith, the place mentioned in proposition (h) happens really to be the place where Brown is. If these two conditions hold then Smith does *not* know that (h) is true, even though (*i*) (h) is true, (*ii*) Smith does believe that (h) is true, and (*iii*) Smith is justified in believing that (h) is true.

1.2.3.1 Knowledge Acquisition

Beginning with the Age of Enlightenment (seventeenth and eighteenth centuries), the *empiricist* tradition of inquiry exhibited new vigor. Important thinkers associated with the maturation of empiricism include Francis Bacon (1561–1626), John Locke (1632–1704), David Hume (1711–1776), and John Stuart Mill (1806–1873). From the empiricist perspective, we acquire knowledge only through experience, particularly as gained by observations of the natural world and carefully designed experiments. Thus, from a pure empiricist perspective, humans cannot know except by experience. Experience, however, can mean more than just counting or measuring things. For example, we can know by our senses that a fire is hot without measuring its precise temperature. Thus, physically sensing a phenomenon and employing metrics designed to quantify the magnitude of the phenomenon are both experiential.

Also during this period, philosophers informed by the *rationalist* tradition were busily honing their epistemological perspective. René Descartes (1596–1650), Baruch Spinoza (1632–1677), Gottfried Leibniz (1646–1716), and others are often associated with this epistemological tradition and were responsible for integrating mathematics into philosophy. For rationalists, reason takes precedence over experience for acquiring knowledge and, in principle, all knowledge can be acquired through reason alone. In practice, however, rationalists realized this was unlikely except in mathematics.

Philosophers during the Classical era probably would not have recognized any crisp distinction between empiricism and rationalism. The seventeenth century debate between Robert Boyle (1627–1691) and Thomas Hobbes (1588–1679) regarding Boyle's air pump experiments and the existence of vacuums fleshed out this division (Shapin and Schaffer 1985). Hobbes argued that only self-evident truths independent of the biophysical could form knowledge, while Boyle promoted experimental verification, where knowledge was reliably produced in a laboratory and independent of the researcher (Latour 1993). Even in the seventeenth century, many rationalists found empirical science important, and some empiricists were closer to Descartes methodologically and theoretically than were certain rationalists (e.g., Spinoza and Leibniz). Further, Immanuel Kant (1724–1804) began as rationalist, then studied Hume and developed an influential blend of rationalist and empiricist traditions. At least two important combinations of empiricism and certain aspects of rationalism followed.

One of these syntheses, *pragmatism*, remains the only major American philosophical movement. Pragmatism originated with Charles Saunders Peirce (1839–1914) in the early 1870s and was further developed and popularized by William James (1842–1910), John Dewey (1859–1952), and others. Peirce, James, and Dewey all were members of The Metaphysical Club in Cambridge, Massachusetts, during the 1870s and undoubtedly discussed pragmatism at length. Their perspectives on pragmatism were influenced by Kant, Mill, and Georg W.F. Hegel (1770–1831), respectively (Haack and Lane 2006, p. 10), although other thinkers such as Bacon and Hume were undoubtedly influential as well. James perceived pragmatism as a synthesis of what he termed the "tough-minded empiricist" (e.g., "materialistic, pessimistic, … pluralistic, skeptical"), and "tender-minded rationalist" (e.g., "idealistic, optimistic, … monistic, dogmatical") traditions of philosophy (1907, p. 12). Similarly, Dewey argued that pragmatism represented a marriage between the best of empiricism and rationalism (Haack 2006, pp. 33–40). James (1912, pp. 41–44) maintained the result of this conjunction was a "radical empiricism" that must be directly experienced. As he put it,

> To be radical, an empiricism must neither admit into its constructions any element that is not directly experienced, nor exclude from them any element that is directly experienced. … a real place must be found for every kind of thing experienced, whether term or relation, in the final philosophic arrangement. (p. 42)

To these classical pragmatists, at least, the merits of even experimentation and observation were weighed by direct experience. Pragmatism is one of the most active fields of philosophy today. For this reason, there are several versions of neopragmatism that differ in substantive ways from the classical pragmatism of Peirce, James, Dewey, or George H. Mead (1863–1931). However, philosophers who consider themselves pragmatists generally hold that truth, knowledge, and theory are inexorably connected with practical consequences, or real effects.

The other important philosophical blend of empiricism and rationalism, *logical positivism* (later members of this movement called themselves *logical empiricists*), emerged during the 1920s and 1930s from the work of Moritz Schlick (1882–1936) and his Vienna Circle, and Hans Reichenbach (1891–1953) and his Berlin Circle (Rosenberg 2000). Logical positivists maintain that a statement is meaningful only if it is (1) analytical (e.g., mathematical equations) or (2) can reasonably be verified empirically. To logical positivists, *ethics* and *aesthetics*, for example, are *metaphysical* and thus scientifically meaningless because one cannot evaluate such arguments analytically or empirically. A common, often implicit assumption of those informed by logical positivism is that given sufficient ingenuity, technology, and time, scientists can ultimately come to understand material reality in all its complexity. Similarly, the notion that researchers should work down to the ultimate elements of the system of interest (to either natural or social scientists), and then build the causal relationships back to eventually develop a complete explanation of the universe in question, tends to characterize logical positivism as well. The recently completed mapping of the human genome and promised medical breakthroughs related to this genomic map characterizes this tendency.

The publication of Karl R. Popper's (1902–1994) *Logik der Forschung* by the Vienna Circle in 1934 (given a 1935 imprint) called into question the sufficiency of logical positivism. After the chaos of WWII, Popper translated the book into English and published it as *The Logic of Scientific Discovery* in 1959. Popper's (1962) perspectives were further developed in *Conjectures and Refutations: The Growth of Scientific Knowledge*. Unlike most positivists, Popper was not concerned with distinguishing meaningful from meaningless statements or verification, but rather distinguishing scientific from metaphysical statements using falsification. For him, metaphysical statements were unfalsifiable, while scientific statements could potentially be falsified. On this basis, scientists should ignore metaphysical contentions; instead, they should deductively derive tests for *hypotheses* that could lead to falsification. This approach often is called the *hypothetico–deductive model* of science. Popper argued that hypotheses that did not withstand a rigorous test should immediately be rejected and researchers should then move on to alternatives that were more productive. He acknowledged, however, that metaphysical statements in one era could become scientific later if they became falsifiable (e.g., due to changes in technology). Under Popper's model of science, while material reality probably exists, the best scientists can do is determine what it is not, by systematically falsifying hypotheses related to the topic of interest. Thus, for Popperians, knowledge regarding an issue is approximated by the explanatory hypothesis that has best survived substantive experimental challenges to date. From this perspective, often called *postpositivism*, knowledge ultimately is conjectural and can be modified based on further investigation.

Physicist Thomas Kuhn (1922–1996), in *The Structure of Scientific Revolutions* (1962), also argued that because science contains a social dimension it does not operate under the simple logical framework outlined by the logical positivists. His publication originally was part of the *International Encyclopedia of Unified Science* begun by the Vienna Circle. Kuhn's model of science includes "*normal science*," or periods where there is general consensus within a scientific community regarding theory, methods, terminology, and types of experiments likely to contribute useful insights. He argued that although advances occur during normal science, they are typically incremental in nature. Normal science at some point is interrupted by "*revolutionary science*," where a shift in *paradigm* occurs, followed by a new version of normal science, and eventually another paradigm shift, and so on. Kuhn argued that transition from an old to a new paradigm is neither rapid nor seamless, largely because the two paradigms are incommensurable. That is, a paradigm shift is not just about transformation of theory, but includes fundamental changes in terminology, how scientists perceive their field, and perhaps most importantly, what questions are deemed valid and what decision rules and methodological approaches are determined appropriate for evaluating scientific concepts. Thence new paradigms are not simply extensions of the old, but radically new worldviews, or as he put it, "*scientific revolutions*." Despite the importance of societal influences, Kuhn's model of science resonated with scientists because it provided a workable explanation for the obvious revolutionary changes observed historically in science (e.g., ecology from the Linnaean versus Darwinian perspective; Worster 1994).

