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EDITORS' CHOICE

For Truth and Realism in Management Research

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Is truth a research goal and regulative ideal that is appropriate for management research? Is realism an appropriate philosophical foundation for management research? This article answers in the affirmative on both of these questions by (a) using historical method to develop the four fundamental tenets of scientific realism, (b) using the historical material to show how some management writers have presented a historically false view of scientific realism, (c) developing a scientific realist model of truth, and (d) using trust as a foundation, arguing for truth and realism in management research.

Keywords: *truth; realism; trust; positivism; constructivism; objectivity; historical method*

Are truth and objectivity appropriate research goals and regulative ideals for social constructivists in administrative science? Meckler and Baillie (hereafter, M&B; 2003a) revisited the, much discussed, truth and objectivity questions and argued that "neither the strong positivist nor strong constructivist perspective is quite correct" (p. 274). They then "propose a 'middle way' between the postmodern rejection of notions of truth and objectivity and [the strong positivist position]" (p. 274). Their "middle way" entails, among other things, adopting a form of the correspondence theory of truth and employing the work of Searle (1995), who contrasted (a) epistemic objectivity with epistemic subjectivity and (b) ontological objectivity with ontological subjectivity. This approach, argued M&B, enables them to agree with constructivists that many of the knowledge claims in social science deal with ontologically subjective entities (e.g., money, profits, etc.), while maintaining that such an ontological subjectivity implies neither abandoning truth nor adopting the epistemic subjectivity of relativism and postmodernism. Rather, they concluded, "the claims of organizational science can be true in the sense of expressing epistemically objective facts while acknowledging that the realm of [i.e., the entities studied in] organizational science as a whole is [are] ontologically subjective" (p. 283).

The commentaries by Gioia (2003) and Lounsbury (2003), when joined with the reply of M&B (2003b), provide a useful reminder of the chasm that separates constructivist researchers from others in administrative science. Gioia (2003) accused M&B of "ravaging . . . a hapless straw man" and using "interesting linguistic gymnastics" that have "trivialized even the notion of truth itself" and "prostitute[d] Weick's treatise" (pp. 285, 287). For Gioia, M&B's "apparently well-intended, but dishearteningly misguided effort" appears to be "yet another thinly disguised attempt to bring interpretivism under positivism's wing," and he

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urged them to "give up the attempt" (pp. 285, 286, 291). Similarly, Lounsbury (2003) accused M&B of being "anti-social constructivists" who "distort the work of Astley" (p. 293). Furthermore, M&B "use pompous language" and "seem quite unsympathetic to social constructivist approaches" (Lounsbury, 2003, p. 293). Constructivist approaches, for Lounsbury (2003), are the result of "incommensurable paradigms" (p. 295), and he recommended that "instead of looking for ways to make various approaches commensurable, as suggested by M&B (2003a), it would be much more fruitful to celebrate and engage in the plurality of styles" (p. 297).

M&B (2003b), in their reply, claimed that they "came to praise social constructionism, not to bury it." Specifically, they wish to save "it from some social constructionists who saddle it with implausible claims regarding relativism, truth, and objectivity" (p. 299). Among the many points they made, they disputed Lounsbury's claim of incommensurability, for "if two paradigms are incommensurable, then it is logically impossible for their claims to disagree with each other" (p. 300). Furthermore, M&B argued that their middle way shows "how constructivists can fully reclaim notions of truth and objectivity and eschew the obnoxious and intellectually fraudulent habit of putting these terms in scare quotes" (p. 301). M&B closed their reply by agreeing that "social reality is a constructed reality" and arguing that "we don't need to treat social structures *as if* they were real, because they *are* real" (p. 303; italics in original).

Three observations on the truth and objectivity debate motivate this article. First, note that Gioia (2003), again and again, described M&B's view as the nefarious "positivist" approach. This is despite the fact that M&B's approach is not appropriately described as positivist, as pointed out in this journal by Hunt (1994) and acknowledged elsewhere by Azevedo (2002), McKelvey (1999, 2002), and Moldoveanu and Baum (2002). As Baum and Rowley (2002) pointed out,

> Much of the "paradigm war" has been driven by mistaken views of positivism (e.g., that it is synonymous with quantitative methods, determinism, reification, and causal laws), as well as by ignorance of philosophers' abandonment of *both* positivism and relativism as incoherent decades ago. (p. 20)

Alas, despite some progress, the rhetoric of positivism bashing identified in Hunt (1994) continues.

Second, note also that the approach advocated by M&B drew heavily on the realist philosophy of Searle (1995), who maintained that "realism and a correspondence conception [of truth] are essential presuppositions of any sane philosophy, not to mention of any science" (p. xiii). Thus, M&B's approach seems inappropriately described as a middle way between the strong positivist and strong constructivist views, for Searle and other realist philosophers do not position scientific realism as between positivism and constructivism.

