2013

Finding Practical Knowledge in Entrepreneurship

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ABSTRACT

Research in the pre-paradigmatic, applied scientific field of entrepreneurship is characterized mainly as exploratory. This article advocates for a considerable shift toward a more effective applied research agenda. An applied research program is proposed based on modifications to a Lakatosian research program. The agenda extends beyond typical calls for more replication work to include a focus on practical outcomes, practical significance, and surprising findings among other things. The intent is to produce substantially more practical knowledge – knowledge that is useful to entrepreneurs, policy makers, educators and scholars.

INTRODUCTION

Venkataraman (1997), Shane and Venkataraman (2000) and Shane (2003) have attempted to realign research in the field of entrepreneurship around the idea of practical dependent variables focused on opportunity identification and opportunity execution. Shane's (2003) interpretation of opportunity execution included various practical entrepreneurial outcomes ranging from starting a company to going public. Defining an applied social science such as entrepreneurship in terms of practical dependent variables is a crucial first step in the search for practical knowledge.

A pragmatic person might wonder to what extent entrepreneurship researchers pursue practical research programs in order to uncover knowledge useful for entrepreneurs, policy makers, educators and scholars. If a pragmatist closely examined research in the emerging field of entrepreneurship over the last thirty years, s/he would likely notice that the field better approximates a world of loosely connected single studies than one of meaningfully-integrated research programs – Forscher's (1963) brickyard problem. Further, many researchers come into the field very briefly in order to publish a single article (c.f. Cornelius et al., 2006). Many of those who remain frequently switch topics rather than steadfastly focus on a single research area for an extended period of time. Additionally, a very small percentage of scholars contributes more than a couple of peer-reviewed articles during a career (Gartner et al., 2006).

In addition to scholar transience, topic-hopping and low productivity concerns, the field must also face problems related to replication. Replication studies are often overlooked in favor of original work in peer-reviewed journals. Moreover, when conducted, replication studies usually do not support initial findings (Hubbard & Vetter, 1996; Evanschitzky et al., 2007). The failure of various types of replications (across management fields) alone should be enough to raise concern with respect to the bulk of our practical entrepreneurship knowledge.

There are a number of other steps that the field must take. Our practical knowledge must be reliable and robust. It is not enough for a single study to be done to identify a predictor of one or more desirable entrepreneurial outcomes. Entrepreneurs should be provided with replicated evidence of the validity of constructs and the confidence that desirable outcomes follow from key predictors (Aldrich, 1992; Chandler & Lyon, 2001; Davidsson, 2004; Kane, 1984; Rosenthal and Rosnow, 1984). Entrepreneurs also need to know that that a predictor is broadly useful, rather than limited to a very narrow set of circumstances (Hubbard & Vetter, 1996). Entrepreneurs want new knowledge that is not blatantly obvious – it should be rather surprising instead of common sense (Ladik & Stewart, 2008; Armstrong, 2003). Entrepreneurs want a predictor to be cost effective (Gouldner, 1957; Drummond & McGuire, 1997), as well as something that they can directly influence (Gouldner, 1957). Further, entrepreneurs would like to believe that it all makes sense – that there are good and compelling reasons for believing that changing a predictor, A, will result in a change in a desired outcome, B.

As most researchers know, the list of practical demands above is a tall order for any research program. Nonetheless, it is important to establish specific objectives for any applied social science in order to more effectively direct research efforts. This article is proposed as a step in the direction toward more effectively organizing work in the field of entrepreneurship[1]. It is shown below that the field of entrepreneurship, like so many social sciences, is not yet able to make use of the intellectual devices developed by Popper, Kuhn and Lakatos. Instead, our proposal for a new 'applied research program agenda' here may allow the field to evolve to a point at which substantial pragmatically useful knowledge is commonly generated[2].

