As the foundation of our modern world, innovation has generated a seemingly endless ocean of new products, new processes, new thoughts, and new ways of doing things. Every day, we enhance our innovation and its effects – and we advance, accomplish and constantly seek more! Generally, we tend to live well based on our innovation outputs.

This suggests that we think we know what we are doing, and that we know where we are headed. We do know what we’re doing, don’t we? Most would say: yes, we do; indeed, we are inclined to be certain of it.

But: can we be certain about what we know about innovation?

To address this question, we search for evidence of any useful outputs of the work of philosophy. Such outputs should help us better understand if we can, indeed, be certain about what we do, and where we are going. Is there any evidence of this? Alas! – philosophy is nowhere to be found! As a tool of rigorous reflection and understanding, even where some of the most exciting and forward-looking innovation enterprise in science, engineering and organizational structuring takes place, philosophy seems to have vanished – if it was ever there in the first place.

Today, this seems somehow normal, and quite all right. But is it? Of course, we are aware that our history of philosophy illuminates the earlier pathways we once followed to achieve our modernity, and that is fine; but, where is philosophy and its work today? Where has philosophy gone?

In this book we explore these questions, and more: why is philosophy vanishing, or even entirely absent from our world today? What has happened? If, at one time, philosophy was so very important, why would it no longer be in evidence, if it is there at all? Where is the work of philosophy today as we push forward with innovation in our astonishing, leading-edge realms? Do we really understand what we are doing? Do we have any idea where we are going? And, most chillingly, regardless of the answers – does it matter?

The claim is made in this book that the disappearance of philosophy does matter, and alarm bells ought to be ringing. Why? Because the work of philosophy, work we seem to have forgotten, is essential for us to know where we are going. If we are truly serious about surviving and thriving, especially by being so innovative in so many spectacular and challenging ways, we cannot afford to have philosophy and its works disappear and then be forgotten. Said plainly, we cannot deny and then lose the maps and compass of philosophy applied to the challenges of today and tomorrow. If we do, we lose any reason for any journey, anywhere. And, more broadly, we are in danger of losing reason generally.

To continue denying philosophy – and then, in the end, to deny that very denial – is a move with no hope of benefit. But, the lack of evidence for the work of philosophy indicates that move is underway. We are destroying any useful link between innovation and philosophy. In so doing, we are seriously reducing the value of innovation (no matter how wonderful we think it might be) while blindly forgetting the critical importance of philosophy and its work. This move will guarantee that the path to our future will be fraught with unnecessary hardship and difficulty, and then, if it is permanent, will deal a fatal blow.

If we truly wish to thrive and persevere, we are compelled to avoid the fatal error of philosophical denial. To do so, we must rediscover, revitalize and apply anew the rigorous work of philosophy to innovation in our modern era.
ADVANCES IN INNOVATION EDUCATION

AIMS AND SCOPE
Industry, Government-sanctioned Research and development and the Private sectors have historically been the champions of fostering innovation with the aim of addressing changing human needs as well as economic gain. The connectivity of the 21st century coupled with advances in information systems and the unchecked advent of globalization have resulted in challenges to existing institutional structures in place as well as a greater awareness of inequities within and across different regions of the world. Innovation and innovation education are the new buzz words increasingly inundating popular discourses in different media. The aim of this avant-garde book series is to unfold the conceptual foundations of innovation from historical, socio-political, economic, scientific and ethical perspectives, as well as apply these foundations towards issues confronting education, science and society in the 21st century.

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Raising the Alarm

An Examination of Innovation and Philosophical Denial

Robert Andrew Este
University of Calgary, Canada
DEDICATION

First, to my parents, Jouko William and Sara Ellen. They very early on instilled in me a deep appreciation, a sense of wonder, and a profound respect for nature, for science and philosophy and their endless investigation, and especially for the pursuit of knowledge. Their memory continues to show the way.

Second, to my dear wife, Theresa. She has always urged me focus when my mind has wandered, while at the same time encouraging me to fly when my heart had become so heavy I was certain I could not. Without her, this book would not have been possible.

Third, to my students who have taught and continue to teach me what it means to be “switched on” so that it will never be possible to ever switch off. We have shared the journey to engage genuinely in exploration, discovery, reflection, doubting, questioning, and then knowing; and especially, knowing and sharing the joys of learning, never abandoning the quest, learning how to ask good questions, and how to always encourage others to do the same.

Most importantly, to our number one grandson, Jadyn. Without any conditions whatsoever he has always invited me to learn what it means to embrace life, to be fearless, brave, loving and thankful, to persevere and thrive even when the odds seem hopelessly impossible, to push forward regardless, to fail and to succeed, to learn anything at all, to find wonder in all we see and do, to understand and seek even more understanding — and especially, to love without limits. I hope that eventually I might achieve at least a small part of what comes so naturally to him.

Finally, to Ian Winchester, my Ph.D supervisor at the University of Calgary. For much longer than he might have originally planned or ever thought necessary, he has always provided consistent, critical feedback and support, and an open-ended freedom and partnership to think, explore and create. I am grateful for his intellectual partnership and the opportunity to commence and carry out this work, which task I am quite sure will continue for the rest of my life.
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SERIES FOREWORD

Innovation and Philosophical Denial signals the inaugural volume of Advances in Innovation Education, authored by Bob Este. The book is based on Este’s doctoral dissertation completed at The University of Calgary. In a nutshell, the book examines the absence of philosophy in a world inundated and driven by technological and scientific innovation. Does philosophy have a place anymore? The many contexts presented in the book serve as a means of identifying the role of philosophy and vice versa. In other words, instead of philosophical questions arising from the unchecked drive towards innovation, are there cases where philosophical inquiry can propel innovation? The readers holding this book may find answers to these enticing questions.