Third, M&B argued for truth using the standard, Tarskian biconditional approach (Tarski, 1956); that is, using M&B's example, "Portland is rainy in January" is true if and only if *Portland is rainy in January*. For Tarski (and much of philosophy after Tarski's seminal work), what is within the quotes refers to a linguistic expression, and what is italicized is independent of the language used to form the linguistic expression; that is, "A statement (proposition, belief, . . .) is true if and only if what the statement says to be the case actually is the case" (Alston, 1996, p. 6). Gioia's (2003) claim is unwarranted that M&B's starting from a Tarskian biconditional stance results in a so-called trivialized notion of truth. However, I argue in this article that there is much more that modern versions of realism, such as those associated with Boyd (1984), Harré (1986), Leplin (1984), Levin (1984), Manicas (1987), McMullin (1984), Niiniluoto (1999), and Siegel (1983, 1987) can offer with regard to explicating the concept of truth.

Therefore, the purpose of this article is to extend the analysis of M&B on the role of truth in administrative science.[1] In a manner similar to Hunt's (1994) summary of the historical development of logical positivism, I begin by providing a ruthlessly brief overview of the historical development of scientific realism. The overview will conclude with four theses that, I argue, serve as the fundamental tenets of scientific realism: classical realism, fallibilistic realism, critical realism, and inductive realism. I use this historical material to show how some advocates of scientific realism in the management literature, for example, Godfrey and Hill (1995), have presented a historically false view of scientific realism. Next, I develop and argue for a model of truth and scientific realism that is based on inductive realism's focus on the successes and failures of empirical tests. To illustrate the model, I use an example from the recent assessment by David and Han (2004) of the empirical support for the transaction-cost economics approach to strategic management. By using this example, I aim to encourage future writers to move beyond exemplars such as “Portland is rainy in January” and move toward instances of actual management research. Finally, using the importance of trust as a foundation, I argue for truth and scientific realism.

HISTORICAL DEVELOPMENT OF SCIENTIFIC REALISM

From the very beginnings of the scientific revolution in the 16th century, science and philosophy were closely related.[2] Indeed, prior to the 19th century, science was a branch of philosophy, and scientists were referred to as “natural philosophers.” However, this situation changed in the latter half of the 19th century when philosophy came to be dominated by Hegel (1770-1883) and his idealism: “He ruled the philosophical world as indisputably as Goethe the world of literature, and Beethoven the realm of music” (Durant, 1954, p. 222). Hegel’s idealism was hostile to mathematics and unsympathetic to science. Its central tenet was that the external world does not exist unperceived: “All reality is mental (spiritual, psychical). Matter the physical, does not exist” (Angeles, 1981, p. 120). Thus, Hegel’s so-called identity of reason and reality denied the existence of tangible objects (e.g., rocks and trees) and proclaimed only reason to be real.

Hegelian idealism’s dominance in philosophy began to crack at the turn of the century from the efforts of G. E. Moore (1873-1958) and Bertrand Russell (1872-1970), who offered three major arguments against idealism: First, idealism confuses the act of perception with the object being perceived. When the object of a mental act is distinguished from the awareness of it, there is no reason to deny the existence of the object independently from its being perceived. Second, idealism uses the concept *real* in ways that violate principles of intelligible discourse; that is, the meaning of the term *real* derives from such exemplars as “this table exists.” Denying the fundamental examples that give meaning to a term, while continuing its use in other contexts produces unintelligible speech. Third, idealism constitutes sophistry, for the behaviors of idealists are inconsistent with their stated beliefs. Although they claim that objects such as chairs do not exist, when entering rooms, idealists approach and sit on chairs, just as if they believe such chairs do exist. The philosophy that Moore and Russell argued for was, in today’s terminology, classical or commonsense realism, whose central tenet is that the external world of tangible objects exists independent of perception.

The second crack in idealism’s philosophical hegemony developed from a discussion group at the University of Vienna that was formed in 1907 by the mathematician Hans Hahn, the physicist Philipp Frank, and the social scientist Otto Neurath. By the 1920s, the so-called Vienna Circle group had added other physicists, including Moritz Schlick (1882-1936), who had studied under Max Planck and who had already received acclaim for his interpretations of Einsteinian relativity. Under Schlick’s leadership, the Vienna Circle sought a philosophy that would (a) heal the rift between science and philosophy and (b) provide a means for interpreting quantum mechanics. The philosophy they developed, logical positivism, was not opposed to the commonsense realism of Moore and Russell. Indeed, the positivists were allies with the realists in their philosophical battles with advocates of Hegelian idealism. Schlick’s (1932/1959) classic article on the foundations of logical positivism framed the idealism-realism question as “If the phrase ‘external world’ is taken with the signification it has in everyday life, . . . [then] are there in addition to memories, desires, and ideas also stars, clouds, plants, animals, and my own body?” He answered: “It would be simply absurd to answer this question in the negative” (p. 101). Therefore, “logical positivism and realism are not in opposition; whoever acknowledges our fundamental principle must be an empirical realist” (p. 107).

