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Research Methods in Comparative Psychology: A Tutorial

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The purpose of this contribution is to acquaint social science students with some of the principle research methods associated with comparative psychology. Comparative psychology is the oldest of the organized social sciences and the myriad issues of experimental design routinely faced by comparative psychologists are directly applicable to all social sciences. Issues discussed include how to determine if a comparison is worth conducting, the relationship between quantitative and qualitative comparisons, how to apply systematic variation to evaluate several possible explanations, and why the cautionary tale provided by "Morgan's canon" is still relevant. Other issues include the importance of a universally accepted definition of behavioral phenomena, the need for behavioral taxonomies, and the significance of including examples of behavioral observations. Of particular interest is a discussion of the comparative method and the presentation of guidelines for designing experiments. This article can be incorporated into any course that is relevant to the comparative analysis of behavior as either primary source material or supplemental readings.

Keywords: comparative psychology, replication, research methods

The goal of this article is to familiarize students with some of the methods used in comparative psychology. The writing style is colloquial and suitable for both undergraduate and graduate students interested in learning how to conduct proper comparisons. This article is useful as a supplemental reading in a variety of courses including comparative psychology, evolutionary psychology, research methods, cross cultural psychology, developmental psychology, and as a basis of independent study (Abramson, 2018). A unique aspect of this tutorial is that I provide the student with advice. The advice is based on almost 40 years of practical experience as a comparative psychologist.

One of the most exciting aspects of science is when we have the opportunity to compare aspects of the world around us. The natural and social sciences use the scientific method as a guide to evaluate the similarities and differences between,

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and among, various phenomena. However, it is in the behavioral sciences where the skill in conducting comparisons reaches its most challenging aspects. Consider, for example, a simple experiment comparing the learning of an ant to the learning of a honey bee. How does one equate apparatus, stimuli, rewards, and previous experience? If we add to our example the laboratory rat and human animals into our comparison of learning in ants and honey bees, does it even make sense to compare them at all? How does one make and/or recognize a scientifically valid comparison?

Comparing species and subspecies offers many interesting challenges. Comparisons within a species are no less of a challenge. In studying *Homo sapiens* for example, the developmental psychologist, the cultural anthropologist, the historian, the political scientist, and the social psychologist face important challenges. How do these scholars equate the myriad of educational, economic, and family factors in a comparative study of culture? How do they know what is worth comparing and what is not? When do they know to limit exaggerated explanations of their results?

Incorrect and sloppy comparisons lead to squandered grant money, lost opportunities, and wasted research. However, the consequences are greatest when the subjects are humans. One just needs to look at the early history of psychology to see how slipshod comparisons produced a generation of racist and sexist social scientists (Abramson and Lack, 2014). The research of many early psychologists was so egregious that the American Psychological Association's Council of Representatives issued a resolution apologizing for "promoting and perpetuating" racism (https://www.apa.org/about/policy/racism-apology). How does one even begin to make proper comparisons? Fortunately, guidelines are available. These are collectively known as the comparative method (Bornstein, 1980; Denny and Ratner, 1970).

One way to conceptualize the comparative method is to consider it an extension of the scientific method specifically directed at making proper comparisons and revealing those that are improper. Yildirim (1971) characterizes the scientific method as a problem solving procedure where the practitioner maintains a critical and rationale outlook. The same is true with the comparative method. Moreover, the comparative method includes such notions as: whether two behaviors are homologous or analogous, the use of quantitative vs. qualitative comparisons, the application of systematic variation to evaluate several possible explanations, and the cautionary tale provided by "Morgan's canon."

Comparative psychology, which I define as the application of the comparative method to problems in psychology (Abramson, 2018), maintains an emphasis on reliability. Perhaps not generally known is that replication is problematic in the behavioral sciences. For example, an article published in *Science* attempted to replicate 100 studies in the areas of cognitive and social psychology (Open Science Collaboration, 2015). The results were alarming. Of 100 published experiments appearing in three highly ranked journals, half of the cognitive experiments were not replicated and many of those that were replicated had reduced effect sizes. The social psychology experiments were even more alarming with only 30% replicable. I suggest that an understanding of comparative psychology and the comparative method will help ameliorate the replication crises.

Comparative psychology is the oldest of the organized social science disciplines (Abramson, 2018, d'Isa and Abramson, 2023). In practice, this means that comparative psychology has the most experience using the comparative method. The myriad experimental design issues routinely faced by comparative psychologists are directly applicable to all behavioral sciences. No other social science has had as many challenges as comparative psychology and its lessons related to research design are well worth knowing.

If we consider 1879 as the date of the formal founding of psychology as an experimental discipline (Heidbreder, 1933), the phrase "comparative psychology" was known as early as 1778 (Hissmann, 1778). It also appeared in 1808 (Liebsch, 1808), 1812 (Hoffbauer, 1812) and in 1827 (Poli, 1827). See d'Isa and Abramson (2023) for a detailed history of the term "comparative psychology."

