The Case for Inductive Theory Building*

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ABSTRACT

This paper argues that theory building in the social sciences, management and psychology included, should be inductive. It begins by critiquing contemporary philosophy of science, e.g., Popper's falsifiability theory, his stress on deduction, and the hypothetico-deductive method. Next I present some history of the concept of induction in philosophy and of inductive theory building in the hard sciences (e.g., Aristotle, Bacon, Newton). This is followed by three examples of successful theory building by induction in psychology and management (Beck's theory, Bandura's social-cognitive theory, goal setting theory). The paper concludes with some suggested guidelines for successful theory building through induction and some new policies that journal editors might encourage.

THE CASE FOR INDUCTIVE THEORY BUILDING

Everyone who publishes in professional journals in the social sciences knows that you are supposed to start your article with a theory, then make deductions from it, then test it, and then revise the theory. At least that is the policy that journal editors and textbooks routinely support. In practice, however, I believe that this policy encourages—in fact demands-- premature theorizing and often leads to making up hypotheses after the fact—which is contrary to the intent of hypothetico-deductive method.

Contemporary Philosophy of Science

My thesis, however, is that the hypothetico-deductive method, even if practiced, actually retards the progress of science. Where did this method come from? Its roots lie in the ancient world, especially Plato and his followers who asserted that the senses were invalid and that knowledge came by intuitively identifying innate forms in the mind from which further knowledge was deduced. Deduction involves going from the general to the particular. The worship of deduction in the social sciences was reinforced by philosophical skepticism starting with Hume and Kant.

Kant represented a major turning point in philosophy. The key result he brought about was the destruction of confidence in the power of the human mind, specifically the capacity of the mind to know reality (i. e., the noumenal world.) According to Kant we can only know the world as it appears to us, the phenomenal world (Harriman, 2006; Ghate, 2003; Peikoff, 1982). The end result, after a long series of intermediary philosophers, was postmodern skepticism-- which necessarily ends in nihilism (Ghate & Locke, 2003).

Influenced by Kant's views, modern philosophers of science like Kuhn and Popper claimed that scientific certainty about the real world was impossible and that induction was invalid. "…a principle of induction is superfluous, and it must lead to logical inconsistencies" (Popper, 2003, p.5). The problem for Popper was that making a universal statement from specific experiences would require further experiences, thus leading in the end to an infinite regress. (Of course, it would only be a theory of experiences, not facts, since reality was unknowable.) To him "Theories are…never empirically verifiable." (Popper, p. 2003, p. 18.). Instead, Popper argues that empirical "testing" of a scientific theory can involve no more than seeing if it can be falsified. Nor does Popper embrace the notion of causality, "I shall, therefore, neither adopt nor reject the 'principle of causality'; I shall be content simply to exclude it as 'metaphysical', from the sphere of science." (2003, p. 39). Metaphysical means pertaining to the basic nature of reality, which, as noted, Kant said was unknowable. Popper also rejected the idea of objective concept formation, "it is widely believed that it is possible to rise by a process called 'abstraction' from individual concepts. This view is a near relation to inductive logic….Logically these procedures are equally impracticable" (2003, p. 45).

In sum, Popper (2003) rejected not only induction but everything that makes induction possible: reality (specifically, the ability to know it), causality and objective concept formation. Popper rejected the whole concept of proof and claimed that science "advances" only by disproving theories. This brings up the question: where do theories come from, if not from integrating observations and discoveries by induction? Popper had no real answer to this, "…there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying

that every discovery contains ‘an irrational element’, or ‘a creative intuition’” (2003, p. 8). Observe the Platonic element in this theory, although Plato would have argued that rational intuitions can be made by philosophers.

Popper’s (2003) replacement for induction was deduction. Since there was no rational method of formulating a theory inductively, one could, in effect, start from anywhere one wanted, since one theoretical starting point was as good as another--deduce a theory, and then try to falsify it.

Popper’s (2203) views were reinforced in an influential article written by Platt in 1964 called, ironically, “Strong Inference.” It claimed to explain the rapid progress made in some fields of science like biology by identifying the method involved. Step 1 involved making “intellectual inventions” from which hypotheses were then deduced (Step 2). Then they were tested and the theories or hypotheses were adjusted accordingly, the erroneous ones being falsified. (Step 3). Platt stresses the process of testing competitive hypotheses, but he gives virtually no account of how one develops theories or hypotheses in the first place. For example, he writes, “But if you stop doing experiments for a little while and *think* how proteins can possibly be synthesized, there are only about 5 different ways, not 50! And it will take only a few experiments to distinguish these” (Platt, 1964, p. 348).

But how could one know there could be only 5 ways and not 50 unless it was based on evidence gained from prior research? Otherwise such a claim would be totally arbitrary. How does one formulate these hypotheses or theories in the first place? Platt (1964) never says; presumably he agrees with Popper (2003) that “intellectual inventions” represent some intuitive process. Although Platt (1964) himself claims to be

advocating an inductive process, the process of testing hypotheses, competitively or otherwise, can only occur *after* one has gathered evidence from which the predictions are deduced. Thus there can be a rational use of intuition based on previously gained knowledge stored in the subconscious, but the subconscious must contain material based on reality in order for it to be potentially useful. Nor can the subconscious substitute for the hard work of thinking and discovering.

Furthermore, once we have falsified a hypothesis, where are we? Unfortunately, nowhere. Platt even undermines his own method in agreeing with Popper that "there is no such thing as proof in science—because some later alternative explanation may be as good or better—so science advances only by disproofs" (Platt, 1964, p. 350). This means that science advances, in effect, by showing theories not to be true.

The falsifiability approach falls apart even on its own terms. Your falsification might itself be false. After all, there is no certainty even in falsification, because it involves gathering actual evidence that disproves a theory. But how do you know whether that evidence is valid? You would need to see if you can falsify your falsification and so on. Logically, the falsification approach must lead to an infinite regress—the very problem that Popper (2003) claimed was the fatal weakness of induction.

More fundamentally, however, how could science ever advance by showing that something is *not* true? What would we gain by showing that genes were *not* controlled by four-leaf clovers or that the planets were *not* kept in orbit by tiny gold strings? The history of science clearly shows that it advances by discovering things that are true. If this were not the case, we would still be living in the dark ages. In reality, science has

not, and could not have, progressed by the process of falsification; it progressed only by the process of making positive discoveries.

When dealing with causal relationships, one does have to rule out alternative explanations (by controlling variables), but that is what the experimental method is for. However, ruling out alternatives is virtually useless unless there is something positive to account for the relationship. Ruling out 1000 hypothesized causes of the plague may be slightly helpful, but this does not explain what actually causes the plague or how, i.e., an infection by the bacterium *Yersinia pestis* through personal contact or flea bites. When Thomas Edison found that hundreds of different materials failed to work as light bulb filaments, this was useful to know because those materials could then be ignored. But he still had to find a filament (a cotton thread coated with carbon) that did work (Josephson, 1959). The invention of a practically workable light bulb represented true scientific progress.