However, if the logical positivists had no problems with according reality status to tangible, observable entities, they strongly questioned giving such status to any so-called transcendent world that allegedly stood behind the observable world, but about which nothing could be verified by observational means. Because the positivists’ verifiability principle equated the meaningfulness of a proposition with the possibility of its verification, for Schlick (1932/1959),

> The denial of the existence of a transcendent external world would be just as much a metaphysical statement as its affirmation. Hence, the consistent empiricist does not deny the transcendent world, but shows that both its denial and affirmation are meaningless. (p. 107)

A major reason the positivists questioned the meaningfulness of any proposition in which transcendent or unobservable concepts are included is that they believed that this was the best interpretation of quantum mechanics. Understanding how they came to this conclusion requires at least some understanding of the world implied by quantum mechanics—a world that is anything but commonsensical.

Quantum Mechanics, Realism, and Positivism

The development of quantum mechanics began with attempts to solve the so-called black body problem at the turn of the century. A black body is one that perfectly absorbs and then reemits all radiation falling on it. In the smoothly continuous world of classical physics, the radiation emitted from a black body would also be perfectly continuous. Max Planck, however, proposed in 1900 that the radiant energy emitted takes place only in the form of discrete packets, which he called energy quanta. Electromagnetic radiation, he proposed, is made up of a whole number of packets of energy, with each packet having the energy hv, where h is Planck's constant and $v$is the frequency of oscillation. Einstein used Planck's idea of energy quanta in 1905 to discredit the (then firmly established) view that light is fundamentally wavelike. He theorized that construing light as being made up of individual particles or photons would explain how electrons are emitted from metals by an incident beam of light. Thus was born what has become known as the wave-particle duality: light is simultaneously wavelike and particle-like.

In 1911, Ernest Rutherford developed his solar system model of the atom, in which negatively charged electrons orbit a positively charged, nuclear so-called sun. His model, however, had a major problem: If electrons could occupy any of the infinite number of possible orbits, they would spiral ever closer to the nucleus, and the atom would be unstable. A young Dane, Niels Bohr, solved this problem by applying quantum theory. He theorized that electrons could occupy only discrete orbits around the nucleus, and he used Planck's constant to identify those specific orbits that would be possible. In 1923, Lewis de Broglie proposed that all subatomic particles, not just photons, are actually wave particles and developed equations that connected the energy and momentum of any such particle with the frequency of its associated wave. Erwin Schrödinger then used de Broglie's ideas in 1926 as a basis for accommodating the wave-particle duality through his justly celebrated wave-function equation. In 1927, Heisenberg proposed his indeterminacy principle: the experimental act of investigating the position (momentum) of a subatomic particle necessarily destroys the possibility of measuring its momentum (position) to arbitrary accuracy. At the limit, if one knows precisely where any subatomic particle is, one has absolutely no idea what it is doing. Dirac then used wave mechanics in 1928 to develop quantum field theory. If interrogated in a particle-like way, the formalism of quantum field theory gives probability predictions of particle behavior; however, if interrogated in a wavelike way, the theory gives probability predictions of wavelike behavior.

Since the late 1920s, predictions of quantum mechanics have been confirmed in thousands of experiments. Given its radical break with classical mechanics, its interpretation prompted a great debate between Einstein, who argued for a realist interpretation, and Bohr, who, influenced by the Vienna Circle, argued for a positivist view. Bohr and his positivist allies developed an interpretation of quantum mechanics that is now referred to as the Copenhagen interpretation, which is often used interchangeably with instrumentalist interpretation and positivist interpretation. Its basic premise is that what we can know about the quantum world is only the effects we can observe after an intervention. As Bohr put it, "The entire formalism is to be regarded as a tool for deriving predictions . . . under experimental conditions" (Bohr, quoted in Polkinghorne, 1984, p. 79); that is, the uncertainty described in Heisenberg's principle does not reflect science's ignorance of the laws of nature—uncertainty is a law of nature. Prior to an act of measurement (observation) it is meaningless speculation even to talk about where a subatomic particle really is, or its momentum, or the direction of its spin. All particles exist in a superposition of potential states.

Einstein and his realist allies attacked the Copenhagen view with appeals to rhetoric (e.g., Einstein's famous claim that God does not "play dice" with the universe), so-called hidden variable theories that posited entities standing behind the wave-particle duality (e.g., Bohm's hypothesized "pilot wave"), and numerous thought experiments. Of the thought experiments that attempted to undermine the view that uncertainty is a law of nature, Einstein's most famous one, with Boris Podolsky and Nathan Rosen (hence the "EPR" experiment), argued that quantum mechanics implied, at times, that the information that a particle is being investigated would be transmitted instantaneously to a second particle. Because speeds in excess of the speed of light are impossible, argued EPR, quantum mechanics violates "local reality" and must be deficient.

Bohr responded to Einstein's rhetoric with the gentle chide that it is not for scientists to prescribe to God how He should run the world. As to the various hidden variable theories, Bohr and his positivist allies argued that such theories were ad hoc and, in any case, the hidden variables (e.g., Bohm's so-called pilot wave) seemed even more bizarre than the Copenhagen view. The Hungarian mathematician John von Neumann then joined the argument and argued that any hidden variable theory was bound to disagree with some of the verified empirical results of quantum mechanics' experiments. After Bohr et al. had rebutted Einstein's thought experiments, John Bell in the 1960s developed some experimentally testable consequences of the EPR thought experiment. Since then, the results of experiments have tended to favor the Copenhagen interpretation: Einsteinian local reality seems incorrect. As the realist philosopher Putnam (1990) put it, "One cannot emphasize too strongly that only a small minority—an extremely small minority—feels any discomfort with the Copenhagen interpretation to the present day" (p. 8). Indeed, the positivist, Copenhagen view, as unsettling as its nonrealistic interpretation is to many, continues to reign supreme among physicists.