Imre Lakatos (1922–1974) attempted to resolve the perceived conflict between Popper's falsification and Kuhn's revolutionary models of science in *Falsification and the Methodology of Scientific Research Programmes* (1970). He held that groups of scientists involved in a research program shielded the theoretical core of their efforts with a "protective belt" of hypotheses related to their central area of inquiry. Hypotheses within this belt could be found inadequate while the core theoretical construct remained protected from falsification. This approach protected the core ideas from premature rejection due to anomalies or other problems, something many viewed as a shortcoming of Popper's model of science. Under Lakatos' model, the question is not whether a given hypothesis is false, but whether a research program is progressive (marked by growth and discovery and potentially leading to a shift in paradigm) or degenerative (marked by lack of growth and novel facts and leading to oblivion).

Paul Feyerabend (1924–1994) took the cultural aspects of science further. In *Against Method* and *Science in a Free Society* (Feyerabend 1975, 1978, respectively), he argued that there was no single prescriptive scientific method, that such a method – if it existed – would seriously limit scientists and thus scientific progress, and that science would benefit from theoretical anarchism in large part because of its obsession with its own mythology. Feyerabend maintained that Lakatos' philosophy of research programs was actually "anarchism in disguise" (Feyerabend 1975, p. 14), because it essentially argued that there was no single, prescriptive scientific method. Feyerabend also challenged the notion that scientists or anyone else could objectively compare scientific theories. After all, as Kuhn had previously pointed out, scientific paradigms were incommensurable and so they were incomparable by definition. Feyerabend went on to argue that the condescending attitudes many scientists exhibited toward astrology, voodoo, folk magic, or alternative medicine had more to do with elitism and racism than to the superiority of science as an epistemological approach.

While Popper placed a small wedge in the door of natural science's near immunity to social criticism, Kuhn, Lakatos, Feyerabend, and other philosophers of science working in the 1960s and 1970s tore it from its hinges. Those interested in learning about these philosophies should read *Criticism and the Growth of Knowledge* (Lakatos and Musgrave 1970). This volume is based on a 1965 symposium, chaired by Popper, where the leading philosophers of science, including Popper, Lakatos, and Feyerabend, critiqued Kuhn's revolutionary model of science, and he responded to their criticisms. Similarly, Lakatos and Feyerabend's (1999) posthumous work, *For and Against Method*, further clarifies these authors' perspectives toward the philosophy of natural science from 1968 through 1974 (Lakatos died in February 1974). Whatever the merit of these philosophies, the juggernaut of critical studies of science, grounded in constructivist epistemology, had been unleashed.

Social constructivism is based on the philosophical perspective that all knowledge is ultimately a social construction regardless of whether material reality exists (Berger and Luckmann 1966; Lincoln and Guba 1985). After all, humans cannot escape being human; they know only through the lens of experience, perception, and social convention. For this reason, our individual and collective perspectives toward race, ethnicity, gender, sexuality, and *anthropocentrism*, to name only a few, form an important component of our knowledge on any topic, including science. Although the thinking of Hegel, Karl Marx (1818–1883), and Émile Durkheim (1858–1917) were important to the development of constructivism, Peter L. Berger and Thomas Luckmann's (1966) *The Social Construction of Reality* greatly enhanced the prominence of social constructionism, particularly in the United States. Constructionist critiques make use of *dialectic approaches*, or discussion and reasoning by logical dialogue, as the method of intellectual investigation. There are now an imposing array of subdisciplines and related academic journals in the humanities and social sciences informed by social constructionism that are dedicated to the critical study of science. These include ethnographic accounts of science, feminist studies of science, the rhetoric of science, and social studies of science. For those who take epistemology seriously, social constructivism has moved into a mainstream position from where it functions as an integrator for researchers working from empiricist, rationalist, pragmatist, and logical positivist perspectives.

1.2.3.2 Inductive, Deductive, and Retroductive Reasoning

Regardless of the epistemological approach one is informed by, logical thought remains an integral component of the process. During the Classical era, Aristotle and others developed important aspects of logical reasoning (Table 1.1). *Induction* con-

Table 1.1 The purpose, logical definition, and verbal description of inductive, deductive, and retroductive reasoning, given the preconditions α, postconditions β, and the rule R_1: $\alpha \rightarrow \beta$ (α therefore β; after Menzies 1996)

Method	Purpose	Definition[a,b]	Description
Induction	Determining R_1	$\alpha \rightarrow \beta \Rightarrow R_1$	Learning the rule (R_1) after numerous examples of α and β
Deduction	Determining β	$\alpha \wedge R_1 \Rightarrow \beta$	Using the rule (R_1) and its preconditions (α) to deterministically make a conclusion (β)
Retroduction	Determining α	$\beta \wedge R_1 \Rightarrow \alpha$	Using the postcondition (β) and the rule (R_1) to hypothesize the preconditions (α) that could best explain the observed postconditions (β)

[a] $\rightarrow$, $\wedge$, and $\Rightarrow$ signify "therefore," "and," and "logically implies," respectively

[b] Note that deduction and retroduction employ the same form of logical statement to determine either the post- or precondition, respectively

sists of forming general conclusions based on multiple instances, where a class of facts appears to entail another [e.g., each of thousands of common ravens (*Corvus corax*) observed were black, therefore all common ravens are black]. Stated differently, we believe the premises of the argument support the conclusion, but they cannot ensure it, and the strength of the induction depends in part on how large and representative the collection of facts is that we have to work with. *Deduction* consists of deriving a conclusion necessitated by general or universal premises, often in the form of a *syllogism* [e.g., all animals are mortal, northern bobwhites (*Colinus virginianus*) are animals, therefore northern bobwhites are mortal]. If the premises indeed are true, then the conclusion by definition must be true as well. Although philosophers of previous generations often perceived induction and deduction to be competing methods of reasoning, Peirce demonstrated that they actually were complementary (Haack 2006). Essentially, inductively derived general rules serve as the basis for deductions; similarly, should the deductive consequences turn out experimentally other than predicted, then the inductively derived general rule is called into question. Peirce also proposed a third type of logical reasoning he initially called *abduction*; he later referred to this concept as *retroduction* (retroduction hereafter; some philosophers argue that retroduction is a special case of induction and others argue that Peirce did not always use abduction and retroduction synonymously). Retroduction consists of developing a hypothesis that would, if true, best explain a particular set of observations (Table 1.1). Retroductive reasoning begins with a set of observations or facts, and then infers the most likely or best explanation to account for these facts (e.g., all the eggs in a northern bobwhite nest disappeared overnight and there were no shell fragments, animal tracks, or disturbance of leaf litter at the nest site, therefore a snake is the most likely predator).

All three forms of reasoning are important epistemologically. Retroductive reasoning is in many ways the most interesting because it is much more likely to result in novel explanations for puzzling phenomena than are induction or deduction. It is also much more likely to be wrong! Inductive reasoning is an effective way to derive important principles of association and is less likely to prove incorrect than retroduction. It has been the workhorse of science for centuries. Deductively derived conclusions are uninteresting in themselves; after all, they follow deterministically from the major premise. Instead, the value of deductive reasoning is that it allows us to devise ways to critically challenge and evaluate retroductively developed hypotheses or inductively derived rules of association.

1.2.4 Values and Science

Axiology is the study of value or quality. The nature, types, and criteria of values and value judgments are critical to science. At least three aspects of value are directly relevant to our discussion: (1) researcher ethics, (2) personal values researchers bring to science, and (3) how we determine the quality of research.

Ethics in science runs the gambit from humane and appropriate treatment of animal or human subjects to honesty in recording, evaluating, and reporting data. Plagiarism, fabrication and falsification of data, and misallocation of credit by scientists are all too often news headlines. While these ethical problems are rare, any fraud or deception by scientists undermines the entire scientific enterprise. Ethical concerns led the National Academy of Sciences (USA) to form the Committee on the Conduct of Science to provide guidelines primarily for students beginning careers in scientific research (Committee on the Conduct of Science 1989). All graduate students should read the updated and expanded version of this report (Committee on Science, Engineering, and Public Policy 1995). It also serves as a brief refresher for more seasoned scientists.