THE PHILOSOPHY OF SCIENCE

It has appeared to some observers that the dominant philosophy of science in the business disciplines continues to be logical positivism (c.f. Cohen, 2007; Meyer, 2009). Core logical positivist values include a distrust of metaphysics (i.e. theory) and a pursuit of objective facts. The unit of analysis is the protocol statement, a single factual statement linking together tightly-operationalized variables. The nature of truth is based on correspondence theory in which empirical findings correspond to an external reality according to their truth value. Hence, the

[1] We wish to thank one of our reviewers for suggesting that the field of entrepreneurship consider the Cochrane Collaboration in the field of medicine as a model for effectively organizing research.
[2] Our fundamental interest, to advance scholarship, aligns closely with that of Peng & Dess (2010).

role of the scientist is to uncover disparate pieces of concrete conceptual knowledge about the real world.

Logical positivism began to lose its appeal amongst philosophers of science for a variety of reasons in the 1960s. Ultimately, most philosophers adopted a perspective that neither subjectivity nor theory could be avoided in the scientific process. Logical positivism, it should be noted, can offer value to a new field searching for basic factual knowledge. For example, it influences scientists to be wary of the slippage between concept and operationalization, encouraging them to collect multiple observations.

The dominant replacements for logical positivism are widely considered to be the intellectual contributions of Sir Karl Popper (1963) and of Thomas S. Kuhn (1962). One might also include the work of Imre Lakatos (1978), who attempted to reconcile the works of both Popper and Kuhn. These three philosophers each worked from units of analysis more encompassing than the protocol statements emphasized by logical positivists. Popper separated theory into two categories: scientific theory, which made bold/risky predictions; and pre-scientific theory, which could not claim such predictions. Kuhn developed the notions of prescience and scientific paradigm. Lakatos emphasized research programs, which involved complex clusters of interconnected theories. Popper and Lakatos adopted the correspondence theory of truth and were decidedly prescriptive in their philosophies. Kuhn, on the other hand, adopted a consensus theory of truth and in the process, imbued his philosophy of science with a decidedly descriptive emphasis upon the realpolitik of scientific decision-making.

For Popper's philosophy of conjecture and refutation to be meaningfully employed in an emerging applied social science such as entrepreneurship, there are two necessary prerequisites. First, entrepreneurship researchers would need to draw a strong line between predictive, scientific theories and explanatory, pre-scientific theories. Empirical research would largely be confined to testing the former. Currently, there is little indication of such a distinction being made within the entrepreneurship field.

Second, research undertaken on predictive theory must strongly test the predictive relationships advocated by a theory. The extent to which such work occurs in entrepreneurship is debatable. A recent review of tests of human capital theory, one of the most used theories in the top entrepreneurship journals (Kenworthy & McMullan, 2010), suggests that strong theory testing may not be commonplace. Human capital theory (HCT) clearly predicts that financial investments in, e.g. formal education and experience, will yield positive financial returns over time. Researchers in entrepreneurship, unlike those in economics, chose to not measure investment costs in any of the fifty-six studies reviewed. Instead, entrepreneurship researchers loosely invoked HCT in order to draw linkages between human capital variables and a wide variety of other phenomena. Furthermore, failures to support hypotheses were typically treated as un-interpretable or invalid. Such pre-scientific behaviors are evidence that the field of entrepreneurship may not be prepared to adopt a Popperian philosophy.

Kuhn, like Popper, has much to offer, but not at this time to the field of entrepreneurship. A scientific field needs a paradigm in order to use Kuhn's ideas. In the 1960s, Kuhn viewed his work as mainly contributing to the hard sciences, as in his estimation, the social sciences lacked any paradigms. Though Kuhn may be debated today, there is little doubt that the field of entrepreneurship is pre-paradigmatic (c.f. Zahra, 2005) and therefore, not yet ready for Kuhnian analysis.

Lakatos' provocative research program concept relies on the existence of at least one strongly-predictive scientific theory. It involves, among other things, a number of auxiliary theories that must also be taken into consideration in order to comment on the progressive or degenerative nature of a research program. According to Blaug (1975), the notion of a Lakatosian research program has extremely limited usage within the entirety of the social sciences.

