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Entrepreneurship's Creation School and Its Comparison-Based Approach: Assessing the Lessons for Theory's Progression

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Abstract: The creation school of opportunity formation is a relatively new partial theory of entrepreneurship built upon a relatively new approach to theorizing. Given their significant respective impacts, we critically evaluate both that school and that approach to better understand their respective values to theorizing. We apply a comprehensive, critical framework to evaluate the creation school. We determine that it is not yet a theory, but that it raises several important theoretical questions, from the origins of valuable heterogeneity to which meta-heuristics should be assumed and to what types of uncertainty are involved in entrepreneurial opportunities. We then describe its comparison-based approach for theorizing, delineating it from similar approaches that also contrast against the given benchmark ideas in a field, to determine its benefits and costs to advancing the modeling of phenomena. We determine that this new approach differs from problematization and contrastivity; instead of developing assumptions from induction, it uses a strawman to simply assert them. We finish by discussing the implications for how we can take more control over what kinds of theories affect the definition, and the legitimacy earned, in important fields of research like entrepreneurship.

Keywords: creation school; comparison-based approach; theory building; strategic entrepreneurship; 3E framework



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1. Introduction

The creation school depicts an attractive vision of an entrepreneurial and strategic process of generating new (potential) value, one primarily based on a collection of three overlapping papers published between 2007 and 2013 (i.e., [Alvarez and Barney 2007, 2010](#); and [Alvarez et al. 2013](#)). Its presentation was provocative at the time because, much like the opportunity-focused effectuation logic that preceded it ([Sarasvathy 2001](#)), it was defined through a stark contrast with a strawman version of an older idealized benchmark—in the present case, with the so-called discovery school of opportunities (e.g., [Kirzner 1989](#); [Shane and Venkataraman 2000](#)). In the interim fifteen years, the creation school has made a recognizable impact on entrepreneurship research, most recently by helping the field refocus on opportunity definition, generation, and exploitation.

But, if we are to build on its ideas, it is time for the creation school to stand on its own—to be evaluated as an independent partial theory rather than as a set of ideas expressed primarily through contrast. It is time for the creation school to be critiqued. And this is only fair given it is itself the embodiment of a critique (i.e., of the discovery school). Therefore, in that same spirit—i.e., that critical reflection of recently established ideas allows for research and theorizing in our field to better advance—we offer this timely and comprehensive critique of one of the most recent and popular partial theories in entrepreneurship, a field that struggles to find its own independent conceptual models (e.g., [Arend 2018](#)). Doing so in this case makes us more informed about this new school's own weaknesses, gaps, and biases, so that we can focus on and analyze opportunity-related phenomena better. Doing so also provides the opportunity to analyze the recent

shift in theorizing approaches toward more radical–subjectivist and observation-inspired narratives, a shift that raises meta-level questions about what are the better ways for our theory-building to evolve. To summarize, then, we address two research questions in this paper—is the creation school a valid theory of entrepreneurship? and is the comparison-based approach a valid means of theory-building?

The primary objective of this paper is to deliver a fair and critical assessment of what the creation school is, and could be, for theorizing about opportunity generation for entrepreneurs and strategists. The secondary objective of this paper is to initiate the larger discussion over the meta-theoretical question that its theory-building approach raises—the question of whether a comparison-based approach is the right evolutionary step. Each objective addresses a problem in our field—the problem of the incompleteness of a popular model of aggressive opportunity generation, and the problem of the dangers of theory-building by comparison alone. The first objective does so by providing a post-publication check on a mature partial theory—one that has gained traction in recent years—so that its weaknesses are made more explicit to those considering building upon it (to make it more complete), testing it, or prescribing from it. The second objective does so by initiating a much-needed debate over our expectations of new partial theories and the acceptable approaches for building them.

2. A Critical Assessment of the Creation School

A fair, comprehensive, and critical assessment of the creation school can establish what it is and what it is not. The creation school offers a constructivist argument that opportunities do not exist without entrepreneurs and their actions, where those actions are mainly directed at influencing social entities (e.g., [Wood and McKinley 2010](#)). To determine whether that argument is sufficient, original, and useful, we analyze its completeness as a theory as well as its novelty as a narrative.

2.1. The Creation School's Process Model

Because the three foundational papers upon which this school is built provide no diagram of the model that they collectively describe, we must create one here as an interpretation of those writings. Figure 1 illustrates the process model of the creation school. It is a feedback loop with one entry and three possible exit points. The entry point finds the entrepreneur (which is a label covering a wide range of possible entities, but mostly described in terms of a new venture team) facing a situation of Knightian uncertainty (i.e., involving unknown future states and their probabilities), endowed with (heterogeneous) beliefs and resources, and about to take action. Presumably, the intended action is within a calculated affordable loss, is targeted at a specific market for a specific purpose (i.e., to elicit useful feedback), and is feasible with the current resources available. The action is then taken on its intended market, that action comprising elements meant to affect the social construction of the market in the entrepreneur's favor. The market then digests the action—by altering its own social reality within the confines of any relevant objective realities (e.g., laws of physics)—and produces a reaction. To complete the feedback cycle, that reaction is then digested by the entrepreneur—who updates beliefs and resources ([Edelman and Yli-Renko 2010](#)), and then considers a further action (if it is within a new affordable loss limit). The cycle can end in three ways: (i) when all desired action options exceed the affordable loss limit; (ii) when the market reaction clearly indicates a failure of the entrepreneur to produce an opportunity; or (iii) when the opportunity-as-a-new-and-exploitable-market-imperfection is successfully created. The next stage in that latter value creation process, i.e., the steps in exploiting the newly-created opportunity—are not provided by the creation school and so we complete the diagram there.

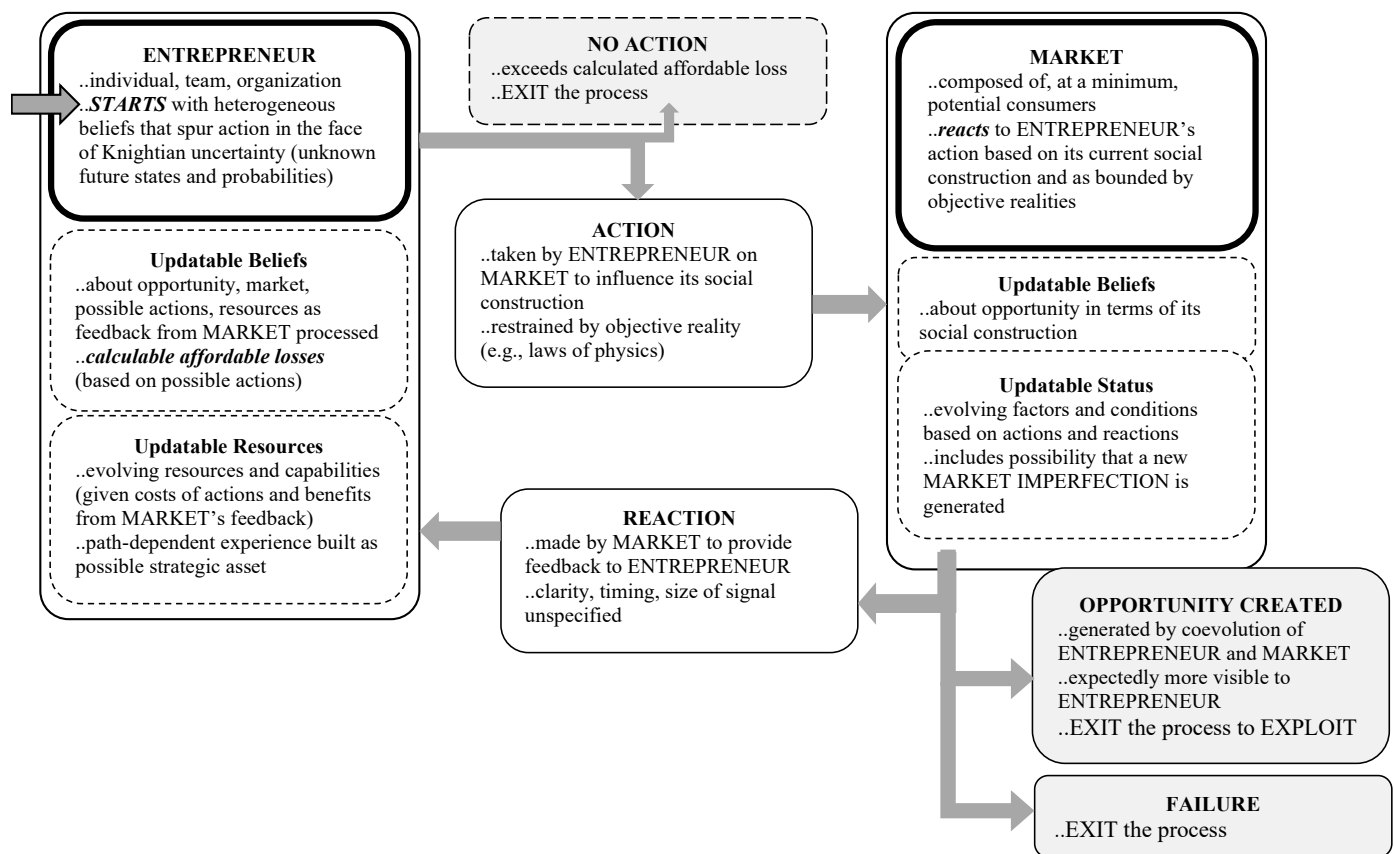


Figure 1. The creation school process model (interpreted).

2.2. The Framework for Evaluating a Proposed Theory

We evaluate the creation school's theoretical *bona fides* by applying a recent and comprehensive analysis tool—the 3E theory-assessment framework (Arend et al. 2015). This tool summarizes and extends decades of previous approaches for evaluating theory, capturing, and organizing all of those criteria (e.g., Bacharach 1989; Boxenbaum and Rouleau 2011; Dubin 1969; Eisenhardt 1989; Gioia and Pitre 1990; McKelvey 1997; Mohr 1982; Priem and Butler 2001; Suddaby 2010; Sutton and Staw 1995; Thomas and Tymon 1982; Whetten 1989). It does so in one cohesive and exhaustive checklist, covering a theory's full embodiment—from its origins to its model's components to its value-in-use. “The three Es represent the natural order of theory building—that is, input, throughput, and output—that we label “experience,” “explain,” and “establish.” Researchers experience the focal phenomenon they wish to theorize about through observation and literature review. They then explain the phenomenon through a model (i.e., defining the units, laws, bounds, and so on) of the causal processes and relationships involved. Finally, they establish the viability and value of the proposed theory through empirical testing, idea diffusion, and practical application” (Arend et al. 2015, p. 634). For each of the 3Es, there are multiple criteria upon which to evaluate a candidate theory; for example, for the second E of *explaining*, the assessment criteria include all of Dubin's (1969) set of requirements for constituting a valid theoretical model.