Perhaps two brief case studies will help put bookends around ethical issues and concerns in science. The first involves Hwang Woo-suk's meteoric rise to the pinnacle of fame as a stem-cell researcher, and his even more rapid fall from grace. He and his colleagues published two articles in *Science* reporting truly remarkable results in 2004 and 2005. These publications brought his laboratory, Seoul National University, and South Korea to the global forefront in stem cell research, and Professor Hwang became a national hero nearly overnight. The only problem was that Woo-suk and his coauthors fabricated data used in the two papers (Kennedy 2006). Additional ethical problems relating to sources of human embryos also soon surfaced. In less than a month (beginning on 23 December 2005), a governmental probe found the data were fabricated, Dr. Hwang admitted culpability and resigned his professorship in disgrace, and the editors of *Science* retracted the two articles with an apology to referees and those attempting to replicate the two studies. This episode was a severe disgrace for Professor Hwang, Seoul National University, the nation of South Korea, and the entire scientific community.

Although breaches of ethics similar to those in the previous example receive considerable media attention and near universal condemnation, ethical problems in science often are more insidious and thence less easily recognized and condemned. Wolff-Michael Roth and Michael Bowen (2001) described an excellent example of the latter. They used ethnographic approaches to explore the enculturation process of upper division undergraduate and entry level graduate student researchers beginning their careers in field ecology. These students typically had little or no direct supervision at their study areas and had to grapple independently with the myriad problems inherent to fieldwork. Although they had reproduced experiments as part of highly choreographed laboratory courses (e.g., chemistry), these exercises probably were more a hindrance than a help. In these choreographed exercises, the correct results were never in doubt, only the students' ability to reproduce them was in question. Roth and Bowen found that the desire to obtain the "right" or expected results carried over to fieldwork. Specifically, one student was to replicate a 17-year old study. He had a concise description of the layout, including maps. Unfortunately, he was unable to interpret the description and maps well enough to lay out transects identical to those used previously, despite the fact that most of the steel posts marking the original transects still were in place (he overlooked the effects of topographical variation and other issues). He knew the layout was incorrect, as older trees were not where he expected them to be. Instead of obtaining expert assistance and starting over, he bent "linear" transects to make things work out, assumed the previous researcher had incorrectly identified trees, and that published field guides contained major errors. " 'Creative solutions,' 'fibbing,' and differences that 'do not matter' characterized his work …" (p. 537). He also hid a major error out of concern for grades. As he put it

> I am programmed to save my ass. And saving my ass manifests itself in getting the best mark I can by compromising the scruples that others hold dear….That's what I am made of. That is what life taught me. (p. 543)

Of course, his "replication" was not a replication at all, but this fact would not be obvious to anyone reading a final report. Roth and Bowen (2001) concluded that

> … the culture of university ecology may actually encourage students to produce 'creative solutions' to make discrepancies disappear. The pressures that arise from getting right answers encourage students to 'fib' and hide the errors that they know they have committed. (p. 552)

While this example of unethical behavior by a student researcher might not seem as egregious as the previous example, it actually is exactly the same ethical problem; both researchers produced data fraudulently so that their work would appear better than it actually was for purposes of self-aggrandizement.

Another important axiological area relates to the values researchers bring to science. For example, Thomas Chrowder Chamberlin (1890; 1843–1928) argued that scientists should make use of multiple working hypotheses to help protect themselves from their own biases and to ensure they did not develop tunnel vision. John R. Platt (1964) rediscovered Chamberlin's contention and presented it to a new generation of scientists (see Sect. 1.4.1 for details). That researchers' values impinge to some degree upon their science cannot be doubted. This is one of the reasons philosophers such as Kuhn, Lakatos, and Feyerabend maintained there were cultural aspects of science regardless of scientists' attempts to be "objective" and "unbiased" (see Sect. 1.2.3.1). Moreover, scientists' values are directly relevant to social constructionism and thence critical studies of science.

Finally, how do scientists determine whether knowledge they are developing, however they define such knowledge, matters? To whom does it matter (e.g., themselves, colleagues, some constituency, society)? How do scientists and the public determine whether science is of high quality? These also are axiological questions without clear answers. Even within the scientific community, researchers debate the answers (e.g., the merits of basic versus applied science). Such judgments hinge on one's values, and thus are axiological. To complicate matters further, these questions must be answered at multiple scales. One person might think that science that actually changes things on the ground to directly benefit wildlife conservation, for example, is the most valuable sort of scientific inquiry. If this person is an academician, however, he or she cannot safely ignore what colleagues working for funding agencies, peer reviewed journals, or tenure and promotion committees perceive to be valuable work. We could make the same sort of argument for scientists who maintain that theoretical breakthroughs are the ultimate metric of quality in science. Moreover, society is influenced by, and influences, these value judgments. Society ultimately controls the purse strings for governmental, industrial, and nongovernmental organizations and thus indirectly, scientific funding. In sum, the quality of scientific knowledge is important to scientists and nonscientists alike, and social influence on the scientific process – at whatever scale – is axiomatic.

1.3 Science and Method

In a general sense, science is a process used to learn how the world works. As discussed in Sect. 1.2, humans have used a variety of approaches for explaining the world around them, including mysticism, religion, sorcery, and astrology, as well as science. The scientific revolution propelled science to the forefront during the last few centuries, and despite its shortcomings, natural science (the physical and life sciences) has been remarkably effective in explaining the world around us (Haack 2003). What is it about the methods of natural science that has proven so successful? Here, we address this question for natural sciences in general and wildlife science in particular. We begin this task by discussing research studies designed to evaluate *research hypotheses* or *conceptual models*. We end this section by contextualizing how *impact assessment* and studies designed to *inventory* or *monitor* species of interest fit within the methods of natural science. The remainder of the book addresses specifics as they apply to wildlife science.

1.3.1 Natural Science Research

We avoided the temptation to label Sect. 1.3 "The Scientific Method." After all, as Sect. 1.2 amply illustrates, there is no single philosophy, let alone method, of science. As philosopher of science Susan Haack (2003, p. 95) put it, "Controlled

Table 1.2 Typical steps used in the process of conducting natural science

1	Observe the system of interest
2	Identify a broad research problem or general question of interest
3	Conduct a thorough review of the refereed literature
4	Identify general research objectives
5	In light of these objectives, theory, published research results, and possibly a pilot study, formulate specific research hypotheses and/or a conceptual model
6	Design (1) a manipulative experiment to test whether conclusions derived deductively from each research hypothesis are supported by data or (2) another type of study to evaluate one or more aspects of each hypothesis or the conceptual model
7	Obtain peer reviews of the research proposal and revise as needed.
8	Conduct a pilot study if needed to ensure the design is practicable. If necessary, circle back to steps 6 or 5
9	Conduct the study
10	Analyze the data
11	Evaluate and interpret the data in light of the hypotheses or model being evaluated. Draw conclusions based on data evaluation and interpretation as well as previously published literature
12	Publish results in refereed outlets and present results at scientific meetings
13	In light of the results and feedback from the scientific community, circle back and repeat the process beginning with steps 5, 4, or even steps 3, 2, or 1, as appropriate

experiments, for example – sometimes thought of as distinctive of the sciences – aren't used by all scientists, or only scientists; astronomers and evolutionary theorists don't use them, but auto mechanics, plumbers, and cooks do." The lack of a single, universal scientific method, however, does not imply that the natural sciences do not employ certain intellectual and methodological approaches in common. Here, we discuss general steps (Table 1.2) and the feedback process typically used during research in the natural sciences.

Readers should not take the precise number of steps we presented in Table 1.2 too literally. Others have suggested taxonomies for scientific research with as few as 4 and as many as 16 steps (e.g., Platt 1964; Ford 2000; Garton et al. 2005). These differences are typically matters of lumping or splitting to emphasize points the authors wished to make. Instead, readers should focus on (1) the importance of familiarity with the system of interest, the question being addressed, and the related scientific literature, (2) the role of research hypotheses and/or conceptual models and how they relate to theory and objectives, (3) appropriate study design, execution, and data analysis, (4) obtaining feedback from other scientists at various stages in the process, including through publication in referred outlets, and (5) the circular nature of science.