The term also appeared in 1858 in a discussion of research methods of comparative psychology (Weinland, 1858). In 1876, Herbert Spencer published "The Comparative Psychology of Man" (Spencer, 1876) and in 1882 George Romanes used the term in his famous book *Animal Intelligence* (Romanes, 1882). The first comparative psychology society appeared in 1885 (Mills, 1887; see also Abramson, 2015, 2018). It is interesting to note that before Alfred Binet developed his famous tests with Theodore Simon, he published a book on the comparative psychology of microorganisms (Abramson and McCarthy IV, 2022)

Compare these dates with those associated with the founding of professional societies in the behavioral sciences. A Google search reveals that: the American Statistical Association was founded in 1839; the American Historical Association was founded in 1884; the American Psychological Association in 1892; the American Economic Association in 1885; the American Philosophical Association in 1900; the American Anthropology Association in 1902; the American Association of Political Science in 1903; and the American Sociology Association in 1905. As these dates show, comparative psychology, compared to other behavioral sciences, has the most experience conducting research, developing experimental designs, and confronting a myriad of research related problems. A study of comparative psychology for example, can guide the researcher on what comparisons are the most fruitful, how to apply systematic variation to evaluate several possible explanations, and why the cautionary tale provided by "Morgan's canon" is still relevant.

The Comparative Method

A discussion of the comparative method is not in any research methods textbook that I am familiar with. This is unfortunate as the comparative method is applicable to a wide range of research areas and social science disciplines. Consider that comparative psychologists contribute to such diverse areas as cultural anthropology, political science, neurobiology, computer engineering, app development, cross-cultural psychology, developmental psychology, animal–human interactions, law, behavioral neuroscience, clinical psychology, agriculture, enrichment, philosophy, mathematical modeling, learning, and history of science along with a host of others (Abramson, 2018, 2023; Bornstein, 1980; Moran, 1987).

As noted by Denny and Ratner (1970) the comparative method comprises the following six sequential stages (see Table 1).

Table 1

The Stages of the Comparative Method

Stage	Function	Sample Activity
1	Background and perspective	Review formal and informal sources
2	Classification of behavior	Determine major classes of behavior
3	Research preparations	Find clear examples of each class of behavior
4	Parametric manipulation	Use preparations to find effects of variables
5	Relations and comparisons	Show relations among behavior classes
6	General theory	Postulate general mechanisms relative to the behavior of organism

Research Methods Associated with Each Stage

1. Background and perspective. Common to every experimental design in the behavioral sciences is the requirement to gather background information. The researcher should acquire all possible information that bears upon the research question or risks conducting an experiment that not only wastes time and resources but also produces erroneous and unreliable data. It is especially important to gather information regarding what is termed "subject or participant variables." Participant variables include those associated with economic variables, evolutionary history, physical characteristics, natural history, social structures, and physiology.

Where can a researcher acquire the necessary background information? One ready source of information is the ethogram. An ethogram is a catalog of observations. Other names for ethograms include behavioral observations and behavioral profiles. As an ethogram consists only of observations, there is no manipulation of independent variables and no “cause and effect” answers. When constructing an ethogram, it is important that the researcher not influence the behavior under examination. How to create ethograms and examples of ethograms are readily available on YouTube and other social media. A Google search on the entry “ethogram” returned approximately 226,000 hits. These hits include examples of human ethograms, how to construct ethograms, and where to purchase equipment to automate ethograms.

There are literally thousands of ethogram examples. These include observations of social behavior, food-seeking behavior, defense strategies, animal–human interactions, aggressive behavior, and behavior associated with development and reproductive activities. For example, an ethogram is available to estimate the interest of an elephant for various enrichment devices (Abramson and Carden, 1998). A unique feature of some ethograms is that they record behavior across different chronological periods to gain an historical perspective on a research project. Additional information useful for the construction of ethograms is available from an analysis of film, art, news outlets, original source material, literature reviews and, of course, personal experience. An important aspect of ethograms that should not be overlooked is that they stimulate the creation of research ideas.

What can a researcher do with these observations? The answer is that background information will help determine the parameters of an experiment. These parameters include the type and intensity of various stimuli and the amount and quality of a reward. An ethogram will also assist the researcher in identifying relevant subject and environmental variables. Obviously, the researcher must make some estimation of the reliability of the observations included in an ethogram.

Particularly important during the background stage is that the ethogram can help the behavioral scientist develop apparatuses. For example, when we were developing a Skinner box for the green crab (Abramson and Feinman, 1990), observing the crab in its natural environment helped us to determine the shape of the box, the rewards that we thought would be effective, and the size and shape of the manipuladum. There is probably no better way of knowing your study organism than by building an apparatus to explore its behavior. Apparatus construction is a lost art in the social sciences (Varnon, Lang, and Abramson, 2018).

My advice: “Know everything you can about the background of your research area and subject/participant before designing an experiment or study.”

2. Classification of behavior. One of the hardest lessons learned by comparative psychologists, and one that should concern all behavioral scientists, is the need for a classification or taxonomy of behavior. As Bitterman (1962) noted 60 years ago “Classification is not merely a matter of taste” (p. 81). Incredulously, there is no universally accepted classification of behavior. This problem is especially acute in the area of the comparative analysis of learning. Several schemes have been proposed (e.g., Bitterman, 1962; Dyal and Corning, 1973; Gormezano and Kehoe, 1975; Woods, 1974) but none are in use.