ASPECTS OF APPLIED RESEARCH PROGRAMS

The problem that we face may be formulated in the following terms: how does a disparate group of occasionally interacting social scientists participate in meaningful, cumulative programs of applied research? How does our research culture shift towards cumulative programmatic research and away from a predominant focus on single studies? Our proposed solution is for scholars to have a common understanding of the requirements of an applied research program and therefore a common knowledge development agenda

We contend that our proposed solution is viable and achievable in part because of the existence of a few largely uncoordinated applied research programs that have produced some of our most important entrepreneurship knowledge. Hence, it would seem that at least some researchers are already asking the types of practical questions about research findings that we discuss here. Some questions such as whether or not a research finding is replicable are standard scientific concerns. Others, such as whether or not specific control variables can be manipulated in a cost effective manner, are less standard.

It is worth pointing out that applied research program agendas are not always directed by explicit, formalized theory. This may be surprising to at least some people who tend to see theory as the dominant research-directing mechanism (c.f. Popper, 1963; Zahra, 2007). Sometimes, the types of research programs that we argue for here, e.g. job generation research, occur largely in the absence of explicit theory. Of course, this is not to say that there is no relevant theory. It is simply suggested that theoretical guidance need not be driven by formally explicated theory. To be scientifically useful, theory must be used in an informed fashion and an applied research agenda (such as that which we advocate) should provide more comprehensive direction to the testing of theory.

An Applied Research Program Agenda

The construction of an applied research program (ARP) agenda involves both philosophy of science and practical concerns. As such, defining characteristics of an ARP are subject to criticism. We offer a set of eight characteristics that indicate the nature of scientific work to be included in applied research programs. Four of the characteristics are fundamental for the definition of an ARP. The other four characteristics arguably designate a desirable knowledge expansion strategy.

The four fundamental characteristics of an ARP are, as follows:

Practical Outcomes In order for a set of studies to constitute an applied research program, at least some of the studies need to focus on practical outcomes. A focus on practical outcomes implies that the dependent variables in tested models need grounding in practical issues typically of concern to entrepreneurs (c.f. Shane and Venkataraman, 2000; Shane, 2003). Entrepreneurs are interested in investing in variables that most influence economic outcomes such as revenue and profit growth.

There can also be instances in which dependent variables in the field of entrepreneurship are of less concern to entrepreneurs but still of concern to scholars, educators and policy makers. Job creation (Birch, 1979), for example, is strongly associated with entrepreneurship, but it is of little interest to specific entrepreneurs. Instead, the job creation findings substantially influence government support and programming for new ventures.

Evidence of Replicability In order for a set of studies to constitute an applied research program they collectively must exhibit evidence of replicability. In 2002, Dov Eden (p. 844), an editor of the Academy of Management Journal, proclaimed that, "…replication research is indispensable for scientific progress." His strong belief is held across the social sciences (Dewald, Thursby & Anderson, 1986; Feigenbaum & Levy, 1993; Tsang & Kwan, 1999). Unfortunately, the publication of replication studies work is uncommon across many scientific fields, including the business disciplines. According to Hubbard and Vetter (1996), replication work in leading business journals represents less than 10% of published empirical work in accounting, economics, and finance and 5% or less of published empirical work in management and marketing.

To identify a number of studies as a progressive applied research stream, many of those studies must have developed evidence of replicability. There must be credible evidence to convince scholars and practitioners that research findings will remain consistent across time, contexts and cultures, and research methodologies. Exceptions should only exist when explicitly predicted or otherwise appreciated as ad hoc adjustments to the generalizability of the findings.