We primarily consider how well the school meets the *explaining* set of criteria in that framework, which assesses whether the school properly addresses the *why* questions about the functioning of the focal phenomenon. We then consider the main *establishing* criteria in the framework to assess the testing of the school. We complete the assessment by analyzing the *experiencing* criteria outlined in the framework; we do so to determine upon what grounds the creation school was established, as that naturally leads into the question of the novelty of its ideas¹. Table 1 summarizes the assessment, noting for each criterion both the ways it is and is not satisfied by the school, in addition to suggestions for addressing

the main gaps. (The assessment is presented to be as factual as possible, e.g., if formal propositions or hypotheses or process flow diagrams are not provided in the foundational papers (or later), then they are factually missing; it is not opinion. The overall assessment—weighing of the satisfaction of the criteria—involves more judgment, but it is a judgment based on the facts.)

Table 1. 3E framework-based evaluation of creation school theorizing.

Stage	Criteria	Assessment
Experience	Built upon the existing literature	- It is missing many important references upon which the core ideas are built (e.g., the ontology; the epistemology; and the creativity concept in entrepreneurship), reducing its novelty. - The three core papers contain 215 unique cites (with only 17 self-cites) indicating some significant drawing on of the then-existing literature, but with the caveat that many of these refer to the discovery school's processes rather than its own. - It would be useful to acknowledge past work more, and to explain the differentiation, if any.
	Built on valid observation	- It does not appear to build on any specific (scientific) observation. There is no lab, no study, no data cited. Instead, it seems like it leverages stylized facts about how products (and their ventures) newly emerged where none existed prior (e.g., <i>Airbnb? Twitter? Google?</i>). - Regardless, there is an audience that feels like such an active generation of opportunities has and does occur and deserves the kind of attention that this school provides. - It would be useful to illustrate the phenomenon with more case studies to illustrate and prove the process (relative to alternative explanations).
Explain	Units: - Comprehensive - Parsimonious	- Few units are provided, and even those seem under-defined (e.g., the "Market"). Some seem to exist because they are attached to primary units (e.g., the "Beliefs" held by the "Entrepreneur"). Several possible units are missing (e.g., rivals, partners, spillover knowledge, the updated state of uncertainty, and so on). Opportunity exploitation—as a separate but dependent process—is under-developed (including its unique units). - A few units are defined (e.g., the "Entrepreneur" as any entity). There is little redundancy and so parsimony is good. - It would be useful to offer additional details about all relevant units. Adding units would help to flesh out the theorized story, especially involving how competition works (i.e., how others acting in opposition to the influence of the entrepreneur on the market affect the process).
	Clear laws (about unit interaction)	- Laws are formally absent. There are mentions of some general feedback learning mechanism; the co-evolution of the entrepreneur and market; some ideas about diversity of the knowledge base of the entrepreneur; and a linear feedback loop. Specific mechanisms of learning (Bayes?) are absent, as are specific selection drivers of evolution and any specific mechanisms that convert social pressures and experiments into a market imperfection. There is nothing about functional forms, time delays (discounting), or possible interactions with rivals, deception, or noise in the feedback process. - There are some relationships implied (rather than formalized as laws), including the fact that updated affordable loss calculations can lead to a "no go" action (and the end of the process); that the generation of a new market imperfection ends this process and initiates the next (exploitation) one; and that the process involves path-dependency—creating a possible source of (sustained) competitive advantage to the entrepreneur for exploiting the opportunity. - It would be very useful to make all of the fundamental laws of the school explicit so they can be used to write propositions and testable hypotheses. Sub-processes need to be better defined (e.g., learning). Loose descriptions do not qualify as theory; the relationships among units—whether process-oriented (e.g., based on steps-sequence logic) or variance-oriented (e.g., based on cause-and-effect), from system state to state, involving possible inputs to decisions—need to be provided.

Table 1. Cont.

Stage	Criteria	Assessment
Explain	Boundaries specified: precise rules	• Greatest theorizing clarity in this criterion—with clarity provided in comparison to the discovery school on items like search, expected value calculation, initial uncertainty level, source of market imperfection, and <i>ex ante</i> entrepreneur differences. • Regardless, some under-definition over some of those comparison terms (e.g., search—via inquisition or experimentation?). Also, many boundaries are impractical (i.e., based on epistemology and ontology or not <i>ex post</i> identifiable). Furthermore, the process itself is not “clean” to set boundaries from, given some “parts” of any opportunity will not be “new-to-the-world” and not subject to Knightian uncertainty. • Although it may severely limit this school’s application, there is a need to exemplify and bound more tightly to the process to the parts (rather than the whole) of the opportunity that is “created” rather than “discovered”. There is also a need to list more contingencies (e.g., over the feedback signals that the market provides being true, over the timing being right, and so on) given many of the implied <i>ceteris paribus</i> conditions for this process are not likely to be (i.e., given the stated co-evolution/ dynamics/ instabilities involved).
	System states exist	• States lack practical precision: although easy to state, identifying when the entrepreneur obtains the belief that potentially spurs action is not straightforward nor is it when a relevant market imperfection occurs (given markets are continually imperfect). The co-evolution process is ongoing, so also presents challenges for intermediate states (e.g., when the market has completed a cycle of reaction feedback to the entrepreneur). States for opportunity exploitation are missing—which is problematic for any full model of the entrepreneurship value-creation process. • At least two states explicitly defined (i.e., the start and the stop-as-success). Others are implicit (e.g., the stop of action when an excessive expected loss is calculated, the stop-as-failure, the action taken by the entrepreneur, the reaction provided by the market, and the updating of their beliefs). • It would be useful to add precision to the system states (especially at a practical level), and to more formally identify any intermediate states that would help in analyzing the system and acting upon the system while that process is actually occurring (versus only at the start or end).
	Propositions consistent with model	• No formal propositions provided. Makes the theorizing untestable/ unfalsifiable. With no reasons provided for this choice. • Several statements made that hint at possible propositions (e.g., about the preferred characteristics of the entrepreneurial team being diversity of knowledge and social environment understanding (but even these appear to contradict other statements about <i>ex ante</i> differences over characteristics being unimportant as a contrast to the discovery school); and, about a basis for competitive advantage in the opportunity exploitation stage based on the path-dependent experience of the entrepreneur). • Without propositions, this is not theorizing in a strict scientific sense; it is an incomplete description of one possible way some specific opportunities arise (i.e., those products and services that seemingly emerge from nowhere to an outsider). It is important to translate the ideas and the co-evolutionary process involved into specifics that are testable, consistent, and lead to different conclusions than existing models.
	Reasonable assumptions	• Many assumptions, including those that should be important to the process, are missing (formally). These include ones that underlie the learning logics used by the entrepreneur and the market; the action and reaction forms and filters; the competition; the experimentation formulated; the ability to calculate affordable loss; and the emergence of the supply chain supporting the opportunity. • Several assumptions are provided explicitly, including those involving the epistemology; the uncertainty; belief heterogeneity; and what a created opportunity embodies. • It would be useful to add further assumptions to supplement the model. (This suggestion would then also help clarify laws and system states, and form propositions.) An implicit background assumption cannot be <i>ceteris paribus</i> here because most parts of the process continue to change (through co-evolution) rather than staying the same (or otherwise equal).

Table 1. Cont.

Stage	Criteria	Assessment
Explain	Logic: • Causality explicit • No tautologies • Coherent	• Due to deficiencies in the preceding “Explain” criteria, the logic is also lacking in many places: Causality is not often explicit (e.g., in terms of how the entrepreneur influences the market sufficiently when others are also trying to do so; in terms of how that influence leads to a market imperfection rather than simply to a market adjustment; in terms of how the actions reduce the unknowables of Knightian uncertainty into knowables; in terms of timing in the casual chain; and in terms of what causes initial action). The opportunity is defined in a tautological way, and in a way that has no practical use. Logical coherence is questionable—the entrepreneur acts both rationally (i.e., with the ability to calculate affordable losses for possible actions, and with the ability to conduct the right experiments within budget and time constraints) and irrationally (i.e., by stating the process despite being confronted by Knightian uncertainty). • The logic nicely follows an experiment-based active-learning cycle that meshes with some evolution-based ideas (e.g., about selection based on existing social constructs and objective realities) in a way that is comforting. • It would be better to avoid the tautology, to be consistent in the rationality levels involved, and to identify and explain in more detail the causes and effects and the correct sequencing and underlying updating flows (e.g., detailing what factors are involved, or providing a functional form).
	Empirically testable	• The theorizing is difficult if not impossible to properly test; this is because there are no formal propositions, there is no process diagram (with specifics of stages, factors to measure), and there are no formalized laws of interaction. Many boundaries are impossible to determine <i>ex post</i>. • The school (in the core three papers) calls for more testing. Some testing has been attempted since. (It has been indirect and <i>ex post</i>, which is problematic for a school that states its process cannot be proven to have caused an outcome rather than a different process.) • It would be useful to clearly establish testable hypotheses and have non-affiliated scholars test them in real-time cases (where the <i>ex post</i> identification problem is avoided).
Establish	Diffused in the literature	• The school has been successful in its diffusion in the relevant literature. • The way the school has been cited may be an issue—with references made more to the idea of creative entrepreneurship rather than to specific elements of the process modeled. This may mask the influence of the theorizing-as-explanatory-model versus the influence of being a ready contrast to speaking about opportunity generation arising from search (or versus the influence of referring to entrepreneurship as making decisions under uncertainties). • It would be useful to assess the citation sources (e.g., their independence) and quality (i.e., regarding whether it is the model or the contrastive “philosophy” being referenced) to better understand the impact of the school and where it should aim to clarify or improve its message.
	Practitioner value: • Understandable • Nonobvious • Implementable	• No formal prescriptions appear, so there is little to apply in the field. The language of the modeling is at too high a level of dialogue (i.e., involving ontologies and epistemologies) for understanding at a practical level. Nothing modeled appears non-obvious here; in fact, the appeal seems to come from describing observed behaviors of fast-and-frugal experimentation and adjustment rather than suggesting anything new to the process. • The core experimentation cycle is an understandable process that contrasts with alternative processes like standard information search or environmental monitoring-with-alertness. • It would be valuable for the school to provide more practical prescriptions and modeling of the opportunity exploitation stage based on a successful creation stage where the entrepreneur attempts to leverage their unique, path-dependent experience.