Step 1 (Table 1.2) is an obvious place to start because progress in science begins with researchers becoming familiar with the system of interest. This helps one to identify a research area of interest as well as develop important questions to be answered. One way to enhance this familiarity is to conduct a thorough review of the relevant scientific literature. This facilitates a better understanding of existing *theory* and previous research results relevant to the system and research objectives. By making numerous observations over time and studying similar systems in the scientific literature, one can inductively derive rules of association among classes of facts based on theory regarding how some aspect of the system works (see Guthery (2004) for a discussion of facts and science). Similarly, one can retroductively derive hypotheses that account for interesting phenomena observed (see Guthery et al. (2004) for a discussion of hypotheses in wildlife science). The development of research hypotheses and/or conceptual models that explain observed phenomena is a key attribute of the scientific process.

Step 6 (Table 1.2) is the principal topic of this book; we discuss the details in subsequent chapters. In general, one either designs a manipulative experiment to test whether conclusions derived deductively from one or more research hypotheses are supported by data, or designs another type of study to evaluate one or more aspects of each hypothesis or conceptual model. There are basic principles of design that are appropriate for any application, but researchers must customize the details to fit specific objectives, the scope of their study, and the system or subsystem being studied. It is critically important at this juncture to formally draft a research proposal and have it critically reviewed by knowledgeable peers. It is also important to consider how much effort will be required to achieve the study objectives. This is an exercise in approximation and requires consideration of how the researcher will analyze collected data, but can help identify cases where the effort required is beyond the capabilities and budget of the investigator, and perhaps thereby prevent wasted effort. *Pilot studies* can be critical here; they help researchers determine whether data collection methods are workable and appropriate, and also serve as sources of data for sample size calculations. We consider sample size further in Sect. 2.5.7.

Once the design is evaluated and revised, the researcher conducts the study and analyzes the resulting data (steps 9–10, Table 1.2). In subsequent chapters, we discuss practical tips and pitfalls in conducting wildlife field studies, in addition to general design considerations. We do not emphasize analytic methods because an adequate exposition of statistical methodology is beyond the scope of this book. Regardless, researchers must consider some aspects of statistical inference during the design stage. In fact, the investigator should think about the entire study process, including data analysis and even manuscript preparation (including table and figure layout), in as much detail as possible from the beginning. This will have implications for study design, especially sampling effort.

On the basis of the results of data analysis, predictions derived from the hypotheses or conceptual models are compared against the results, and interpretations are made and conclusions drawn (step 11, Table 1.2). The researcher then compares and contrasts these results and conclusions with those of similar work published in the refereed literature. Researchers then must present their results at professional meetings and publish them in refereed journals. A key aspect of science is obtaining feedback from other scientists. It is difficult to adequately accomplish this goal without publishing in scientific journals. Remember, if a research project was worth conducting in the first place, the results are worth publishing in a refereed outlet. We hasten to add that sometimes field research studies, particularly, do not work out as planned. This fact does not necessarily imply that the researcher did not learn something useful or that the effort was unscientific. In subsequent chapters, we discuss ways to salvage field studies that went awry. Similarly, some management-oriented studies do not lend themselves to publication in refereed outlets (see Sect. 1.3.2 for more details).

This brings us to possibly the single most important aspect of the scientific process, the feedback loop inherent to scientific thinking (step 13, Table 1.2). Once researchers complete a study and publish the results, they take advantage of what they learned and feedback from the scientific community. They then use this new perspective to circle back and repeat the process beginning with steps 5, 4, or possibly even steps 3, 2, or 1 (Table 1.2). In other words, researchers might need to begin by formulating new hypotheses or by modifying conceptual models addressing the same objectives used previously. In some cases, however, they might need to rethink the objectives or conduct additional literature reviews and descriptive studies. This reflexive and reflective thinking is the essence of science.

Although a broad research program typically uses all the steps outlined in Table 1.2 and discussed above, not all individual research projects or publications necessarily do so. Instead, different researchers often address different aspects of the same research program. For example, the landmark publications on the equilibrium theory of island biogeography by Robert H. MacArthur and Edward O. Wilson (1967) and MacArthur (1972) focused primarily on steps 1–5 (Table 1.2). They conducted thorough literature reviews and used the results of numerous observational studies to develop their theoretical perspective. From it, they deductively derived four major predictions. Experimental tests and other evaluations of these predictions were left primarily to others (e.g., Simberloff and Wilson 1969; Wilson and Simberloff 1969; Diamond 1972; Simberloff 1976a,b; Wilcox 1978; Williamson 1981). These and other publications provided feedback on equilibrium theory. At a more practical level, this continuously modified theoretical perspective toward the nature of islands still informs protected area design, linkage, and management, because wildlife refuges and other protected areas are increasingly becoming islands in seas of cultivation, urban sprawl, or other *anthropogenic* landscape changes (see Diamond 1975, 1976; Simberloff and Abele 1982; Whittaker et al. 2005). The point here is that not all useful research projects must employ all 13 of our steps (Table 1.2). Some might produce descriptive data that other researchers use to develop theoretical breakthroughs, while other researchers experimentally test or otherwise evaluate theoretically driven hypotheses, and still others could employ this information to produce important syntheses that close the feedback loop or support specific applications.

1.3.2 Impact Assessment, Inventorying, and Monitoring

Natural resource management agencies often implement field studies to collect data needed for management decision making (often required to do so by statute) rather than to test hypotheses or evaluate conceptual models. For example, agencies may need to determine which species of interest occur on a state wildlife management area or another tract of land (*inventory*). They also commonly need to monitor species of interest. After all, it is difficult to know whether management plans designed to increase abundance of an endangered species are effective without reliably *monitoring* the species' abundance over time. Similarly, state wildlife agencies must monitor intensely hunted elk (*Cervus elaphus*) populations if they are to regulate harvest safely and effectively. Further, state or federal management agencies or environmental consulting companies might need to determine the impact of proposed wind plants, highways, or other developments on wildlife or their habitat. Agencies also might want to evaluate the impact of an intense wildfire, a 100-year flood on a riparian area, or a proposed management treatment. Such *impact assessment* often cannot be conducted using replicated manipulative experiments with adequate controls; moreover, one rarely can assign treatments (e.g., floods, wind turbine locations) probabilistically. Despite the limitations of surveys (e.g., inventorying, monitoring) and impact assessment, these are among the most common types of wildlife studies and are important for natural resource management.

These management-oriented studies typically must employ more constrained study designs than those used for "ideal" replicated manipulative experiments. This does not imply, however, that wildlife scientists can safely ignore scientific methodology when designing these studies. Close attention to all details under the biologist's control is still critical. When study planning begins for inventorying, monitoring, and impact assessments, biologists typically already have much of the information listed for steps 1–5 in Table 1.2, although additional review of the literature probably is required. Appropriate study design, the importance of peer reviews of the proposed design, possibly a pilot study, data analysis, and data evaluation are just as important as with other sorts of wildlife research (see Sect. 1.3.1). Some impact assessments and extensive inventories lend themselves to publication in refereed outlets, and steps 12–13 (Table 1.2) follow as outlined in Sect. 1.3.1. In other cases, however, a single impact analysis, an inventory of a state wildlife management area, or the first few years of monitoring data are not suitable for publication in refereed outlets. This does not imply that these data were collected inappropriately or are unimportant. Instead, the purposes for data collection were different. In these cases, however, it is still critical for wildlife scientists to obtain feedback from peers not involved with these projects by presenting results at scientific meetings or via other approaches so that the feedback loop represented by steps 12–13 (Table 1.2) is completed.