Practical Significance In order for a collection of studies to be considered an applied research program, the findings must exhibit practical significance, rather than mere statistical significance. "Statistical significance," according to Kirk (1996, p. 746), "is concerned with whether a research result is due to chance or sampling variability; practical significance is concerned with whether the result is useful in the real world." Statistical significance is often investigated via null hypothesis significance testing, a technique that has been heavily criticized by leading social scientists (c.f. Carver, 1978; Cohen, 1990; Meehl, 1967; Rozeboom, 1960; Schmidt, 1996).

Kirk (1996) refers to the measures of practical significance as measures of effect magnitude. The magnitude or strength of a relationship, i.e. the amount of variance accounted for in the dependent variable, can be measured in a variety of ways, the most popular being Cohen's d.

It appears that effect sizes are already being reported to some extent in entrepreneurship studies and that effect sizes are higher than expected (Connelly et al., 2010). Such findings bode well for the development of practically-significant knowledge that is useful to entrepreneurs and other relevant stakeholders.

Unexpected Findings In order for a set of studies to constitute an applied research program they must provide findings that go beyond common sense.

> Surprising findings differ from current practice or current beliefs…Surprising findings may be innovative or new, although not always. When the problem is important, surprising findings are likely to be controversial or unpopular.
>
> Armstrong (2003, p. 71-72)

An applied research program cannot only accept findings that support existing theories and contentions. It must be open to surprising and counter-intuitive findings. Such findings can be generated from implicitly theoretical research or from strongly predictive theories that, when right, set up 'damn strange coincidences' (Salmon, 1984). Counterintuitive findings, according to Lindsay et al. (1998, p. 215), "…demonstrate the superiority of science over common sense." In the early 1960s, the field of social psychology, from which entrepreneurship research draws a number of theories, went through a transition from demonstrating the obvious to focusing on counterintuitive effects. The shift toward non-obvious findings led to a number of productive research consequences including increased researcher enthusiasm; unexpected research results; and, productive debate (Kelley, 1992).

Such a set of defining characteristics should help us recognize findings, theoretically supported or otherwise, that hold practical potential for entrepreneurs. When identifying a current research program we recommend that scholars appreciate and accommodate the factors that tend to bias published research findings (Meehl, 1990). Most commonly, we suggest that researchers take into consideration the totality of both supporting and non-supporting findings despite some additional difficulties associated with the interpretation of non-supporting findings.

The next four agenda characteristics expand the usefulness of entrepreneurship knowledge:

Address Why and How An applied research program attempts to answer the how and why questions. According to Whetten (1989), how and why are two of the essential building blocks for effective theory development. The how building block handles the manner in which factors (i.e. variables, constructs and/or concepts) are related to each other. The key relationships, and their complexities, are typically depicted graphically for purposes of presentation, testing and refinement. The why building block offers a rhetorical explanation for the underlying logic (i.e.

a theory's assumptions) of the causal model. According to Sutton and Staw (1995, p. 376), good theory should be, "…rich enough that processes have to be described with sentences and paragraphs so as to convey the logical nuances behind the causal arrow."

Controllable Variables

> …the applied social scientist is concerned not merely with identifying predictively potent independent variables, but also with discovering some that are accessible to control.
>
> Gouldner (1957, p. 97)

An applied research program makes an attempt to identify variables that can be manipulated by practitioners (Bauer, 1951; Gouldner, 1957). Controllable variables such as business strategies are desirable because they enable entrepreneurs to directly influence survival, growth and success.

The distinction between uncontrollable and controllable variables does not, however, imply that uncontrollable variables should be dismissed. According to Hobbs (1969, p. 243), "…uncontrollable variables are important from the standpoint that they may establish the context of change, or the limits of effect of controllable variables."

Eliminate Alternative Explanations

> For those of us who grant that theory testing is meaningful…this lack of testing is an undesirable state of affairs…these tests should be comparative - that is, against a rival paradigm or research program (Kuhn, 1962; Lakatos, 1970).
>
> (Pahre, 1996, p. 221)

An applied research program attempts to eliminate alternative explanations for phenomena under observation. The ubiquitous existence of multiple explanations is typically known as under-determination. The process to reduce under-determination involves strong attempts to refute rival scientific theories. Though refutation may be a strong word, the idea is that the type of applied research program described here positions the field of entrepreneurship to examine scientific evidence and weigh the relative strengths of competing theories against each other. The outcome is stronger applied knowledge about, e.g. new venture survival and success.