The Concept of Induction and the Basis of Scientific Progress: Some Historical Observations

To see more specifically how science really progressed, let's go back in time to before Hume and Kant, starting with Aristotle (384-322 BC), the first ancient philosopher of science, and Francis Bacon (1561-1626), the first modern philosopher of science (Farrington, 1961). Aristotle credits Socrates with the discovery of the method of induction: the process of proceeding from particulars to the general (universals). It is not known whether Socrates believed in the validity of the senses, but Aristotle did. His view was that one groups entities according to their perceived similarities and identifies their essential characteristics, the essence of a kind (McCaskey, 2006). This included the formulation of definitions based on genus and differentia. Aristotle's theory did not go much beyond this, though be believed that induction was logically prior to deduction.

Over the following centuries, Aristotle's view of induction became widely misrepresented, e.g., it was claimed that he viewed induction as complete enumeration and that it was reducible to deduction (McCaskey, 2006). Bacon strongly criticized this misrepresented view as well as the views of all previous scholastics and philosophers. The prevalent view of how to gain knowledge at Bacon's time consisted mainly of blind acceptance of or deduction from the ancient philosophers. This accepted method was called by various names: scholasticism, dogmatism, rationalism. The flaw was that the method was divorced from reality, i.e., facts. Note the rationalistic influence of Plato: he did not think the senses were valid; thus reality could not be grasped except by deduction.

Bacon championed induction, based on Aristotle's actual approach (even without fully realizing it; McCaskey, 2006) of using the senses to observe similarities and differences between existents. Bacon's most important and original contribution to the theory of induction, however, was to argue that it must entail not only detailed observations of nature, including similarities and differences, but also (going beyond Aristotle) the discovery of causal relationships by experiment. This was the method for discovering the essential nature of things and thus made generalization possible without complete enumeration. Although Bacon favored active intervention into nature (experiments), he did not think it was necessary or possible in all cases (e.g., astronomy).

Neither Aristotle nor Bacon developed a fully valid theory of concept-formation, the method by which one integrates sensory material. That discovery was to come later. But Bacon's ideas were enough of a foundation to start the physical science on the road to stunning discoveries.

Perhaps the best historical example of an inductive genius (prior to the 20th century) was Isaac Newton. Newton (1642-1727) discovered the nature of white light through experiments with prisms. He showed that prevailing theories of the nature of white light were false, but only as a by-product of showing that white light actually was: a mixture of different colors (Harriman, 2002). Newton, explains Harriman (2002, p. 15):

> built on the knowledge and proper methods discovered by Galileo and Kepler, while explicitly rejecting the Cartesian approach [see below]. He didn't start from "innate" ideas and demand that reality conform to them, but instead, following Aristotle, he looked out at the world and inquired about its actual nature. He rejected the Platonic dichotomies between reality and appearance, reason and sense perception. He understood that the data of the senses are valid, and that all scientific knowledge is based on such data. He was the modern era's greatest exponent of the inductive method, which proceeds from the observed effects to the causes.
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> ….Galileo [see Harriman, 2000, for more on Galileo] pioneered the experimental and mathematical method of physics, but Newton carried the new method to a level none of his predecessors even imagined possible. His experimental work served as a model for how physical science ought to be done, and he invented a new branch of mathematics, which served as an indispensable part of the language of physics. Furthermore, the scope of Newton's integrations set a new standard in science

What were these inductions? He did experiments on circular motion showing that bodies move with constant speed in a straight line (which Galileo had not grasped) unless some external cause affected them. Building on Galileo, he worked to understand circular acceleration. He invented calculus which allowed him to develop the uniform law of circular motion. He the integrated this discovery with Kepler's third law and discovered the law of planetary motion: the sun accelerates the planets with a force that is inversely proportional to the planet's distance from the sun. He then showed, using additional data, that the same law applies to the moon's movement around the earth.

Newton then sought to discover how a body's mass affects motion. He showed with his own pendulum experiments that mass is proportional to weight. Further experiments with colliding pendulums showed that the pendulum bobs exert forces on one another that are equal and oppositely directed. Further experiments showed that the same principle works at a distance, e.g., magnets and iron attract each other.

In order to formulate his laws Newton needed not only new concepts (e.g.,

mass) but "a variety of experiments that studied free fall, inclined plane motion, pendulums, projectiles, air pressure, double pendulums and floating magnets" (Harriman, 2006, p. 18) plus observations about the movement of the planets, the distance to the moon, and instruments to measure force. "His laws apply to everything that we observe in motion, and he induced them from knowledge ranging across that enormous data base." (*Ibid*.). In the Principia Newton explicitly rejected the idea of formulating hypotheses unsupported by evidence, that is hypotheses that were arbitrary (Harriman, 2002, pp. 16-17). Contrast Newton to Rene Descartes (1596-1650) whose approach to science was the opposite of the inductive method. According to Harriman (2002, p. 13) Descartes:

> explicitly rejected the Aristotelian method … and he criticized Galileo for using such an approach, "He seems to me very faulty in . . . that he has not examined things in order, and that without having considered the first causes of nature he has only sought the reasons of some particular effects, and thus he has built without foundation" [Drake, 1995, pp. 387-8]. In contrast, Descartes explained that his goal was "to deduce an account of effects from their causes…" [Cottingham, Stoothoff & Murdoch, 1985, p.249]. Of course, Galileo had realized that we arrive at knowledge of the causes only after a long process of observation and reasoning. But Descartes believed he had direct access to the first causes by means of his innate ideas.

Based on nothing more than such "innate ideas" Descartes (obviously influenced by Plato) developed theories on numerous subjects in physical science. He developed theories of: the components of matter, the planets, the causes of movement, the formation of the solar system, the nature of light, the formation of stars, the nature of tides, the causes of earthquakes and mountains, the nature of magnetism and static electricity, and the nature of chemical interactions. He developed all his theories by deduction from arbitrary first principles [creative intuition?], and, as a consequence, they were all wrong. As Harriman (2002, p. 14) explains, Descartes "made no observations, did no

experiments, and engaged in no reasoning from effects to underlying causes. Instead, he looked inward and offered a 'clear and distinct' make-believe world that was more imaginative than any fairy tale." Of course, Descartes could have done experiments but they would have only served to expose the futility of his approach. (His method did not work any better in proving the existence of God or that the soul meets the body in the pineal gland).

One could show that induction was the method used by other famous scientists such as Einstein (e.g., 1921, p. 238), Darwin (Mayr, 2001) and Galileo (Harriman, 2000) but that is beyond the scope of this paper. The fact that inductive scientists made errors does not invalidate induction. Errors can be made due to making inductions without sufficient evidence (e.g., Galileo had a totally wrong theory of tides). Such errors are corrected by later inductive work (e.g., Newton discovered the actual causes of tides). Also one's conclusions can be correct based on all the knowledge available at the time but may later have to be modified in an expanded context of knowledge (e.g., Newton's Laws had to be modified by Einstein's equations).

It is critical to recognize that science does not progress by going suddenly from total ignorance to omniscience. There is no such thing as omniscience. If this is what Popper meant by a "universal statement" then it is a false (irrational) standard. Science develops incrementally—by a process of continuous discovery. Errors are made but are corrected based on attempts at replication and on new discoveries.