2.3. Assessing the Creation School across the Explaining Criteria for Theory

We first consider how well the creation school meets the criteria involving the provision of formal explanations for the workings of the focal phenomenon. The core of theory-

building is explanation (Gioia and Pitre 1990); without it there simply is no theory (Sutton and Staw 1995). The bulk of the criteria for the second “E” of *explaining* come from Dubin’s (1969) five-phase assessment—a set of requirements that has been applied to theorizing, including in entrepreneurship (Ardichvili et al. 2003).

Units are the things from which theories are built; it is only when units are assembled into models of the observed world are theories recognized (Dubin 1969). Furthermore, the model can be judged on whether it contains all of the right units and only them (Whetten 1989). For the creation school, the criterion for formalizing *theoretical units* is only partially met, with only three primary ones significantly considered—the entrepreneur–manager (EM henceforth), the opportunity, and the market. The EM is an actor seeking to create and exploit an opportunity². The opportunity is defined in terms of its necessary condition for existence, i.e., it embodies a new market imperfection. The market is acted upon by the EM in order to provide reactions—from which either the opportunity, or further information that can be used by the EM, emerges³. There are also secondary units considered—these being attached to two of the primary units as their main relevant characteristics. For the EM, these are its updatable beliefs and its changing resources and capabilities. For the market, these are its updatable beliefs (i.e., its state of social construction constrained by objectively real natural laws) and its changing status (e.g., involving its composition, resources, and state of economic perfection).

Important units that require more formalization include the action(s) taken by the EM; the reaction(s) of the market forces; any factors that capture the EM’s learning; the signals communicated (and accompanying issues like noise and filtering); the time lapse; the value of time; rivals; information (and its spillover); and the nature of the venture itself. Although the creation school is defined in terms of opportunity formation *and* exploitation (Alvarez and Barney 2007, p. 22), it is exclusively described in terms of the formation; consequently, the units involved in exploitation are also missing their formalization (e.g., regarding the complementary resources required—see Teece 1986). The lack of unit formalization is significant because, without a precise description of all of the main factors, the theorized relationships cannot be properly explained, measured, tested, or applied.

A further issue at the unit level concerns the *level of analysis* being applied. While the creation school focuses on the formation of an opportunity, it occurs through the actions of the EM, where the end result is only defined at the market level (Garud et al. 2014). Depicting a multi-level (i.e., individual, organizational, and market) analysis *without* a clear translation among those levels makes it difficult to understand how the explanations work across the units (e.g., how does an EM affect similar-level entities in the market unit to generate a response at that collective higher-level unit—is it based on “swarm” mechanics, or adoption models, or something else?).

A theory’s laws of interaction details how the units are linked (Dubin 1969). A systematic linkage is specified to account for how one unit varies (or follows in sequence) with at least one other, where characteristics like directionality, magnitude, and linearity indicate more valued laws. For the creation school, the criterion for formalizing the *laws of unit interaction* is not met. In its place is a loose process-like description that does not specify the laws of the co-evolution involved in the EM’s cycle with the market—a cycle composed of acting, then of waiting to receive feedback, then of adjusting and acting again. That description consists of simple linearly sequenced steps, but there are no laws governing how the units interact; there are no laws that make explicit how the directions, patterns, limits, and driving forces of each interaction work. As a result, unanswered questions arise over how the timing of the steps affects the outcome, how the size of the actions affects the market, and whether Bayesian updating is appropriate when the EM’s initial priors stem from ignorance (Edelman and Yli-Renko 2010). The lack of laws of unit interaction is significant because such laws form the basis for the focal relationships that are supposed to explain how the phenomenon evolves within its context; without these laws, knowing what to test of the theory and how to apply it are each made much more difficult. Essentially, without these laws, there is just description. Indeed, without a solid

model of the co-evolution process involved, there is no logical reason to expect that the process can lead to attractive outcomes like the formation of a new and feasibly exploitable market imperfection (*aka* an opportunity).

Boundaries define the specific domain modeled by the theory, delineating what it explains from what it does not (Ardichvili et al. 2003; Dubin 1969). For the creation school, this is the criterion where most of the writing focused—using the discovery school to mark the differences in opportunity generation processes that one school covers that the other does not. Thus, the creation process does not apply where pure search is effective, where expected values can be computed, or where market failures arise from exogenous shocks. Regardless, the criterion for formalizing the *boundaries* is not fully met because several of those relevant terms are under-defined, like *search*, *exogenous*, and *shock*⁴; also under-defined is the transition point between when expected values are un-computable and when they are. This failure to fully set the bounds for where the proposed theory applies (establishing the limit of the phenomenological system it seeks to explain) is significant. That is because, if there are differences in “what works better” under creation versus discovery processes, but there is no way to tell which should apply because of poorly defined boundaries, then following either school’s prescriptions could be harmful. Even if the bounds were theoretically clearer, it seems impractical to expect an actual EM to be able to appreciate the subtleties currently involved in the creation school’s definition (i.e., to differentiate between an endogenous and exogenous-but-relevant shock, or between a search-of-sources and a search-by-experiment approach, or between a risky and an ambiguous decision). And that is strange for a school that sells itself as being more true-to-life than its contrast.

A system state occurs when all of the model’s units have determinant, measurable values at one time, and the collection of those values can be captured as a possible holistic condition of the model—one of a finite set (Dubin 1969). For the creation school, the criterion to formalize *system states* is largely ignored. Described are two states—an initial state where the EM faces Knight (1921) uncertainty with a heterogeneous belief, and an outcome state where either the opportunity is formed or it is not. Intermediate and post-formation states are not described with any precision. That poses a substantial problem if this school is to be considered process-type theorizing. This is because, when the process stages cannot be properly identified, then there are no delineated times to take system measures or to pull levers, to make decisions, to assess uncertainty levels, to calibrate affordable losses, to engage partners, or to consider new actions; and that makes proper testing and practical application of the theory very difficult. Indeed, failing to meet this criterion is serious because, without identifiable system state points—especially intermediate ones—it is difficult to control or even predict the system’s behavior, let alone begin to understand any temporal issues in the relationships being explained. Without the stages or states defined, it is just not feasible to understand the feedback loops, learning mechanisms, and other interactions of what is proposed as a co-evolutionary activity among social actors.

A proposition is “a truth statement about a model that is fully specified in its units, laws of interaction, boundary, and system states” (Dubin 1969, p. 166). Propositions provide the basis for testing the model to validate its practical use (Eisenhardt 1989). For the creation school, the criterion to formalize the *propositions* that arise from the model is ignored. While there are several statements that read like implications of their narrative (e.g., teams with more diverse knowledge will have a higher likelihood of success in the opportunity creation process—see Alvarez et al. 2013, p. 309), there are no propositions argued or explicitly stated in their three papers. That is a significant failure because, without propositions, their theorizing is difficult to accurately test. And without such deeper empirical validation, it is difficult to sell the subsequent prescriptions as worth implementing or the underlying logic as worth building upon.

Theorizing builds up from self-evident facts—assumptions—and these should be explicit, specific, and credible (e.g., Bacharach 1989; Whetten 1989). For the creation school, the criterion to formalize the core *assumptions* and to argue their reasonableness is partially met. There are clear statements concerning the EM’s initial heterogeneity

(e.g., of the EM's beliefs); the kind of uncertainty at the initiation of the process (i.e., as unknown future states and their unknown probabilities); the main characteristics of the opportunity formation outcome (i.e., as the generation of a new market failure with computable exploitation-relevant expected values); and the pertinent epistemology (i.e., as evolutionary realism). No further formal statements of the premises that underlie the main processes and the relevant relationships involved are provided. Instead, the three papers are inconsistent over what exactly is unknown (e.g., is it demand, regulation, or something else?), for how long, and by whom, as well as over how that is reduced through experimentation, how quickly, and at what cost. While powerful statements are made about "creation *ex nihilo*" (which is impossible according to the laws of physics and logic), market-making, endogeneity, unique path-dependency, and so on, no such statements enjoy any proper supporting premises. There are no assumptions stated to justify the origins of the materials from which the EM creates actions that end in opportunities, to explain how a new and efficient supply chain suddenly appears to support the EM, to explain how the EM's legitimacy is generated (e.g., Aldrich and Fiol 1994), to describe what the co-evolutionary interdependencies are, or to explain what creates uniqueness versus conformity in such a process. This partial failure in explicitly stating premises and in arguing their reasonableness is significant because, without credible foundations, no path to theoretical conclusions through deductive reasoning is possible; that leaves *only* a path through induction—which is a logical reasoning method upon which the creation school claims no basis.