Cost-effective Solutions An applied research program should uncover numerous controllable variables that vary in magnitude and implementation costs. Hence, an applied research program will also undertake to examine and compare the cost-effectiveness of the most powerful controllable variables. Cost-effectiveness analysis (c.f. the field of medicine – McClellan & Newhouse, 1997; Stinnett and Mullahy, 1998) compares incremental cost and incremental effectiveness of existing versus new interventions based on empirical results. The results of cost-effectiveness analysis allow entrepreneurs to make more informed decisions about key aspects of starting and growing new ventures.

ILLUSTRATIONS OF APPLIED RESEARCH PROGRAMS

Two illustrations of applied research programs are described below. The first illustration involves the development of knowledge with respect to entrepreneurship and job generation. In this case, a highly provocative finding directly influenced scholars from around the world to seek replications and/or refutations. The second illustration, that of using creativity to predict entrepreneurial performance, is interesting because it appears that the researcher efforts involved were much less coordinated than in the job generation case.

Entrepreneurship and Job Generation

David Birch (1979) found that 81.5% of all net new jobs created in the US from 1969 to 1976 were located in small firms with less than 100 employees. Birch's finding was not just statistically significant but practically significant, as well. The evidence influenced policy makers around the world to vigorously promote various small business initiatives. For many neo-classical economists, however, the counter-intuitive findings flew in the face of both theory and ideology. Replications, cross-validations, extensions and practical significance studies ensued. An early US-based replication study (Armington & Odle, 1982) indicated that only 35.8% of net new jobs created were by small firms, and that Birch was wrong. However, the study used a traditional, static measure of job creation and further, it was restricted to data from a recessionary period.

Armington (1983) and Armington and Odle (1983) conducted more sophisticated replications of the Birch (1979) study and found in favor of the dominance of entrepreneurial (i.e. new small business) job generation. In 1987, Birch found that companies with 1-19 employees accounted for 82% of net job creation and large firms (i.e. with 5000+ employees) suffered net losses of 13.5%.

In the UK, Doyle & Gallagher (1987) found that only companies in the 1-to-19 employee category showed employment growth. Blanchflower and Burgess (1996) found that establishments with less than 100 employees accounted for a disproportionately large share of job creation and for destruction, a disproportionately small share. In Canada, Picot, Baldwin & Dupuy (1994, 1998) found that a disproportionate share of employment was created over the short and long runs by small firms during recessions and recoveries. Over a three year period, 5% of small firms created 43% of jobs. In Norway, Spilling (1996) found that 1% of new establishments accounted for 44.8% of total job creation over a nine-year time frame. Further, 2.4% of new establishments accounted for 57.9% of total job creation. In Sweden, Davidsson et al. (1998) found that small firms outperformed large firms in terms of gross and net job generation.

Back in the US, the Small Business Administration (1998, 1999) developed a database for more accurately tracking interclass movement. An analysis of the data revealed that 75% of net new jobs from 1990 to 1995 were created by small businesses (<500 employees). Further, the small business job generation rate was almost triple the large business (500+) rate: 10.5% and 3.7%, respectively.

Over the years, only a small number of studies have not replicated the Birch (1979) finding. White and Osterman (1991) reviewed Wisconsin unemployment records and found that two and four-digit SIC analyses reveal that, longitudinally, small firms do not account for the bulk of net job generation. Davis, Haltiwanger and Schuh (1993, 1996) tackled what they considered the job generation myth by investigating the US manufacturing sector and reported that firms with at least 500 employees account for half of jobs generated. Interestingly, the Davis, Haltiwanger and Schuh (1993, 1996) studies were heavily criticized by numerous economists and entrepreneurship scholars for data and method limitations and problems (c.f. Davidsson et al., 1998; Carree and Klomp, 1996; Kirchhoff and Greene, 1998).

