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The Depth of History and Explanation as Benefit and Bane for Psychological Control Theories

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A longstanding debate has recently re-erupted in the self-regulation literature around the concept of self-efficacy. This article presents an argument that the debate emerges from a lack of understanding the long and varied history of control theories within both the social and physical sciences, and the various levels of explanation to which phenomena can be subjected. This history, coupled with the issues of determinism, materialism, and empiricism evoked by the deeper level of explanation some versions of control theory provide, have led some critics to misapply non-psychological properties to control theories and obscure their usefulness. Here the usefulness of a deeper control theory level of explanation is illustrated via comparisons with explanations found in goal-setting theory and the use of self-efficacy in social cognitive theory.

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Does one's belief in their capacity to perform positively or negatively affect performance? It seems the answer to this question is more complex than one might think. Whereas recent meta-analyses (e.g., Stajkovic and Luthans, 1998) find the relationship to be positive under various conditions (e.g., simple or complex tasks), a series of four studies described in two subsequent papers found evidence in support of both a negative and positive causal influence of self-efficacy on performance (Vancouver, Thompson, & Williams, 2001; Vancouver, Thompson, Tischner, & Putka, 2002). In response, Bandura and Locke (2003) took serious issue with the Vancouver et al. papers, and in particular, with the control theory perspective used to develop the hypotheses. This exchange brings to the surface a longstanding debate among self-regulation theorists regarding the use of control theories, as opposed to symbolic processing theories, in the explanations of these processes. The purpose of this paper is to lay bare the underlying sources of the conflict fueling the debate in the hopes of moving the field beyond it.

Specifically, I address the question posed at the close of Bandura and Locke's (2003) paper concerning the core and unique contribution of control theory to understanding human behavior. First, I note that control theories are actually a class of theories and briefly trace their long history. Next, I more precisely the particular kind of theory Powers (1973) wanted his control theory to be (i.e., its level of explanation). Powers' version is the one that is embedded in many modern versions of control theory. I claim that both its history and its level of explanation have invited, with and without merit, some of the criticisms it has received. In particular, I address the misperception that control theories are inconsistent with concepts like volition. Finally, I describe some of the fundamental contributions I believe control theories can make to understanding human behavior, and the more specific contribution our set of studies attempted to make. This latter discussion involves addressing some specific criticisms raised by Bandura and Locke (2003) that were not discussed in the original papers.

What Is Control Theory?

The Ontology of Control Theory

At the end of Bandura and Locke's (2003) critique, they asked about the ontology (i.e., development) of control theory. This is a fair question and useful for understanding the reactions it has received in the literature. To begin, it is helpful to understand that conventions for naming theories in psychology are not well specified. Bandura's (1986) social cognitive theory provides an example of the primary issue, specifically that the name of a theory often reflects the currently popular name for a *class* of theories to which it belongs. Thus, for example, Bandura's social cognitive theory was at one time just *a* social cognitive theory belonging to a class of theories including Mischel's (1973), Kanfer's (1977), and others (see Beck, 2000). These theories had a similar perspective that dealt with a serious flaw associated with the social learning theories popular at the time (e.g., Bandura, 1973; Rotter, 1954). Bandura's social cognitive theory could also have been named expectancy-value theory or self-regulation theory after the larger classes of theories to which it belongs (Pittman, 1998; Vancouver 2000). Or it could be named self-efficacy theory owing to the central role of that construct within Bandura's social cognitive theory. Indeed, Bandura often uses the label self-efficacy theory, but it is not always clear whether he is referring to his entire social cognitive theory or merely some specific aspect of it. Moreover, theories of specific problems or within specific domains have arisen that use Bandura' social cognitive theory as a basis and borrow the name in the process (e.g., Howe, 1994).

This proliferation of social cognitive theories compliments the parent concept, but potentially confuses the "core" of the concept when the branching children theories include misapplications of the parent concept or include concepts from other theories. There are two lessons that can be drawn from this example: 1) theories fall within a hierarchy of classifications that can be useful for understanding the inheritance or grouping of concepts and principles, but that 2) the organization of this classification hierarchy is not pre-ordained or well structured, which can lead to ambiguities regarding the concepts and principles that comprise the members and classes within the loosely organized hierarchy.

With that preamble, let me describe my understanding of the hierarchy in which the control theory *class* of theories falls and the principles and concepts associated with the levels in the hierarchy. I classify control theories as belonging to the cybernetic-systems paradigm (Vancouver, 2000a). Cybernetic-systems theories share, at their origin, the desire to understand the observation of stability in open systems when their environments would suggest instability (Richardson, 1991). In addition, these systems are seen as being composed of subsystems that interact with each other and the environment over time. Hence, systems theories are interactional and dynamic in that they describe how properties of the system and properties of the environment *feed back* and change each other over time. Some of the system properties could be associated with subsystems, but others only *emerge* via the interaction of subsystems or the system with its environment (e.g., harmony can only emerge from the combination of sounds). A key promise of systems theories is the notion that understanding across a wide range of phenomena can occur from the application of only a few simple principles used to describe basic subsystems. This perspective was epitomized perhaps most dramatically in Miller's (1978) living systems theory and, for applied psychologists, in Katz and Kahn's (1978) social psychology of the organization, but it can also be found in more recent theorizing (e.g., Wolfram's, 2002, theory of cellular automata).

Initially independent of the systems perspective, Rosenblueth, Wiener, and Bigelow (1943) developed a set of principles to describe the operation of a particular information processing structure, eventually called the control system (see Figure 1 for one rendition of this structure). The control system provides an elegantly simple explanation for the primary observation of stability that intrigued the systems theorists. Specifically, it elaborated upon the concept that some variable in this simple system's environment can be maintained at or moved to a desired level, called the goal level in Figure 1, by acting on the variable when the perception of the variable deviates from the goal. Within this conceptualization the input function creates the perception, the comparator function creates the deviation or "error" when perception and goal differ, and the output function creates the actions by the system on the variable when it receives the error signal. Meanwhile, disturbances from the environment can also be acting on the variable. The system need not know the nature of the disturbances, only the current state of the variable. It also must have enough wherewithal (i.e., power) to counter disturbances or to close the discrepancies between the current state of the variable and a new desired state (i.e., goal level) when the desire state is changed. A natural example of this is seen in how humans and other warm-blooded organisms maintain their body temperature by continually monitoring it and activating mechanisms to maintain or dissipate heat as discrepancies between desired and perceived body temperature are detected. These discrepancies are reduced without concern for the source of the changes, provided that the changes are not so great as to not overwhelm the mechanisms at their disposal (a point I will return to later).

The control system structure has in it a number of features that have interested the systems theorists. First, it was general in that the control system structure described the flow of information (i.e., communication) between organisms (or machines) and their environment for the purpose of maintaining stability of some variable in the environment (or the organism's internal environment). When Wiener (1948) subsequently launched a field of study focused on this communication process, he called it "cybernetics" and that name for the control system, and theories using it, is also popular. Also relevant to systems theorists was the ease with which control systems could be included as parts of larger systems (Boulding, 1956). Because of these two features (i.e., simple accounting for stability and the ability to be incorporated as subsystems), cybernetic and systems perspectives quickly merged into an imperfectly integrated system of scientific thinking around the middle of the 20th century (Richardson, 1991). At this point the control system was recognized within older biological (e.g., to explain homeostasis for some, but not all, variables in which it is observed, Cannon, 1939) and psychological thinking (e.g., Dewey's, 1896, attempt to add a functional perspective using the structural reflex arc). It also motivated substantial new theorizing like Miller, Galanter and Pribram's (1960) test-operate-test-exit (TOTE) model that so influenced the cognitive revolution in psychology.

Yet, it was in the engineering field where the more formal theorizing occurred (e.g., Wiener, 1948). Specifically, engineers sought ways to set and then keep regular (i.e., control) some variable (e.g., engine torque, car speed). If a control system is constructed with the goal level determined by the user, the constant monitoring of and adjusting to the variable done by human operators could be eliminated (e.g., as in an automobile's cruise control). This freed up human operators to do other things (e.g., watch the road), or eliminated them altogether. It was this field that popularized the name "control theory" to describe the function and principles associated with it. Now theories that use the structure as a core explanatory subsystem tend to call themselves, or to be called, control theories. The proliferation of control theories across and within a wide variety of fields validated the promise of systems thinking, but revealed some problems as well.