I do not think it is any accident that the fields of physics, biology and medicine have progressed much faster than fields within the social sciences. Granted, the social

sciences came much later historically. On the other hand, they allegedly had the entire history of the "hard" sciences to learn from.

Unfortunately, social scientists learned the wrong thing from the hard sciences. For example, since consciousness could not be measured by the methods of physics, it was considered unscientific by the behaviorists. This led to a rejection (after Watson) of introspection as a legitimate method. Its rejection robbed psychology of a key source of data for inductive theorizing.

Further, the social sciences accepted the hypothetico-deductive model which eventually, as the inductive method came increasingly under attack, became the official doctrine endorsed by philosophers of science. As noted earlier, this meant that researchers often had to pretend that they had theories before they had a firm basis for any. This method makes for quick and often short-lived theories; in contrast, true inductive theorizing takes many years, even decades, and, I believe is far more likely to withstand the test of time.

Successful Inductive Theory Building in Management and Psychology: Three Examples

There are many theories in management and psychology, but many would agree that there are a limited number of good theories—by which I mean theories that have been well-validated and have withstood the test of time. I will discuss three of them here, each from a different specialty area: Beck's (Clark & Beck, 1999) cognitive theory of depression was developed in psychology, but its implications, such as training people in positive self talk has managerial applications (e.g., Millman & Latham, 2001). Bandura's social-cognitive theory (1986, 1997) has been applied to many realms, including management (e.g., Wood, Bandura & Bailey, 1990). Goal setting theory, developed by

myself and Latham (Locke & Latham, 1990, 2005), fits within both I/O psychology and OB. My focus will be in showing how these theories were developed as reported by the theorists themselves. I make no claim that these are the only valid theories in management and psychology. (Relevant references are provided in each section below).

Beck's Cognitive Theory of Depression. Aaron Beck's cognitive theory of depression is well known (see Clark & Beck, 1999, for the most recent presentation of the theory). Furthermore, the practical success of his theory, in terms of successful treatment of depression, is firmly established (Butler, Chapman, Forman & Beck, 2006). How did the theory come about? In "Cognitive Therapy of Depression: A Personal Reflection," (Beck, 1993, p. 1) states: "Cognitive therapy did not emerge full-blown but went through many tortuous paths before it reached its present form"

He claims in his reflection that his diary notes from 1956 reveal his first discovery regarding the importance of cognition. He was practicing psychoanalysis. A patient, M, was free-associating in line with "good" practice and was angrily criticizing Beck. Beck asked him what he was feeling. Besides anger, the patient was experiencing something else which he had not previously mentioned. This other stream of thought consistent of self-criticisms such as "I said the wrong thing…I should not have said that…I'm wrong to criticize him. I'm bad…" (Beck, 1993, p. 2).

> This incident constituted my *first surprise* and also presented me with an anomaly. If the patient was actually reporting everything that came to mind, how could he have experienced a conscious flow of associations and not report it? Further, how could two streams of thoughts occur simultaneously?
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> The answer to the question embodies an important principle. There can be more than one stream of thoughts running parallel to each other in the patient's stream of consciousness. The first stream, which was more readily expressed in free association, represented the most conscious component. The second stream, more

> at the periphery of awareness and not generally reported, probably corresponded to what Freud had described as "preconscious" [subconscious].
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> My formulation of this observation was that M's critical thoughts were an intermediate variable between his angry expressions and his guilty feelings (Beck, 1993, p. 2).

Beck proceeded to verify this conclusion with other patients. They too experienced double streams of thinking. The second stream he labeled "automatic thoughts." Here is how Beck described them, "First, they tended to be very fleeting. Second, they were just on the fringe of consciousness. Third, they were not the kinds of thoughts that individuals were accustomed to verbalize to other people" (1993, p. 3).

Beck asked patients to start noticing these thoughts, specifically the ones that occurred just before they experienced a particular feeling. He started this practice with the very next patient after M. He discovered that just prior to experiencing anxiety regarding a sexual relationship, she reported thoughts such as, "He is bored with me….He will probably get rid of me." (1993, p. 3)

Beck then made similar observations in other patients, in friends and relatives and added these to his own introspective observations. He concluded that these subconscious thoughts involved rapid, automatic interpretations of events and evaluations of the self. Furthermore, these subconscious thoughts were not confined to reactions to the therapist ("transference") but widely generalized. The patient who believed she was boring believed she was boring in all social situations.

Beck then trained all his patients to report subconscious thoughts. He observed that "in ambiguous situations, the depressed patients were particularly prone to make a negative interpretation when a positive one would seem to be more appropriate."

(1993, p. 5). This material from patients' thoughts "provided me with the raw data for constructing a theory of psychopathology as well as a therapy" (1993, p. 5)

He went on to discover a variety of other errors in the thinking of depressives: selective abstraction, over-generalization, [inappropriate] dichotomous thinking, exaggeration, and negative expectations. Beck observed that the depressed themes expressed a "negative cognitive triad…a negative view of themselves, their personal worlds and their futures" (1993, p. 6). Furthermore, these same themes characterized the patients' waking experiences and appeared across all types of depression. Strong negative expectations, he observed, could lead to feelings of hopelessness which were predictive of suicide.

Another source of data came from Beck's studies of patients' dreams, dream analysis being an important part of psychoanalysis. The theory was that depressed patients needed to punish themselves due to guilt over hostility towards others. This hostility towards others would presumably be revealed in patients' dreams— Freud's "royal road to the unconscious." To his surprise, however, he found that depressed patients tended to have dreams in which they were the victims of unpleasant events rather than aggressors. "[T]he depressed patients showed less hostility in their dreams than did the non-depressed patients." 1993, (p. 8)[1]. The same negative themes appeared in dreams as had appeared in patients' reports of their automatic thoughts.

Of course, psychoanalysts could readily claim that these were masochistic dreams in which patients were directing their hostility against themselves rather than others, and Beck found that an objective scoring system did reveal more masochistic

dreams among depressives as compared to non-depressives. However, several other studies, using a variety of methodologies, failed to confirm the original results.

Beck reports, “In trying to put all of these findings together, I posed the question: could we take the simple-minded view that the manifest content of dreams instead of being an expression of a deep-seated need for punishment or inverted hostility just reflected the way patients viewed themselves and their experiences?”(1993, p. 11).

He then made another key breakthrough: the data he had gathered from his study of automatic thoughts matched perfectly with what he had found from his studies of dreams: both represented “a negative distortion of reality”(1993, p. 11) Beck says, “This phase of discovery was in many ways the most thrilling of my professional career.”(p. 11)

The next question Beck asked himself was: how can this discovery be used in therapy? He decided to ignore the unconscious and focus directly on the patients’ automatic thoughts. Rather than trying to give patients insight into the deeper meaning of their thoughts and dreams, he worked with them to test the validity of their negative thoughts and conclusions, viz., I am bad.

Through discussion and joint problem-solving, patients’ conclusions were subjected to empirical test in order to uncover distorted thinking. Patients were given homework consisting of reading and graded assignments—each assignment being a step towards a goal.