Finally, the value of many impact assessments, inventories, and monitoring goes beyond immediate relevance to wildlife management, although this certainly is reason enough to conduct these studies. Researchers interested in complex ecological phenomena, for example, could conduct a metaanalysis (Arnqvist and Wooster 1995; Osenberg et al. 1999; Gurevitch and Hedges 2001; Johnson 2002) using numerous impact assessments that address the same sort of impacts. These studies also could serve as part of a metareplication (Johnson 2002). If researchers have access to raw data from multiple impact assessments or surveys, they can evaluate these data to address ecological and conservation questions beyond the scope of an individual field survey. Syntheses using multiple sets of data include some of the more influential ecology and conservation publications in recent years (e.g., Costanza et al. 1997; Vitousek et al. 1997; Myers et al. 2000; Jackson et al. 2001). Such analyses can be extraordinarily effective approaches epistemologically, and typically would not be possible without basic long-term survey data, impact assessments, and other studies that individually might have limited scope. At any rate, impact assessment, inventorying, and monitoring are so important to wildlife ecology and management that we deal with these topics to some extent in all subsequent chapters. Moreover, Chaps. 6 and 7 are devoted entirely to discussions of impact assessment and inventory and monitoring studies, respectively.

1.4 Wildlife Science, Method, and Knowledge

Thus far, we primarily have addressed natural science generally. Here we attempt to place wildlife science more specifically within the context of the philosophy of natural science. One way wildlife scientists have contextualized their discipline is by comparing what is actually done to what they consider to be ideal based on the philosophy of science. As we have seen in Sect. 1.2, however, the ideal was somewhat a moving target during the twentieth century. Additionally, the understandable tendency of wildlife scientists to cite one another's second, third, or fourth hand summaries of Popper or Kuhn's ideas, for example, rather than read these philosophers' writings themselves, further clouded this target. For this reason, many publications citing Popper or Kuhn do not accurately represent these authors' ideas. Here we discuss a few critiques of science by scientists that influenced how researchers conduct wildlife science. We then attempt to contextualize where wildlife science falls today within the philosophy of natural science.

1.4.1 Methodological Challenges

Critiques of scientific methodology written by natural scientists, as opposed to philosophers or social scientists, have greatly influenced how investigators conduct wildlife ecology and conservation research. One reason these publications were so influential is they were more accessible to wildlife scientists than philosophical tomes or social studies of science that some might argue were more agenda-driven deconstructions of science than constructive criticisms.

One of the most influential critiques of science by a scientist was "Strong Inference" by Platt (1964; originally titled "The New Baconians"). One reason Platt's essay in *Science* was so influential was that, directly or indirectly, it introduced wildlife scientists to Poppers' hypothetico–deductive method of science (1959, 1962), Kuhn's (1962) idea of normal versus revolutionary science, and Chamberlin's (1890) call for multiple working hypotheses. Briefly, Platt (1964) argued that The New Baconians, exemplified by leading researchers in molecular biology and high-energy physics, made much more rapid scientific progress and significant breakthroughs than did those working in other natural sciences because they utilized an approach he called strong inference. Platt maintained that strong inference was nothing more than an updated version of Bacon's method of inductive inference. Specifically, he argued, researchers should (1) inductively develop multiple alternative hypotheses (after Chamberlin 1890), (2) deduce from these a critical series of outcomes for each hypothesis, then devise a crucial experiment or series of experiments that could lead to the elimination of one or more of the hypotheses (after Popper 1959, 1962), (3) obtain decisive results through experimentation, and (4) recycle the procedure to eliminate subsidiary hypotheses. He also argued that these New Baconians used logic trees to work out what sort of hypotheses and questions they should address next. He provided numerous examples of extraordinarily productive scientists whom he felt had used this approach. As Platt concluded (1964, p. 352)

> The man to watch, the man to put your money on, is not the man who wants to make "a survey" or a "more detailed study" but the man with the notebook, the man with the alternative hypotheses and the crucial experiments, the man who knows how to answer your Question of disproof and is already working on it.

Rowland H. Davis (2006) maintained that while Platt's (1964) essay was influential in an array of natural and social sciences, it probably had its greatest impact in ecology. One reason was that in 1983, the *American Naturalist* prepared a dedicated issue titled "A Round Table on Research in Ecology and Evolutionary Biology" that included some of the most highly cited theoretical papers in ecology till that date. Some of these authors directly suggested that researchers use Platt's method of strong inference to address their theoretical questions (Quinn and Dunham 1983; Simberloff 1983) and others made similar suggestions somewhat less directly (Roughgarden 1983; Salt 1983; Strong 1983). Several other essays invoking aspects of Platt's approach also appeared in ecology and evolutionary biology outlets during the 1980s (e.g., Romesburg 1981; Atkinson 1985; Loehle 1987; Wenner 1989). There is little doubt that wildlife ecology and conservation researchers were inspired directly or indirectly to improve the sophistication of their study designs by Platt's essay.

Strong inference (Platt 1964) was not without problems, however, including some that were quite serious. Only one year after its publication, a physicist and a historian (Hafner and Presswood 1965) demonstrated in *Science* that historical evidence did not support the contention that strong inference had been used in the high-energy physics examples that Platt provided. Instead, they maintained "… that strong inference is an idealized scheme to which scientific developments seldom conform" (p. 503). More recently, two psychologists concluded that (1) Platt failed to demonstrate that strong inference was used more frequently in rapidly versus slowly progressing sciences, (2) Platt's historiography was fatally flawed, and (3) numerous other scientific approaches had been used as or more successfully than strong inference (O'Donohue and Buchanan 2001). Davis (2006, p. 247) concluded that "… the strongest critiques of his [Platt's] recommendations were entirely justified."

One might logically ask why Platt's essay was so influential, given its many shortcomings. The answer, as Davis (2006, p. 238) put it, is "that the article was more an inspirational tract than the development of a formal scientific methodology." It was effective because it "imparted to many natural and social scientists an ambition to test hypotheses rather than to prove them" (p. 244). Davis concluded that the value of "Strong Inference" was that it "encouraged better ideas, better choices of research problems, better model systems, and thus better science overall, even in the fields relatively resistant to the rigors of strong inference" (p. 248). This undoubtedly was true for wildlife science.

Numerous influential essays more directly targeting how wildlife scientists should conduct research also appeared during the last few decades. For example, H. Charles Romesburg (1981) pointed out that wildlife scientists had used induction to generate numerous rules of association among classes of facts, and had retroductively developed many intriguing hypotheses. Unfortunately, he argued, these "research hypotheses either are forgotten, or they gain credence and the status of laws through rhetoric, taste, authority, and verbal repetition" (p. 295). He recommended that wildlife science attempt to falsify retroductively derived research hypotheses more often using the hypothetico–deductive approach to science championed by Popper (1959, 1962) and discussed by Platt (1964). Similarly, Stuart H. Hurlbert (1984) maintained that far too many ecological researchers, when attempting to implement the hypothetico–deductive method using replicated field experiments, actually employed pseudoreplicated designs (see Sect. 2.2 for details). Because of these design flaws, he argued, researchers were much more likely to find differences between treatments and controls than actually occurred.

One of the difficulties faced by wildlife science and ecology is that ecological systems typically involve middle-number systems, or systems made up of too many parts for a complete individual accounting (*census*), but too few parts for these parts to be substituted for by averages (an approach successfully used by high-energy physics) without yielding fuzzy results (Allen and Starr 1982; O'Neill et al. 1986). For this reason, wildlife scientists often rely on statistical approaches or modeling to make sense of these problematic data. Thence, the plethora of criticisms regarding how wildlife scientists evaluate data should come as no surprise. For example, Romesburg (1981) argued that wildlife scientists had a "fixation on statistical methods" and noted that "scientific studies that lacked thought… were dressed in quantitative trappings as compensation" (307). Robert K. Swihart and Norman A. Slade (1985) argued that sequential locations of radiotelemetered animals often lacked statistical independence and that many researchers evaluated such data inappropriately. Douglas H. Johnson (1995) maintained that ecologists were too easily swayed by the allure of nonparametric statistics and used these tools when others were more appropriate. Patrick D. Gerard and others (1998) held that wildlife scientists should not use retrospective power analysis in the manner *The Journal of Wildlife Management* editors had insisted they should (The Wildlife Society 1995). Steve Cherry (1998), Johnson (1999), and David R. Anderson and others (2000) maintained that null hypothesis significance testing was typically used inappropriately in wildlife science and related fields, resulting in far too many *p*-values in refereed journals (See Sect. 2.5.2 for details). Anderson (2001, 2003) also made a compelling argument that wildlife field studies relied far too much on (1) convenience sampling and (2) index values. Since one leads to the other and neither are based on probabilistic sampling designs, there is no valid way to make inference to the population of interest or assess the precision of these parameter estimates. Finally, Fred S. Guthery and others (2001, 2005) argued that wildlife scientists still ritualistically applied statistical methods and that this tended to transmute means (statistical tools) into ends. They also echoed the view of previous critics (e.g., Romesburg 1981; Johnson 1999) that wildlife scientists should give *scientific hypotheses* and *research hypotheses* a much more prominent place in their research programs, while deemphasizing *statistical hypotheses* and other statistical tools because they are just that – tools. These and similar critiques will be dealt with in more detail in subsequent chapters. The take home message is that wildlife science is still struggling to figure out how best to conduct its science, and where to position itself within the firmament of the natural sciences.