Problems of Proliferation

The ubiquitous nature of control theories in the natural and social sciences is telling in regards to the cybernetic structure's explanatory power (Katzell, 1994), but it also creates problems. For example, its popularity in engineering created two unfortunate outcomes for psychology. One evident in Bandura and Locke's (2003) critique, as well as other critiques of psychological control theories (e.g., Sandeland, Glynn, & Larson, 1991), is the association of the theories with machines. This association creates what I call the machine-analogy problem (Vancouver, 2000b). One aspect of the problem is that the machines, which are much more limited than humans, when used as analogies for understanding humans evoke errors of omission (e.g., no imagination, consciousness, or volition). In some cases (i.e., for some control theories), the limits are not unreasonable because of the narrowness of the phenomena the theory is designed to explain. Indeed, the field of ergonomics is replete with these types of theories (Jagacinski & Flach, 2003). However, the concern here is related to a very comprehensive version of control theory and later I describe why the error of omission only *seems* to occur.

Another aspect of the machine-analogy problem pertains to errors of commission. Too often psychological control theorists borrowed too much from engineering versions of the theory (cf. Powers, 1978). These errors of commission give some credence to the criticism that some control theories included elements that, although reasonably applied to machines, were unreasonably applied to humans. For example, Wiener's cybernetic structure had to be rearranged in some fundamental ways before it was appropriately applied to human behavior (Powers, 1978). On the other hand, the criticisms were often misapplied when engineering versions of the theory were confused with psychological ones by the critics (e.g., Locke, 1991).

Beyond engineering, control theories in psychology and management have focused on a plethora of phenomena including motor control (e.g., Pressing, 1999), physiological aspects of motivation (e.g., Mook, 1996), cognitive aspects of motivation (e.g., Frese & Zapf, 1994; Klein, 1989; Lord & Levy, 1994), man-machine interfaces (e.g., Howell, 1993), organizational stress (e.g., Edwards, 1992), and management processes (e.g., Bozeman & Kacmar, 1997; Kluger & DeNisi, 1996; Tsui & Ashford, 1994; Wright & Snell, 1991). Each is unique, owing to the specificity of the phenomena on which it is focused and on the differences in linage from which it is derived (i.e., one's control theory parent). Moreover, several present-day theories include control theory principles as either a major (e.g., Beach's, 1990, image theory) or a minor player (Katzell, 1994). I put social cognitive theory in that later category given Bandura's (1986) acknowledgement of what he calls the discrepancy-reducing element of self-regulation.

This differing linage, degree of incorporation, or even style of psychological control theories creates the moving and ambiguous entity to which Bandura and Locke (2003) refer. For instance, many control theories do not conceive of the control system as a subsystem within a larger system but as a heuristic with which to organize findings and theories (e.g., Edwards, 1992; Hyland, 1988; Klein, 1989). Others attribute different properties to control systems or different interactions with respect to those properties (e.g., see Vancouver, 2000a, for a comparison of Powers', 1973, version with Miller, Galanter, and Pribram's, 1960, version, and with action theory as described by Frese & Zapf, 1994). Still others add small modifications or elaborations onto existing versions. Thus, it is not surprising that Bandura and Locke (2003) question the meaning of control theory in psychology.

Yet, does this state of affairs indicate control theories are a poor class of theories for human behavior? Clearly the existence of a plethora of theories within a class is not a *prima facie* reason to reject any particular theory or the class as a whole. In this case, given the breadth of phenomena control theories have been developed to explain, it seems reasonable that a class with multiple members will always exist. Indeed, if the multitude of theories indicates anything about the quality of the core concept it is that it seems useful (Katzell, 1994). On the other hand, the large number of theories within the class does not indicate that any one of the theories is valid. In particular, when members of the class address a similar set of phenomena, some culling may be appropriate. Indeed, working through the details regarding which theory or theory elements are best is what we do as scientists.

Toward that end, and specifically for the purpose of a more perfect integration within the cybernetic-systems paradigm, Richardson (1991) reviewed the feedback concept in theories within the social sciences. He found many variations in how feedback was approached and that these approaches ranged in quality. For instance, Richardson was very impressed with the way Powers (1973; 1978) handled the feedback concept within his version of control theory, stating that "he has by-passed the issues associated with meta-languages that so influenced Beer, Miller, Galanter, and Pribram, and others" (Richardson, 1991, p. 263). Richardson further noted that, "Powers's interpretations and uses of the feedback concept suggest the need for fundamental changes in the ways social scientists do business. In particular, Powers's work correctly identifies significant flaws in the way some have interpreted and made use of the feedback concept" (p. 263). I agree with this assessment and hence focus most of this discussion on

Powers' version of control theory, which he now calls perceptual control theory (PCT). One of the most fundamental differences between PCT and many other control theories of psychological phenomena is the kind of theory it is. Powers' is a subsystem-level theory. In the next section, I describe the difference between system and subsystem-level theories, and why this difference has contributed to misunderstandings regarding PCT.

System and Subsystem-Level Theories

Theory, as it turns out, is a fairly ambiguous concept in psychology and management (Sutton & Staw, 1995). An informal review of research methods textbooks in psychology will quickly show that the modal definition of theory reads something like this: "theory is a set of statements about relationships between variables" (Whitley, 2002, p. 8). Fairly uncontroversial is the belief that better theory describes causal relationships, abstract variables (i.e., constructs), and boundary conditions (e.g., Bacharach, 1989). More controversial is the nature of the justification required to make the statements (DiMaggio, 1995). Some argue that the statements may be or *should be* extrapolations of past empirical findings (e.g., Locke, 1991). Within this understanding, quality is assessed in terms of the number of dependent variables covered by the abstractions (i.e., comprehensiveness, where the more the better), the percent of variance explained in these variables (i.e., predictive ability, where the more the better), and the number of independent variables used to do the explaining (i.e., parsimony, where the fewer the better).

Others argue that theories are to explain why a set of relationships exist, not the amount of variance in the set of variables at one end of the relationships (Bacharach, 1989; Sutton & Staw, 1995). Within this understanding, quality is often assessed using the criteria described above, but explanations could be "plausible accounts of how the actions of real humans could produce the associations predicted and observed . . .[or] . . . formal models of the human behavior that specify principles of individual or group action that through computer simulation generate observed distributions of outcomes" (DiMaggio, 1995. pp. 391-392). The first level of explanation focuses on the properties of the system of interest (e.g., organizational performance is a function of the selection system, which influence the composition of the organization's human resources); the second focuses on showing how the properties and interactions of the subsystems affect the properties of the system of interest (e.g., when powerful organizational members have knowledge of how to validate selection systems, the organization's selection system is of high quality and improves).

What constitutes a system and a subsystem is relative. DiMaggio's (1995) was referring to sociological theories, in which organizations or societies are the systems of interest and humans are the subsystems nested within them. Katz and Kahn's (1978) theorizing took a similar perspective (i.e., understanding the nature of organizational phenomena via the understanding of interacting humans). At a very different level, Powers (1973) uses an example from physics to make the same point. He noted that the kinetic theory of heat describes how heat capacity, a property of *substances*, arises from interacting properties of *molecules* (a subsystem within substances), and explained the relationship between other substance-level variables (e.g., mass) and heat capacity. Powers noted that generalizations regarding substance-level properties were and are still useful, but that physical theories did not develop their power of explanation until the subsystem-level theorizing was accepted.

For applied and social psychologists the idea that humans can be the focus of understanding organizational phenomena is intuitively appealing. In high school we learned that the behavior and properties of substances is a function of the molecules nested within the substances, and that the properties of molecules are a function of the atoms nested within them. However, pertaining to human behavior, determining what subsystem conceptualizations are useful for increasing our understanding remains controversial. For one thing, what are these subsystems? In Powers' theory he describes the properties of control systems that enable human systems to maintain the dynamic homeostasis necessary to sustain life as well as how the control systems' interactions may explain much of human behavior. Thus, the core schematic (Figure 1) of this theory does not represent how properties (i.e., constructs) of individuals relate to each other, but instead information flow through a control system allows the system obtain control. In contrast, social cognitive and goal-setting theories explain concepts at the system level. That is, in these theories the properties of interest are properties of the human (e.g., self-efficacy, performance levels), not the subsystems within the human. Below I argue that this distinction is important in terms of explaining 1) human behavior and 2) the negative reaction control theory often garners in the literature.

One might question my categorization of social cognitive and goal-setting theories as system-level on the grounds that social cognitive theory includes human subfunctions (Bandura, 1986) and goal-setting theory describes mediating mechanisms (Locke & Latham, 1990). Indeed, both theories seem to be on the *verge* of incorporating subsystems, particularly control subsystems, to explain the relationships they describe. For example, Bandura often acknowledges the discrepancy reduction element of self-regulation and Locke often speaks to the comparison between goals and feedback. Yet, they frequently explicitly reject control system explanation (Bandura, 1989; 1991; Bandura & Locke, 2003; Locke, 1991; 1994; 1996; Locke & Latham, 1990; 2002), which I partially attribute to their rejection of subsystem theorizing in general. Moreover, their rejection of subsystem theorizing has implications for the nature of the explanations found in these theories. A particular example of this is seen in the differing descriptions of what constitutes mediation. In system-level theorizing mediators are defined as construct, while in subsystem-level theorizing mediators are seen as mechanisms. For example, many cognitive theories describe the mediating role of perceptions on action; however, control theory describes how perceptions *are used* to affect action.