The fact that there is no classification scheme has directly led to problems with definitions of psychological phenomena. The problems associated with inconsistent definitions have stimulated debate related to, for example, the definitions of species, sexual selection, eusociality, and tool use (Crain, Giray, and Abramson, 2013). As we will see in a latter section, there are also no consistent definitions of behavior (Abramson and Place, 2005) and personality (Sternberg and Detterman, 1986).

In the area of learning, there is little formal discussion regarding the inconsistent definitions of conditioning procedures. The lack of a universally agreed-upon system of classification negatively influences behavioral science research and leads to problems of replication.

One of the most egregious examples is classical conditioning. Contemporary accounts often fail to mention that there are at least four different methods to produce classical conditioning. These methods focus on the degree of experimental control and the relationship between the conditioned and unconditioned response (Gormezano and Kehoe, 1975). There is no research directly comparing these procedures and it is doubtful that these four methods all produce the same behavioral phenomena — that is, classical conditioning.

Another definitional issue concerns operant conditioning. For many researchers, operant behavior is any "behavior controlled by its consequences" (Abramson, 1994). The hallmark of operant conditioning is whether the organism can demonstrate not only the use of a manipulandum such as a lever, but that it can use that manipulandum in novel ways (Lee, 1988).

Novelty is studied by training the organism to manipulate a device with different degrees of force or speed, moving the device up, down, or from side to side. In other words, the organism shows the researcher it "knows how to use the device." As operant behavior is now synonymous with instrumental behavior, an entire generation of behavioral scientists and their students consider instrumental and operant behavior as equivalent. They are not. Comparative research demonstrates that instrumental behavior be restricted to situations such as the maze, runway, and shuttle box where response novelty is not the primary concern (Abramson, 1994, 1997; Abramson and Levin, 2021; Abramson and Wells, 2018).

Let us consider a simple runway situation. A runway contains a start box, an alley, and goal box segments and is often defined as a "maze without choice points." A rat or human can easily be trained to traverse the alley segment at various speeds depending upon the contingencies of reinforcement. These type of reinforcement schedules are known as differential reinforcement of high rates of responding (DRH) and differential reinforcement of low rates of responding (DRL). If the reinforcement is contingent upon increasing running speed they can do that; if contingent upon decreasing running speed, they can do that also

(Logan, 1960). Despite numerous runway experiments with invertebrates, not one has ever demonstrated that an invertebrate can adjust its running speed dependent upon the contingencies of reinforcement. In other words, an ant can hurdle down a runway to obtain food, but the ant cannot adjust its speed to do so. This situation would represent instrumental conditioning rather than operant conditioning (Abramson, 1994).

The same logic applies to lever press situations developed for invertebrates. While some invertebrates can learn to press a lever to obtain food, it has never been demonstrated that an invertebrate can "use" the device in novel ways. For example, a green crab can press a lever to obtain a small piece of squid pâté but it has never been demonstrated that the crab can be taught to press the lever with varying degrees of force or of position (Abramson and Feinman, 1990).

My advice: "Be aware of inconsistent definitions."

3. Research preparations. In Stage 2 (see Table 1) it was noted that there are inconsistent definitions of phenomena in the behavioral sciences and there is no generally accepted behavioral taxonomy. This is problematic because in the research preparation phase (Stage 3), the successful research preparation requires a direct relationship with the behavior identified in Stage 2. If the definitions used to classify the behavior are suspect, changing, and unreliable, it will be difficult to find a reliable and meaningful research preparation.

Denny and Ratner (1970) identify four characteristics of a robust research preparation. These are: a) the behavior under study is reliably emitted/elicited; b) the behavior under study is noticeable; c) the behavior under study is reliably measured; and d) the behavior under study is accessible.

A solid research preparation has a number of advantages. These include the development of standard research designs and the sharing of background information with other behavioral scientists. Moreover, a standard research design will help the researcher: 1) direct time and resources toward conducting parametric manipulations; 2) consider how the results compare and contrast within and between research preparations; and 3) provide reliable data for the construction of general theories. Lahue and Corning (1971) provide an excellent example of this strategy in a study of habituation in the horseshoe crab to airpuff. They first develop a reliable intact preparation (i.e., a fully functioning organism) and then examine the effect of airpuff on the ventral nerve cord and then on an isolated ganglion.

An example of a solid research preparation "gone badly" is the classical conditioning of the proboscis extension (PER) in honey bees (Bitterman, Menzel, Fietz, and Schäfer, 1983). In PER conditioning, a bee is presented with an odor which is then closely followed by a sucrose feeding. After a number of odor–food pairings, the bee extends its proboscis to the odor prior to receiving a feeding (Abramson, 1990). This procedure has produced many interesting findings but there are major inconsistencies among laboratories. For example, some laboratories use only three trials of odor-food pairing while others use more. Some deprive the bees of food for 24 hours, other laboratories condition the bees as soon as they are captured; some routinely use controls while others rely only on published reports of control animals. These, and other differences in procedures, make it extremely difficult to compare PER results across laboratories (Abramson, Sokolowski, and Wells, 2011).