Logic is what builds a theory up from its premises to describe the rules of how model units interact within boundaries to move the system from one state to another; it needs to be explicit, non-tautological, and coherent (e.g., Priem and Butler 2001; Suddaby 2010; Whetten 1989). Meeting this multi-characteristic criterion for the sufficient formalism of the theorizing's logic is a concern for the creation school (but a concern which is to be expected given the school's failure to meet many of the preceding criteria). First, *causality* is not often explicit, which is a result of not properly formalizing the units, laws, boundaries, and propositions. Besides the school's general implication that "entrepreneurial action" triggers all else, every other causal detail is unclear—particularly regarding what those actions are, what makes them so effective in reducing uncertainty (e.g., how do they elicit what must be unambiguous feedback from only those parties that can only help the EM at the right time?), and how the EM has access to the resources to take those actions at those times. Even the big question of what exactly causes that specific EM to act remains unanswered. There is no origin for the heterogeneous belief that sparks the initial decision to act, and there are no heuristics provided for determining when estimated gains outweigh the EM's evolving affordable losses. Second, the main unit is depicted as a logical *tautology*—with opportunity defined by market imperfection, essentially arguing that "possible profits exist only when possible profits exist". The definition is also problematic in practice, because market imperfections *always* exist in reality (see Venkataraman 1997), and so the school's use of the term opportunity cannot be helpful in delineating a set of special conditions for triggering EM action. Third, the *coherence* of the logic is often questionable. For example, how can the EM always know the affordable losses but cannot estimate the possible gains (even roughly)? Or how can the EM learn from the repeated experiments with the market without raising the attention of rivals who have relationships with the same customers, suppliers, distributors, and regulators? Or how can the EM be boundedly rational (e.g., applying an affordable loss heuristic), yet super-rational (e.g., by being able to set up actionable, feasible experiments to efficiently reduce uncertainties without spillovers), yet irrational (e.g., in deciding to pursue the creation of an opportunity under complete ignorance of the possible outcomes and their probabilities)? The failure to meet this logic criterion is significant because, without knowing the main cause–effect relationships or the necessary process sequencing involved, without non-self-referential definitions of key terms, and without consistency in the narrative, the phenomenon cannot be explained in a clear, or new, manner⁵.

2.4. Assessing the Creation School across the Establishing Criteria for Theory

We now consider how well the creation school meets the main criteria for establishing its presence; specifically, relating to how well it has provided pathways for applying its offered explanations in the real world.

Empirical testability is one pathway for a proposed theory to establish itself; the theory should be falsifiable with clearly defined boundaries and units, and well-written propositions (e.g., [Bacharach 1989](#); [Boxenbaum and Rouleau 2011](#); [McKelvey 1997](#)). For the creation school, the criterion for sufficient *empirical testability* is not met. Due to the fact that the formalization of units is incomplete, the formalization of laws is absent and the formal boundaries are mainly implied, there are no foundational propositions from which to accurately form testable hypotheses nor any solid bases for measuring the model units and determining the suitable contexts for investigating their relationships⁶. And the little empirical testing thus far attempted has been, at best, indirect in its support⁷. To their credit, the school's authors have called for more testing (e.g., [Alvarez et al. 2017](#)). That call, however, comes with a caveat, because the creation school originally sought to justify its untestableness. One argument for its untestability relied on a Heisenberg-like uncertainty principle where any empirical observation would affect the opportunity's type, possibly shifting it from a creation to a discovery type, and so making identification suspect ([Alvarez and Barney 2010](#), p. 570). Another argument relied on the assertion that any opportunity identified *ex post* could be explained as being the result of either a discovery or a creation process, giving the theory no empirical content ([Edelman and Yli-Renko 2010](#)). Such arguments make recall-reliant case studies and standard backwards-looking empirical analyses unreliable. Regardless of the reasons for difficulties in testing, the resulting failure to fully meet this testability criterion is not just inconvenient, it is significant. This is because it shields the theorizing from empirical falsification, which remains a standard validation procedure. It also reduces the value of associated conceptualizations and relational modifications (e.g., moderators) because the untestability arguments provided raise concerns over building upon the theory through the use of standard induction techniques.

Another path for establishing presence is through appealing to practice ([Thomas and Tymon 1982](#)); but, to do that, the theory needs to be understandable to managers, non-obvious, and implementable (e.g., [Bacharach 1989](#); [Suddaby 2010](#)). For the creation school, the criterion of *practitioner value* is also not met in a sufficient manner. There are some minor descriptions (e.g., in Table 2 in [Alvarez and Barney 2010](#)) and a small set of "are more likely" statements about creation-friendly characteristics ([Alvarez et al. 2013](#), pp. 309–12), but these are insufficient to seriously guide practice. Furthermore, it appears that the level of discourse in the papers is squarely aimed above that of the practitioner, making the school's theoretical concepts and processes difficult for them to understand and apply. Real EMs do not care about epistemology, or about distinguishing among types of uncertainty that they may or may not perceive correctly, or about delineating whether a shock came from inside or outside what they perceive as their market; nor do most appreciate what the formation of a new market failure means (given such failures continuously exist in reality—see [Venkataraman 1997](#)).

To be valuable to the practitioner, the school must detail *non-obvious* and novel actions. However, real EMs seem to be doing much of what is described in the school already, whether they be expert EMs (e.g., [Sarasvathy 2001](#)) or successful EMs depicted in relevant case studies (e.g., [Vaghely and Julien 2010](#)). Real EMs do conduct experiments in the market, do adjust their approaches to feedback, do apply influence tactics, and do try to minimize losses, but all of those behaviors were well known *prior* to the creation school (and often based on observation of expert EMs). While real EMs behave in ways that appear to implement some of the *basics* of what the school advocates (e.g., being consistent with the informal descriptions of what creation teams are more prone to look like), the implementability of several *specifics* are harder to assess simply because these are not detailed in the theorizing. This failure to meet the practitioner value criterion is significant

because, without it, the only value it can provide is to academia. Unfortunately, that has been put into question by the school's failures to fulfill the *explaining* criteria already⁸.

The third path to establishing presence is through academic means—being cited in the relevant research literature (for the right reasons). For the creation school, the criterion of *diffused in the literature* is seemingly met in a sufficient manner (i.e., with over 4000 citations across the three foundational papers in Google scholar in mid-2022). The three foundational papers are well-referenced in entrepreneurship research, especially involving discussions over opportunities. That said, “what the school is cited for” is more often the over-arching (and pre-existing) notion that entrepreneurs actively create new things (e.g., by influencing social construction) or do so under Knightian uncertainty, and less often for the actual model itself (e.g., for its propositions, its process unit, or its system states). Therefore, the question remains of whether being cited more for the input (i.e., the assumed level of uncertainty) and output (i.e., an observed opportunity that the entrepreneur takes the most credit for in shaping) of a model, rather than the explanation of the phenomenon, indicates that a theory has had a significant academic impact (or not).

2.5. Assessing the Creation School across the Experiencing Criteria for Theory

In the last assessment from the 3E framework, we consider how well the creation school meets the main criteria that support the need for a *new* theory; specifically, the need for a new theory to explain the focal phenomenon of opportunity generation and exploitation.

One path to supporting the need for a new theory—an inductive path—is to build on valid observations of the phenomenon that do not seem to be explained by existing models⁹. For the creation school, the criterion for *building on valid observation* is only met in the most general terms. The creation school is *not* built on induction; clearly, there are no foundations for it claimed in the then-recent empirical work of any kind. Instead, it appears to be built on stylized facts (e.g., the creation of blue oceans by strategic EMs—Kim and Mauborgne 2004) and descriptive analogies (e.g., of mountain-building versus mountain-climbing). The school grounds itself in the existence of general enactment processes (e.g., Aldrich and Ruef 2006) and the general active learning cycles documented by others (e.g., Choi 1993; Huber 1991; Rindova and Kotha 2001). Specific firm examples are not used. Clearly, the impetus for this school was not based on “new and surprising” empirical observations. Of course, failing to meet this induction-oriented criterion is not fatal, but it does imply that the impetus for the new theorizing must have emerged from newly discovered gaps or inconsistencies in the then-current literature; that implication leads to the assessment of the second experiencing criterion.

The deductive path to supporting the need for a new theory is based on an understanding of what theories already exist relevant to the focal phenomenon, in order to recognize, compare to, and build on them (Bacharach 1989; Suddaby 2010). For the creation school, the criterion to *build on the existing literature* is partially met. The creation school clearly does build on the relevant then-existing research. Across the three establishing papers, there are 215 unique cites (only 17 of which are self-cites). However, there are two issues that blunt this determination. The first is that a significant fraction of the citations exist to describe and explain the discovery school and are provided so that the creation school can more effectively detail a contrast to that. The second is, despite the high number, there are many relevant references missing. While some oversight of previous research is inevitable, there are some omissions that put into question the claims over the originality and validity of some of the school's content. For example, regarding the use of an evolutionary epistemology, the works of Quine (1969), Baldwin (1906), and Holland and O'Hear (1984) are ignored. Bradie's (1990) work is uncited although it describes the metaphorical basis for applying biology-based processes to explain the evolution of ideas, which is what the creation school does. The original *meme* concept (Dawkins 1976; Huxley 1880) is also not referenced, but is clearly relevant to the selling of ideas, including the commercializable kind (i.e., opportunities)¹⁰. Perhaps it would be more transparent to admit that the impetus

for the creation school was a basic objection to the unfounded neoclassical general equilibrium theory's assumptions about the knowability of the future, and that a more realistic alternative describes many markets as being buffeted by an evolving process that is continuously originated by changes in human choices and actions that emerge only within the process itself (Littlechild 1979). The creation school could also have been more open about its adherence to the subjectivist epistemology, given Wiseman (1989, p. 230)—who they do not cite—seems to provide the basis for its ontological perspective with comments like “the future is not simply ‘unknown,’ but is ‘nonexistent’ or ‘indeterminate’ at the point of decision”, but is then created in the process by human choice and action made in the expectation of benefits. It is wasteful to not have built on such research.

In summary, considering the factual assessment over full range of the 3E framework's criteria, it is our opinion that the creation school is simply *not* yet a theory. This is because it fails to adequately address most of the requirements for proper theorizing, despite its attractive narrative.

2.6. Assessing the Novelty of the Creation School's Ideas

With the more-structured analysis complete, we can now drill down on the creation school regarding its originality. This assessment goes beyond the above question of how well it acknowledged and complemented past work; we now assess how much of the relevant work was *not* acknowledged.