In practice this distinction may often be unimportant. Both types of theories attempt to describe what the mediator is, but only subsystem theories attempt to describe how the mediator works. If we seek understanding, this distinction is critical. Below, I consider a key element in Locke and Latham's (1990) goal setting theory to exemplify this distinction and highlight the implications of the subsystem level of theorizing. Then, I describe self-efficacy from a subsystems perspective in order to illustrate the difference and connection between system and subsystem theorizing.

A System-Level Theory

Locke and Latham (1990) provide a clear example of a theory of systems (i.e., abstract generalized relationships) owing to the theory building method they used. Using the term grounded theory, Locke and Latham discuss the construction of goal-setting theory as arising from considering and testing specific relationships. Only after each relationship was found repeatedly using several different operationalizations, and abstracted to several different contexts, did it become part of their theory. To its credit, the relationships depicted within it are often strong and practically relevant. In this way, goal-setting theory is a well-established, practical theory of the human system.

Despite this process of theory building, Locke and Latham (1990) claim to provide explanations for the relationships described within their theory. Specifically, Locke and Latham (1990) argue that the mechanisms explaining goal setting constructs (e.g., goal difficulty) affects on performance include effort (i.e., intensity of behavior), persistence (i.e., time engaged in behavior), direction (i.e., target of behavior) and, in the case of complex tasks, strategies. Yet, these terms have little meaning beyond being mere labels for the results of the underlying processes. Consider the following thought experiment. Suppose you had two empty water glasses. For one glass, your goal is to fill it halfway, and for the other, to fill it to the top. To reach your goals, you use a pitcher that limits the flow of water, always pouring at the maximum rate, thus controlling for the intensity of the behavior (i.e., effort). Can we make a hypothesis about the difference in time (i.e., persistence) it will take you to fill each glass? Of course, the glass that you desire to fill to the top will take twice as long to fill as the other glass. This is a basic function of the laws of physics, not the laws of psychology.

Now, let me add a little humanity to this thought experiment. Specifically, suppose we add a little human imperfection such that when pouring into the glass that should be half full, the pourer gets it *around* half full, and for the glass that is to be filled, the pourer *nearly* fills it. Suppose also that we repeat this experiment with either different sets of glasses (i.e., a within-subjects design), different pourers (i.e., a mixed design with goal level as the within factor), or different groups of pourers for each glass (i.e., a between-subjects design). Dutifully, we measure the levels of water and the goals for the glasses and conduct the appropriate statistical analysis. No doubt we would find a strong relationship between goal and performance (i.e., final water level). In addition, using the definitions for effort, persistence and direction provided by Locke and Latham (1990), we also note that effort (i.e., intensity of water flow), direction (i.e., fill the glasses), and strategy (i.e., pour from container) were held constant in our design, whereas persistence (i.e., duration of water flow) was allowed to vary. Because we are clever researchers, armed with a theory, we measured persistence as well as goal and performance. This allows us to test for the mediational role of persistence in the goal-to-performance relationship. What do you suppose we would find? Full mediation, no doubt.

Does this finding of mediation mean that persistence *explains* the effect? Yes and no. Yes, to the extent that the goal-to-performance relationship involved a physical process and the physical process, given the constraints we placed on it, required persistence (i.e., duration) to vary as a function of the goal. No, to the extent it explained anything psychological. Instead, the presumed psychological mediating mechanism persistence is suppose to represent is actually a function of a physical mechanism involved.

At this point let me reiterate that I believe goals' influence on performance is a hugely important psychological phenomenon (Austin & Vancouver, 1996). However, to explain psychologically *how* goals affect performance, it seems we need a different theory then the one described by Locke and Latham (1990). Figure 2a depicts this problem. Specifically, in Locke and Latham's proposed model there is a black box between goal and performance—labeled, but obscured nonetheless. As the thought experiment shows, the labels of effort, persistence, direction, and task strategies do not help much in understanding the underlying processes. On the other hand, Figure 2b depicts Campion and Lord's (1982) control theory explanation of the goal difficulty effect. In this model, the pourer constantly monitors the level of the water while pouring. This is accomplished by the input function within the control system. When the difference between the goal and the perception of the current state becomes zero, action (i.e., pour) stops. The comparator function makes the comparison, while the output function determines the nature of the actions on the environment (i.e., pouring).

The similarities between this example and Powers' (1973) heat capacity example are telling. Like the kinetic theory's main competitor, the caloric theory of heat involved a mysterious mediating substance (i.e. caloric), which determined the heat capacity of a substance. This capacity was believed to be an internal property of the substance for more than a century even though the kinetic theory of heat was also present within the field. It was only through the confluence of some key experiments and the rise of the molecular theory of matter in other contexts that finally convinced the scientific community to accept the kinetic theory of heat in the middle of the 19th century. Even then; however, several theorists (e.g., J. R. Mayer and E. Mach) rejected it based on an antireductionist stance.

In physics the antireductionist stance was not popular such that those voices had little impact on kinetic theory. In psychology, however, antireductionism seems to be more prevalent due to its association with determinism and materialism (e.g., Locke, 1995). The next section discusses why these are attributed, inaccurately, to subsystem theories and what reductionism means within systems theories. Further, the empirical implications of subsystem-level theories that work against their acceptance in psychology are discussed.

Implications of a Subsystem-Level Theory

The position that I take in this paper is that much of the animosity directed toward control theory stems from its explanatory level. In particular, I suspect that attempts to reduce system-level explanations to subsystem-level explanations raises questions of determinism and materialism (Locke, 1995). In this context, determinism relates to the issue of will (i.e., its freedom) and materialism relates to the role of symbolic cognitive processes (i.e., are they epiphenomological and non-causal). Bandura (1995) and Locke (1995) are sensitive to deterministic and materialistic arguments against self-efficacy that have arisen from both radical behaviorism (e.g., Lee, 1989) and physiological perspectives (e.g., Eysenck, 1978). Indeed, Bandura and Locke's (2003) attribution of these positions to control theory seems the most fundamental reason why they reject any control theory explanations. *However, any subsystem-level explanation, including control theory, is ambivalent with regards to determinism and unequivocally non-materialistic*. Determinism and materialism, as they relate to subsystem theories, are each described in turn.

Determinism. Determinism has several meanings in psychology, but the critical meaning here relates to the opportunity for human behavior to be somewhat a matter of free will (Locke, 1995; 1996). Free will means that choices (or at least a subset of them) are made non-deterministically, such as the choice to think or not (i.e., volition). The debates between the libertarians (i.e., those who believe in free will), the soft determinists (i.e., those who believe humans make choices, but that those choices are determined by internal and external factors), and hard determinists (i.e., those who believe human behavior is completely determined by factors outside the individual) are still raging (Sappington, 1990). The libertarians and soft determinists share the belief that there is utility in considering choice and purpose in theories of psychology. For instance, Sappington (1990) interprets Powers' (1978) data comparing the behavior of his models and that of humans, *and* Bandura's theories and data to argue for the utility of considering choice and purposeful behavior. Specifically, Powers and his colleagues (e.g., Marken, 1992) as well as other researchers (Vancouver, Putka, & Scherbaum, 2000) have obtained correlations in the 0.90's between their models and participants' behavior when the participants' choices (e.g., goals) are known.

However, libertarians go further than soft determinists in suggesting that humans are somehow different from the rest of nature, stating that our behavior cannot be explained lawfully. To a libertarian, free will is a meta-assumption so powerful that any description of humans that does not include free will must be inadequate (i.e., an error of omission;

Binswanger, 1990). This stands in contrast to the meta-assumption in much of scientific psychology that "human behavior and mental life are lawful" (p. 295, Lindsey, 1991).

Ironically, the current reality of scientific thinking is that the question of determinism is indeterminable. That is, while subsystem-level theories illuminate black boxes within system-level theories or provide explanation where there was none before, as is illustrated in Figure 2, no one has ever completely illuminated all the black boxes that are contained within a system. Theorists often find that opening one black box reveals more black boxes. Einstein noted this situation when he described that knowledge is like a sphere of illumination, with the dark surface representing what is unknown. As knowledge expands, it reveals exponentially what is unknown. Subsystem theories, while potentially increasing our knowledge, also highlight our ignorance. In terms of psychological phenomena, this implies that we will never know all that determines behavior. Even if we are able to completely reduce the mind to the physical medium in which it occurs, it will still be within a physical system and hence not completely predictable due to Heisenberg's uncertainty principle. The uncertainty principle states that it is impossible to specify simultaneously the position and momentum of a particle, creating a barrier to absolute knowledge. Nonetheless, as Lindsey (1991) points out, psychologists should seek as much understanding and prediction as we can. Black boxes should not be off limits.