Patients began to improve rapidly and usually after twelve sessions they were well enough to terminate therapy, although booster sessions were recommended. Beck then conducted controlled experimental studies using other therapists who were

trained in his newly discovered methods. Cognitive therapy was compared to treatment with drugs, behavior therapy and other types of therapy. As noted earlier, the results were very favorable to cognitive therapy. Beck and others also developed questionnaires to measure depression which were found to be very accurate in distinguishing depressive from non-depressives.

It should be made clear that Beck considers negative thinking to be a proximal cause of depression, not necessarily the ultimate cause. He hypothesizes that more remote causes could include genetic predisposition, early life experiences, and negative core beliefs. Nevertheless, changing the proximal causes is effective in changing the depressive symptomology and the benefits are long lasting.

Bandura's Social-Cognitive Theory. The initial focus of social-cognitive theory (Bandura, 1962) was on role modeling. Bandura says (personal communication):

> I rejected the behavioristic conception of social modeling and the experimental paradigm used to test it for two reasons. 1. The necessary conditions they posited for modeling (social cue, matching response, and reinforcement) violated the conditions under which observational learning occurs in everyday life. 2. They misconstrued observational learning of new patterns of behavior as a special case of discriminative performance of pre-existing behavior under social stimulus and reinforcement control.…
> A theory that dismisses cognitive determinants as explanatory fictions has problems explaining how people learn complex patterns of behavior by observation without performing responses, without being reinforced for performing them, and exhibiting the modeled style of behavior on later occasions in the absence of a cueing model.

The behaviorist model, argues Bandura, required an inefficient trial and error procedure whereas modeling in fact short cuts this tedious process."I found this [behaviorist] conception seriously wanting on the determinants, mechanisms and scope of

observational learning. We launched a program of research on observational learning as it typically occurs in the absence of reinforced performance" (Bandura, 2005, p. 11).

Unlike Beck, Bandura rejected the prevailing paradigm (behaviorism) in his sub-field in advance of his actual experiments. He and his colleagues then proceeded to design studies to show how modeling actually worked. He identified four classes of cognitive functions that regulated it (Bandura, 1986): what the subjects paid attention to; what the observers retained in memory (often through mental rehearsal) and how they structured what they retained (e.g., in the form of images, rules, principles, etc.); how they translated conception to appropriate action; and whether they were motivated to act based on expected rewards, social evaluative reactions or self-evaluative reactions. Behavioristic theory was replaced by studies showing that modeling was cognitively mediated. Bandura and others also found that thinking itself could be modeled and that modeling could promote creativity.

Bandura (2005) was interested in social change and integrated his work with the insights of social network theory. With the aid of others he saw modeling as a method of changing the behavior of people in other cultures regarding literacy, family planning, curtailing AIDS and the like.

Coming from a small Canadian town with poor educational resources, he made his own way in the world of education, ending up as a professor at Stanford. His own life experiences made him aware of the importance of what he calls personal agency (what I call volition). Rejecting the behavioristic theory of environmental determinism, he distinguished between environments that were: imposed, selected and created—the last

two were a matter of one's own choices. Personal choices can be made and are important, he argues, even in the face of fortuitous events (Bandura, 2005).

What is his theory of theory building? "The goal in theory building is to identify a small number of explanatory principles that can account for a wide range of phenomena" (Bandura, 2005, p. 23). Modeling was one of those core principles. Self-efficacy was another. This discovery of its importance was somewhat fortuitous, but in agreement with Pasteur he says that, "Chance favors the prepared mind" (Bandura 2005, p. 20).

In the course of treating snake phobics using his modeling techniques (guided mastery, etc.), he observed that the debilitated phobics were not only totally cured—in only a few hours-- but that they had acquired fundamentally altered beliefs about their ability to control their own lives. This prompted him to start a program of research on self-efficacy, defined as domain-linked belief in one's capability.

The experiments went in many directions (Bandura, 1997). Some were designed to show, using a variety of methodologies, that self-efficacy has causal efficacy and was not an epiphenomenal by-product of other causes or simply a description of past performance. Other studies were designed to find out the best method of measuring self-efficacy. Still others demonstrated relationships between self-efficacy and many outcomes such as: task performance, skill acquisition, response to feedback, goal choice and goal commitment, and the use of efficacious task strategies.

Self-efficacy was also applied to many different domains: mastery of phobias, work performance, career choice, anxiety and other mental disorders, eating disorders, substance abuse, athletic performance, educational attainment, and recovery from

surgery, to name a few. The efficacy concept was also extended to the group and institutional levels. It was also applied to people of different ages and genders and across cultures. In other studies the determinants of self-efficacy were identified, the three most important being enactive mastery, role modeling, and social persuasion.

There is more to social-cognitive theory that what I have discussed (e.g., it encompasses goals, rewards, triadic causality, moral disengagement, developmental self-efficacy and more) but I have presented the two core concepts, which, as noted, are interconnected.

As to induction vs. deduction Bandura (2005, p. 29) makes this telling observation:

> A prominent group of social scientists was once brought to a mountain retreat to prepare a report on how they went about their theory building. After a couple of days of idealized show and tell they began to confess that they did not construct their theories by deductive formalism. A problem sparked their interest. They had some preliminary hunches that suggested experiments to test them. The findings from verification tests led to refinements of their conception, that, in turn, pointed to further experiments that could provide additional insights into the determinants and mechanisms governing the phenomena of interest. Theory building is for the long haul, not for the short winded. The formal version of the theory, that appears in print, is the distilled product of a lengthy interplay of empirically based inductive activity and conceptually based deductive activity.
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> (I will come back to the issue of deduction below.)

Locke and Latham's Goal Setting Theory. To avoid the accusation of egocentric bias in regard to including our own theory, suffice it to say that goal theory was ranked number 1 in importance out of 73 management theories by organizational behavior professors (Miner, 2003). Like Bandura, I rejected behaviorism from the outset despite its dominance over the field of psychology when I started my graduate work in industrial-

organizational psychology in 1960. As I viewed it, its falsity was self-evident. It could be refuted by 30 seconds of introspection. It was also refuted philosophically by Ayn Rand who demonstrated that consciousness was a philosophical axiom which was self-evident to perception and could not be escaped (Peikoff, 1991; see more on this below). She also showed that volition was a derivative axiom (Binswanger, 1991; Peikoff, 1991). Psychological determinism, as Bandura also notes (2005, pp. 16-17), is self-refuting. A doctrine which claims to be true based, presumably as a result of looking at the relevant evidence, cannot at the same time assert that people are compelled to believe and act as they do by forces outside their control. The doctrine of determinism is incompatible with its own content. (The opposite of determinism is not acausality but self caused action, specifically the choice to think; Binswanger, 1991).

My interest in goal setting was sparked by a study in a textbook written by my two mentors at Cornell, Ryan and Smith (1954). The study had been done in 1935 by Mace, an Englishman, and compared the effects on performance of telling the members of one group of subjects to each do their best on a task while telling another group to each aim for a specific goal. The subjects with goals did better, according to the graph, but in 1935 researchers apparently did not perform statistical tests.