Realism Since the 1930s

Realism suffered a heavy blow in the quantum mechanics debate. However, beginning in the 1960s, the so-called received view of the logical positivists as to the nature of all theories (not just quantum mechanics) began steadily losing ground to the realism now generally referred to as "scientific realism" (Suppe, 1977), which is associated with such philosophers as Maxwell (1962), Sellars (1963), Putnam (1962, 1990), Bhaskar (1979), MacKinnon (1979), Siegel (1983, 1987), McMullin (1984), Boyd (1984), Levin (1984), Leplin (1984), Harré (1986), Manicas (1987), and Niinniluoto (1999). However, there is no "grand theory" of science to which all scientific realists ideologically adhere: "Scientific realism is a majority position whose advocates are so divided as to appear a minority" (Leplin, 1984, p. 1). The absence of a scientific realist grand theory of science notwithstanding, Hunt (1990, 2003) argued that four theses serve as the fundamental tenets of scientific realism: classical realism, fallibilistic realism, critical realism, and inductive realism.

Classical realism holds that the world exists independently of its being perceived. This is the "external realism" advocated by Searle (1995), who is cited by M&B (2003a). For Searle (1995), "The world (or alternatively, reality or the universe) exists independently of our representations of it" (p. 150). Contrasted with idealism and postmodernist relativism, classical realism maintains that there really is something "out there" for science to theorize about. Nonetheless, scientific realism rejects direct realism, which holds that knowledge about external objects can be known with certainty because our perceptual processes necessarily result in a veridical representation of external objects (Hooker, 1985). Scientific realism, in contrast, argues for fallibilistic realism, which maintains that, though the job of science is to develop genuine knowledge about the world, such knowledge will never be known with certainty: there is no "God's eye" view. Similarly, critical realism, recognizing the fallibility of our perceptual (measurement) processes, maintains that science must critically evaluate and test its knowledge claims to determine their truth content. Finally, inductive realism maintains that "the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists" McMullin (1984, p. 26).

Because the logical positivists believed that science's implicit acceptance of inductive realism in the 19th century had wrongly encouraged it to believe in the absolute truth of Newtonian mechanics, they rejected inductive realism and accepted Humean skepticism's position with respect to unobservable constructs (McMullin, 1984; Stove, 1982; Suppe, 1977). Scientific realism, in contrast, maintains that Humean skepticism, which "denies that one can progress by logical reasoning from perceptual experience to any genuine knowledge of an external world" (Watkins, 1984, p. 3), is wrong headed. For scientific realism, the positivists were "throwing out the baby with the bath water."

ON SCIENTIFIC REALISM AND MANAGEMENT RESEARCH

Many management writers implicitly advocate a form of scientific realism, and some do so explicitly. An example of explicit advocacy is the work of Godfrey and Hill (1995) (hereafter, G&H). They, quite rightly, argued:

> According to realists, the scientific enterprise can give us knowledge about the existence of unobservable entities. Realists argue that when a theory that contains unobservable entities is well corroborated by scientific evidence, then we may have good reason for believing that those unobservable entities have a correspondence in reality. Thus, the realist believes that we can make statements about the truth value of theories that contain unobservables—the more skeptical logical positivist does not. (p. 520)

Unfortunately, however, the other arguments of G&H for scientific realism are so historically incorrect that the management discipline risks adopting scientific realism for reasons that can easily be attacked by antirealist writers of all kinds; that is, it risks a repeat of the historically and philosophically uninformed "paradigm wars" (Baum & Rowley, 2002). First, as the preceding discussion shows, it is historically incorrect for G&H to argue for scientific realism and against logical positivism on the grounds that logical positivism "has been roundly attacked for its inability to handle theories that rely on unobservable constructs, such as quantum physics" (p. 524). Indeed, it would be more accurate to say that logical positivism was invented by the Vienna Circle to handle the unobservable constructs in quantum physics. Furthermore, the positivist interpretation of quantum mechanics still dominates the views of physicists.

Second, it is historically false to claim that it "has not helped the positivist cause that some of these theories, and particularly quantum mechanics, have been spectacularly successful in making predictions that are subsequently confirmed by empirical observation" (G&H, 1995, p. 524). Just the opposite is true: it has helped the positivist cause enormously that quantum mechanics has been spectacularly successful. If (a) physics is the prototypical science, (b) quantum mechanics is spectacularly successful, and (c) the positivist interpretation of quantum mechanics dominates, then (d) should not all theories be interpreted positivistically?