In psychology, we are far from illuminating all the black boxes that determine human behavior, allowing one to substitute free will, God's will, or whosever will one wishes within the black boxes. Subsystem theories, including control theories, usually make no attempt to include these meta-physical wills, but they do not, and cannot, eliminate their possibility. Hence, libertarians cannot criticize subsystem theories for eliminating the *possibility* of free will. What subsystem models try to do is push the unknowns deeper into the system in an attempt to increase predictability and understanding in the process. A mistaken association with illuminating subsystem processes is in the assumption that causality also moves to the subsystem level. This mistake brings us to the issue of materialism.

Materialism. According to Locke (1995) materialism is the concept that humans are *only* physical entities, and that cognitive constructs like perceptions, memories, and self-efficacy are just labels for neural events with no true causal role. This position is antithetical to the cognitive perspective in psychology that seeks to understand behavior, at least partially, as a function of the information and symbol processing in the mind. This position is also antithetical to the general systems perspective in science (von Bertalanffy, 1950). Specifically, the systems perspective holds that the properties of systems, while arising or emerging from the properties of the subsystems that form them, may be unique to each level of the system (Boulding, 1956). For example, temperature is a property not of molecules (i.e., the subsystems within substances), but of the ensemble (e.g., distribution of the energies among the molecules).

Hence, in cognitive psychology, a theory of symbolic processing can be articulated at the level of the meaning of the symbols and how those meanings affect one another and behavior. Alternatively, *a theory of symbolic processing can be articulated at the level of the subsystems that bring the symbols about, give the symbols their meaning, and explain how they are translated into action*. Social cognitive theory is of the former type and perceptual control theory is of the latter. Of course, the subsystem theory may reveal some epiphenomological labeling (e.g., as in the case described above of effort and persistence as they are used in goal-setting theory), or it can reveal the role of a symbolic construct more clearly (e.g., as in the case of self-efficacy described below). What subsystem theorists cannot abide by is having no description of the interior processes of the system under consideration. Hence, radical behavioristic approaches, while discovering interesting relationships that might need to be explained, provide no useful constructs that might represent properties of subsystems.

Meanwhile, although theories at different levels of explanation may seem inherently connected, they can exist somewhat independently. For example, Darwin and Mendel did not need to know about the structure of genes to develop their theories (though it would have helped). Computer programmers do not need to understand the architecture of computers to write their programs and engineers do not need to understand the physics of electronics to build computers (although these understandings would help). Doctors do not need to know why certain drugs have their effect, only that they do (although it can help drug researchers find new drugs).

Along the same lines, social and I/O psychologists do not need theories of cognitive subsystems to articulate theories of the relationships among cognitive constructs. Over their careers, Bandura and Locke have clearly shown the possibility of this independence. Likewise, Powers (1973) put aside issues of how the functions in his theory are instantiated in the neuro-chemical medium of the mind. Powers (1973), and others (e.g., Cannon, 1939) demonstrated that the functions *can be* instantiated in such a medium, and that various sub-processes have been demonstrated in a variety of mediums, to substantiate the plausibility of the theorizing. Yet, system theories do not require subsystem theories, and subsystem theories do not obviate systems theories.

In sum, it is true that, to some extent, each level of explanation can be reduced to a lower level, but it is equally true that some properties only emerge as one moves up the levels. This is a basic tenet of systems theories (Katz & Kahn, 1978; von Bertalanffy, 1950). It is also true that if the focus of explanation for a phenomenon is too low, it may be too tedious or intractable to be of much use. Unfortunately, conflict often arises at the confluence of these levels. For instance, psychologists are often trying to convince economists and sociologists that an understanding of the individual will improve their theories. Similarly, I am arguing that control theory models of underlying symbol processing subsystems will improve theories of symbolic processing. Economists, sociologists, and psychologist can make substantial theoretical contributions to their fields without these subsystem theories, but suppressing subsystem theorists' work serves no one.

Empirical issues. Recall that one element of theory quality, regardless of the level of explanation, was the predictive ability of the theory. A final consideration of subsystem-level theories concerns their apparent lack of unique contribution in explained variance over and above system-level explanations. Specifically, subsystem theories must account for known relationships and known system-level explanations of those relationships. As a result, their predictions are often overlapping (e.g., most psychological models of decision making predict rational decisions most of the time, just as the economists' assume). On the other hand, they are often more precise because they can clarify ambiguities in the system-level explanations. Hence, they may make unique predictions that appear as attacks on the system-level theory rather than an elaboration of it.

Operationalizing more unique tests of subsystem theories are certainly possible, but they create complications 1) specific to the ontology of control theories and 2) the prevailing research paradigms in psychology. The ontological complication relates to the use of simulations to test and operationalize subsystem theories (see the DiMaggio, 1995, quote above). That is, with subsystem-level theories, one should be able to build simulations, whether on the computer or in physical representations, that confirm that the theory can produce observations of the system being modeled. For example, consider the water pouring experiment. A subsystem researcher might construct a water pouring apparatus in which the theoretical subsystem properties are represented (e.g., a mechanism that monitors water level in the glass, a parameter that represents desired water level, and a mechanism that pours the water as long as the water level in the glass is less than the desired water level). If the apparatus stopped pouring like the humans stopped, it is at least feasible that the properties represented in the apparatus might represent the properties in humans and account for the behavior. Unfortunately, this methodological tool adds to the materialistic impression of subsystem models, and in combination with the mechanistic impression exacerbated by the engineering uses of control theory, it is not surprising that control theories appear non-psychological.

The prevailing paradigms complication arises because of the current predominance of system-level theories. Specifically, two methodological paradigms currently hold reign in psychology: the experiment and the passive observational study (Cronbach, 1957). The experiment generally involves assigning *individuals* to conditions and assessing the differences between the individuals after one or several manipulations. Meanwhile, the passive observational study generally involves measuring properties from samples of different *individuals* and correlating the properties. When the relationships of interest are the properties of the system (i.e., individual), these paradigms make sense. However, when the theory of interest refers to subsystem properties, it makes more sense to measure these properties within an individual, usually over time, to assess relationships among properties as well as whether they can account for individual-level properties. For example, Jagacinski and Flach (2003) describe within-person studies that have been used to verify how control theory accounts for Fitt's law (1954), a theorem that Newell (1990) describes as so robust and neat it should be in "every first-year textbook as a paradigm example of a psychological quantitative law" (p. 3). Unfortunately, this alternative, case intensive paradigm, where numerous observations from an individual is fit to predicted observations, appears to be so foreign to most applied psychologists that most tests of control theory in the applied arena do not use it. The result is that most "tests of the theory" cannot be uniquely separated from system-level explanations, or when these alternative tests are used, complaints arise that the results are not practical (e.g., consider the within-between debate in expectancy theory, Mitchell, 1974).

The irony of these methodological reservations is that the increased use of both would likely improve psychology. For instance, Simon (1992) noted that the computational modeling required for most simulations is a dramatic improvement over the verbal descriptions used to explain various predictions common to other types of theories. For instance, the mathematics and precise specification of the relationships between functions found in some control theories allow one to build these types of models into simulations (e.g., Jagacinski & Flach, 2003). This aids us in understanding how the system operates over time and in grasping complex scenarios, where unaided predictions are difficult (Hulin & Ilgen, 2000). Moreover, the case intensive designs suited for testing subsystem theories tract more naturally to our understanding of humans as interacting systems with reciprocal relationships described as assumptions in most current theorizing, including Bandura's (1986) social cognitive theory. Instead, the traditional paradigm, particularly the experimental one, is based on closing an *open system* and opening a *closed loop of causality* in order to work (Cook & Campbell, 1979). This problem is the focus of Runkel's (1990) book on research paradigms in psychology, which concluded that case intensive designs are the method of choice if we wish to test theories of the human species.

These empirical issues are clearly relevant with regards to the present debate, foreshadowing the final section of this article. Specifically, I address the question of control theory's apparent lack of a unique contribution to psychology and the studies that have spurred the ongoing debate.