Ryan was writing a book at the time on intentional behavior and his key argument, based on introspection, was that the most immediate determinant of most of what we do is our intention to take a certain action. So, he said, why not start building motivation theory with that as the starting point and then gradually move back in the sequence to the more remote determinants? This is just how Mace had started (though he did only one set of studies).

I thought that made perfect sense, and it agreed with my own introspective knowledge. So I did my dissertation following up on Mace's work. I used the term goal rather than intention, though they have similar meanings, because, as an I/O psychologist, I was interested in work performance, and I knew in that realm people thought in terms of goals for work outcomes. (Fishbein & Ajzen,1975, and others developed theories based on intentions).

In my doctoral dissertation I replicated Mace's results, using statistical significance tests. Goals which were specific—and challenging—led to better performance that "do your best" goals. I also found a relatively linear function between goal difficulty and performance (delimited by subjects' ability and commitment). This contradicted Atkinson's (1958) theory that task difficulty is most motivating when the chances of success are 50/50. However, Atkinson did not measure goals, and we never replicated his curvilinear function unless it was due to lack of knowledge (skill) or lack of commitment. Later with my assistant Bryan and others (see Locke & Latham, 1990, for references), I replicated the dissertation results in the laboratory using other tasks. Years later, we found that performance was undermined if subjects were trying for consciously conflicting goals. We did several studies showing that feedback in the form of knowledge of results, was mediated by the goals that were set in response to the feedback.

Another apparent contradiction was that expectancy theory (Vroom, 1964) predicted a negative relationship between expectancy and performance (other factors being held constant), whereas goal theory found that higher goals with a lower probability of success led to higher performance than easy goals with a higher probability

of success. This was resolved by showing that within any given goal level, the expectancy- performance relation was positive (Locke & Latham, 1990).

Despite the success of the laboratory studies, many commentators were skeptical that they would replicate in the real world of work. Fortunately, Gary Latham, who became my life-long research colleague, had discovered the importance of goal setting independently in a field setting and subsequently did many additional field (as well as laboratory) studies which replicated my laboratory studies and also added new knowledge. Subsequently goal setting studies were conducted by many others.

Some of Latham's work studied the effects participation in goal setting, which he did not find to have very powerful effects unless participation led to setting higher goals than were assigned by others. However, research by Erez and her colleagues found powerful effects for participation. To resolve the apparent contradiction, Erez and Latham designed joint experiments, with me serving as mediator, and resolved the apparent contradiction--which turned out to be caused by different experimental procedures (Latham, Erez & Locke, 1988).

Subsequent studies by Latham, Erez, and many others identified the moderators of goal effects: feedback, to track progress; goal commitment, task complexity and situational constraints. Research also identified goal mediators: effort, direction of attention and action, persistence, and task knowledge (e.g., task strategies).

In examining the relation of goal to affect (satisfaction) another anomaly arose. Goal success was strongly related to satisfaction for any given goal level. However, if you measure anticipated satisfaction across goal levels, goal level was negatively related to affect. The paradox was resolved by recognizing that high goals set the bar for

satisfaction higher. Thus people with easy goals are easier to satisfy; those with hard goals are harder to satisfy. Nevertheless, in real life many people set high goals, because they bring more psychological (pride) and practical (good jobs, career advancement) benefits than low or easy goals.

In the course of our research we discovered social-cognitive theory and incorporated self-efficacy into our own work (just as social-cognitive theory incorporated goals). Self-efficacy, in addition to having main effects on performance, affected—as noted earlier-- the level (difficulty) of self-set goals, goal commitment, response to feedback and the use of effective task strategies. We also discovered that goals, along with self-efficacy, mediated or partly mediated the effects of other motivators and incentives, such as personality, leadership, participation, and money incentives on performance.

It was only after twenty-five years of research and some 400 studies by ourselves and others that Latham and I felt ready to actually develop a theory (Locke & Latham 1990; see Locke & Latham, 2002, for an update and summary). It was done strictly by induction. There was no advance theory. Rather we simply asked our selves a series of questions based on a single core idea: the importance of goals. Our actual thinking process was something like: I wonder what would happen if….? Or, in the case of Latham's field studies, I wonder if goals could be beneficial in dealing with a practical problem? Of course, journal editors compelled us to list formal hypotheses, but in our own minds we were less formal and more just questioning. Building the theory entailed an inductive integration of every goal setting study that had been conducted, including an

attempt to resolve all contradictions and to explain all null studies (Locke & Latham, 1990).

Communalities among the Theories

Despite the fact that the three theories summarized above are from three different fields of psychology and management, there are interesting commonalities among them, over and above their being built by induction. All focused primarily on proximal rather that remote causes of actions. All started—and stayed with-- with a core idea (negative thinking in the subconscious; role modeling which led to self-efficacy; goals). All took it as a given that consciousness has causal efficacy and took pains to establish the causal efficacy of their core variable(s). All had applied applications. All the theories were developed over a period of many years based on the accumulation and non-contradictory integration of a large body of evidence.

Mostly notably, none of these theories advanced by the method of falsification. Both social-cognitive and goal theory rejected behaviorism before the research was begun. Beck did reject psychoanalysis as a result of his research, but this was a by-product of his positive findings regarding the role of negative thinking in depression. I make no claim that all valid psychological theories have to share the above commonalities, but it would be an interesting idea to explore.

Some Suggested Guidelines for the Development of Inductive Theories

The present author is not knowledgeable enough to prevent a full-fledged theory of induction. But I can suggest some guidelines, that, if followed, may help move psychological science forward more rapidly.

Start with valid philosophical axioms as the base. Popper (2003) and many philosophers scorn anything smacking of the "metaphysical," but, in fact, all science—and all knowledge-- is based on certain philosophical premises, i.e., axioms, whether held explicitly or implicitly. Ayn Rand (1990, p. 55) writes:

> An axiomatic concept is the identification of a primary fact of reality, which cannot be analyzed, i.e., reduced to other facts or broken into component parts. It is implicit in all facts and in all knowledge. It is the fundamentally given and directly perceived or experienced, which requires no proof or explanation, but on which all proofs and explanation rest….[*axioms*] *are perceived or experienced directly but grasped conceptually*. They are implicit in every state of awareness, from the first sensation to the first percept to the sum of all concepts

Axioms are self-evident and cannot be contradicted without accepting them in the process (Peikoff, 1991). They are grasped inductively; they are implicit in one's first perceptions of reality. They are both true and non-falsifiable, though Popper considered this impossible. The three primary axioms are: existence (reality), identity (everything has a specific nature, to be is to be something specific), and consciousness (awareness). Without these axioms as the base, all knowledge would be impossible. If there is no reality out there, there is nothing to be conscious of. If things have no identity, what you discover about them today could be dissolved tomorrow; a thing would not be itself Without consciousness, there is no means to discover knowledge. Rand also stresses that existence is primary; the function of consciousness is to perceive reality not to create it. Existence exists and is what it is independent of consciousness. (For a discussion of man-made facts and why these do not contradict the primacy of existence principle, see Ghate & Locke, 2003, and Peikoff, 1991).The law of causality is implicit in the law of identity: if everything has a specific nature, then it must act in specific ways under specific

conditions. Acorns cannot turn into elephants and pencils cannot become hot fudge Sundays-even if one wishes them to.