Third, it is historically false to maintain that "modern realism emerged in the twentieth century as the dominant response to the philosophical problems that quantum mechanics created for logical positivism" (G&H, 1995, p. 523). Indeed, the more accurate statement is exactly the reverse: Logical positivism emerged in the early 20th century as the dominant response to the philosophical problems that quantum mechanics created for classical realism. As discussed, scientific realism has developed since the 1960s despite its problems with quantum mechanics. Even today, when such antirealist philosophers as Fine (1986) and Van Fraassen (1980) attack the realist view, they associate it with the many failed attempts to find hidden variables that stand behind the observed consequences of experimental evidence supporting quantum mechanics. Even today, advocates of realism—including this one—admit that there is as yet no satisfactory realist interpretation of such constructs as the wave-particle duality.

Fourth, it cannot be argued cogently that "A strict application of the [positivist] verificationist theory of meaning to agency, transaction costs, and resource-based theory would doom these theories to the realm of metaphysics, along with the likes of quantum mechanics" (G&H, 1995, p. 524). Even though the dominant positivist interpretation of quantum mechanics has its critics, no one can argue that it has doomed quantum mechanics to metaphysics. There is no reason, therefore, to suspect that interpreting management theories positivistically would doom them either. Fifth, it cannot be argued cogently that if one adopts the positivist view then "the derivation of normative rules for managerial action from such theories constitutes an unscientific endeavor" (G&H, 1995, p. 520). Practicing physicists use quantum mechanics every day to make recommendations on everything from transistors to fiber optics to nuclear fission. And no one claims, nor should claim, that they are unscientific in doing so.

In short, there are good grounds for management researchers to adopt scientific realism. However, these grounds do not include many of the arguments in G&H (1995), and, thus, management researchers should not use them as grounds for adopting realism as a philosophical foundation for research.

A SCIENTIFIC REALIST MODEL OF TRUTH

The preceding section introduced the four basic tenets of scientific realism. This section develops a scientific realist model of truth that focuses on the successes and failures of empirical tests. To articulate the model, I use an example from strategic management. David and Han (2004) reviewed the transaction cost economics (TCE) approach to strategic management, identified its six core tenets, and assessed the extent to which 308 empirical tests (found in 63 articles) support the core tenets of TCE. One tenet they examined was "As asset specificity increases, hybrids and hierarchies become preferred over markets; at high levels of asset specificity, hierarchy becomes the preferred governance form" (p. 41). They reported that the 63 articles they reviewed contained 58 tests in which asset specificity was the independent variable and the choices of market, hierarchy, or hybrid were the dependent variables (see their Table 3). Of the 58 tests, 32 (55%) supported the specificity tenet, 3 (5%) were counter to the tenet, and 23 (40%) were not significant. I used the findings of David and Han (2004) on the asset specificity tenet, in particular, and the implications of their findings for TCE, in general, as continuing examples to illustrate the scientific realist model of truth.

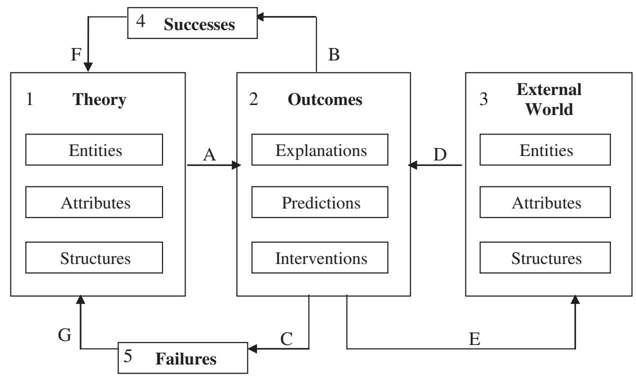

Figure 1: Truth and Scientific Realism: Successes, Failures, and the External World
Source: Hunt (2002). Reprinted by permission of the author.

The Model

Scientific realism, in viewing administrative science as a truth-seeking enterprise, conceptualizes *truth* as not an entity, but an attribute. It is an attribute of beliefs and linguistic expressions. For example, it is an attribute of such linguistic expressions as those denoted by the labels *theories*, *laws*, *propositions*, and *hypotheses*. Recall that the inductive realism tenet of scientific realism maintains that the long-run success of a theory gives reason to believe that something similar to the entities and structure postulated by the theory actually exists. Figure 1 is a model that explicates the meaning of "Theory *X*is likely true" and "Theory *X*is likely false" in the scientific realism approach to science.[3]

Assume that Box 1 in Figure 1 contains the linguistic expression denoted by transaction cost economics theory or TCE. The TCE theory posits entities (e.g., assets, transaction-specific assets, market governance, hierarchy, and hybrid), attributes of entities (e.g., the identifiable characteristics or properties of assets, transaction-specific assets, markets, hierarchies, and hybrids), and structures (e.g., the proposition that, as asset specificity increases, hybrids and hierarchies become preferred over markets). The theory (i.e., TCE) posits that the entities, attributes, and structures referred to in Box 1 exist in the world external to the theory (i.e., Box 3); that is, the linguistic expressions that constitute the theory in Box 1 are about the world in Box 3.