1.4.2 The Puzzle of Scientific Evidence

Even if we ignore the serious deficiencies Kuhn, Lakatos, Feyerabend, and other philosophers found in Popper's model of science (see Sect. 1.2.3.1), and argue that hypothesis falsification defines science, there is still a major disconnect between this ideal and what respected wildlife researchers actually do. For example, although most wildlife scientists extol the hypothetico–deductive model of science, Fig. 1.1 represents common study designs actually employed by wildlife researchers.

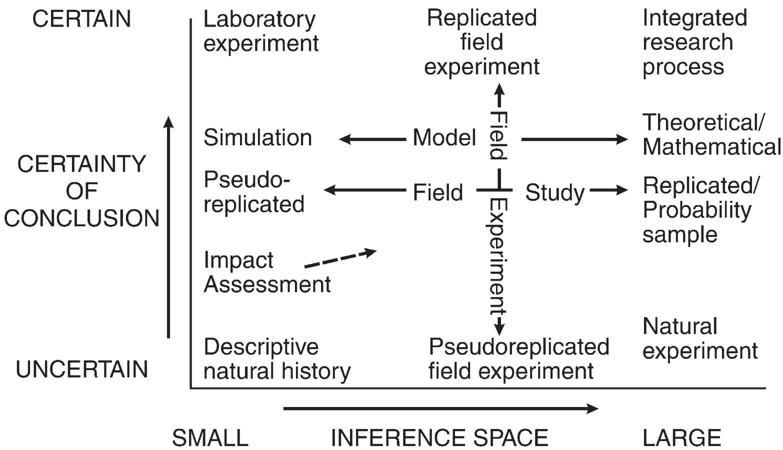

Fig. 1.1 The potential for various wildlife study designs to produce conclusions with high certainty (few plausible alternative hypotheses) and widespread applicability (diversity of populations where inferences apply). Reproduced from Garton et al. (2005), with kind permission from The Wildlife Society

Only a few of these can be construed as clearly Popperian. This does not imply the remaining designs are not useful. In fact, much of the remaining chapters deal with how to implement these and related designs. Instead, although Popper's postpositivist model of science is sometimes useful, it is often insufficient for the scope of wildlife science.

Is there a philosophical model of science that better encompasses what wildlife researchers do? Yes, there probably are several. For example, Lakatos' (1970) attempt to reconcile Popper (1959, 1962) and Kuhn's (1962) representations of science resulted in what we expect many wildlife scientists assume was Popper's model of science. Lakatos' formulation still gives falsification its due, but also makes a place for historically obvious paradigm shifts and addressed other potential deficiencies in Popper's model (see Sect. 1.2.3.1 for details). Lakatos' model, however, still cannot encompass the array of designs represented in Fig. 1.1 (and discussed in subsequent chapters). Although Feyerabend's (1975, 1978) "anything goes" approach to science certainly can cover any contingency, it offers wildlife scientists little philosophical guidance.

Haack (2003) developed a model of scientific evidence that offers a unified philosophical foundation for natural science. Further, Fig. 1.1 makes perfect sense in light of this model. Essentially, she argues that, from an epistemological perspective (see Sect. 1.2.3.1), natural science is a pragmatic undertaking. Her retro-classical version of American pragmatism places science firmly within the empiricist sphere of epistemology as well, due to the criticality of experience. She developed an apt analogy, beginning in the early 1990s (Haack 1990, 1993), which should help contextualize her model. Haack (2003) maintained that natural science research programs are conducted in much the way one completes a crossword puzzle, with warranted scientific claims anchored by experiential evidence (analogous to clues) and enmeshed in reasons (analogous to the matrix of completed entries). As she put it

> How reasonable a crossword entry is depends not only on how well it fits with the clue and any already-completed intersecting entries, but also on how plausible those other entries are, independent of the entry in question, and how much of the crossword has been completed. Analogously, the degree of warrant of a [scientific] claim for a person at a time depends not only on how supportive his evidence is, but also on how comprehensive it is, and how secure his reasons are, independent of the claim itself. (p. 67)

Following the crossword analogy, a group of researchers, each with 20 years of experience working with a system of interest, should be able to solve a scientific puzzle more easily than a first semester graduate student. While some writers find the social nature of science problematic (see Sect. 1.2.3.1), Haack (2003) maintained it is beneficial. After all, "scientific work … is much like carrying a heavy log, which can be lifted by several people but not by one. It is complex, intricate, multi-faceted – yes! – like working on a vast crossword puzzle" (p. 106). Different investigators employing different study designs and different methodologies might solve different portions of the puzzle. Because many researchers work on the puzzle simultaneously, there also must be "ways of discriminating the nut and the incompetent from the competent inquirer – credentials, peer review – so as to ensure that what the journals make available is not rubbish but worthwhile work"

(p. 107). Further, just as someone completing a crossword puzzle might make inappropriate entries, and be forced to rethink their approach, scientists are fallible as well. In fact, learning from mistaken results, concepts, or theories, and having to begin certain aspects of a research program repeatedly, seems to characterize natural science (Hafner and Presswood 1965; Haack 2003).

We hasten to point out that others noted the puzzle-like nature of natural science prior to Haack (1990, 1993, 2003). For example, Albert Einstein (1879–1955; 1936, pp. 353–354) wrote that

> The liberty of choice [of scientific concepts and theories], however, is of a special kind; it is not in any way similar to the liberty of a writer of fiction. Rather, it is similar to that of a man engaged in solving a well-designed word puzzle. He may, it is true, propose any word as the solution; but, there is only *one* word which really solves the puzzle in all it forms. It is an outcome of faith that nature – as she is perceptible to our five senses – takes the character of such a well-formulated puzzle. The successes reaped up to now by science do, it is true, give a certain encouragement to this faith.

Haack (2003) added that "scientific inquiry is a highly sophisticated, complex, subtle, and socially organized extension of our everyday reliance on experience and reasoning" (pp. 124–125). She also clarified that "there is a real world knowable to some extent by creatures with sensory and intellectual powers such as ours" (p. 125), despite the fact that our understanding of this world is to some degree a social construction (see Sect. 1.2.3.1). Despite the fact that scientists sometimes blunder about while attempting to solve scientific puzzles, there is a world outside of us we can come to know to some degree, and natural science is one of the more effective ways to acquire this knowledge. In fact, "unless theories in mature science were at least approximately true, their predictive power would be miraculous" (p. 145). This should give us hope, if nothing else.

In sum, Haack (2003) provides wildlife science a pragmatic model for knowledge acquisition (see Sect. 1.2.3.1). It does more than explain why wildlife scientists commonly employ study designs incongruent with Popper's (1959, 1962) falsification approach (e.g., Fig. 1.1). Her pragmatic model of science allows for material reality on Earth before (and possibly after) human existence, despite the contentions of radical social constructionists. It allows the social aspects of science to be explicitly included within the enterprise. It also permits any study design that can provide reliable solutions to the scientific puzzle, including various types of descriptive research, impact assessment, information–theoretic approaches using model selection, replicated manipulative experiments attempting to falsify retroductively derived research hypotheses, and qualitative designs to name just a few examples. She did not argue that each of these study designs was equally likely to provide reliable information in all circumstances. Instead, researchers must determine the best approach for each individual study, given specific constraints. There is no rote checklist for effective wildlife research. Finally, for Haack's pragmatic epistemology, truth, knowledge, and theory are inexorably connected with practical consequences, or real effects. This should resonate with wildlife scientists for whom practical conservation consequences are the ultimate metric of success.