Observe that these principles entail a total rejection of the philosophical premises Kant (Harriman, 2006; Ghate, 2003) and the skeptics who followed him, including Popper. These principles lay the foundation for objective knowledge. The question of how one gains knowledge belongs to the field of epistemology and includes the need for a valid theory of concepts: what they are and how they are formed (see below).

Develop a substantial body of observations or data. Use, at least in management and psychology, a variety of methods, subjects, tasks, and time spans. Aside from the usefulness of replication, this will help identify possible boundary conditions for the phenomenon (see below) and also possible problems with measurements.

An important source of data in psychology (and parts of management) comes from introspection. As noted, psychologists have considered this method to be illegitimate, because a given individual's consciousness can only be observed by that person. This precludes its being "inter-subjectively verifiable," but I believe this is a wrong standard. First, a given person can learn a great deal working alone. One person can be wrong but so can 100 or 1000. Further, we can infer another person's internal states from various types of evidence, including certain of their actions in certain contexts and the person's introspective reports. All sciences use the method of inference. And what we discover in our own minds can be replicated by others' introspection and vice versa. In addition, we can determine to what extent introspective reports are lawfully related to subsequent beliefs, emotions and actions. Finally, it should be noted that introspection is the means—the only means—by which we are able to grasp

psychological concepts. In the absence of introspection we could neither form nor grasp concepts like thought, emotion, perception, belief, goal, etc. It is time that introspection came out of the closet. Psychologists should study the conditions under which it is most valid (e.g., see Ericcson & Simon, 1980).

Formulate valid concepts. It is regards as a virtual axiom today that concepts, and therefore definitions, are subjective or arbitrary (cf. Popper)--that there are no objective principles for forming them. Ayn Rand (1990) disagrees. Very briefly, her theory is that all valid concepts begin at and are ultimately traceable to the perceptual level. Entities are grouped together based on their similarity in contrast to differences on some attribute (e.g., chairs are grouped together and differentiated from tables based on their shape.) But how do you form the concept chair when in reality every chair is in some way different from every other (e.g., even "identical" chairs will differ somewhere by a millimeter). Her original insight was that, although the shape and dimensions of chairs entail a range of measurements, in forming the concept, the measurements are omitted. They are assumed to exist but are not stated. The process of abstraction entails the integration of the similar entities, with measurements omitted, into a single mental unit. Higher level or more abstract concept are formed by integrating lower level ones. The concept of chair or man stands for all chairs or men that exist, have ever existed and will exist in the future, regardless of their size, gender, color, age, etc.

The concept-formation process is completed by the choice of a word (a single mental unit) to stand for the concept plus a definition. Objective definitions entail (from Aristotle) a genus: integrating the concept into a wider category, and a differentia: differentiating the concept from other existents in that genus, viz. man is the rational

animal--meaning he is the animal who has the capacity to reason. (For the treatment of borderline cases, see Rand 1990). The purpose of a definition is to tie the concept to reality and to differentiate it from other concepts.

The definition is not the same as the concept. The concept of man includes everything known about man and everything that will be discovered in the future. In this respect, concepts are open-ended. Rand rejects Aristotle's idea that things have metaphysical essences discovered intuitively (e.g., an ineffable man-ness inside every man)--as well as the currently popular premise that concepts are subjective. To her essences are epistemological. An essentialized definition states the most fundamental attribute (the attribute on which the most other attributes depend based on one's present context of knowledge) of the entity. Objective definitions, therefore, are based on reality—not subjective feelings. I don't think I am being controversial to state that much confusion in psychology and management has been caused by careless definitions and careless concept formation (Locke, 2003). It should be stressed that valid measurement presupposes valid concepts and definitions.

Look for evidence of causality and identify causal mechanisms. Concepts are formed inductively, from observing reality. The facts discovered about members of the class are generalized to all members, including those not yet seen. How then do we discover causal generalizations?

A provocative new approach to induction (Peikoff, 2007) argues that contrary to Hume, our first-discovered causal relationships are perceived directly, as when a child pushes a ball away from him. Even the phenomenon of gravity has a perceptual base, as when a child drops an object to the floor. More advanced causal generalizations require

valid concepts. For example, Galileo, despite his great achievements, never grasped the concept of gravity, and this led him into errors (Harriman, 2007). Early psychologists never grasped what consciousness was, and this prevented them from being able to form and use valid psychological concepts and thus really understand human action, e.g., that goals affect how people act.

If this new approach is valid, it means that, like concepts, causal generalizations are based on inductions starting at the perceptual level. I noted earlier, however, that Bacon had said that induction is not a total enumeration of instances. We all know that demonstrating that x is often found with y is not evidence for causality. To show that x causes, or is a causal factor in y, based on the nature of x and y, is a critical step in making valid inductive generalizations.

Experimentation is one key method, based on actively controlling certain factors while actively manipulating others (a method unknown to the ancient Greeks). Experimentation is not always possible in management, but, even if it were, I would argue strongly that management (including leadership) is as much of an art as a science, so it could never be reduced just to scientific formulas.

Finding lawful mathematical relationships between x and y is another method. For example, Newton's laws of motion represented a stunning breakthrough in physics, because they involved mathematical formulas which described the actions of an enormous range of phenomena based on gravitational attraction. It must be noted that statistical methods, used heavily in the social sciences, even when other factors are controlled, reveal only probabilistic relationships. There are three main reasons for this: (1) the human mind is extraordinarily complex; (2) there are usually multiple causes of

human action; and (3) volition: people make choices that other people and circumstances do not pre-determine (Bandura, 1986; Binswanger, 1991; Peikoff, 1991). This does not stop the social science from progressing, however. Predictions can still be made contingent upon an individual's premises and choices and within a range of error.

Discovering how x causes y (mediators) is yet another step. For example, we know the psychological mechanisms by which goals affect action. It should be stressed that causal explanations exist in layers. Each new layer of causal explanation increases our causal understanding and our ability to generalize. Not all layers are discovered at once. Newton discovered the laws of gravity but was not able to explain the actual nature of gravity (what it actually is, other than a force), and this is still not known today.

Tie in valid concepts from other sources and theories where applicable. Goal theory incorporated self-efficacy from social-cognitive theory. Social cognitive theory incorporated goals and elements of network theory. Beck, though not a behaviorist, incorporated ideas from (so-called) "behavior" therapy in his treatment program. I must stress that tying concepts together does not mean putting boxes around words and connecting them with arrows. The words must stand for concepts based on reality, and the relationships between the concepts must be demonstrable.

Integrate the totality of findings and concepts into a non-contradictory whole. This is the beginning of theory building[2]. The law of contradiction is critical. If a finding does not come out as expected, either the theory is wrong and needs modification or replacement, or the theory was tested incorrectly. If two findings or theories about a given phenomenon are contradictory, at least one of them must be wrong. Negative findings can be useful-- if they lead ultimately to new, positive insights.