Path A, from Box 1 to Box 2, shows that some theory (e.g., TCE) has certain implications or outcomes; that is, the theory can be used to explain some phenomena (e.g., "Why did Firm *X* use a hierarchical governance mechanism?" "It did so because it had transaction-specific assets and, therefore, it feared the opportunism of market governance"). The theory can also be used to predict some phenomena (e.g., "The relationship between asset specificity and the adoption of hybrids and hierarchies should be positive and significant"). The theory can also be used to suggest interventions (e.g., "If a new venture has high transaction-specific assets, be wary of the potential opportunism of venture partners and protect yourself accordingly").

Paths B and C show that the theory's outcomes are sometimes successful (Box 4) and sometimes not (Box 5). For example, as to explanations, some firms that have transaction-specific assets are known to use hierarchies (which would constitute explanatory successes); however, others are known to use market governance (which would constitute explanatory failures). As to predictions, recall that David and Han (2004) reported that, of the 58 tests with asset specificity as the independent variable, 55% were successes (Box 4) and 45% were failures (Box 5). As to interventions, at times the suggestions of TCE are right (e.g., the venture partners are indeed not trustworthy and the costs associated with protecting one's firm are warranted), and at times the suggestions are wrong (e.g., the venture partners are trustworthy and the costs of protecting one's firm from potential opportunism are unwarranted). The successes and failures are affected by the entities, attributes, and structures that exist in the external world (Box 3), as shown by Path D. In turn, the outcomes in Box 2 affect (by way of Path E) the entities, attributes, and structures in Box 3 (e.g., when managers in firms interpret the outcomes in Box 2 as supporting the truth of TCE, and this belief then guides their future patterns of behavior).

What, then, is the import of a high or low proportion of successes (Box 4), and a low or high proportion of failures (Box 5)? Paths F and G represent inferences from a theory's successes and failures to the truth content and falsity content of a theory. For scientific realism, a high proportion of successes, relative to failures, gives reason to believe that something similar to the entities, attributes, and structures posited by the theory in Box 1 (e.g., the tenets of TCE) actually exist in the world external to the theory (i.e., they exist in Box 3); that is, we infer that something similar to the theory posited in Box 1 is likely true. The "something like," then, equates with TCE being "approximately true" or "having truth content." From a high proportion of failures, relative to successes, we infer that something similar to the theory (e.g., again, TCE) is likely false. In a sense, Paths F and G depict a "weighing" of evidence, as Bunge (1967, p. 319) so aptly put it. However, scientific realism, as a theory of science, does not imply that "a high proportion of successes, relative to failures" means "true with probability 'p'." Likewise, it does not imply that "a high proportion of failures, relative to successes" means "false with probability 'p'." Indeed, most scientific realists are highly skeptical of efforts that attempt to apply the logic of probability to the weighing of evidence involved in the empirical testing of theories. In addition, the realist approach to truth does not equate *truth* with *truth with certainty*. As the realist Siegel (1983) put it, "To claim that a scientific proposition is true is not to claim that it is certain; rather, it is to claim that the world is as the proposition says it is" (p. 82).

Returning to the study by David and Han (2004) on the tenets of TCE, note that the authors framed their conclusions consistent with scientific realism. They found that, as an independent variable, asset specificity seemed to fare best: "This construct was quite successful at predicting the make-vs.-buy choice (58%), and was even better at predicting the degree of integration between independent buyers and sellers (79%)" (p. 52). Furthermore, their findings regarding uncertainty as an independent variable showed that it fared poorly: "In fact, there was almost as much evidence that increasing uncertainty led to results in the *opposite* direction predicted by the theory (e.g., towards less integrated governance forms)" (p. 52; italics in original). Moreover, when they, as scientific realism puts it, "weighed" the total empirical evidence, they found overall support to be at 47%, and they concluded:

> We found this [47% support] surprising, especially given our conservative sampling methodology (i.e., selecting only published journal articles with clear and direct relevance to TCE). We expected that a theory of such prominence and disciplinary-spanning power would have clear-cut support. (p. 51)

Therefore, David and Han (2004) disagreed with Williamson's claim (1996) that "Transaction cost economics is an empirical success story" (p. 55). Indeed, for them, the empirical evidence to date shows TCE to be on "shaky ground" (David & Han, 2004, p. 55).

On The Scientific Realist Model of Truth

Readers should note that truth is an attribute of beliefs and linguistic expressions, it is not an entity in the external world (i.e., truth is not in Box 3 in Figure 1). Therefore, *truth* is not an entity that researchers do (or can) study. To treat truth as an entity in Box 3 is to engage in reification, that is, it is "to postulate as an entity fallaciously" (Levin, 1991, p. 57). For example, with regard to truth, Anderson (1988) asked, "Indeed, how would we know truth even if we held it in our hands?" (p. 404). His query is (one suspects) meant to be taken as just an instance of colorful, relativist rhetoric. Nevertheless, his reification of truth vividly illustrates the conceptual danger of treating an unobservable, intangible concept, such as truth, as if it referred to an observable, tangible object, such as an apple. By wrongly leading us to believe that truth could be held in our hands, his reification of truth, absurdly, leads us to inquire how we could recognize it with our eyes.[4]

A common accusation by relativists is that those holding that science should seek true theories must also reify truth. For example, Zinkhan and Hirscheim (1992) maintained that those who hold truth in high regard must assume that "there is an immutable truth out there which scientists can study" (p. 83). However, they provided no quotations of realists or any other evidence of instances of reification—and for good reason: it is likely that no such evidence exists. Truth for scientific realists is not an entity for study, let alone, an "immutable" entity.