1.5 What is it We Study?

If the objective of a wildlife study is to make inference, it is important to ask the following question: "To what biological entity do I wish to make inference"? Researchers must define this entity specifically. Is it a biological population, a species, the set of animals (of possibly different species) that serve as prey for a predator of interest, the trees in a patch of forest? The entity that is defined will be the entity that you will try to measure in the study, and the extent to which you access it will determine how well you can make inference to it from the results. Defining and accessing the entity of interest requires tools of both biology and sampling.

In designing wildlife studies, we are faced with two sets of definitions related to *populations*, one biological, and the other statistical. We start with statistical definitions, as they underpin all inference in wildlife studies. We then move on to biological definitions, the notion of significance, and whether one's focus is on wildlife or wildlife habitat.

1.5.1 Statistical Definitions

A *target population* is the collection of all *sampling* or *experimental units* about which one would like to make an inference. With respect to wildlife studies, this could be all the individuals in a biological population, subspecies, or species, all individuals or species in a community, or their habitat. The target population is just that, a target. If you had the ability and desire to measure every element in the target population, that would be a *census*. This is rarely the case in ecological systems.

In many cases, there is a subset of the target population not accessible using chosen field methods. In this case, the subset of the target population that is accessible is the *sampled population*. Because a census of even the sampled population is rarely feasible, researchers take a representative sample. A *sample* is the collection of experimental or *sampling units* from the sampled population that are actually measured. If researchers choose the sample appropriately, then they can make statistical inferences about the sampled population. However, to extend inference to the target population, they must argue that the sampled population is representative of the target population. For example, suppose you are studying the recruitment of wood ducks (*Aix sponsa*) in the Mississippi Alluvial Valley over time and your annual measure is the occupancy rate of nest boxes. To draw inference for the entire Mississippi Alluvial Valley, the target population would be all potential wood duck nesting sites in the valley. The sampled population is already smaller than the target population because the study is restricted to nesting boxes, thus ignoring natural cavities in trees. If, in addition, you only had access to the wood duck boxes found on government-owned land, such as state wildlife management areas, the sampled population would be further restricted to all wood duck boxes on government-owned land. Therefore, even with sophisticated methods of sampling design, the only resulting statistical inference strictly justified by the study design would be to recruitment in nest boxes on government-owned land. Extension of that inference to the entire Mississippi Alluvial Valley would require an argument, based on subjective expertise or previous studies where both nest boxes and natural cavities on both public and private lands were included, that trends in recruitment should be equivalent between public and private lands, and between nest boxes and natural cavities.

1.5.2 Biological Definitions

The target population of a wildlife study could include a broad array of biological entities. It is important to be clear and specific in defining what that entity is. It is just as important to identify this before the study begins as when it is explained in a manuscript or report at the end, because the sampled population and thus the sample stem directly from the target population. If some part of the target population is ignored when setting up the study, then there will be no chance of sampling that portion, and therefore drawing statistical inference to the entire population of interest cannot be done appropriately, and any inference to the target population is strictly a matter of professional judgment.

If a target population is well defined, and the desirable situation where the sampled population matches the target population is achieved, then the statistical inference will be valid, regardless of whether the target matches an orthodox definition of a biological grouping in wildlife science. Nevertheless, we believe that reviewing general definitions of biological groupings will assist the reader in thinking about the target population he or she would like to study.

In ecology, a *population* is a group of individuals of one species in an area at a given time (Begon et al. 2006, p. 94). We assume these individuals have the potential to breed with one another, implying there is some chance they will encounter one another. Dale R. McCullough (1996, p. 1) describes the distinction between *panmictic populations*, where the interactions between individuals (including potential mating opportunities) are relatively continuous throughout the space occupied by the population, and metapopulations. A *metapopulation* (Levins 1969, 1970) is a population subdivided into segments occupying patches of habitat in a fragmented landscape. An environment hostile to the species of interest separates these patches. Movement, and presumably gene flow, between patches is inhibited, but still occurs. Metapopulations provide a good example of where the design of a population study could go awry. Suppose a metapopulation consists of sources and sinks (Pulliam 1988), where the species remains on all patches and the metapopulation is stable, but those that are sinks have low productivity and must rely on dispersal from the sources to avoid local extinction. If an investigator considers individuals on just one patch to constitute the entire population, then a demographic study of this subpopulation could be misleading, as it could not address subpopulations on other patches. By considering only natality and survival of this subpopulation, the investigator might conclude that the population will either grow exponentially (if a source) or decline to extinction (if a sink).

This example illustrates the importance of including all elements of population dynamics when studying populations, metapopulations, or subpopulations. Births, deaths, immigration from other areas, or emigration to other areas defines the state of the population. Emigration can be permanent, as in dispersal of young to find new territories, or temporary. We must consider all these population parameters, in addition to other measures such as age structure and age at first reproduction, to study population dynamics properly. The population picture becomes more complicated for migratory populations, where animals that breed in distinct breeding populations often mix in staging or wintering areas. These additional dimensions must be taken into account to understand their dynamics.

The *biotic community* is "an assemblage of species populations that occur together in space and time" (Begon et al. 2006, p. 469). Sometimes the usage is more specific, such as a plant community or a small-mammal community. There are concepts of community dynamics that parallel those of population dynamics. *Species richness* is the number of species in the community at a given time, and *species diversity* refers to indices of community diversity that take into account both species richness and the relative abundance of species (Begon et al. 2006, pp. 470–471). *Local extinction probability* is the probability that a species currently present will not be in the community by the next time period. The *number of colonizing species* is the number of species currently in the community that were absent during the previous time period.

Biodiversity is one of the most commonly used ecological terms in both the scientific literature and the popular media today. Unfortunately, it rarely appears with an unambiguous definition. In its most general sense, biodiversity refers to all aspects of variety in the living world (Begon et al. 2006, p. 602). More specifically, the term is used to describe the number of species (species richness), the amount of genetic variation, or the number of community types present in an area of interest.

Habitat also has many definitions in the literature. For our purposes, it is "the physical space within which an organism lives, and the abiotic and biotic entities (e.g., resources) it uses and selects in that space" (Morrison et al. 2006, p. 448). Further, because habitat is organism-specific, "it relates the presence of a species, population, or individual (animal or plant) to an area's physical and biological characteristics" (Hall et al. 1997, p. 175).

1.5.3 Biological vs. Statistical vs. Social Significance

John Macnab (1985) argued that wildlife science was plagued with "slippery shibboleths," or code words having different meanings for individuals or subgroups within the field. "Significance" is as slippery as any shibboleth in wildlife science. We typically use this term in one of three ways: biological, statistical, or social significance. All too often, authors either do not specify what they mean when they say something is significant, or appear to imply that simply because results are (or are not) statistically significant, they must also be (or not be) significant biologically and/or socially.

When wildlife scientists say that something is biologically significant, they mean that it matters biologically. Because one of wildlife sciences' primary objectives is to determine what is biologically important, this is not a trivial matter. In fact, the reason we use inferential statistics at all, and sometimes compute statistical significance in the process, is to learn what is biologically important. The problem is that, based on a particular study, not all statistically significant differences matter biologically, and just because we cannot find statistically significant differences does not imply that biological differences do not indeed exist in the system being studied (Cherry 1998; Johnson 1999). Further, as Johnson (1999, p. 767) maintained, "the hypotheses usually tested by wildlife ecologists… are statistical hypotheses [see glossary].… Unlike scientific hypotheses [see glossary], the truth of which is truly in question, most statistical hypotheses are known a priori to be false." For example, successful hunter–gathers in North America since the Pleistocene have known that white-tailed deer (*Odocoileus virginianus*) do not use habitat at random, so designing a study to determine whether deer use habitat in proportion to availability is silly; it is also silly to consider this question for any species known well by humans. The more relevant question is "how much time are animals spending in available habitats" (Cherry 1998, p. 948), and what important life requisites do each of these cover types provide. Much of the time, wildlife scientists are actually attempting to find the magnitude of some effect rather than determine whether the effect actually exists – we already know that answer.