It should be emphasized that integration does not just include knowledge gathered in one's own research or even that of other theorists in one's field. One's conclusions must be consistent with all of one's knowledge. The failure to make such integrations seems especially common today among researchers who study so-called language acquisition in the lower animals (chimps, parrots). They do everything in their power to show that chimps, for example, are like humans, e.g., by teaching them things they could never learn on their own, and ignoring differences. But, they never acknowledge that in the six million years since they split off genetically from man, chimps never developed even the rudiments of a primitive culture. (I do not consider adult chimps learning to use twigs to stick in ant or termite holes or to wash food to constitute a culture). This failure is fully consistent with chimps' genetic make-up which is very similar to man's but has critically important differences, including a 67% smaller brain. The lack of cultural achievements is also consistent with chimps' cognitive limitations (e.g., they don't write books or build universities.) This quantitative difference between the two species is so enormous that it overwhelms any similarities, whatever those might be, and constitutes, in reality, a qualitative difference

Goal theory was not only based on experimental findings, but also on introspection: we can observe directly that our daily actions, unless we are totally passive, have some purpose. At a deeper level it was based on the nature of life itself. "Life is a process of self-sustaining and self-generated action" (Rand, 1964, p. 15). Such action must be goal-directed. That is, the action must be aimed, directly or indirectly, toward the ultimate goal of promoting or sustaining life or the organism sickens and/or dies. Goal-directed action occurs automatically at the vegetative level (the beating of the

heart; Binswanger, 1990) and can be regulated volitionally at the human (molar) level (Binswanger, 1991).

Integration does not mean that particle physicists should have rejected their results, because the phenomena they observed were inconsistent with Newton. Newton was concerned with "large," perceivable objects and his laws were not based on studies at the atomic level.

Identify the domain and boundary conditions for the theory. There is no theory of everything, so every valid theory always pertains to a certain subject matter domain (e.g., evolution, gravity, depression). This domain needs to be clearly articulated. Furthermore, even within the relevant domain, boundary conditions need to be discovered. This cannot always be done at the outset, but it needs to be done eventually. Boundary conditions are the same thing as interactions or moderator variables. Goal effects depend on feedback, commitment and knowledge. Self-efficacy effects also depend on feedback. The success of cognitive therapy, Beck (1993) observed, depends on having a good relationship with the patient.

Make theory-building a careful, painstaking and gradual process. Anyone can invent a theory off the cuff. But for it to be tied firmly to reality and therefore have lasting value, a valid theory needs to be built gradually from an accumulating body of evidence. (There may be exceptions if a relevant and substantial body of knowledge already exists and a genius like Einstein is able to integrate it.)

Although I did not make it part of the above list, it may be helpful in management and psychology, as a relatively young science, to start theory building with a simple, core idea. The core idea is not a theory but the potential foundation of one. The core idea may

develop into a theory over time. At the outset, the core idea may simply be the premise: I think this concept is important—let's see where it leads. Or, it can be a question: I wonder what would happen if…?

Where do you get core ideas? Not, I think, from some irrational or arbitrary intuition. Core ideas can come from many sources: an interesting finding in the literature, an unexpected finding in one's own work research, an integration made from existing data, a finding in another field, etc. There are no fixed rules here so long as one is tied to reality. Having an active, curious mind and being a good observer may be the keys. Furthermore, core ideas in psychology and parts of management can come from or entail introspection.

Once inductive theory building has occurred, of course, one is in a position to made deductions from the theory and apply them to new situations. Such deductions do not come out of the blue but from a solid body of evidence that is tied to reality. Since theories are properly open-ended, one can go back and forth between induction and deduction, expanding the theory when deductions prove correct and revising it when deductions prove incorrect.

Can One Attain Certainty?

Scientists (like everyone else) are not and can never be omniscient, but they do discover things, and these discoveries are of demonstrable benefit to human life—a goal very much in consonance with Bacon's view of the proper role of science. Knowledge must be discovered gradually and by a certain process (Rand, 1990). Knowledge begins with the evidence of the senses (to deny the validity of the senses is a self-contradiction)

which, in the sciences, includes evidence gathered by instruments which can be read by the senses. Perceptual knowledge is integrated into concepts (Rand, 1990). Conceptual knowledge is integrated, obeying the law of contradiction, into principles, formulas, and theories. This is a gradual process and there is never a point at which a human mind can claim "I know everything."

The proper epistemological standard to use in judging scientific discoveries is not omniscient certainty but contextual certainty. One attains contextual certainty when there is an accumulation of great deal of positive evidence supporting a conclusion and no contradictory evidence (Peikoff, 1991, see ch. 5). It is precisely because people are not omniscient they must identify the context in which they make claims of knowledge. One can properly say, "On the basis of the available evidence, i.e., within the context of the factors so far discovered, the following is the proper conclusion to draw" (Peikoff, 1991, p. 172). Peikoff gives the example of the discovery of blood types, A, B, AB and O. A type bloods were at first considered to be compatible until the RH factor was discovered. The first proper conclusion was that "type A bloods are compatible based on what we know now." Later this had to be qualified with, "providing their RH factors are matched." The first conclusion was contextually certain and so was the qualification.

To make this principle concrete let me summarize the essence of goal setting theory based on the full context of knowledge we have to date: Unless there are conflicting goals, for individuals: specific, high (or hard) goals will lead to higher performance that do best, moderate or easy goals by affecting direction of attention and action, effort and persistence, providing there is commitment to the goal, feedback

showing progress in relation to the goal, relevant task knowledge (ability, skill) and there are no environmental factors which block goal-directed action..

This claim is contextually certain based on what we know now. Will future results require additional qualifications? Undoubtedly. Goal setting works with groups but groups bring in a new factor: social influence and knowledge sharing (Quigley, Tesluk, Locke & Bartol, 2007). Further, subconscious factors may play a role in goal setting (e.g., see Stajkovic & Locke, 2006) as might mental health. But the point is that we do know something.

The principle of contextual certainty does not overthrow the possibility of human knowledge, but rather it is what makes claims of knowledge possible. It establishes that all knowledge depends on a cognitive context and demands that the context be identified. New discoveries enlarge or modify the context and allow science to progress. If science demanded omniscience at the outset and then people saw that arbitrary theory after arbitrary theory kept being contradicted or overthrown, it would lead to total skepticism. Observe also the similarity of theory building to concept formation: both are open-ended, thus allowing for the possibility of new discoveries.

If we look at reality, specifically the history of science, it is obvious that scientific progress has been made and that more discoveries are being made everyday. Progress is made (despite mistakes) in increments, some small, some large. The proper goal is not to pursue the hopeless grail of omniscience, but to constantly discover more. And the key process is induction. As Bandura implies, deduction is always necessary in science and there is an interplay between induction and deduction (e.g., x implies y), but my point in this essay is to show that induction is primary. Consider the famous syllogism about

Socrates being mortal: observe that the premise that "all men are mortal" is a generalization that is made through induction. Without it, there could be no valid syllogistic deduction (that is, one that applies to reality). Making deductions from arbitrary premises leads nowhere.