The idea that the discovery school was flawed and needed to be challenged was not new at the time (see Davidsson 2003; Fletcher 2006; and Sarasvathy 2001). The core idea that some opportunities are created rather than discovered was not new (see Ardichvili et al. 2003; Knight 1921; Penrose 1959; and Weick 1979). Nor was the detail that EMs do so with innovation (e.g., Schumpeter 1934, 1939) and, thus, create the future (e.g., Kirzner 1985). The focus on opportunities was not new (see Shane and Venkataraman 2000; and Venkataraman 1997, with perspective by Arend 2014). Nor was the focus on entrepreneurial action (see Bird 1988; Hmieleski and Corbett 2003; McMullen and Shepherd 2006; and Santos and Eisenhardt 2004). The idea that some artifacts are socially constructed, including perceived market imperfections, was not new; it is the essence of social constructivism (which has been around since the ancient Greeks, like Protagoras). Complementing that perspective, the idea that not all items people value are objectively real was not new (see Branthwaite 1975; Hoorens et al. 1999; Raghubir 2006; and Sweezy 1934). And so, the idea that EMs actively engage in *social engineering* to push their agendas (i.e., to obtain private value) was also not new (see Bijker 1987).

The idea that entrepreneurship can arise from non-risk uncertainty was not new (see Knight 1921). The idea that humans learn through an action–feedback–adjustment loop was not new (see Argyris 1976; Butler and Winne 1995; and Ridderinkhof et al. 2004). The idea that better ideas, including commercial ones, are selected in an evolutionary manner, was not new (see Dawkins 1976). The idea that initial unknowns can become knowns through experimental interactions with the environment was not new (see Lindley 1956; Nordhaus and Popp 1997; and Smith 1962). The idea that path dependencies can exaggerate initial differences was not new (e.g., see chaos theory—Gleick 1987). The idea that human action creates shocks to focal and secondary markets that are exploitable was not new (see Clark 2003; McGrath and MacMillan 2000; and Zahra et al. 2006). The idea that markets are co-evolutionary, dynamic, disequilibrium systems was not new (see Buchanan and Vanberg 1991). The idea of affordable loss and other heuristics being used by EMs to make decisions rapidly in complex contexts was not new (see Kahneman et al. 1982; Sarasvathy 2001; and Tversky and Kahneman 1974). And the idea of social constructivism being a contrast to scientific realism was not new, nor was the application of evolutionary epistemology to knowledge objects new.

Thus, it is our opinion—one based on the facts outlined above—that the creation school is not new in terms of its main ideas. This list is only partial, but it does beg the question about what exactly in the creation school was and is new, especially if it is not

the core ideas. Furthermore, there are those who believe that the creation school was not an appropriate challenger to the discovery school, arguing that it did not advance a rival perspective, but instead rested upon questionable linguistic practices (Ramoglou and Tsang 2017, p. 742). One way to address the question of its value is to move from our now-completed analysis of *what it is not* onto an analysis of what the creation school is, or at least what it represents, to entrepreneurship and management theorizing, and then also to theory-building more generally.

2.7. Assessing the Questions Raised by the Creation School for Entrepreneurship and Strategy

Although not yet a theory, the creation school does provide a very useful basis for questions about theoretical issues in *entrepreneurship*, *strategy*, and more.

At its most fine-grained level, the school sketches out one possible coherent path to entrepreneurial success by a managerial team. Pulling together the descriptions over the school's primary three papers, this path may be identifiable by the characteristics of the team members (i.e., they have diverse technical knowledge, and deep knowledge of the social environment and of experimental learning); the organizational form (i.e., a clan, commitment-based, organically grown); and the approach to building barriers (i.e., that are less reliant on simple items like brand and more reliant on more complex ones like causal ambiguity). This path provides a valuable critique and contrast to alternatives like pure information search (i.e., not involving active experimentation) and patience-with-alertness (Garud et al. 2014)¹¹. This path could even be testable *if* the additional assumption is made that synergies exist among those characteristics uniquely for that path. The interesting questions that such theorizing raise the concern of how many of these bundles of characteristics are viable, under which conditions involving competition and uncertainty, and at what costs.

Continuing at this level, one important way that the creation school could take the next steps for improvement would be in better acknowledging, including, and explicitly building on the relevant past research. Research espousing the school's core idea that opportunities are created rather than discovered, through effort and luck (e.g., Chiles et al. 2007; Denrell et al. 2003; Hsieh et al. 2007) and Schumpeterian innovation (e.g., Noteboom 2000), in a multi-stage process (e.g., Wood and McKinley (2010) is too often missing or under-attributed. That a creative process such as theirs occurs in a co-evolving context where the structuration theory is applicable (Chiasson and Saunders 2005; Sarason et al. 2006) is absent from the school's consideration. Also missing are several important and highly relevant papers that describe that process in terms of social construction (e.g., Krueger 2000, 2003), including Bijker's (1987, p. 183) detailed use of social constructivist analysis to understand technological artifacts as opportunities in an evolutionary approach with selection and stabilization of variants. The subjective judgment concept over information (Foss and Klein 2005; Foss et al. 2008; Knight 1921) is under-explored. The same lack of acknowledgement in the creation school of deep previous research is also noted for the cognitive and mental-modeling processes described in the managerial and entrepreneurship research (e.g., Casson 2000; Edelman and Yli-Renko 2010; Sarason et al. 2006) and the role of experiential learning, specifically in opportunity recognition (Corbett 2005).

At a more phenomenological level, the creation school raises questions about the semantics employed in our field's research. Much of the debate over what opportunities are and how they are recognized, formed, generated, or otherwise made to come into existence, appears to be about what one means by key words like *discover*, *search*, *create*, *exogenous*, *shock*, *uncertainty*, *knowledge*, and even *reality* (with these last two terms re-opening debates over epistemology and ontology). Unfortunately, some of the entrepreneurship and management theorizing can be imprecise in their attempts to define these terms. As such, *school wars* are often based on misunderstandings or misinterpretations (e.g., Ramoglou and Tsang 2017), and so add little to the progress or synthesis in our fields. Take the example of how the creation school defines *opportunity*—it opts for a tautological, conditional characterization. This raises the question about whether we should treat such difficult

core terms as a *black boxes* instead (Abell et al. 2008) in order to make progress. In the case of terms like opportunity, for example, perhaps they should be delineated by their effects—for opportunity, perhaps that should be by its effects on any relevant markets, measured either as generating instantaneous arbitrage between different parts of a focal market, or as inducing longer-term coordination of markets as the future unravels.

At the level of model-building, the creation school process description leads to several difficult questions that need to be addressed in our field. The first question involves *the origins of valuable heterogeneity*, where the answer could apply to both what causes the differentiation of an EM from a non-EM, and what causes the differentiation of a successful venture from an unsuccessful one. Although no resolution as to origins is provided by the school, a possible explanation for why small initial differences can grow to significant levels over time is offered. Of course, if the theorizing in entrepreneurship and strategic management is to be complete, then that origin story will need to be described beyond the currently recognized one of luck (Arend 2015). Furthermore, it will be important to then explain how a sufficiently complete collection of such differences (e.g., in initial beliefs and in the abilities to form opportunities, and then in the abilities to exploit and protect them) are gathered and are made to interact effectively as they are exploited.

A second set of questions that the creation school raises relates to *what the proper level of EM rationality should be, and how it can be captured in our models*. There is a discomfort with full rationality, and some consensus on bounded rationality, with several heuristics offered that have enjoyed some empirical support. But that still leaves open the issues over the selection of the heuristics—which would be a meta-heuristic—and over how those heuristics that are attributed to EMs are applied when doing so can require a level of information-gathering, interpretation, or calculation that assumes a greater working expertise than that assumed in the enveloping rule. For example, the heuristic of affordable loss could mean a simple calculation over what financial investments have been sunk into a project thus far, or it could entail a much more complicated estimation of the possible downsides of a project including its legal liabilities, loss of reputation, loss of network trust, opportunity costs, and so on. (And, if the former, why is such a simple estimation calculus not possible for what the feasible maximum gains could be—say, by adding the market values of the top three firms in the closest industry?) Getting to more realistic theorizing in entrepreneurship will require committing to, and then empirically verifying, an explicit level or strata of rationality, from which unique behaviors arise and are guided.

A third question that the creation school raises involves *how complete the modeling of a competitive contest over socially constructed alternative futures needs to be*. Short and general descriptions, peppered with references to winners of such contests, are insufficient because such competitions are very complex in their underlying mechanics. Consider just the core idea—that any party who wishes to can then actually influence any other party's subjective reality—which is an idea that is easy enough to model as a one-way relationship, and possibly even a sequential two-way one, but once simultaneity, rivals, and disinformation enter the picture, it is a model with extensive complications. Having multiple parties trying to influence each other's realities while trying to keep track of what they perceive as real or not, all with different goals and prior beliefs, and all trying to interpret different signals and update their beliefs, involves a great deal of record-keeping, let alone accurate calculation. It is likely that non-linearities, discontinuities, and unknowns would also arise in the analysis and make the outcomes unpredictable and, consequently, the optimal prescriptions incalculable. When combined with boundedly rational decision-makers—who by default assume that any simple rules they are applying at least have true inputs when that may not actually be the case—then the implications become even less clear. This modeling question is highly challenging to any proposed theory that does not greatly and precisely constrain how much social constructivism can matter in a competitive context (e.g., by restricting its influence to only specific products and services, like fashion or music, and to only specific decisions within them).

A fourth question that the creation school raises concerns *which of the big picture dimensions—space* (i.e., cutting across industries) and/ *or time* (i.e., cutting across temporal periods)—*needs to be considered in theorizing*. To the former, if the exogenous shocks to a discovery process in one market are generated by the endogenous shocks of a creation process in a different market, then can these processes be combined into a wider explanation of entrepreneurial activity in the economy (e.g., for policy purposes)? To the latter, if the creation process continuously generates new market failures—that are, by definition, harmful economic inefficiencies—then is it not also important to model the balancing processes (given our markets are arguably increasing in their efficiencies overall)? Where is there a grand accounting of the benefits of new products, services, and organizational forms against the costs of at least temporarily less-competitive markets, let alone the costs of those transitions (that tend to disproportionately burden the weakest in society) and of their unintended surprise consequences (which are also often negative and unfairly distributed)?