Fundamental Implications of Control Theories as Subsystem Theories

Because system-level theories and subsystem-level theories are often two levels of explanation for the same phenomena, they frequently overlap in terms of their predictions. This can create the impression that subsystem-level theories are simply redundancies of the system-level theories. Indeed, to begin to be accepted, new theories often begin by explaining phenomena already recognized (Kuhn, 1970). The introduction of control theories to understanding motivation in work settings took this form. Specifically, Campion and Lord (1982) used a control theory to account for a level of aspirations effect (Lewin, Dembo, Festinger & Sears, 1944). Shortly after, Lord and Hanges (1987) used a control theory to explain goal-setting effects. That is, they illuminated the black box in Figure 2a with the structure in Figure 2b to account for the varying performance levels. Since then others have conducted additional empirical studies using various control theories as their impetus (e.g., Hollenbeck, 1989, Kernan & Lord, 1991). Although these studies confirm that control theory can inspire empirical investigation, which is a function of good theory (Whetten, 1989), most have used research paradigms that obfuscated the unique predictions that control theory might make. Thus, if the predictions are essentially the same, what might be gained by the deeper and more precise level of theorizing provided by a subsystem theory like perceptual control theory?

Clearly, theorists of applied problems find this general approach useful. The number of references to control theories and cybernetics prompted Katzell (1994) to call it a meta-trend in his review of the chapters in the *Handbook of Industrial and Organizational Psychology*.

Reviewing the propositions found there and elsewhere would be redundant. However, on a more general level there are fundamental differences between a subsystem-level control theory and other self-regulation theories that are likely to have profound implications for psychology (Richardson, 1991), the primary being the unit of investigation. Psychology is defined as the science of human behavior; however, for too long "behavior" has been considered the overt actions of individuals, rather than that of the human system. The implication of this difference is discussed first, laying down the foundation of the more specific contribution regarding self-efficacy's role in behavior discussed later. Additional differences between these theories relate to the role of consciousness and emotions, theorizing about multiple-goal contexts, and the conceptualization of thinking.

The Meaning of Behavior

Powers' (1973) position is that we should not be studying the self-regulation *of* behavior, but rather how behavior is a function of self-regulation. Perhaps this myopic perspective is a vestige of behaviorism; perhaps a cognitive heuristic to which we, including control theorists, sometimes succumb (e.g., the title of Carver & Scheier's 1998 book: *The Self-Regulation of Behavior*). In any case, Powers (1973) argued that perception, not action, should be the unit of investigation. Actions can serve numerous goals and goals can be achieved by various actions. Actions stop, start, and vary in degree. Unless our actions are understood within the proper negative feedback loops (i.e., within the subsystems acting to maintain perceptions at varying levels), they may often seem erratic, or simply a function of changes in the environment.

To understand the behavior of systems based on control subsystems, the crucial question is what perceptions are the subsystems trying to maintain and how are these subsystems interacting with one another. The content of the perceptions for a particular system determines the variable that system is trying to control and the content of its goal. Within this system, the goal level may change (i.e., a fever results from the body raising its temperature goal level); the way we might reach it may change (e.g., a blanket is or is not available); and for learned perceptions, content may change (e.g., one's understanding of what constitutes a quality paper changes as one moves through graduate school). So, if one is interested in determining whether human behavior is a function of striving for goals, as are all of the antagonists in this debate, one should be focusing on measuring and determining the perceptions that are controlled, not the actions.

The Role of Consciousness

A second difference between social cognitive and control theories involves the role of conscious and non-conscious processes. Bandura and Locke entered the field at a time when acognitive approaches to psychology dominated the field (i.e., the mind did not matter). They countered this dominance with evidence supporting the power of conscious, cognitive processes (i.e., symbolic processing and intentionality) in explaining psychological phenomena. The success of their efforts can be seen in the current dominance of the cognitive perspectives focused on volition and intention. Yet, to a new generation of researchers the pendulum of focus has swung too far. Non-conscious, though not necessarily acognitive, processes clearly hold some sway in determining the behavior of the human system (e.g., Bargh & Chartrand, 1999; Wegner & Wheatley, 1999). Whereas there is some acknowledgment of these types of processes in social cognitive theory and goal-setting theory, there is no theorizing regarding how they operate. As my earlier example of how the body regulates temperature illustrates, however, control theory can account for these non-conscious processes. Thus, if we wish to be more complete in our analysis, it seems that both types of processes need to be given consideration.

The Role of Emotions

A third difference between social cognitive and control theories relates to the role of emotion in mediating behavior. Social cognitive theories are hedonistic; that is, emotion mediates the relationship between discrepancies and behavior. On the other hand, the negative feedback loops in control theories do not require an emotional mechanism. Humans control variables like body temperature and muscle tensions without emotional mediation. Nonetheless, emotions clearly play important roles in human regulation and several control theories include one or more of these possible roles in this regulation. For example, emotions are described as mediating *some* relationships (e.g., allocation of attention; Powers, 1992), targets of regulation (e.g., maintain some level of happiness; Ford, 1992), consequences of second-order goal-perception discrepancies (e.g., rate of discrepancy reduction; Carver & Scheier, 1990), and information regarding types of goal-perception discrepancies (e.g., specific emotions arising from the control of ought versus ideal goals; Higgins, 1987). Clear elucidation of the role of emotional subsystems within human behavior has not happened, but the potential of control theory principles in providing some illumination is likely if the above list is any sign.

Multiple Goal Contexts

For many in applied psychology, one of the most potentially useful elements of control theory is its ability to deal with multiple goal contexts (e.g., Kernan & Lord, 1991), because goal-setting theory is of little use for deducing or explaining such contexts. Decision-making models, like expectancy-value theories, have provided a way to consider multiple-goal contexts (i.e., in the form of multiple outcomes associated with options), but they are largely static, incapable of handling the dynamic interaction between the individual and his or her environment (Luce, 1995). Control theories' models of hierarchically arranged negative feedback loops of goal-striving systems thus seem more appropriate (e.g., Beach, 1990). Yet, I have found that the level of understanding required to operationalize control theory models of specific phenomenon is much deeper than is typically needed to operationalize a system-level theory. Moreover, the sophistication of our common methodologies and analysis procedures were not yet matched to the dynamic, interactional nature of the phenomenon a subsystem level of control theory was ready to illuminate. Fortunately this situation is rapidly changing with the introduction of multiple-level analytic models (e.g., Bryk & Raudenbush, 1992) and relatively simple computational modeling techniques (e.g., Levine & Fritzgerald, 1992). Armed with these tools, we can now begin to understand the implications of the multiple goal/control systems in everyday work lives.

Ironically, one of the simplest examples of the importance of understanding multiple goal striving contexts can be found in what Bandura (1986) calls discrepancy production. Perhaps the most common criticism of control theories is that the discrepancy reducing control subsystem does not explain discrepancy production (Bandura & Locke, 2003). On the surface this seems obviously correct. Yet, like many systems theories, we know that the properties of a whole are not limited to the properties of the individual parts. Within psychological control theories, hierarchies of control subsystems are described that not only account for various forms of discrepancy production, but also claim it as a key emergent property of the interacting subsystems.

Despite numerous control theorists making this point (e.g., Klein, 1991; Lord & Levy, 1994; Powers, 1973), and research on discrepancy production based on the control theory perspective (Campion & Lord, 1982; Scherbaum & Vancouver, 2002), Bandura and Locke (2003) continue to use the argument. I suspect one reason for this persistence is that they include in the concept of discrepancy production more than the term suggests. Instead, they use it to refer to the "proactive," or forethought component of their theorizing. Forethought is two separate issues (Bandura, 1986). Forethought relates to both unrealized goals (i.e., desired future states), and to anticipated outcomes (i.e., anticipated future states). Unrealized goals, as an aspect of forethought, can be found in any theory of goals, with control theory being no exception. Campion and Lord (1982) described a multiple, unrealized goals context within a control theory perspective to explain discrepancy production in a classroom setting. More recently, Scherbaum and Vancouver (2002) created a computational model involving three control subsystems with unrealized goals. Simulations of that model produced discrepancies in one of the control subsystems, negating the argument that a theory of only negative feedback loops could not create such an effect. More recently, I created a computational model where discrepancies were produced as two discrepancy reducing (i.e., control) subsystems were operating in contexts similar to the one Campion and Lord described. These models confirm the plausibility of a property of the system *emerging* from the interaction of subsystems with different properties.

Even with the added benefit of explaining behavior in multiple goal contexts with unrealized goals, one could argue that control theories are limited because they cannot account for the other aspect of forethought (Bandura & Locke, 2003). Yet, I think perceptual control theory's conceptualization of thinking is likely one of its most promising and most misunderstood contributions.