Moreover, there is a question of whether the PER situation is indeed an example of classical conditioning. While it is correct that the bee extends its proboscis to an odor after a number of odor–feeding pairings, it is also true that the procedure has a built in instrumental component. This is because to administer an unconditioned stimulus (US) of sucrose, the researcher first touches the antennae with sucrose and then, now that the proboscis is extended, allows it to feed. In the best examples of classical conditioning, the US is directly presented without an instrumental component (Gormezano and Kehoe, 1975). If the PER situation is to become similar to Pavlov's original procedure, the sucrose (US) must be directly pumped into the bee. This has never been done. Moreover, as the sucrose is applied to the antenna over repeated trials, there is a build-up of sucrose on the antenna, which will change the sensitivity of the sucrose receptors on the antennae as the experiment progresses (Abramson, Sokolowski, and Wells, 2011).

My advice: "Spend significant time developing a reliable research preparation."

4. Parametric manipulations of variables. When a reliable research preparation is developed, it is now possible to systematically manipulate independent variables. The manipulation of independent variables and their interactions are the hallmarks of Stage 4. One of the best examples of this in the comparative literature is the work of Ferster and Skinner (1957) who present cumulative records describing the key peck behavior of adult pigeons on various schedules of reinforcement. In an earlier article, Skinner (1956) presented three cumulative records from monkey, rat, and pigeon and challenged the reader to determine which cumulative record goes with which animal. The curves were of similar shape. The key peck, and its close relation the lever press, are now standard research preparations in many areas of behavioral science.

With such a preparation, it is simple to manipulate independent variables such as type of reward and amount and schedule of reward. Moreover, the robustness of the preparation has led other researchers to develop similar devices for organisms such as bees (Pessotti, 1972), crabs (Abramson and Feinman, 1990), cockroaches (Rubadeau and Conrad, 1963), fish (Longo and Bitterman 1959), humans (Macht, 1971) and sea hares (Downey and Jahan–Parwar, 1972). Varnon, Lang, and Abramson (2018) provide a discussion of the issues and problems associated with apparatus in comparative psychology.

One of the prohibitive factors hindering parametric research is the cost of apparatus and control equipment. With the advent of 3D printing behavioral apparatus is inexpensive and easily made (Hitesh and Abramson, 2020). Once the apparatus is constructed, the researcher must deal with the problem of how to attach the apparatus to equipment that controls the experiment. Such control equipment often costs several thousand dollars. With the advent of microcontrollers, this is no longer a problem. The parallax microcontroller, for example, can be configured in such a way that a sophisticated laboratory can literally be placed in the palm of one's hand for under $200.00, is suitable for field experiments, can accommodate many types of experimental designs, and is easily programmed (Varnon and Abramson 2013, 2018).

My advice: "Once a reliable research preparation is developed, spend time investigating independent variables."

5. Relations and comparisons. After obtaining background information, classifying the behavior of interest, developing a robust research preparation, and using the preparation to manipulate independent variables, the task of making comparisons and uncovering relationships begins. This is the goal of Stage 5. Stage 5 is arguably the most interesting stage and perhaps the most frustrating.

A behavioral science researcher must grapple with the question of whether the field has advanced enough to make meaningful comparisons. All a researcher has to do is type in a browser the phrase "problems of replication in psychology" (or some other social science field) and the results will be disappointing. Unfortunately, in many aspects of behavioral science, the necessary universally accepted definitions, behavioral taxonomies, and reliable research preparations are not available. Therefore, any meaningful comparisons risk a replication failure.

A behavioral science researcher also wastes time and resources trying to discover relationships, or make comparisons, if the research preparation on which conclusions are based is faulty, unreliable, and incomplete. It is better to spend time developing a robust and reliable research preparation. Without such a preparation, a behavioral scientist risks contributing to the literature data that cannot be replicated and is misleading.

My advice: "Think carefully about making comparisons if the necessary background information is not available; the definitions of phenomena differ among social behavioral scientists, and no robust and reliable preparations exist. Your conclusions will rest upon a house of cards and will easily be refuted by any competent scientist."

6. General theory. Perhaps the most difficult stage of the comparative method is the creation of a general theory. A good example are the learning theories developed by behaviorists such as Delbar Bindra, Edward Guthrie, Clark Hull, Neil Miller, B. F. Skinner, Kenneth Spence, and Edward Tolman (Abramson, 2013). Each of these theorists attempts to explain human behavior in terms of behaviorist and/or neo-behaviorist principles using comparative data. Each theorist met with mixed success and now, unfortunately, despite many aspects of their systems that are still viable, are discarded. What is clear is that no general theory can be developed without a consideration of the previous stages of the comparative method.

One method to create general theories are mathematical models. Such models have a number of advantages including the ability to summarize a wide range of data. A model that I have found useful is the application of the first order transfer function (Stepanov and Abramson, 2008, 2022). This model successfully characterizes subspecies differences in the maze performance of rats, and can characterize deficits in recall associated with Type 2 diabetes, multiple sclerosis, drug/alcohol abuse, and pesticide exposure. It has also been successfully applied to the California Verbal Learning Scale for both children and adults (Abramson and Stepanov, 2007; DeStefano, Stepanov, and Abramson, 2014; Stepanov and Abramson, 2022; Stepanov, Abramson, Hoogs, and Benedict, 2012; Stepanov, Abramson, Wolf, and Convit, 2010).