The scientific realist model of truth in Figure 1 prompts several observations. First, the model seems consistent with the kind of arguments advanced in the review of TCE by David and Han (2004).[5] This is unsurprising, for many philosophers of science, as well as most philosophically oriented social science researchers, believe that only some version of realism can explain the actual workings of science without reducing it to a shameful charade. For example, because no rational person searches for the characteristics of a nonexisting entity, what other than the warranted belief that the entity labeled *transaction-specific assets* exists could motivate the search for whether such assets prompt firms to adopt hybrids and hierarchies? Are we to believe that researchers say to themselves:

> Even though I do not believe in the existence of transaction-specific assets, I shall pretend they exist and then pretend to investigate whether such assets impact on the decision to adopt hybrids and hierarchies, whose existence I also shall pretend to believe in?

Although disingenuous activities do take place in science, scientific realism maintains that the totality of physical and social science is not best described as an elaborate charade.

Second, the scientific realist model of truth is inconsistent with logical positivism and logical empiricism. Even though the logical positivists and logical empiricists held truth in high regard, both were under the spell of Humean skepticism with respect to induction (Stove, 1982). Therefore, both refused to countenance the real existence of entities that were, in principle, unobservable. Because, for the logical empiricists, only observables are real, the concept of transaction-specific assets must be simply a shorthand way of talking about a collection of observable entities; that is, the logical empiricists made a sharp distinction between theoretical terms and observation terms, with only the latter referring directly to some aspect of the world. Theoretical terms would have to be given meaning by being defined through so-called correspondence rules with observation terms. However, this posed a major problem for the logical empiricists: the problem of theoretical dispensability. Called the "theoretician's dilemma" by Hempel (1965), the first one half of the dilemma is as follows: If all theoretical terms can be defined through correspondence with observation terms, and if the purpose of science is to determine relationships among observation terms, then theoretical terms are unnecessary in science. The second half of the dilemma is as follows: If theoretical terms cannot be defined through correspondence with observation terms, then theoretical terms are meaningless and, surely, are unnecessary in science.

For scientific realism, the theoretician's dilemma is no dilemma at all. Scientific realism dismisses the theoretical term and/or observation term dichotomy as a false dichotomy; that is, scientific realism acknowledges that all the terms in a theory are, properly speaking, theoretical terms. The expression *theoretical term* means nothing more than "a term in a theory." For scientific realism, some terms in a theory may denote something more observable, more detectable, more easily measurable than other terms. In fact, some terms may denote nothing, in principle, observable at all. However, all the terms in a theory (excepting, of course, mathematical and logical terms) can legitimately claim to denote the existence of some entity, such claims being based on (a) the senses (classical realism) and/or (b) the success of a theory (inductive realism).

Third, the model in Figure 1 is inconsistent with strict falsificationism. As were the logical empiricists, Popper (1972) was strongly influenced by Humean skepticism (Stove, 1982). For Popper (1972),

> I regard Hume's formulation and treatment of the logical problem of induction . . . as a flawless gem . . . a gem of priceless value . . . a simple, straightforward, logical refutation of any claim that induction could be a valid argument, *or a justifiable way of reasoning*. (pp. 86, 88; italics added)

Thus, Popper (1972), by claiming that all positive results of a theory test are irrelevant to science (not a justifiable way of reasoning), fell into a form of irrationalism (Stove, 1982). In terms of Figure 1, falsificationism would maintain that, whereas the inferences of Path F are not a justifiable way of reasoning, the inferences of Path G are justifiable. The scientific realist model of truth—in accord with, I argue, the actual practice of science—maintains that Paths F and G are defensible.

FOR TRUTH AND SCIENTIFIC REALISM

The preceding shows how scientific realism approaches the concept of truth. I turn now to arguing for truth as a research objective and regulative ideal in management research. The argument is based on trust. What is trust? Trust exists when one has confidence in another's reliability and integrity (Hosmer, 1995; Morgan & Hunt, 1994; Pearce, 2001; Rousseau, Sitkin, Burt, & Camerer, 1998). In turn, the confidence of the trusting party in the trustworthy party's reliability and integrity is associated with the belief that the trustworthy party has such attributes as being consistent, competent, honest, fair, helpful, and benevolent. In organizational behavior, the study of so-called norms of trust is considered a characteristic that distinguishes management theory from organizational economics (Barney, 1990; Donaldson, 1990).