A fifth question that the creation school raises involves *which uncertainties need to be or are actually being dealt with in the opportunity generation process*. That question is posed to address, at any one point in time and across time, what is known and what is knowable by whom and why. In fact, the delineation of the creation school from the discovery school is based on an “uncertainty assumption”—that of the initial unknowability of the future states and their probabilities relevant to the EM’s opportunity exists under creation but not discovery. That contrast raises many issues, the main one being the reliance on one specific type of factor—the *expectedly knowable but initially unknown* type (i.e., as defined by the creation process’s initial unknowability and the conclusive knowability). That type of factor does not meet what Knight (1921) described as *sufficiently unknowable* to be uninsurable and, thus, a basis for *entrepreneurial* action. If what is initially unknown is (highly) expected to be knowable by a known method (e.g., experimentation), and a resource-poor EM can arrive to that knowability level in some reasonable period of time, then why would a better-resourced rival not get there (i.e., to the exploitable known opportunity) much faster, reducing the time and possibly the intermediate and low-stakes decisions that would have to be made under uninsurable conditions and, if that is the case, why would EMs need to exist? An acid test would involve proving that the transition between the initial unknownness and the subsequent knownness must entail additional characteristics—about the uncertainties involved—that are (i) profitable to an EM and (ii) unattractive to rivals. But the creation school does *not* address the conditions for passing this test (e.g., through first-mover advantage requirements), and so does not prove that the EM is necessary.

To be clear on this point, entrepreneurship-related theorizing needs to address the uncertainty-related issues because they are existential. Knight (1921) carved out a role for the EM based on non-risk uncertainty, and the creation school uses a version of such uncertainty as a primary delineation from the discovery school. The theorizing over what has been termed *Knightian uncertainty* (KU henceforth) and its relationship to entrepreneurial activity continues to the present. But the creation school has set a dangerous precedent for modeling KU as being convertible into non-KU. Knight (1921, p. 239) did *not* describe such a conversion; instead, KU was to be met and borne. However, in the creation school, KU can be methodically transformed into risk through experimentation and interaction with the market, as well as through social influence. As such, the outcome of such theorizing has caused considerable confusion over the question of *what is unknowable*¹².

In sum, the creation school is not theory nor are its core ideas new. However, it does deserve credit for raising important critiques of the discovery school, and for implicitly raising significant theoretical questions for the field.

In terms of improving the creation school, we have elaborated on two clear paths: first, address the gaps in its presentation that the 3E framework analysis has identified; second, address the set of five questions raised by it that we have started to explore. Some of this can be achieved with more specific modeling, and some of it can be achieved through empirical work that tests its (mostly implied) premises (e.g., regarding the heuristics and

meta-heuristics involved), process steps/sequencing (see Figure 1), and unit relationships (e.g., regarding the characteristics of teams better suited to being successful in that process).

The creation school can also be considered as a solid jumping-off point to continue the debate over our expectations for new theorizing, especially when the foundational theorizing in our field remains represented by a deduction-based, idealized benchmark model that has been “stretched too far”. We characterize the approach exploited by the creation school below—an approach that was used to challenge the benchmark discovery school—in order to move that debate forward now.

3. Comparison-Based Theorizing

What is the best way to evolve our theorizing in entrepreneurship? Given what we know from Kuhn (1962) and others, we as theorizers, reviewers, editors, teachers, and readers, have ways to be active in whether or not that evolution continues along the punctuated, episodic path observed in other scientific fields. We do have the knowledge to more actively shape a new path toward accepting better (e.g., more logical, more empirically supported) theories faster, and abandoning poorer theories more rapidly and easily, even if that means admitting ignorance about some phenomena rather than continuing the use of bad theories.

In the entrepreneurship field, a *comparison-based* approach to theorizing has been effective in making an impact (e.g., Sarasvathy’s (2001) effectuation), regardless of the debated quality of the theory, logic, or process model that was produced. Here, we use the creation school—as a more recent example of that approach—to analyze whether it provides an acceptable path for shaping any field’s theorizing. What we term the comparison-based approach draws from both the *problematization* approach (Alvesson and Sandberg 2011; Suddaby et al. 2011) and the *contrastive* approach (Tsang and Ellsaesser 2011), but it does *not* fully adhere to either.

The creation school defines itself through an explicit comparison to the discovery school, or at least its version of what was then a largely deductive and partial theory explaining opportunity generation and exploitation. The creation school differentiates itself from the discovery school via epistemology, as well as through the nature of opportunities, EMs, and decision-making context. That said, it also claims explicit similarities to discovery; with similarities over the goal of EMs, the definition of opportunities, and the social aspects of extra-individual realism. And regardless of valid concerns about the accuracy of such differentiation, or about whether their characterization of a discovery school is fair, the creation school does follow a major step in the problematization approach by identifying several debatable paradigm-level assumptions underlying its target. That said, it does not fully assess those assumptions, nor does it develop (e.g., through induction) its own alternative assumptions; instead, it only lays out the two sets of different assumptions, providing a limited historical context and a few implications. That is *not* problematization. The creation school does depict a contrasting explanation for the phenomenon of how opportunities come into existence, but it does not do that in the contrastive form. It does not address the question of “*why X rather than Y?*” (where X (the fact) and the negation of Y (the foil) are true statements—as in the question of *why is opportunism present in markets rather than in hierarchies?*—Tsang and Ellsaesser 2011, p. 412). Instead, many of the research questions they do pose already presume the existence of what they seek to explain, i.e., a creation process where markets can be shaped. Thus, the approach taken by the creation school is *not* contrastive either.

What the comparison-based approach appears to be then is an efficient means to introduce an alternative narrative when existing theorizing has led to a benchmark grounded in idealisms (e.g., near full rationality; nearly complete information to those alert enough to it; optimizable decisions over risky choices). The approach is efficient because it decreases the need for new and original explanations. It decreases that need by drawing on existing ideas, from the benchmark (e.g., opportunity generation is a process), as well as from alternative perspectives (e.g., social constructivism) and recent stylized facts (e.g., some

newer ventures appear to be heavily built upon social impressioning; EMs use heuristics in uncertain contexts to their advantage—Gigerenzer and Gaissmaier 2011). That saves on building theory from scratch and offers legitimacy by association. Besides being efficient, such a comparison approach can also be effective when it exploits the tendency of audiences to be more receptive to narratives that retain some of their existing ideas (e.g., about informational market failures) when introducing newer ones (e.g., about the endogeneity of the opportunity to the EM). One problem with such an approach, however, is that its effectiveness and efficiency both increase when the comparison target is made simpler and starker, i.e., when it becomes more like a strawman, but that leads to a reduction in the depth and quality of modeling because there are fewer relevant details to explain and to juxtapose against.

Unfortunately, the popularity of the comparison-based approach's strawman-contrast ploy is growing, even though it involves an explicit crutch that past theory-building did not rely upon¹³. Worse is that such strawmanning is not really needed. For example, other relatively recent comparison-based theorizing, like the relational view of the firm (Dyer and Singh 1998), explicitly leverages the structure of the resource-based view's logic but does so in a complementary, non-strawman way. Regardless, strawman comparisons are popular, though, for several reasons: it is easier to do than arguing stand-alone new theory; it gives a ready-made research question and structure to the theorizing; it exposes real gaps to address; and it lends legitimacy through association. It also allows for a less-specified level of theorizing to exist; doing so by focusing on statements about *what it is not* (i.e., the benchmark) rather than *what it is*. At its worst, its strawmanning ignores the more subtle and stronger version(s) of the benchmark theory that it should be compared to and robs the audience of a fair choice over the options for explaining the focal phenomenon.

But why are the benchmark theories such easy targets for comparison-based approaches to attack? Idealizations tend to precede more realistic models in the progressions of many fields' theories. That initial use of ideal models, ironically, occurs for similar reasons that afflict recent examples of the comparison-based approach—because the modeling is easier to understand and simpler to analyze while still providing a viable story that is consistent with at least some big patterns observed for the focal phenomenon. If that idealization survives initial scrutiny (e.g., because it appears intuitively logical and based on transparent premises), then it becomes a benchmark for the field. However, the power of its generalizability comes at the expense of realism. For example, significant realism is sacrificed when one assumes an objectively real ontology, agent super-rationality, exogenously given initial conditions, non-focal homogeneity (aka the *ceteris paribus* contextualization), and the availability of sufficiently complete information to make decisions, regardless of whether doing so saves a significant amount of formal modeling. It saves on specifying the differences in abilities, information, resources, and networks that increase the complexities of interactions and processes. It saves on specifying the learning processes, and on specifying the accounting of the costs and time of information-gathering, processing, and calculation. It saves on specifying the possible mistakes along the way. It saves on any modeling of the origins of conditions, where such origins often involve messy and regressive dynamics. And it saves on the modeling of the many complexities of interactions among processes and across non-focal variables.

That sacrifice of realism, however, has a significant upside—it makes any formal model constructed highly meaningful, because the consequent simple, linear algebra of that model clearly leads to the identification of better or best options for affecting the phenomenon in a particular direction. All that simplification also means that a benchmark theory can often be presented in a single paper, and in a form that a sufficiently wide audience can easily understand, with intuition satisfied by relating the model's main (linearized) relationships to established relationships in other fields, and with specific prescriptions that are aligned with a few select, famous examples.

It is not just the fact that these idealized benchmarks are often built on multiple unrealistic premises that has made them easy targets for critique and comparison, it is also

that they are very often over-extended by follow-on research. While some extensions are in the form of add-ons that make the benchmark better (e.g., by adding moderators and mediators to primary relationships), other extensions make the benchmark *more* vulnerable to critique because they are simply applications of those primary relationships, unmodified, further afield than originally intended. Unfortunately, such “over-extensioing” is tempting. For example, *if neoclassical economic principles apply to corporations and to mature markets, then should they not apply to their earlier versions—to entrepreneurial ventures and embryonic markets—as well?* On the one hand, over-extensions have occurred because some are willing to answer “yes”. On the other hand, it is relatively easy for a comparison-based approach to answer the question with a resounding *no*, because the accompanying assumptions of neoclassical models (e.g., relating to rationality, informational completeness, resource access, and objectivity) are almost always violated in the early venture-forging stages. Pointing out the folly embodied in the artifacts of such over-extensions, including the resulting inevitably optimistic and often highly inaccurate long-term forecasts emerging from formal new venture business planning that relies on unrealistic assumptions, provided an opportunity that was all too easy for many comparison-based approaches to exploit.