My advice: "Look into the contributions of early theoretical systems and attempt to create general theories only when reliable data are available and the model is framed in the language of mathematics."

Below I present guidelines for summarizing an article or designing an experiment. The guidelines can be modified to fit the needs of individual researchers and students. Townsend (1953) developed these guidelines and I have modified them previously (Abramson, 1994) and again for this contribution.

Guidelines for Planning or Reporting Experimentation

1. Determine the problem(s)?
2. State the problem in terms of an hypothesis.
3. Provide background information on the problem.
    a. Perform literature review.
    b. Contact other researchers with similar interests.
    c. Describe the subject (e.g., age, sex, gender, developmental stage, natural history).
    d. Determine if the definitions of the phenomena of interest are consistent in the literature.
4. Define the independent variable(s).
5. Determine the dependent variable(s).
6. Decide how the dependent variable(s) is measured.
7. Consider how the independent variable(s) is presented.
8. Assess the environmental variables associated with the experiment (ex., light levels, temperature, humidity)
9. Relate the procedure to a behavioral category.
10. Determine what controls are necessary. Describe how they are instituted.
11. Describe the subject variables.

12. Describe the environmental variables associated with the experiment (temperature, humidity, time of day, etc.).
13. Describe the procedure to be followed in conducting the experiment.
    a. Determine how the procedure relates to others in the literature.
    b. Diagram the apparatus.
    c. Explain exactly what you plan to do.
    d. Determine how the results should be analyzed.
14. Review the research design.
    a. What results, if obtained, would support the hypothesis?
    b. What results, if obtained, would fail to support the hypothesis?
    c. Does the preparation accurately reflect the response class supposedly under investigation?
    d. Does the design lead to a clear example of the behavior under investigation?
15. Conduct the experiment.
    a. Explain unplanned occurrences that were present and that may have influenced the results.
    b. Illustrate the behavior of individual subjects.
    c. Summarize the research results in tables, graphs, and/or other clear means of presentation.
16. Interpret the results.
    a. Describe the tables, graphs, and statistical analysis from the point of view of supporting or not supporting the hypothesis.
    b. Determine whether the experiment yielded reliable information about the response system under investigation.
    c. Present samples of individual data.
    d. State the conclusions and relate back to the research problem under consideration.
17. Can the results relate to a general theory?
18. Can the results be characterized by a mathematical model?

Some Additional Considerations Related to Experimental Design

In the previous sections, I presented information on the comparative method and provided some guidelines for designing and summarizing experiments. In this section, I briefly discuss some supplemental issues related to experimental design in the social sciences.

1. Definitional issues.

As mentioned, one of the problems hindering replicable research in the behavioral sciences is inconsistent and changing definitions of behavioral phenomena. This is obvious in the areas of operant and classical conditioning. Some of the more humorous examples are found in a collection of B. F. Skinner's papers with commentary (Catania and Harnad, 1988). A unique feature of the book is that Skinner comments on the commentaries. His responses make it clear that many scientists misunderstand his system.

It may be surprising to discover that even the definition of learning is constantly changing. An early discussion of attempts to define learning is found in Bullock and Quarton (1966). As Kimble (1961) notes, as early as 1961 definitions of learning span a spectrum of those that concentrate on "facts" to those that focus on the theoretical. All one needs to do is to look at the glossaries of several textbooks on learning to see that the problem persists.

Especially concerning for researchers interested in "cognition" is the fact that there is no consistent definition of cognition. In one study, 12 leading cognitive textbooks were examined and 12 different definitions were found (Abramson, 2013). How can one rationally study "cognition" when there is no universally accepted definition? Frankly, the answer is: "You cannot."

As if definitional issues related to learning and cognition are not problematic enough, the term "behavior" is also fraught with difficulties. I reviewed the glossaries of 138 introductory textbooks in psychology, animal behavior, and biology (Abramson and Place, 2005). Much to my surprise there is no consistency within or among disciplines. Perhaps most surprising is that there is no consensus on a definition of behavior even within the biological sciences (Cvrčková, Žárský, and Markos, 2016). In fact, behavioral biologists do not agree on what constitutes behavior. Levitis, Lidicker, and Freund (2009) found 25 different definitions of behavior. The lack of consistent definitions of behavior across disciplines is reflected in how students defined behavior. For example, those in psychology focused on human behavior while those in biology defined behavior in relation to non-human animals (Abramson and Place, 2005).

One of the most important areas of behavioral science is the study of intelligence. Once again, readers may be surprised to learn that there are over 70 distinct definitions of intelligence (Legg and Hutter, 2007). Sternberg and Detterman (1986) asked 24 social scientists for their definitions and reported 12 different responses.

A cursory reading of the human intelligence literature reveals the problems of defining intelligence (Abramson and Lack, 2014). Schlinger (2003) highlighted this by revealing that definitions of intelligence are not only inconsistent, but that they also change over time, use circular reasoning, and he suggests that "intelligence" has a "physical substrate" rather than being just a psychological concept. Intelligence is also used to describe the behavior of plants with little consideration of the definitional issues of the term (Abramson and Calvo, 2018; Abramson and Chicas–Mosier, 2016).