What is the relationship between trust and scientific realism? Realist philosophy of science views trust as a key construct for understanding the dynamics of scientific disciplines. Trust is essential in science (indeed in all disciplines) because scientific knowledge is a shared form of knowledge; it is shared with its clients. The clients of academic management researchers include not just other academics but also practicing managers, students, government officials, and the public in general. In essence, all researchers who share their research with others state implicitly: "Trust me." One consequence of the importance of trust in science concerns those whose research projects are guided by philosophies maintaining that no research "touches base"—Path D in Figure 1—with a reality external to the researcher's own linguistically encapsulated theory, or paradigm, or research tradition. Thus, philosophies such as relativism, constructivism, and postmodernism, when they deny the reality-theory connection, are self-defeating for the researchers who adopt them. Who could trust the output of research guided by such philosophies?

Philosophers of science are coming to realize that trust and ethics are interrelated keys to understanding scientific communities. Rom Harré has been at the forefront of those who advocate the importance of, in his terms, "moral order" in science. Harré (1986) defined scientific knowledge as "trustworthy knowledge," rather than truth with certainty:

> Science is not a logically coherent body of knowledge in the strict, unforgiving sense of the philosophers' high redefinition, but a cluster of material and cognitive practices, carried on within a distinctive moral order, whose main characteristic is the trust that obtains among its members and [the trust that] should obtain between that community and the larger lay community with which it is interdependent. (p. 6)

What, for him, is trust? "To trust someone is to be able to rely on them in the matter in question. . . . Scientists believe that things personally unknown to them are as *another scientist* says they are" (Harré, 1986, p. 12). However,

> trust is not maintained by telling each other only literal truths. Under that constraint the members of the community would perforce remain forever silent. It is enough that they tell each other what they honestly believe to be the truth. (1986, p. 12)

In this regard, Harré is claiming that the moral order of science implies, among other things, the avoidance of sophistry and deception, as well as outright fraud.

Harré (1986) pointed out that trust in all societies is often role-related: "it is because the trusted one is in the role of parent, guardian, policeman, research supervisor, and so on, that the trust is there until something happens to upset it" (p. 21). Therefore, scientists in their role as researchers producing trustworthy belief are required by their peers and by the lay community to maintain a moral order. This moral order is necessary, Harré argued, because researchers are involved in producing "practically reliable scientific knowledge." This

> reliance might be existential, concerning what there is or what might be, or it might be practical, concerning what can and cannot be done, or both. The moral quality of the product comes through clearly in the kind of outrage felt by the [scientific] community at the disclosure of scientific fraud. (Harré, 1986, p. 13).

Harré (1986) asked: "Is scientific method . . . and scientific morality, the fiduciary act of committing oneself to make one's scientific utterances fiduciary acts, the best way to discipline a community which exists to find out about the natural world?" (p. 26). He answered this question affirmatively on the basis that science is committed to referential realism. This realism holds that

> existence is prior to theory, and that while no ontologies for science could be absolute, nevertheless, ontologies (realized in referential practices) are always, at any moment, less revisable than their associated belief-systems. . . . On this view, truth and falsity migrate from the epistemology of science to the morality of its human community. (Harré, 1986, p. 6)

For Harré, any view of science that claims that scientific knowledge is constructed or created by the scientific community independent of some external reality is to be rejected on moral grounds.

The members of the management academic profession have numerous clients for management knowledge. Concerning management knowledge, its development, and dissemination, does the trust that these constituencies have in management academics imply certain special responsibilities? If so, what is the nature of these responsibilities, and what does it imply about the most appropriate philosophy to guide management inquiry? Philosophies based on relativism and the denial that research connects with any reality external to some linguistically encapsulated theory, paradigm, or research tradition would seem to be unlikely candidates for inspiring trust. Most assuredly, no philosophy of research can guarantee trustworthy knowledge. Nevertheless, researchers can find comfort in the fact that there exist philosophies of science—such as scientific realism—that, at the minimum, are not antithetical to truth and its surrogate, trustworthy knowledge, and, at the maximum, may (fallibly) yield knowledge that is truly worthy of others' trust. Although the clients of management research can ask for no more, they deserve no less.

NOTES

1. I restrict the analysis here to truth. For analyses of objectivity, including the supposed problem of the theory ladenness of observation, see Hunt (1993, 1994, 2003). In addition, as to the issue of the alleged ontological subjectivity of administrative science concepts, see Thomasson (2003).

2. The historical material in this section, except where noted, draws heavily on the works of Ayer (1959), Bergmann (1967), Hunt (2003), Joergensen (1970), Manicas (1987), Polkinghorne (1984), and Suppe (1977).

3. The general approach to the concept *truth* used here, as well as the model depicted in Figure 1, is consistent with the descriptive-success version of the correspondence theory of truth developed in Goldman (1999): "An item X (a proposition, a sentence, a belief, etc.) is true if and only if X is descriptively successful, that is, X purports to describe reality and its content fits reality" (p. 59).

4. For more on reification, see Hunt (1994, 2003).

5. Of course, this does not mean that different evidence or different means of "weighing" the evidence might not have yielded different overall findings; that is, it does not imply that their findings are insensitive to either the specific measures and sample sizes of the individual studies they report on or to M&B's use of simple proportions as a weighing technique instead of, for example, a more formal, meta-analytic procedure. In general, it is always important to keep in mind that scientific realism is a fallibilistic realism.

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