Such an opportunity raises the question of the best way to exploit it—*is it to do so with full pendulum-swing in modeling?*—which is the option embodied in the comparison-based approach. That approach has impact, and that is a good thing because it shines a bright light on those over-extensions that a field must then react to in a meaningful way. It is also good in that it packages those issues in one interesting story that most often draws from recent real-life observations. That forces self-reflection and the updating of the benchmark theory in a more holistic manner, which is also a valuable step to evolving the theorizing of any field. However, such benefits are predicated on the quality of the theorizing that emerges in reaction, noting that such quality is not simply guaranteed through the use of the comparison-based approach.

While the *reaction* to the comparison-based approach being published has benefits, that approach’s own theorizing involves several potential costs. First, comparison-based models often *avoid* difficult-but-necessary specification, which may leave an audience sold on its alternative narrative but sorely lacking in sufficient information about the steps, risks, costs, and contingencies of adopting it. Second, comparison-based models are often difficult to test or falsify due to tautologies, observational difficulties, and a lack of propositions (and their corresponding hypotheses), which raises questions over what requirements need to be fulfilled to be considered legitimate theorizing for a field. Third, such models can displace better theorizing (e.g., theorizing that is testable, that includes all the units, and so on) that could have filled the “journal space” instead, and may even reduce the adoption of newer benchmarks that are in the process of evolving (when such work is prematurely targeted).

Focusing on the first cost, comparison-based theorizing may harm the evolution of the field when it fails to provide the necessary and difficult modeling details. Simply stating different assumptions without proper foundations, and then without actually providing the complexities of how those alternative factors and relationships work, leads to more questions than answers. In the creation school example, that void of modeling encompassed the competing realities; the origins of initial EM heterogeneity (e.g., in initial beliefs or capabilities); the linkage from the formation mode to the exploitation mode in the overall process; and the conversion, in a noisy informational context, of initial unknowable factors into expectedly knowable ones. Such a replacement of formal modeling with positive narration often leads to a false sense of confidence in switching to that alternative story. In order for any field to build legitimacy for its theories, it must enforce the precept that *the level of difficulty in modeling what is can be modeled is not an excuse for a lack of modeling*¹⁴. And when something cannot be modeled, then that must either be admitted to, or the use of added simplifying assumptions that allow modeling must be explained. Celebrating a paper that presents a new story that purports to describe what cannot be modeled while it also attacks an existing theory that *does* use simplifying assumptions to provide one

modeled explanation is clearly untenable. It is not tenable because it will inevitably lead to harmful prescriptions and misunderstandings about complex phenomena.

Table 2 summarizes the concerns we have both with the vulnerable, benchmark, normative target and with the comparison-based, reactive approach to theorizing. We suggest that the costs of that latter, newer approach often outweigh its benefits. The main reason is that the comparison-based approach does *not* guarantee progress in theorizing directly, but only indirectly by intensifying the impetus for new, more comprehensive theorizing. There is a difference; the latter identifies the opportunity for progress, an opportunity that is foregone without proper exploitation—which is a conclusion that poses some irony in the example of the creation school.

Table 2. Pros and cons of the benchmark theory and the comparison-based approach that exploits it.

	Benchmark (Idealized) Theory	Comparison-Based Theorizing
Value	• Easy to model given: ○ Linear relationships among units; ○ Homogeneity across most agents; ○ (Super) rationality; ○ Instantaneous reaction (minimal dynamics); ○ Complete information. • Powerful testing: ○ Results often more generalizable; ○ Reveals how far off the ideal reality is; and often with clues as to why. • Calculable optimizations • Rationality provides measurable goals • Deductive logic provides explicit assumptions ○ Should entail strong limits to application when the assumptions are broken	• Can expose near-current best practices as a foil, as well as analyze them • Describes possible contexts and reasons where the benchmark assumptions are violated, and what then occurs • Can elicit valuable updates to the benchmark theory • More process-oriented, with more dynamics considered • Considers (some) effects of bounded rationality, including the use of specific heuristics and behaviors • More specific empirical support possible, often with use of wider set of methods applied, often with both subjective and objective explanations (to compare) • Paints more active roles for the decision-makers • Inductive logic used (but often based on relatively limited data) Practice-oriented; as such, more marketable
Concerns	• Unrealistic predictions in many non-ideal contexts • Questionable prescriptions at and past the limits • Too easy to mischaracterize as a strawman • Too easily overextended (past its limits)	• Modeling lacking because: ○ Non-simple relationships + dynamics + heterogeneous agents all lead to complexities that formal systems find difficult to capture, let alone optimize. • Over-reliance on implicit assumptions and stories rather than formal models (which can lead to proof-by-tautology) • Lacks optimization, and often solid logic for recommending “better” decisions • Prescriptions based on observations (e.g., of past successful heuristics) • “creativity” takes on a larger role in explanations • Generalizability often questionable given specific context the theory is induced from

4. Discussion

The creation school is not yet a theory, nor are its main ideas novel. That is our conclusion based upon a fair and comprehensive evaluation of how well the school fulfills a set of wide-ranging criteria for theory-building grounded in the literature. Nevertheless, the creation school is a solid example of a recent and impactful *approach* to theorizing. We have newly labelled the approach *comparison-based*, because it differs sufficiently from the two most similar meta-theorizing techniques, i.e., *problematization* and *contrasting* (Suddaby et al. 2011), to be considered as a separate method. We analyzed its pros and cons and concluded that its costs more often outweigh its benefits as a means for evolving the theorizing of a

field. To summarize, the answers to our two research questions are: no, the creation school is not yet a valid theory of entrepreneurship, and no, the theory-building approach it uses (the comparison-based one) is not alone a valid method. The assessments that led to those answers were fair, comprehensive, and clear; each was grounded in the relevant literature.

We now discuss the implications of our analyses below, focusing mainly on how to make better progress in theorizing. We then briefly speak about implications for entrepreneurs and to the limitations of this study.

For academics, our analysis leads to two primary questions on the advancement of theory in a field—when and then how to replace old theory with new. The simple answer to when is *as soon as the old theory is no longer useful*, raising the issue of measuring such usefulness. Less simple to address are follow-up questions like how much modification to the old theory is allowed before it becomes a different theory; how explicit do the listing of its limitations, gaps, and critiques need to be; and when is simply stating ignorance about the theory's "outer" boundaries of applicability better than alternatives (e.g., of continuing to stretch it, or of prematurely replacing it). Other difficult follow-up questions include what replaces the benchmark that could qualify as new theory, or at least sow its seeds; how developed, coherent, or contrasting must that work be; and how connected to the issues plaguing the old theory must it be. But regardless of the raising of such questions, the answers *are* nevertheless being provided—right now—by editors and reviewers, often through whatever politics that incurs in the publication process (Gabriel 2010), rather than being debated openly and widely. Is that what is good for our fields, especially given the current and growing ineffectiveness of post-publication critique and action? We contend that one sign that this situation may not be serving us well is, in fact, that the recent reviewing processes are publishing theorizing papers that exploit the new comparison-based approach, but not often in its best version. Whether it is the politics, or the inherent interest in the confrontation of existing benchmarks, or the pent-up demand for something different to move forward with in a field, such what-we-argue-as-questionable theorizing approaches appear to be working in ways that appeal to the right editors (regardless if that is best for the field).

The editorial attraction to papers using the comparison-based approach is understandable, and as such research may be more likely to be cited for several reasons, including that it summarizes a version of the benchmark (like a review paper); it describes a newer viable alternative explanation for an important phenomenon; it provides explicit comparisons over several important characteristics; it identifies specific weaknesses in the benchmark, often with examples; and it proposes possible heuristics that differ from current prescriptions that often have a marketable appeal, such as portraying the EM in a more active and heroic role. But such research should not be confused with the direct progress of theory¹⁵. Indeed, there is no direct progression of theory under such an approach for several reasons, including the lack of an independent foundation of core ideas; imprecision and incompleteness in the modeling; unclear and, at best, indirect testability; and the forced separation of scholars into often-exploitative camps. In addition, such reasons imply a possibility of negative spillovers that damage the legitimacy of the existing theories in the field.

If the comparison-based approach is not the answer to how to replace theory, but only possibly a signal of when to begin considering that replacement, then what are the appropriate alternative approaches? Such alternatives were the focus of a special-topic forum on meta-theory (see Suddaby et al. 2011) that produced a matrix placing seven new and different theorizing approaches along two dimensions. A more recent call for a similar special-topic forum (see Haveman et al. 2019) described another half-dozen more standard approaches. None were the comparison-based approach we describe here. We suggest that the benefits of a comparison-based approach should be available from less-radical and more-structured challenges to benchmarks (e.g., through analytical critiques), as well as from the dozen-plus alternative and more complete approaches that already exist. Furthermore, we also suggest that using those alternatives will entail fewer costs.

Although the comparison-based approach is not a suitable means forward, there are several possible ways to improve the efficacy of the impact that it does have. One way is to ground it in real-world cases, as well as in experiments in the lab. Such cases could provide a clearer means to induce new theorizing while more specifically indicating where the current benchmark is flawed. Vaghely and Julien (2010, p. 84) exemplify this direction in studying the creation school, concluding that real EMs “combine opportunity construction with opportunity recognition and enactment”. Another way is to generate the alternative theorizing independently and, thus, save the contrasts with the benchmark until the discussion. Wood and McKinley (2010) effectively take this tack with a constructivist perspective that argues a set of ten propositions, all *prior* to discussing the limitations and the comparisons with the objectivist-discovery perspective¹⁶. Still other ways include grounding the contrasting model in a coherent set of the most promising incremental modifications to the benchmark (although this raises a danger of piecemeal rather than holistic advancement); explicitly admitting the limitations of the new modeling, while offering guidelines or analogous patterns on how to proceed; and re-casting the benchmark and the proposed alternative as purely metaphorical concepts rather than as practical bases for decision-making (e.g., Klein 2008).

While the comparison-based approach can reinvigorate a field by taking a bigger-yet-coherent “shot” at the benchmark theory “king” than more incremental, gap-filling, and extension-testing research can, it does *not* provide a direct evolution of theorizing. Our analysis identifies ways to address many of the approach’s concerns, what the better examples look like, and how to put a better editorial check on the weaker attempts. Ultimately, though, it remains up to scholars, and pre- and post-publication processes, to filter out, reward, recognize, put into perspective, critique, and build upon such new work, all within whatever politics such less-usual models and approaches involve. We believe, however, that doing so should be informed by a more explicit set of standards—that are openly and actively debated—for helping to better guide us on how we wish to see progress made in the theoretical bases of our fields.