Social science researchers can now study naturalistic intelligence, musical intelligence, emotional intelligence, interpersonal intelligence, spatial intelligence, analytical intelligence, creative intelligence, and practical intelligence, among others (Gardner, 2006; Sternberg, 1984). If this trend continues it will not be long before someone begins to study "unconscious intelligence." The term intelligence is also widely used in robotics under the canopy of "AI" (artificial intelligence; Nilsson, 2009). How can anyone accurately study or compare the intelligence of animals, "brainless systems," or a machine, if there is no universally accepted definition of what it is? Without consistent and universally accepted definitions, how is a behavioral scientist able to compare these various "intelligences"? The answer is you cannot reliably do so.

My advice: "Be weary of inconsistent definitions."

2. Behavioral taxonomies.

As discussed earlier, there is a need for behavioral taxonomies in the social sciences. In the area of learning, for instance, Tulving (1985) identifies how a taxonomy can advance the field. One of the most important benefits of a behavioral taxonomy is that social scientists can speak "the same language." Ill-defined terms such as classical and operant conditioning would be replaced with descriptions of the procedure. One effect of this strategy is to limit erroneous generalizations of empirical data across species. Another benefit is that theoretical developments are specified with greater accuracy precisely because they are anchored in a strong and universally accepted taxonomy. Moreover, innovative procedures and fresh results can readily be incorporated into a well-designed taxonomy. Researchers need to develop a classification scheme to systematize the various procedures used in the behavioral sciences.

My advice: "Consider how your procedure relates to the existing behavioral taxonomies."

3. Analogous or homologous comparisons.

The comparative method was developed as a way to compare some feature of one species or subspecies with another. One aspect of the method which has received little attention is how to determine if a comparison is valid. One way to help a researcher to do this is to decide whether the behavior of interest is analogous or homologous. Richard Owen (1843) established the basic rules in 1843 (Boyden, 1943; Camardi, 2001; Gray, 1966) to provide a strategy for rationalizing the comparisons to be made.

Owen was concerned with providing a rationale for making anatomical comparisons. For example, the wings of a bat, the flippers of a sea lion, and the arm of a human can all be rationally compared because, although their functions are different (i.e., flying, swimming, and lifting), the anatomical arrangements of the bones are structurally similar. This is known as an homology and implies that the structures come from a related ancestor of common descent. On the other hand, it is also possible to make rationale comparisons when the function is the same but the structures are different. Consider the wings of honey bees and ravens. Here the structures are different, have similar functions, and are not related to an ancestor of common descent.

Owen suggested that as anatomical structures may or may not be alike, and, as the functions of anatomical structures may or may not be alike, there are four possible comparisons. As summarized by Gray (1966) in his excellent discussion of the application of analogies and homologies to behavior, when two behaviors are being compared and are alike in structure, they are homologous; when not alike in structure, they are not homologous. When the functions of two events being compared are similar, they are analogous, when not alike in function, they are not analogous. These four possibilities are shown in Table 2 below. The example is based on Gray's experiment examining whether imprinting occurs in human infants as it does in birds.

Prior to his experiment on the following behavior of ducks and chicks, Gray (1966) used the first stage of the comparative method to gather background information. Based on his knowledge of birds, he reasoned that there might be an analogous relationship between the following response of a duckling and the smiling response of a human infant. Gray first analyzed the following response of the duckling and chick and determined that the response has a number of components including orientation toward the mother and reducing the distance between the mother and the chick whenever the mother moves away. Therefore, the function of the behavior (following response) and its structure (orientating toward the mother) is both analogous and homologous. This type of comparison is illustrated in panel A of Table 2.

When gathering background information on imprinting (i.e., stage one of the comparative method), Gray realized that the imprinted response of a duckling and the smiling response of the human infant represents a social interchange between the offspring and the parent. This social interchange includes orientating toward the mother and then engaging in a social act. The comparison of the imprinting response in the duckling and the smiling response of the infant is represented in panel B of the table and is called an analogy. Determining whether two behaviors are analogous is difficult, but this is where the most interesting comparisons are made.

Panel C of the table represents a spurious comparison. In the example provided by Gray, consider a situation where the experimenter wants to compare the following response of a duckling with the stalking behavior of a cat. In both instances, the duck and the cat reduce the distance from the object of interest. In the case of the cat it is stalking prey; in the case of the duckling it is following its mother. In this example, the structure of the behavior is similar as both animals are moving and orientating toward some object. However, the function of the behavior is quite different. In the case of the cat, it is moving toward a prey item and in the case of the duckling it is moving toward its mother. While such a comparison can be made, it bears little relevance to the issue whether imprinting in ducklings and humans share some characteristics and is therefore a false or spurious comparison.

Now consider an example where a researcher wants to compare the smiling response of an adult post-puberty human with the following response of the duckling. While such a comparison can be made, it is not fruitful because the smiling responses of the adult human and the following response of the duckling are not similar in structure or function. This is represented in panel D as a comparison not worth conducting, as it would waste resources, and lead to no solid conclusions.

My advice: "Prior to conducting comparisons determine if the behavior you are comparing is analogous or homologous."