I am compelled to address one final issue in this section—an example actually—because it is used repeatedly to show that induction is invalid. It is the black swan example. Professor X looks everywhere for new bird species and discovers white swans. He forms the concept swan and describes them as birds having webbed feet, a long slender neck and white plumage. He generalizes that swans are white not only the basis of his observations but on the basis that bird species are widely classified on the basis of color (e.g., crows are black, cardinals are red—though male and female members of the same species may differ). Then someone discovers black swans in another country. This, it is typically claimed, shows the futility of induction, viz. "You can't really claim swans are white unless you have seen every swan in existence, which means you can't generalize at all."

What's wrong with this conclusion? It ignores the fact that concepts are opening ended. The original definition was contextually valid (certain) and the concept of swan includes all yet to be discovered knowledge. Discovering black swans does not invalidate the concept of swan but simply adds to what we know. The definition could be changed to "usually white" or "white or black." The new definition is again contextually valid. If green swans were then discovered, we would have learned more and the definition would again be modified. But the concept of swan would not be invalidated. We have not

progressed by falsification as such, but by learning more. This is the model for the whole history of science.

Some Suggestion for Changes in Editorial Policies that Could Promote Productive Theory Building

If theory building is properly inductive, journal editors could make useful changes in their editorial policies. Instead of demanding a theory to start with, the Introduction to a research paper could summarize what is known about the phenomenon in question and state the purpose of the proposed study: how it will go beyond what is known. Hypotheses would not be necessary; the author could simply pose questions. Even if the author were working from an established theory but taking the research in a new direction, no hypotheses would have to be made. Deductions, of course, would be allowed if a hypothesis followed clearly from an extant theory. Introductions would be much shorter than they are now, because the author would not need to write pages and pages of justification for hypotheses, so long as it was made clear that something new is being done

Then in the Discussion section, the author would do the work of inductive integration—tying together the new findings with what was known previously. This means that much of the material formerly in the Introduction, if not discarded, would be moved here. The author would show how the study moved the field forward. The author could also identify how far along the field is in developing a theory and what more needs to be done (e.g., identify causal mechanisms, identify moderators). Some of this is done now, but I would suggest it be more explicit. Stating implications of the study would be optional; sometimes there is simply not enough knowledge to make clear deductions.

Requiring researchers to make them encourages unwarranted speculation. A discussion of the limitations of the study would remain as it is in the typical discussion section, but I would suggest more emphasis be placed on the issue of causality (because its key role in induction), viz. how good is the evidence for causality and what is known of the mechanisms? If anything, journal editors should actively discourage premature theorizing.

I believe if this procedure were used in the journals, it would increase the chances that more good inductive theories will be developed and less time will be wasted deducing hypotheses (before or after the fact) from theories that are not really valid theories. It is my observation that the so-called theories that researchers are forced to make deductions from for their articles virtually never grow into full-fledged theories. I believe this is because too many alleged theory builders are using deduction instead of induction.

A Note about Theory Building and the "Ego"

Before closing, I would like to make an important psychological point about theory building: Never tie your self-esteem (ego) to the correctness of your theory. Why? Because self-esteem will the take precedence in your mind over facts and more time will be spent defending the theory against even legitimate criticisms than improving or modifying the theory. When this happens, the theory fails to grow. I have seen this happen to many people over the course of my career. When one's theory is criticized, the first question to ask is: Is the criticism valid? If it is, the theory needs revision, expansion or qualification. If not, then refuting the critics is proper. Of the three psychological theories I discussed, social-cognitive theory, in my opinion, has been unjustly criticized

the most often—usually by people who denied the causal efficacy of self-efficacy despite overwhelming evidence for its causal effects (Bandura, 1997).

On an autobiographical note, I held as a conscious axiom throughout my career: reality first. This is what Rand calls "the primacy of existence" (Peikoff, 1991, p.18). Its opposite is the primacy of consciousness (e.g., feelings, wishes first). Basing self-esteem on adherence to reality is the best route to scientific progress—and to real self-esteem. This should not be taken to imply that the search for truth should be passionless. Without passion we are zombies. It all depends on: passion for what? The ideal is a passionate curiosity and love of our work combined with an insatiable desire to know the truth.

Ego-defensiveness, I believe, would be far less common if people took more time to build theories using induction. They would not get "attached" to theories prematurely and thus would not waste their time defending themselves against an inevitable rash of non-supportive findings. Rather they would be the ones to look at the totality of results before making any theoretical claims.

Conclusion

In philosophy, Plato, Descartes and Kant (and his followers) were all advocates of the primacy of consciousness. To them the senses were not valid and "truth" was discovered by deduction from ideas implanted in the mind independently of experience. Aristotle, Bacon and Rand were advocates of the primacy of existence and believed that knowledge was discovered starting with observation by the senses followed by the inductive integration of sensory material by reason.

So one could say that the battle between deduction and induction as the starting point of science represents, ultimately, a duel between Plato and Aristotle, the two most

eminent, Greek philosophers (see Peikoff, 1991, pp.451-460). The theme of this paper is that Aristotle's approach was right, and Plato's was wrong. I argue that it has always has been and still is Aristotle's inductive approach (despite his errors), aided by Bacon, that has moved science—and the world—forward.

It is appropriate to end with a quote from Galileo (Galilei, 2001, p. 63), who, although aware of the falsity of Aristotle's cosmology (which undoubtedly would have been quite different had he access to a telescope), was in full agreement with his inductive method: "Does he [Aristotle] not…declare that what sensible experience shows ought to be preferred over any [deductive] argument, even one that seems extremely well founded?"

Footnotes

1. Psychoanalysis is often accused of being a poor theory, because it is not falsifiable. For example, if one accepts the existence of the Oedipus complex, this is acceptable to psychoanalysts but, if one rejects it, one is accused of being defensive or repressed. So it's a no win situation for the critic. All this is true. However, I believe the real error lies much deeper. The fundamental problem is that psychoanalysis does not have an objective foundation in reality; it is based heavily on arbitrary inferences on the part of Freud and his followers. To quote Peikoff (1982, p.p. 213-214):

> Freud offers the world…man the defeated plaything of the gutter; man the smutty pawn shaped by sexual aberrations and toilet training, itching to rape his mother and castrate his father, hoard his excrement; man the sordid cheat who pursues science because he is a frustrated voyeur, practices surgery because he is a sublimating sadist, and creates the [statue of] David because he craves, secretly, to mold his own feces.

How does Freud prove this? He doesn't. Because Freud denigrates the power of reason (the ego), he is free to assert anything, even in building his own theory. The irony is that, given its own premises, Freud's entire theory could be viewed as his own rationalizations of unconscious forces which he can neither identify nor control. This wipes out the entire concept of objectivity and detaches psychoanalysis from reality. (This is not to say there is nothing in it of value, e.g., I believe the there is evidence for the phenomenon of repression, but this has to detached from the rest of the theory). Observe the startling contrast between Freud and Beck in terms of adherence to a genuinely scientific approach.

2. The term theory today is used very loosely; some equate it with any arbitrary speculation one thinks up. I use the term is the dictionary sense:

Systematically organized knowledge applicable in a relatively wide variety of circumstances, especially a system of assumptions [based on facts?], accepted principles, and rules of procedure devised to analyze, predict, or otherwise explain the nature of behavior of a specified set of phenomena (*American Heritage Dictionary,* 1992 edition).

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