Ultimately, the initial Birch (1979) finding spawned a substantial program of research to further understand and interpret the macro-economic relevance of entrepreneurs.

Creativity and Entrepreneurial Performance

Joseph Schumpeter (1942) did not formally predict that more creative people will develop more successful businesses. The notion does, however, appear to underlie his thinking. Thus, the post-Schumpeter creativity research in the field of entrepreneurship is more theory-inspired than theory-directed. A recent review of the extant creativity research uncovered 28 empirical studies that linked creativity with entrepreneurship (McMullan, 2009).

The creativity research program has grown slowly, over a thirty year period. The articles are not very well-cited and they tend not to be published in top-rated journals. The facts are, however, that many of the creativity researchers used practically relevant measures of outcomes such as start-up, growth and profitability. The researchers conducted constructive replications and cross-validated findings by using different measures of creativity and substantially different sample populations from many different countries. Moreover, a sizable number of studies found not only statistical significance but evidence of practical significance as well (c.f. Khan, 1987; Khan and MacMillan, 1988; Verhees and Meulenberg, 2004). The proportion of outcome variance often accounted for in the creativity studies is surprisingly high in spite of measurement difficulties in the area of creativity (Clapham, 2004; Kaufman, 2006). For most entrepreneurship scholars it is normal to expect entrepreneurs to be more creative than the average person, though anything but conventional wisdom to expect creativity to account for a substantial portion of entrepreneurial performance.

With respect to the eight research program aspects addressed above, the creativity research evidence does a better job of establishing entrepreneurial creativity as an applied research program (requirements 1 – 4) than it does further developing the usefulness of this established knowledge (requirements 5 – 8). The only attempt to study the enhancement of entrepreneurial creativity is disputably David McClelland's work in India (McClelland and Winter, 1969), in which he attempted to enhance subjects' need for achievement. A cost-benefit analysis of such intervention strategies seems a long way off in part because control is a major problem in this line of research. Nonetheless, there are occasional attempts at building theoretical explanations for such a relationship and useful empirical results may arise from facilitating better self-selection and more effective investment strategies.

In summary, the empirical studies linking creativity with entrepreneurial performance generally meet the minimum requirements of the aforementioned applied research program agenda. The success of this research program occurred in spite of what appears to be a lack of common knowledge about the various empirical findings amongst the participating creativity researchers. This applied program of research may have been, more or less, accidentally spawned by the diversity of concepts and measures characterizing creativity, and by a number of people who envisaged the reasonableness of the idea of the creative entrepreneur.

Comments on Illustrations

The two illustrations above suggest that applied research programs, corresponding to the definition provided here, do occur naturally in the field of entrepreneurship. There are also other examples. The published empirical studies of the SBDC counseling effects is an example of an applied research program that has been coordinated for over 25 years by a single scholar, Jim Chrisman. The need for achievement research is another example of an applied research program that existed due to efforts by a mainly uncoordinated group of researchers.

Some of the applied research programs appear to be more explicitly theory-directed than others. It is interesting to note that many people in the field of entrepreneurship likely believe that McClelland's (1961) theory was the directional force behind the need for achievement research, and to an extent, this is the case. McClelland created a theory that emphasized the importance of need for achievement in entrepreneurship as well as in economic development. Some of his key assumptions were that entrepreneurs represented an important dimension in economic growth and that entrepreneurial success and need for achievement were positively correlated. McClelland's theory, however, predicted that macro-level interventions would promote an entrepreneurially vibrant economy. Hence, his theory did not provide direct effective guidance to the largely micro-level research on individual need to achieve.