Table 2

The Use of Homologies and Analogies[1]

		Appearance	
Function		Similar (Homologous)	Different (Non-Homologous)
	Similar (Analogous)	A. Following response of chick and following response of duckling (This is a homologue)	B. Following response of chick and smiling response of human infant (This is an analogue)
	Different (Non-Homologous)	C. Following response of chick and stalking response of cat (This comparison is spurious)	D. Following response of chick and smiling response of adult human (No comparison)

4. Systematic variation.

Systematic variation refers to a control procedure where the experimenter "systematically varies" possible explanations before reaching a conclusion. Systematic variation is especially important because it serves as a reminder that alternative explanations must be evaluated before inferring that, for example, a species, subspecies, gender, or racial difference actually exist.

Consider a situation in which women outperform men on one of the many types of intelligence tests. Individuals not familiar with comparative research methods might conclude that "women are more intelligent than men." While this

[1] From Gray (1966)

may turn out to be correct, it cannot be concluded before possible explanations are "systematically varied." For example, the male participants may not be motivated to complete the task. In this example, motivation in the men will have to be systematically varied and if the differences among men and women persist, then motivation is ruled out as a possible factor. Once motivation is ruled out, the researcher may direct attention to the properties of the intelligence test. Perhaps the test itself contains some inherent bias favoring women. Here, again, the researcher must systematically vary the type of test.

If using a different intelligence test still leads to women outperforming men, the researcher may be more confident that a real gender difference exists for this particular task. Systematic variation should continue until all possible explanations are systematically explored. Control by systematic variation is an expensive procedure both in time and resources but it needs to be done to limit unsupported and erroneous generalizations.

My advice: "Before generalizing your results, systematically vary possible explanations."

5. Morgan's canon.

Systematic variation is a control method that tries to limit unsupported generalizations of behavioral science researchers. Another approach is known as Morgan's canon. Morgan's canon is not a control procedure as it is a philosophical position that encourages researchers to limit their speculations when making comparisons (Karin–D'Arcy, 2005; Thomas 2019).

The original statement of the canon appeared in C. Lloyd Morgan's introduction to comparative psychology (Morgan, 1894). As the original statement was often misunderstood, he clarified the canon in a latter publication (Morgan, 1903). As Morgan states:

> In no case is an animal activity to be interpreted in terms of higher psychological processes, if it can be fairly interpreted in terms of processes which stand lower in the scale of psychological evolution and development. To this, however, it should be added, lest the range of the principle be misunderstood, that the canon by no means excludes the interpretation of a particular activity in terms of the higher processes, if we already have independent evidence of the occurrence of these higher processes in the animal under observation. (p. 59)

The canon contains several important principles. First, behavioral science researchers are cautioned not to assume a higher level of processing if a lower level can satisfactory account for the data. Secondly, one must view with caution the tendency to anthropomorphize human explanations of behavioral phenomena to animals. Third, a researcher must not overlook the possibility that a more fundamental and reasonable explanation of an animal's behavior may also be appropriate when observing the same behavior in humans.

The history of Morgan's canon is an interesting story as are the issues and controversies the canon has engendered. An excellent discussion of Morgan's canon is available by Karin–D'Arcy (2005) and by Thomas (2019).

My advice: "Be aware of the cautionary tale provided by Morgan's canon."

6. Quantitative vs. qualitative research.

Quantitative and qualitative research are two sides of the same coin. On one side is the parametric research characterized by Stage 4 of the comparative method: parametric investigation of variables. Quantitative research is vital as it provides the researcher with a reliable database to estimate, for example, the values of training variables such as stimulus intensity, number of training trials, and amount of reward. It is impossible to search for qualitative or "process" differences in performance without a solid database of parametric manipulations.

Sometimes these parametric manipulations are used as a basis of comparison. For example, it is common in the comparative psychological literature to see organisms ranked in terms of how many trials are necessary to reach some criteria, the latency to respond to some stimulus, or the time taken to consume some commodity. These are known as quantitative comparisons.

However, the more interesting comparisons are to be found in qualitative research and this is represented on the other side of our research coin. Here, organisms are compared based on their ability to adjust or adapt to changing situations. A major proponent of this research strategy is seen in the work of Morton Edward Bitterman (Bitterman, 1965).

Bitterman and his colleagues tested a number of animals including rats, pigeons, fish, roaches, turtles, earthworms, and bees on two problems they thought would reveal the behavioral strategies these animals used. Moreover, the researchers assumed that these strategies would reveal species and subspecies differences. The problems were reversal learning and probability learning.

In reversal learning, the organism is confronted with a discrimination problem. For example, turning to the left in a T maze leads to food and turning to the right does not. After a few experiences the organism learns to go left. Once some criterion is met, such as five consecutive errorless runs, the conditions are reversed. Now, going to the right leads to food and going to the left is no longer effective. Once again, when the criterion is met, the conditions are reversed. This process is continued throughout the experiment.

The question of interest is how long does it take for the organism to adjust to these changing circumstances. The reversal learning design can be made more complicated by adding visual cues, rather than just position or place cues, but the rationale behind the experiments are the same: how long does it take the organism to adjust.