The Conceptualization of Thinking

At the subsystem level, forethought (i.e., anticipating future states) and thinking require using memory. This can include making predictions of uncertain events, and weighing the value of those events in terms of the focal goal and other affected goals. This process is the focus of decision-making theories (e.g., expectancies and valences; utilities of possible actions or choices); however, this element *appears* to be missing from control theories based on Powers' accounts of human behavior (cf. Miller, Galanter, & Pribram, 1960, or Newell & Simon, 1972, where it is the central phenomena of interest). Indeed, descriptions of the properties of the potential subsystems involved are missing from many popular control theories (e.g., Carver & Scheier, 1981; Klein, 1989). Instead, decision making constructs (e.g., outcome expectancy in the case of Carver & Scheier, 1981) or whole theories (e.g., expectancy theory in the case of Klein, 1989) are often added on to these simple models to account for this generally unassailable aspect of human behavior. It is this adding on process that is one of Bandura and Locke's (2003) central criticisms of control theories.

The add-on nature of decision-making constructs to many modern or "rational" control theories is seen as an important supplement to control theory (Donovan, 2002; Sniezek, 1999). Much of the debate between control theorists and non-control theorists has turned on whether or not supplementing is legitimate. Bandura and Locke (2003) argue that, in the case of control theory, supplementation replaces the core of control theory with a different theory or combination of non-control theories (see also, Locke, 1991). A counter argument to this is that supplementation occurs all the time in theorizing, including both goal-setting and social cognitive theorizing (Pinder, 1998). However, I believe neither position is particularly relevant to the issue at hand. Powers' perceptual control theory is at a different level of explanation than most decision making theories. Thus, the question is not whether these perspectives can supplement each other, but whether control theory can serve as a substrate to decision making theories (Vancouver, 2000a). More specifically, can it account for the processes that require anticipatory estimations that are used in the planning, choosing, and revising of goals?

Consider again the example of body temperature control. Given a scenario in which our body becomes too cold, automatic, non-cognitive mechanisms will cause one to shiver, bringing up our temperature. However, if our memory recalls the effectiveness of a blanket in similar situations one is likely to be used in conjunction with the automatic processes described. Humans, at least, have developed back-up systems for maintaining the stability of key internal variables that involve substantial manipulations of the environment. What subsystem control theorists argue is that evolution has done this by co-opting the basic control system building block and adding a few enhancements. The phenomenological consequence of this appears to be consciousness; the scientific consequence is a domain of inquiry called cognitive psychology. In this particular example, the consequence is that we can *learn* to put on coats—made with an *understanding of what causes* heat loss—*before* going out into the cold.

The subsystem elements that are missing in the simpler accounts of control theory include memory and the control systems that determine how memory is used (Powers, 1973). These elements allow the human system, as hypothesized by Powers, to use alternative modes of processing. One mode, called the passive observation mode, allows the person to record the perceptions created by another (i.e., a model). Another mode, the imagination mode, involves sending signals from an output function of a control system via memory to the system's input function, from which a perception is formulated. Hence, perceptions arising during this mode are based on projections given what has happened in the past. It is via these processes that Powers (1973) theory might account for planning and other forethoughtful activities. Yet, does it?

It is too early to tell the answer to that question. Powers' own work has not focused on this element of his theorizing, but control theorists in other domains have developed numerous models for solving problems of optimal and adaptive control that involve internal estimation and learning (Jagacinski & Flach, 2003). In their book titled *Control Theory for Humans,* Jagacinski and Flach (2003) describe much of this work in relation to human factors and ergonomic problems. In line with the engineering perspective, they use labeling conventions that Powers (1973) thought problematic when referring to humans. Also with regards to adaptive control, the more sophisticated models likely use mathematics more complex than humans would naturally use (Anderson, 1995). However, they also clearly illustrate ways in which cybernetic subsystems, with memory capacities, can handle the kinds of processes psychologists associate with thinking.

The task of developing cybernetic subsystem mechanisms, and assessing their veracity with regards to human processing, will likely require years of dedicated research, theoretical elaboration, and continued debate. For instance, the work on self-efficacy that began this current round of debate is only a small aspect of this issue. Other aspects seem more difficult to work out at the subsystem-level, such as observational learning (Bandura, 1986). Whereas this speaks to the value of system-level theories because they can include the phenomena without explaining the details of how it works, it does not preclude the value of the subsystem theories. Yet, the details might have substantial implications regarding the processes. In the next section, I describe the substantial implication of subsystem-level theorizing about self-efficacy and address some of Bandura and Locke's (2003) specific criticisms of the empirical work.

An Explanation of Self-Efficacy

Earlier I described a control theory elaboration of goal-setting theory's key finding (i.e., the goal-on-performance relationship). In that case, I questioned whether the key mediators (e.g., persistence) were measures of psychological mechanisms or the properties of physical mechanisms relevant to the task. In social cognitive theory, Bandura (1991) describes self-efficacy as the most central *mechanism* of human self-regulation. Specifically, self-efficacy is a construct held responsible for some observed causal relationships. To someone interested in the subsystem level of human cognition, variables, even latent ones, cannot serve as mechanisms, but they can serve as markers or parameters in models of the subsystem mechanisms responsible for the observed relationships. Alternatively, as the example from Powers about heat capacity illustrated, they can serve as a system-level property reflecting an average or some other combination of subsystem properties. The Vancouver et al. (2001; 2002) studies, which were the impetus for Bandura and Locke's (2003) article, were designed to test a control theory elaboration of self-efficacy's role as a key mediator of human behavior (Bandura, 1977).

To accomplish this we had to first describe self-efficacy from the subsystem perspective because Bandura and Locke (2003) were correct when they noted that Powers never included it within his theory (cf., Powers, 1991). We suggested that self-efficacy reflects how signals from the output function and other subsystems are weighted when creating an anticipated or estimated perception from memory for some focal subsystem (Vancouver, et al., 2001). Given our current discussion, it makes more sense to use self-efficacy to refer to *the aggregate* of the weighting process. The specific weighting parameter in the memory substructure of *each* control system might be reasonably labeled expectancy after Tolman's (1932) and Lewin's (1951) notions. That is, self-efficacy more comprehensively reflects the accumulation of expectancies related to the numerous subsystems thought relevant to achieving specified levels of performance. This parallels temperature, which reflects the average movement of molecules within a substance. Self-efficacy likewise reflects a consideration of all of the elements of action and the context in which it is occurring (or being anticipated to occur), which is consistent with Bandura's (1997) conceptualization. Expectancy reflects the specific weights.

Next, using control theory conceptions of thinking as a processing mode for control systems that use memory structures to create perceptions, we hypothesized that self-efficacy, reflecting key expectancy weights, likely influences the anticipated or estimated perceptions used in goal processes. The higher the weights, on average, the higher the perceptions given the same level of the signals. When the goal processes involve planning or revising, the relatively higher anticipated perceptions arising from relatively higher self-efficacy levels would lead individuals to accept or remain with the goals they are thinking about or currently pursuing, respectively. That is, self-efficacy would be expected to positively relate to accepting difficult goals or persistence in the face of frustrations. The second study in Vancouver et al. (2001) confirmed (i.e., replicated) this well-established positive influence of self-efficacy on the task we used in our studies. However, our control theory description of the mechanism and how variance in self-efficacy is used by it led us to a unique implication for self-efficacy that we wanted to highlight in order to distinguish control theory explanations from social cognitive theory predictions. Specifically, we reasoned that when the estimated perceptions are to be used to assess current states in an ongoing activity, the relatively higher estimates associated with high self-efficacy would likely result in the control systems reaching their goal levels more readily than lower estimates. This latter effect could result in a negative relationship between self-efficacy and performance depending on the degree to which current performance perceptions were estimated rather than assessed from direct stimuli. Thus, we did not expect to find, and indeed did not find, social cognitive theory's prediction of a direct positive effect for self-efficacy.

Bandura and Locke (2003) had much to say about the studies and arguments found in Vancouver et al. (2001) and Vancouver et al. (2002). For the most part these articles can stand on their own merits and readers can judge the veracity of Bandura and Locke's comments for themselves. However, three issues require some elaboration because they were not anticipated and have come up in other contexts (Eden, Chen, Kanfer, Mathieu, Stajkovic, & Vancouver, 2002). The three issues are the "guessing game" nature of the task, the possibility of a spurious (i.e., regression to the mean) explanation for the negative effect, and the complexities associated with the feedback concept.

Guessing Game. Bandura and Locke (2003) and others (Eden et al., 2002) have questioned a property of the Mastermind task used in these studies. Specifically, they highlight the element of chance, or luck, inherent in the game. We exacerbated this perception by referring to attempts at finding the solution as "guesses." The result is that Bandura and Locke refer to the task as a guessing game instead of an analytic game. Performance on guessing games, they argue, would not be useful for studying motivated behavior. If luck completely determines performance, the degree of effort or persistence would not have a measurable effect. Therefore, if one wants to see a link between a motivational construct like self-efficacy and performance, performance must be at least partially a function of motivation.