A similar argument could be constructed regarding the weak link between Joseph Schumpeter's macro-level theory and the empirical research linking entrepreneurial creativity with entrepreneurial performance. What is interesting about the insights here is that explicit theory played a limited role in guiding effective research. This suggests that something other than formal theory can be used to direct the generation of useful knowledge. Further, that the job generation and creativity research programs managed to cover the minimal requirements of an applied research program agenda suggests that some researchers already have mental models of useful knowledge that do not vary greatly from the research agenda being advanced here.

PROGRESSIVE VERSUS DEGENERATIVE RESEARCH PROGRAMS IN APPLIED SOCIAL SCIENCES

As previously mentioned, Popper, Kuhn and Lakatos each focused on different units of analysis in order to examine intellectual progress in the natural sciences. They also analyzed the waxing and waning of research programs through those unitsBuilding on Popper and Kuhn, Lakatos (1970) advocated a model for the critical assessment of a research program. He distinguished between advancing and declining (i.e. progressive and degenerative) research programs. From Lakatos' perspective, research programs tend not to be completely refuted. Instead, ongoing ad hoc adjustments are often made to models in order to save them from nonsupportive findings. The accumulation of ad hoc modifications, however, may eventually lead researchers to acknowledge the limitations of a research program or merely lose interest in it.

In the pre-paradigmatic social sciences (Kuhn, 1962; Lakatos, 1999), the Lakatosian notions of progressive and degenerative research programs require some adaptation to better suit the characteristics of applied research programs established above. Below, we suggest six criteria that signal a degenerative program and promote disengagement from faltering research streams.

- Failure to cross-replicate research results with different measures, with different sample population or under different circumstances.
- Weakening of the explanatory power of predictors in better-controlled studies.
- Failure to find controllable variables that can be modified to meaningfully affect new venture performance.
- Failure to eliminate competing explanations, particularly when competing ones explain more of the phenomena of interest or more of the desired outcome variance.
- Failure to find evidence that supports the cost-effective manipulation of controllable variables.
- Increasing doubt due to spurious correlations or the existence of administrative artifacts that explain key relationships.

The last suggested criterion, about spurious correlations, may represent the greatest threat to the long-term progress of promising research programs. A key reason for the threat is that the social sciences do not typically allow for the kinds of controlled experiments found in some of the hard sciences that bolster confidence in the integrity of a cause and effect relationship.

IN SUMMARY

The direction for non-cumulative, solo-study research has come, however implicitly, from logical positivism. The influence of logical positivism has declined in philosophy of science and many scientific fields have sought to fashion research efforts around the ideas of Kuhn, Popper and Lakatos. It is argued that the applied scientific field of entrepreneurship research should also transition away from the influence of logical positivism. It is further argued that the natural replacements to logical positivism do not yet represent a good fit with the entrepreneurship field.

An applied research program agenda is proposed to help direct the development of cumulative research towards practical knowledge. The argument bears some things in common with the position papers advocating for more replication research but extends beyond the single issue of replication. The elements of the proposed applied research program are recognizable as substantial modifications to the Lakatosian research program. The entrepreneurship research program is, therefore, supported or refuted in a manner more appropriate for a pre-paradigmatic applied social science.

The applied research agenda should encourage a critical mindset. It should direct the development of knowledge across studies over time by helping entrepreneurship researchers to more critically appreciate the usefulness of extant and new findings, as well as determine what groupings of studies constitute promising applied research programs.

It is important to point out that we do not suggest all independent studies to be without worth, but rather we demonstrate that the agenda required to develop practical knowledge is sufficiently time consuming and study intensive to require a substantially larger proportion of the global entrepreneurship research effort.

In order to demonstrate that the advocated approach to knowledge development has been used to positive effect in entrepreneurship in the past, job generation and entrepreneurial creativity research efforts are described. The outsider assistance research program spearheaded by Jim Chrisman is offered as another example. Scholars in the entrepreneurship field are hence encouraged to identify additional existing applied research programs, evaluate their progress and propose future directions in line with the philosophy advocated here.

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