The second task is known as probability learning. In this design, the organism is also confronted with a choice. However, the choice is not whether food will always appear in one arm of a maze or the other but what is the probability that it will do so. For example, in a "70–30 problem" using a T maze, food will appear in the left arm 70 percent of the time and on the right 30 percent of the time. The question of interest is how does the animal distribute its responses. It is also possible to incorporate visual stimuli into the design and to look at what the organism does after consuming the reward — does it make the same choice after receiving the reward (reward following), or does it switch to the non-rewarded side (reward adverse).

When designing a discrimination experiment such as probability learning, a decision must be made on how to randomly present the stimuli. One way to do this is to use a totally random sequence. I do not recommend this because a totally random sequence is beyond the control of the experimenter. Instead, I recommend a pseudo-random sequence consisting of the sequence "ABBA BAAB" where "A" is a stimulus or position and "B" is the second stimulus or position. For example, in a simple classical conditioning discrimination situation of eight trials, "A" might represents the CS+ (a conditioned stimulus followed by the unconditioned stimulus) and "B" represents the CS– (a different conditioned stimulus not followed by an unconditioned stimulus). The order of trials is represented below. If more trials are necessary, the researcher adds more ABBA BAAB sequences. In addition to the advantage of knowing the "random" sequence, the researcher can look at the transitions between the As and the Bs.

Table 3

Example of the Use of a Pseudo-Random Sequence for Discrimination Learning

Trial Number	Letter	Condition
1	A	CS+
2	B	CS–
3	B	CS–
4	A	CS+
5	B	CS–
6	A	CS+
7	A	CS+
8	B	CS–

My advice: "Both quantitative and qualitative comparisons are important. Quantitative comparisons form the foundation of qualitative comparisons. You cannot have one without the other."

7. Statistical analysis: Observation oriented modeling.

A difficult challenge facing social science researchers is to decide what statistics to use. I suggest researchers consider Observation Oriented Modeling [OOM] (Grice, 2011; Grice, Barrett, Schlimgen, and Abramson, 2012). Observation Oriented Modeling is a collection of nonparametric methods which requires researchers to hypothesize an expected pattern of results and then determines how many individuals or entities match that predicted pattern. To determine how many organisms behaved as expected, OOM computes a person-centered effect size, referred to as the Percent Correct Classification (PCC) index. The PCC index is the computed proportion of individuals/organisms who conformed or behaved as expected within the hypotheses. Moreover, a randomization test on the PCC index can be conducted to determine whether the resulting value should be explained as having arisen by chance (Grice, 2021).

Observation Oriented Modeling has been used in a number of comparative investigations including social reinforcement delays, timing (Craig, Grice, Varnon, Gibson, Sokolowski, and Abramson, 2012; Craig, Varnon, Sokolowski, Wells, and Abramson, 2014; Craig, Varnon, Pollock, and Abramson, 2015), and taste aversion learning (Varnon, Dinges, Black, Wells, and Abramson, 2018). The program is easy to use and well supported.

My advice: "Consider using Observation Oriented Modeling as a stand-alone statistical package or in conjunction with traditional statistics."

8. Report individual data and behavioral observations.

One of the unfortunate aspects of contemporary behavioral research is that few manuscripts contain examples of individual data and behavioral observations. The former has been addressed somewhat by the increased use of data depositories. Observations or examples of the behavior of interest are rarely available in published manuscripts. However, even here, some journals have the capability of including supplemental video clips (Abramson, 2021).

Observations provide fellow social–behavioral science researchers with additional data. These data are necessary to develop apparatus and to design additional experiments. Observation also serves as an additional check on replication because the observations that the original researcher reported should also be observed in future studies. Moreover, if the original experiment cannot be replicated, an examination of the observations contained therein may reveal why.

My advice: "Include observations of what your subjects and participants are actually doing. Answer the question: 'What does the behavior look like?' "

Conclusions and Discussion

The goal of this tutorial is to highlight some of the comparative research methods useful for research in the behavioral sciences. Many of the lessons learned by comparative psychologists in regards to research are over 100 years old and are well worth re-discovering. The job is made more difficult because comparative psychology is in decline as reflected in the lack of textbooks, few graduate programs, and insufficient undergraduate course offerings (Abramson, 2015, 2018). As a result, I fear that many of the research designs, and the philosophy behind them, will be lost.

I have taught comparative psychology for over 30 years. Over the many years I was struck by how few students ever heard of comparative psychology or its research methods. Perhaps this is not surprising given that many faculty have never heard of comparative psychology even though the first use of the term appeared in 1778 by the German scholar Michael Hissmann (d'Isa and Abramson, 2023). As I result, I believe what is needed is a brief and highly readable tutorial accessible to students and faculty alike, that highlights some of the major contributions to research design made by comparative psychologists.

This tutorial can be incorporated into formal course readings, as directed readings, and as a stand-alone general interest article. I believe this tutorial is especially important because no research method textbook that I am aware of, or introductory psychology text, mentions the comparative method or how to use it. Moreover, concepts such as systematic variation, Morgan's canon, and analogies and homologies are not discussed in research method courses. If the next generation of social science students are expected to have a firm grasp of the research process, a tutorial on the lessons learned from comparative psychology, such as presented here, is necessary.

As the oldest organized psychology, comparative psychology has much to offer the contemporary behavioral scientist. The time is right for these methods, and the hard lessons learned, to be re-discovered.

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