The requirement that performance must be somewhat a function of motivation to study the influence of motivational constructs is completely correct. However, the assertion that performance on the Mastermind game is completely a function of luck is incorrect. First, there would have been no stable individual differences in performance were performance based on pure luck. Yet, the intraclass correlations for performance indicated more variance between individuals than within individuals over time. More importantly, if motivation did not matter, goal-setting manipulations should not have affected performance. Yet in Study 2 of Vancouver et al. (2001), we found a goal-setting effect. Furthermore, in Study 2 of Vancouver et al. (2002), we specified the specific activity where motivated action matters and associated the results of that action to our measures of self-efficacy. Performance on the Mastermind task is clearly somewhat a function of motivation.

On the other hand, it is also somewhat a function of luck. This is an important attribute of the task creating variance on self-efficacy over time. Without this attribute, performance would likely not vary enough to influence self-efficacy, which is needed to test the hypotheses. Moreover, tasks that are a function of motivation, ability, and luck are common in the real world. Consider the door-to-door salesperson. Each potential customer is more or less inclined to buy the product or service at the outset (the luck element); however, the persistence and skills of the salesperson are also likely to matter in determining the final sale.

Bandura and Locke (2003) also claim that learning must take place for the task to be a meaningful one, and for testing the effects of self-efficacy. It is not completely clear what they believe needs to be learned. If it is how to do the task, then this evokes one of the common criticisms of the experimental studies of self-efficacy–that the manipulations confound learning with self-efficacy (Hawkins, 1992; Lee, 1989). For example, the group that learns to play the piano will have higher piano playing self-efficacy than the group that does not learn to play, but their capacity to play the piano *is* higher because they learned something the other group did not.

The other thing people can learn for a task is their capacity to engage in it; in other words, they can learn how good they are at performing. We found evidence that this type of learning occurred in the Mastermind task because average self-efficacy was highly related to the individuals' average performance. There was no evidence that the first type of learning occurred, and this is a good thing given our design, a point upon which I elaborate in the section on statistical artifacts.

However, what is most surprising about Bandura and Locke's (2003) claim is that learning contexts are exactly the contexts in which Bandura (1997) predicts a negative effect for self-efficacy (also mentioned in the 2003 article). Bandura refers to this as the preparatory context within which he claims individuals with high self-efficacy will lack the motivation to prepare because of their belief in their capacity. We noted in our papers that the arguments Bandura (1997) makes regarding preparatory contexts are very much in line with the arguments we where making, but that we believed the effect could occur in a non-preparatory context given the right conditions. Therefore, we purposely designed the study to be a non-preparatory context.

Statistical Artifacts. Bandura and Locke (2003) additionally note that because of the luck element in the Mastermind game, performance will bounce around the mean, with self-efficacy and the personal goal levels following performance. This we found. They also suggest that this produces a saw-tooth pattern, which spuriously accounts for the negative relationship we found between self-efficacy and subsequent performance. Again, it is true that a saw-tooth pattern emerged; otherwise we would not have had the negative effect we found. The central question raised, however, is whether that saw-tooth pattern was a statistical artifact like regression to the mean (Samuels, 1991) or a function of the substantive processes we argued?

The regression to the mean argument seems compelling. Specifically, if one performs especially well (i.e., much above their average), we might expect that there next performance would be less impressive. Likewise, we might expect a particularly poor showing to be followed by something better. Whichever direction, the opposite would be expected because we expect people's performance to hover around their mean. Assuming for the moment that Bandura and Locke are correct about the nature of the Mastermind game, performance at Time 1 would not be related to performance at Time 2 because each performance is a function of luck, which is random. When two variables are not related, the best prediction of the second variable given the first is the mean of the second, regardless of the value of the first. Therefore, the predicted relationship between past and subsequent performance is a flat line at the subsequent performance's mean, not a negative relationship like the one Bandura and Locke (2003) claimed would emerge.

If, on the other hand, performance was either increasing, perhaps because of learning, or decreasing, perhaps because of fatigue, over time, then the immediately subsequent measure of the performance (or its correlates like self-efficacy and personal goal) would likely be closer to performance than to more distant measures of performance (or its correlates). This would cause a spurious *positive* effect. This spurious effect is the autocorrelation effect often feared in longitudinal designs (Jones, 1991). Thus, it turns out Bandura and Locke (2003) were correct in stating that a learning environment would likely show a positive effect; however, it would be the result of a statistical artifact.

The Many Meanings of Feedback

One major source of misunderstanding of control theory involves the term feedback, which has several denotations within the psychological literature. Most of the time distinctions in meaning can be handled with adjectives. For example, Bandura and Locke (2003) correctly distinguish between descriptive feedback (i.e., information about what the individual has done) and normative feedback (i.e., information about how the individual's performance compares to others). However, the adjective "negative" does not delimit a specific meaning. In control theory, the negative refers to *the nature of the operation of the system*. Specifically, a negative feedback loop is one that through its operation reduces the difference between the level of a variable and the reference (i.e., goal) level for the variable. A closely related concept is the negative feedback loop found in causal diagrams within the system dynamics literature. A causal negative feedback loop refers to *a circular series of cause and effect relationships* (i.e., with an odd number of negative relationships; Richardson, 1991). A simple example comes from Adam Smith (cited in Richardson, 1991). Smith argued that as the market price for a product goes up, production goes up (i.e., a positive cause-effect relationship), which in turn causes the supply to go up. However, as supply goes up, market price (the first variable in the loop) goes down, which is a negative cause-effect relationship. The odd number (i.e., one) of negative relationships in this loop indicates that this loop is negative and stable. Yet, no comparison with goals or desired states is explicit within the loop. Richardson (1976) noted this type of negative feedback loop should not be, but often is, confused with the cybernetic negative feedback loop described in control theories.

More common in the psychological literature (e.g., performance appraisal; learning) is the concept of negative feedback as *information communicated to an individual* (i.e., that his or her performance was less than what *the communicator* thought it should be). Moreover, this communication usually does not take place while the individual is engaged in the task, but only after it is completed. Hence, it is not information about the current state of a controllable variable, but about the current (or past) state of a variable on which the individual can no longer act. In cases where the individual can act, information that one has not reached a desired goal (negative feedback?) is met with continued behavior whereas information that one has reached their desired goal (positive feedback?) is met with discontinuation of the behavior, at least until something happens to disturb the current state of the variable, creating a new discrepancy (Vancouver & Putka, 2000). Bandura and Locke (2003) agree with this latter description of what would happen in the goal-striving context, claiming it as an old goal theory finding (although a finding occurring somewhat after control theory models of human behavior were articulated, e.g., Powers, Clark, & McFarland, 1960a; 1960b).

So what do Bandura and Locke (2003) mean by negative feedback? They refer to an unpublished dissertation in which negative feedback was operationalized as a loss frame (-25%) as opposed to a gain frame (+75%) from a goal. This seems to be a different concept altogether (i.e., frame, not feedback sign). The manipulation is often used to manipulate regulatory focus (e.g., Förster, Grant, Idson, & Higgins, 2001). Yet, they use the results of the study to claim, "So much for the driving power of negative feedback" (Bandura & Locke, p. 17). Later they refer to negative response feedback in learning contexts. Here it appears that the word "negative" refers to information suggesting that some approach to a problem does not work. They do not deny this information is useful; simply that it is not very efficient for learning.

Bandura and Locke's conceptual confusion is not surprising. Feedback is a common term with many meanings even within the control theory literature (Richardson, 1991). For example, it is often used to refer to the information that is received about the current state of the variable (e.g., Carver & Scheier, 1981). Yet, that current state may have nothing to do with actions taken by the person; therefore, nothing is being *fed back*. Moreover, information about the current state of some variable is also likely to be used in creating perceptions of a number of variables tied to other control subsystems. This is the concept of feedback space we discussed in Vancouver et al. (2002). The crux issue is how and what information is used by control systems in the operation of negative feedback loops (i.e., to potentially create perceptions that indicate discrepancies from goal levels). A subsystem-level control theory forces one to confront the ambiguities regarding the meaning of feedback. Richardson (1991) might argue this is its most important implication.

Conclusion

In the end, this debate is *not* about whether the interpretations surrounding the series of studies we conducted on self-efficacy are accurate or not. The issue is the value of control theories, particularly Powers' version, for understanding human behavior. Clearly any theory should go through rigorous testing under a cloud of skepticism. This is what we do as scientists. Perceptual control theory, like any theory that is still the focus of research, is a work in progress. We do not know what it might explain or fail to explain. We do not know what past, present, or future revisions will make it better or worse. We certainly do not know what other theories might do better because so few competing subsystem-level theories exist. However, we also know that many system-level explanations need subsystem-level elaboration. In some cases, such as with goal-setting and discrepancy-production effects, the elaboration has begun. In other cases, such as the origin of goals (Pinder, 1998), the elaborations seem to be just around the corner. Still other cases will likely require more experience with modeling the subsystems before reasonable models of the phenomena are created. Bandura's (1986) concept of observational learning (i.e., modeling) falls in this category. In all cases, however, the theories are not in competition with each other, but rather complementary ways of considering the same phenomena.