Another complication is that just because wildlife scientists find something to be biologically significant does not imply that society will reach the same conclusion. Moreover, society might well find something to be extraordinarily important that wildlife scientists do not think matters much biologically. For contentious environmental issues, various segments of society will undoubtedly disagree with one another as well. As case studies amply illustrate, differences in the moral cultures of various segments of society, and disagreement regarding what is or is not socially or biologically significant, contribute greatly to wildlife-related environmental conflicts (Wondolleck and Yaffee 2000; Peterson et al. 2002, 2004, 2006a). These differences also form one of the primary challenges to public participation processes designed to work through such environmental conflicts (Daniels and Walker 2001; Depoe et al. 2004; Peterson and Franks 2005; Peterson et al. 2005, 2006b). Because the majority of wildlife scientists work for regulatory agencies at the state or federal level, for nongovernmental organizations, or for environmental consulting firms – or train those who do – what various publics and related interest groups perceive to be significant, and why they reach these conclusions, are questions central to wildlife science.

1.5.4 Focus on Wildlife vs. Focus on Wildlife Habitat

We have defined the statistical sampling concepts of target population, sampled population, and sample, as well as the biological concepts of population, metapopulation, community, and habitat. The statistical concepts will be applied to the biological ones (i.e., the set of experimental or sampling units will be identified), based on the objectives of the study. We can divide wildlife studies into those whose objectives focus on groupings of animals and those whose objectives focus on the habitat of the animals.

We can further divide studies of animals into those that focus on measuring something about the individual animal (e.g., sex, mass, breeding status) and those that focus on how many animals are there. Consider a study of a population of cotton rats (*Sigmodon hispidus*) in an old field where there are two measures of interest: the size of the population and its sex ratio. The sampling units would be individual rats and the target population would include all the rats in the field (assume the field is isolated enough that this is not part of a metapopulation). If capture probabilities of each sex are the same (perhaps a big assumption), then by placing a set of traps throughout the field one could trap a representative sample and estimate the sex ratio. If the traps are distributed probabilistically, the sampled population would match the target population (and in this case the target population would coincide with a biological population) and therefore the estimated sex ratio should be representative of the population sex ratio.

The estimation of abundance is an atypical sampling problem. Instead of measuring something about the sampling units, the objective is to estimate the total number of units in the target population. Without a census, multiple samples and capture–recapture statistical methodology are required to achieve an unbiased estimate of the population size (see Sect. 2.5.4.). If traps are left in the same location for each sample, it is important that there be enough traps so that each rat has some chance of being captured during each trapping interval.

Estimates of abundance are not limited to the number of individual animals in a population. The estimation of species richness involves the same design considerations. Again, in the absence of a census of the species in a community (i.e., probability of detecting at least one individual of each species is 1.0), designs that allow the use of capture–recapture statistical methodologies might be most appropriate (see reviews by Nichols and Conroy 1996; Nichols et al. 1998a,b; Williams et al. 2002). In this case, the target population is the set of all the species in a community. We discuss accounting for detectability more fully in Sect. 2.4.1.

If wildlife is of ultimate interest, but the proximal source of interest is something associated with the ecosystem of which wildlife is a part, then the target population could be vegetation or some other aspect of the animals' habitat (e.g., Morrison et al. 2006). For example, if the objective of the study is to measure the impact of deer browsing on a given plant in a national park, the target population is not the deer, but the collection of certain plants within the park. The researcher could separate the range of the plant into experimental units consisting of plots; some plots could be left alone but monitored, whereas exclosures could be built around others to prevent deer from browsing. In this way, the researcher could determine the impact of the deer on this food plant by comparing plant measurements on plots with exclosures versus plots without exclosures.

1.6 Summary

Because wildlife scientists conduct research in the pursuit of knowledge, they must understand what knowledge is and how it is acquired. We began Sect. 1.2 using "the science wars" to highlight how different ontological, epistemological, and axiological perspectives can lead to clashes grounded in fundamentally different philosophical perspectives. This example also illustrates practical reasons why wildlife scientists should become familiar with philosophy as it relates to natural science. Differing perspectives on the nature of reality (*ontology*) explain part of this clash of ideas. Most scientists, grounded in the empiricist tradition, hold that reality independent of human thought and culture indeed exists. Conversely, many social scientists and humanists argue that reality ultimately is socially constructed because it is to some degree contingent upon human percepts and social interactions. Several major perspectives toward the nature and scope of knowledge (*epistemology*) have developed in Western philosophy. Influential approaches to knowledge acquisition include empiricism, rationalism, pragmatism, logical positivism, postpositivism, and social constructionism. Regardless of the epistemological perspective one employs, however, logical thought, including inductive, deductive, and retroductive reasoning (Table 1.1), remains an integral component of knowledge acquisition. At least three aspects of value or quality (*axiology*) influence natural science. Ethical behavior by scientists supports the integrity of the scientific enterprise, researchers bring their own values into the scientific process, and both scientists and society must determine the value and quality of scientific research.

As Sect. 1.2 illustrates, there is no single philosophy of science, and so there can be no single method of science either. Regardless, natural science serves as a model of human ingenuity. In Sect. 1.3, we addressed why natural science has proven such a successful enterprise. Much of the reason relates to general steps commonly employed (Table 1.2). These include (1) becoming familiar with the system of interest, the question being addressed, and the related scientific literature, (2) constructing meaningful research hypotheses and/or conceptual models relating to theory and objectives, (3) developing an appropriate study design and executing the design and analyzing the data appropriately, (4) obtaining feedback from other scientists at various stages in the process, such as through publication in referred outlets, and (5) closing the circle of science by going back to steps 3, 2, or 1 as needed. Often, because of the complex nature of scientific research, multiple researchers using a variety of methods address different aspects of the same general research program. Impact assessment, inventorying, and monitoring studies provide important data for decision making by natural resource policy makers and managers. The results of well-designed impact and survey studies often are suitable for publication in refereed outlets, and other researchers can use these data in conjunction with data collected during similar studies to address questions beyond the scope of a single study.

In Sect. 1.4, we discussed how wildlife scientists have honed their approaches to research by studying influential critiques written by other natural scientists (e.g., Platt 1964; Romesburg 1981; Hurlbert 1984). Because ecological systems contain too many parts for a complete individual accounting (*census*), but too few parts for these parts to be substituted for by averages, wildlife scientists typically rely on statistical approaches or modeling to make sense of data. For this reason, numerous critiques specifically addressing how wildlife scientists handle and mishandle data analysis were published in recent decades. These publications continue to shape and reshape how studies are designed, data analyzed, and publications written.

As Fig. 1.1 illustrates, wildlife science commonly employs a number of study designs that do not follow Popper's (1959, 1962) falsification approach to science. Epistemologically, wildlife science probably is better described by Haack's (2003) pragmatic model of natural science, where research programs are conducted in much the same way one completes a crossword puzzle, with warranted scientific claims anchored by experiential evidence (analogous to clues) and enmeshed in reasons (analogous to the matrix of completed entries). This pragmatic model permits any study design that can provide reliable solutions to the scientific puzzle, including various types of descriptive research, impact assessment, information–theoretic approaches using model selection, replicated manipulative experiments attempting to falsify retroductively derived research hypotheses, and qualitative designs to name just a few. Under this pragmatic epistemology, truth, knowledge, and theory are inexorably connected with practical consequences, or real effects.

We ended the chapter by clarifying what it is that wildlife scientists study (Sect. 1.5). We did so by defining a number of statistical, biological, and social terms. This is important as the same English word can describe different entities in each of these three domains (e.g., significance). We hope that these common definitions will make it easier for readers to navigate among chapters. Similarly, this chapter serves as a primer on the philosophy and nature of natural science that should help contextualize the more technical chapters that follow.

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