In terms of managerial implications, our analysis of the creation school offers entrepreneurs some insight. Unfortunately, the creation school itself offers little new for the practicing entrepreneur, given it was inspired by existing cases, especially by successful Internet-based ventures that scaled up their social influence at the right time. Furthermore, we would warn entrepreneurs to be wary of using any of that school’s prescription because the theory is incomplete, especially regarding the of origins of the new venture idea, and what the advantages are that entrepreneurs have in any competition to socially influence outcomes or to make an unknown known. There are better theories and instructions available for co-creation, for fast-and-frugal experimentation, and that are more specific to an industry or context, in practical guidebooks about lean start-ups, from mentors, and through experiential educational courses.

As with any assessment of complex targets, ours faces several limitations. We are limited in the depth of detail we can provide (e.g., by journal page restrictions). We are limited in the width of detail we use (i.e., we focused on the three foundational papers for the creation school, plus some later related papers). We are limited in the set of criteria we applied (although we believe the set was comprehensive). We are limited in the facts available about the targets, and had to apply judgments and interpretations to fill in some significant gaps (e.g., to generate a process diagram; to weigh the criteria met against those not or only partially met), partially because we have to rely on relatively few clear cases (e.g., of comparison-based theorizing). Those limitations noted, the assessments remain fair and comprehensive, and the conclusions strongly supported.

We call for further work to address the concerns about the validity of the two targets here. We identified the two main paths to improve the creation school, and we have called to buttress the comparison-based approach to theory-building with either an additional accepted approach (e.g., problematization) or with the independent development of formal explanations directly from premises. We further call for more empirical work on

entrepreneurial opportunity identification–generation and exploitation (in the field and in the lab, through simulations, surveys, cases, and experiments) to serve as basis for alternative, induced theorizing, especially involving new technologies like AI, or against new powerful gatekeeper firms (e.g., Meta, Amazon, Alphabet, and Apple). We recommend more post-publication assessments of the suitability of current theories, especially in the light of such changes in the competitive landscape. We—as a field—need to see “our” ideas, premises, and models more challenged and open to critique if we are going to be relevant to practitioners and to other academic disciplines. And so, we look forward to seeing that debate occur much more explicitly, and much more often, in entrepreneurship and in management in the very near future.

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Notes

- ¹ The 3E framework is independent of theory type, as explained in [Arend et al. \(2016\)](#). It has been applied to multiple theories and is based on established criteria proposed by many management theory scholars in top journals over decades, with its core in [Dubin's \(1969\)](#) work. We note that one of the creation school's founders even submits her school to Dubin's criteria ([Welter and Alvarez 2015](#); pp. 1402–3), thus verifying that the school's processes can be assessed under criteria like the ones applied here.
- ² Note that the definition of the main unit—the EM as a strategic actor seeking to exploit an opportunity ([Alvarez et al. 2013](#), p. 302)—is problematic because it not only assumes the pre-existence of a different unit—the exploitable *opportunities*—but one that the school is trying to explain the existence of.
- ³ Note that the EM and the market are each likely to be entities embodying many individuals, making the updating of beliefs, internal social constructions, decisions, calculations, and actions potentially complex (as involving interdependent interactions among those embodied individuals). Given that there exist extensive literature modeling such complex internal phenomena, it should not be trivially glossed over. However, the current model simply overlooks those supporting processes, even though they appear at the same level of analysis as the primary process.
- ⁴ Note that the example exogenous shocks listed in the creation school (i.e., changes in technology and consumer demand, and in political and social institutions—see [Alvarez et al. 2013](#), p. 305) are all generated by intentional human actions that are *endogenous* within their own markets, regardless of if they also have shocking effects in outside markets that may be exploitable there. This raises the question that, if all opportunities are created by human action, even the ones that the creation school labels as *discovery opportunities*, then the school-to-school boundary would be non-existent as only *one* process appears to drive *all* entrepreneurial activity—the process that continuously generates incomplete information.
- ⁵ There are a myriad of further questions relating to the logic of the creation school that should also be considered, including: What do the dynamics of the co-evolving process look like? How much does the market co-evolve rather than simply react through judgment ([Garud et al. 2014](#))? How is disinformation from customers and rivals filtered out? How can entrepreneurs use disinformation and other “cheating” tactics to gain an advantage in this context ([Arend 2016](#))? What is the learning process of the EM regarding the various pieces of information that are initially incomplete but evolve toward being sufficiently complete so as to be considered only “risky”? What does that initial decision about the scale, timing, and characteristics of the first action look like, and how do resource constraints affect that? When and why do the gains become estimable—is it the unknowability of the states or their probabilities that is the issue along the way? Why aren't other decision items unknown, like possible alternative actions, or the payoffs of those? Why are losses not malleable, for example, by receiving payments and shared liability from partners or consumers? Why does it matter whether the paths to opportunity formation are dependent or unique, if the opportunity itself becomes known when formed? How does forming the opportunity influence the ability to exploit it otherwise?
- ⁶ Note that the school does hint at potential hypotheses—based upon statements about (i) several team characteristics that would likely lead to success in their process (see [Alvarez et al. 2013](#), pp. 309–12); and (ii) the lower degree of non-causal-ambiguity-related entry barriers built. None of those have been tested.
- ⁷ Work that attempts to test the creation school includes one piece by one of the school's authors and one independent piece. [Hmieleski and Baron \(2008\)](#) do not directly test the school; their *H2b* is about promotion, not creation. [Alvarez et al.'s \(2015\)](#) study consists of one historic case not involving a created opportunity but the evolution of a long-existing product (i.e., king crab) through innovations in processes and in legitimization. No market failure existed; their *P1* is simple self-interest, not creation, and their *P2* involves generating shared beliefs, which is an activity that is not solely attributed to the creation school.

- Note that the most important practitioner concern remains a mystery in the creation school—that of *identifying the origins* of the EM’s differentiated initial belief that sends the EM down that path to forming and then exploiting that new opportunity.
- For human-action-based phenomena (as with the case here), note that the validity of those observations also needs to be assessed (e.g., [Locke 2007](#); [Pentland 1999](#)).
- Besides failing to cite similar work, the creation school also avoids citing (and discussing) relevant work that questions the kind of research that they do cite. For example, the possibility that “the evolution of the ideas can lead to failure” is overlooked, which is somewhat dangerous if we care about truth and social welfare (e.g., [Rowbottom 2010](#)). The overall equivalence of evolutionary realism with scientific (aka *objective*) realism—as even admitted to by [Campbell \(1974, p. 451\)](#)—is downplayed for the sake of contrast. Also un-discussed is that the evolutionary process occurs in a non-equilibrium system ([Buchanan and Vanberg 1991](#)). And the possibility that the process is driven by randomness—which is naturally present in real complex systems ([Allen 1988](#))—is not fully considered (i.e., as this process is based more on luck than on entrepreneurial skill).
- We note that all “opportunity schools” are open to critique—the creation school and effectuation criticize the discovery school while also being critiqued. Each school offers both strengths and weaknesses, and their advocates should be willing to assess such theorizing along the 3E criteria (or equivalent) in order to see where improvements can be made.
- The recent proliferation of inaccurate versions of what KU is, is a product of adhering to the creation school rather than considering alternative theorizing. There are several potential dangers to continuing such a “creation school perspective” that confuses “unknowability” with “what is initially unknown but is actually knowable”. First, if KU is not actually required for the creation school’s entrepreneurial activity, then the main challenge represented by known-unknowns—being the non-optimizability of any decisions involved—is avoided when it should not be, and the main challenge represented by knowable-unknowns—the highly complex modeling required to capture the competition it involves—is also avoided, but should not be. Second, and to the latter, if the end-to-end profit-making process that requires uncertainty-reduction is not fully and explicitly explained, then, from a theoretical standpoint, there is *no* actual profit guaranteed—as not all of the steps can be formally scrutinized to prove that any one venture can succeed—and, as such, from a practical standpoint, there is no real prescription to follow, as managers cannot implement plans that are incomplete.
- We analyze the comparison-based approach because it is a relevant and important addition to theorizing—it is used in the creation school and in effectuation—two recently proposed partial theories that have received significant attention in our field. And one could contend that one of its key traits—that of “strawmanning” a targeted pre-existing theory—has been more visible in many related fields (e.g., with the arguable strawmanning of the individual traits stream by the nexus model in entrepreneurship, or of the resource-based view by the dynamic-capabilities view in strategy, or of game theory by behavioral models in economics).
- Violating the simplifying assumptions of *homogeneity*, *informational completeness*, and *rationality* is often nightmarish in terms of modeling the consequent complexities and non-closed-form-solutionability that arises. However, it is often necessary to do so in order to make progress in building better theory; the alternative of building interesting but informal “stories” is simply not science.
- Another effective tactic of the recent comparison-based approaches, as a further means to draw attention and contrast to themselves, has been to tease that their processes have “solved creativity”. Of course, that can never happen; in fact, such alternatives do not actually give the creative process the full respect it deserves, never getting into the realities of its difficulties, especially at the individual level (e.g., as more fully described by experts as in the case of its multi-stage conceptualization in visual artistry—see [Ehrenzweig 1967](#)).
- In addition to the three primary papers that define the creation school, pieces like [Wood and McKinley’s \(2010\)](#) add some further details (e.g., more boundaries). That said, they also miss several crucial elements (e.g., how competing social influencing works); make several questionable assertions (e.g., that opportunity ideas do not require market failures); and contrast to a strawman version of the discovery school (e.g., presuming, incorrectly, that successful opportunity exploitation does not involve the entrepreneur and their social capital, and that marketing is not used in the latter part of the discovery school’s full process). Therefore, while such papers under the creation school may offer some alternative, specific details that the discovery school does not (e.g., on how entrepreneurs process opportunity abandonment), that does not in and of itself make good theory, or even necessarily provide new insight into the larger picture (e.g., given psychological research also deals with the personal processing of failures).

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