Given how long it took the physicists to accept the kinetic theory of heat, it is not surprising that it took 30 years after the introduction of cybernetics to the sciences (Rosenbueth et al., 1943) for a sophisticated theory of human control subsystems to emerge (Powers, 1973). Nor is it surprising that 30 years later, the theory is still largely unexplored. It involves using multiple, dynamic, open, non-linear subsystems to explain phenomena generally approached empirically assuming a single, static, closed, linear system. While we recognize these latter assumptions do not hold, abandoning them creates methodological conundrums difficult to overcome (Shadish, Cook, & Campbell, 2002). Yet, I conceive of these difficulties as a challenge and an opportunity to move psychological science forward.

Beyond the empirical challenges, however, control theories have been the subject of attack on philosophical grounds. Control theory, at least the subsystem versions, is unapologetically reductionistic. However, psychology's understanding of reductionism has been seriously flawed, creating unfounded associations with determinism and materialism. More specific to control theory is its association with engineering principles, reinforcing the mechanistic associations of the theory. It is around these issues that I have focused this paper. Armed with a different understanding, I hope to remove the philosophical arguments against the theory and increase the belief in the 60-year old promise of cybernetics for understanding purposeful behavior.

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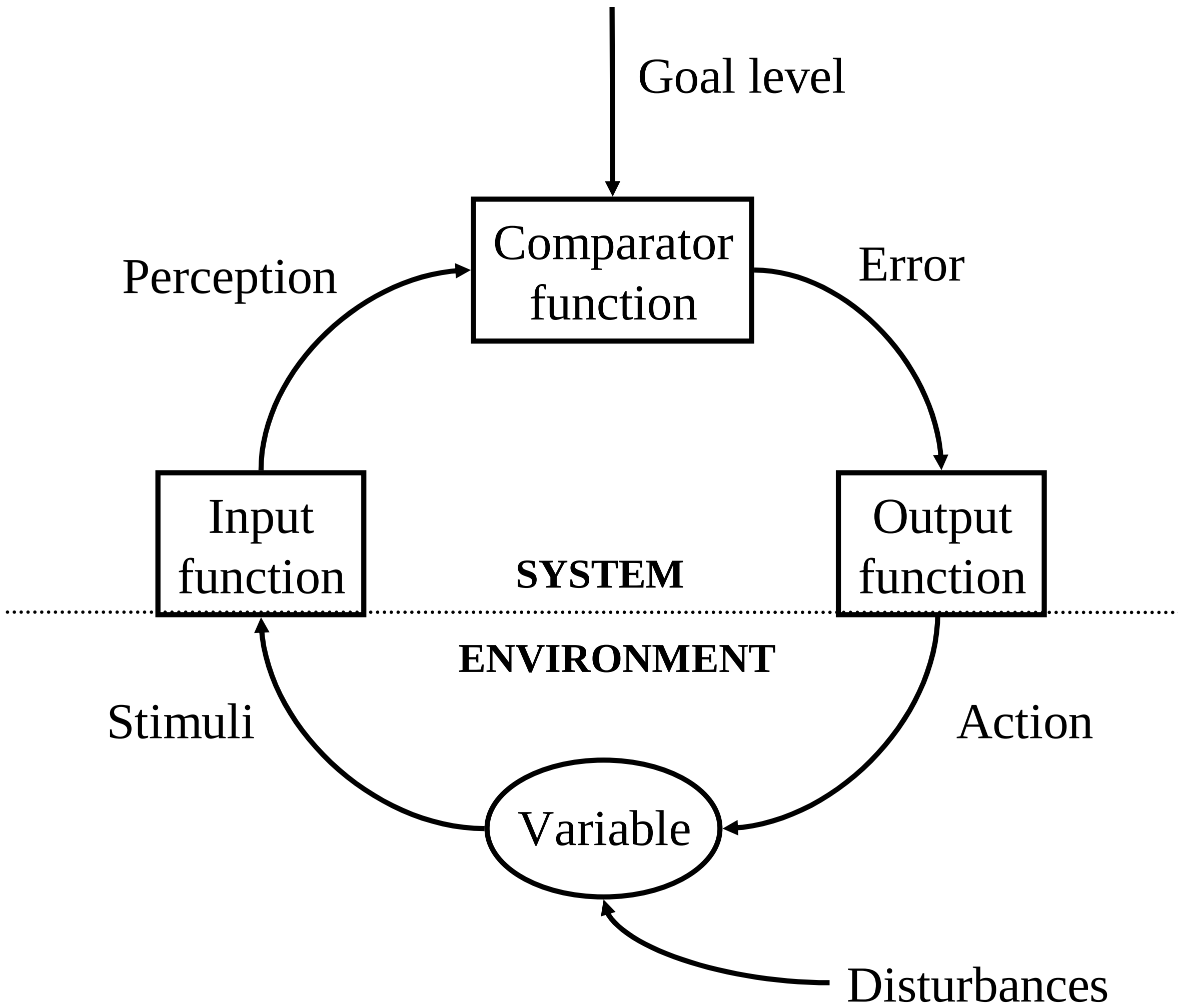
Figure Captions

Figure 1. The cybernetic structure: a single control system.

Figure 2. Goal-setting theory's black box illuminated.


Goal level
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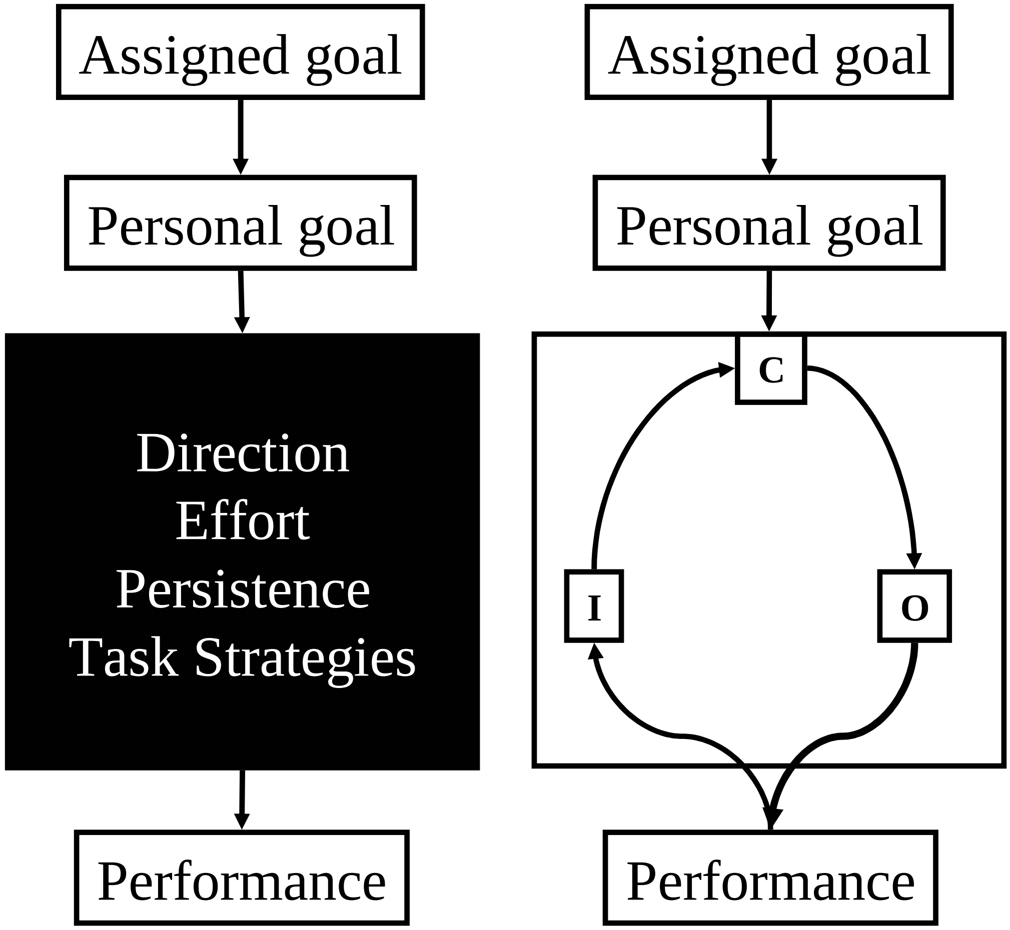
Assigned goal
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Effort
Persistence
Task Strategies
Performance
Assigned goal
Personal goal
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