I will take this opportunity to thank Ian Winchester for chaperoning this manuscript towards its final forms. In addition a warm welcome to the distinguished members of the editorial board that have agreed to give their valuable time to developing the full scope of this new series. Last but not least, I wish to convey my appreciation to Peter de Liefrde for providing Sense as the platform (pun intended).

Bharath Sriraman
Missoula, Montana
02.04.2013
WHAT IS THIS BOOK ABOUT?

In the pages that follow we explore why philosophy, and the work of philosophy, appear to be going through a vanishing act. We do not shy away from pushing the boundaries of pursuing this question, and especially, what we need to do about turning things around.

If only this was theatre! If it were, perhaps we could sit back and enjoy the show. We could engagedly follow the repartee leading to the last punch line. We could feel secure about what happens after the final bows take place, the curtain comes down, the applause fades away and the house lights come up; we could be sure about what we will do after we all head back out onto the neon street. A relaxing and comfortable dinner, with an extended conversation, perhaps?

This all sounds perfectly fine when seen from the perspective of theater. But the vanishing taking place before us is not theatre. What is playing out is very, very real, and is very serious. Philosophy and its work seem to have been disappearing for a while; and, it is argued here, they are vanishing with increasing speed and seeming permanence.

A number of examples of this are provided in what follows. These examples raise the alarm about the seriousness of this vanishing. Today, we cannot live our lives where this vanishing is not taking place. We need to think, and think very well, about what we ought to be doing as a consequence. The vanishing shows no sign of ending – and, in the process, as we shall see later, we are being enveloped by a vast and very seriously dark final curtain. We are not blithely able to escape back to some external world, a world we thought we knew or assumed to be comfortable. If we switch the metaphor to television, we cannot turn it off or change the channel to find a place where the vanishing is not occurring. This is because we are the performance, and the consequences will, as always, define who we are and who we will be.

So, based on this initial complex and alarming claim, the book you have before you takes as its task the exploration and development of fundamental philosophical questions about the role of philosophy in innovation, how the vanishing of philosophy is eating away at our capacities to do new things, and what we ought to do about it. Why focus on innovation? Because this is the term that apparently denotes a realm of newness in so many realms – indeed, it is difficult to think of any realm where such newness does not occur. One would hope that the broad sweep of innovation would embrace a reasonably complete understanding both of what it is and how it works. But the argument here is that we cannot achieve such understanding without the work of philosophy – and if philosophy is vanishing as claimed, where does that leave us with regard to innovation and everything else that we do? Like tail lights of a car rapidly pulling away into the solid darkness of moonless night, philosophy is disappearing before our very eyes; and with this I suggest that our collective abilities to innovate are being eroded to dust.
WHAT IS THIS BOOK ABOUT?

By moving through a number of examples, this book therefore carries out an examination of what I suggest is the growing absence of philosophy – the vanishing act – and then moves towards identifying a potentially useful direction for revitalizing and enhancing philosophical inquiry and thought. All of this takes place against the backdrop of situating what we take to be innovation in the broader context of the work of philosophy. The primary claim underpinning this book is that understanding more of each in the context of the other simultaneously enhances both. And I suggest we cannot do otherwise if we wish to not only survive, but thrive.

The first section of this book provides a very brief high-level overview of two examples of innovation and what we might think of as the “forward march” of progress that can be characterized as being reasonably complex, and, for the most part, has lived on what most would agree is (or was, during earlier times) “technology’s leading edge”. These examples are briefly reviewed from the perspective of the types of developments that took place to achieve them (or, at least, bring them close to fruition). In reviewing these examples, we make the observation that we find little if any explicit consideration of philosophy, or of reflective philosophical thought — and then a question: why would it be that we find the work of philosophy in our achievements (or in our attempts to reach substantial organizational, scientific, engineering, and political goals) is either entirely absent or almost imperceptible?

This sets the stage for an exploration of why explicit consideration of philosophical questions — having to do with rigorous thinking about presuppositions, for example — appears entirely absent from so much of our contemporary enterprise, and, especially, from so much of what we appear to aim to accomplish with what we think of as innovation.

As this exploration is first developed, a speculative question appears: what might have happened if either the approaches to or the steps taken in that development had included philosophical considerations and explicit philosophical thought? That is, we consider the question of whether things might have turned out differently if explicit philosophical work had been a part of combined, leading edge scientific, technical, engineering, and organizational work. This question is examined through some speculative exploration of scenarios where outcomes are sketched that are plausibly different on account of what might have taken place if philosophical thought had been explicitly incorporated.

This is a difficult and highly challenging task. Serious consideration of this formative question in essence creates an argument in support of claims based on the negative example. This is akin to tacking against the wind. Rather than pointing at apparently reliable evidence of what we agree is, or what was, we are pushing ahead (some would suggest it is “pushing sideways in order to move forward”) to think about what might have been. Empiricists both living and dead might spin like high-speed lathes at the thought of such a prospect. Alternative histories, usually considered to have their home in fiction, can be entertaining and might even help us think more clearly about antecedent variables that may not have been previously considered; and, when focused seriously on our thinking about the future, they may
WHAT IS THIS BOOK ABOUT?

occasionally help us map out plausible routes to what we think are desirable outcomes (Schwartz 1990; Brand 1992). Fiction in science and medicine, for example, is built on our capacity to invent and maintain things and concepts in our imaginations that can arguably help shape our thinking of where our world will next be developed (Petersen et al 2005).

But, how can we take seriously any thinking about things — including a range of alternative processes and outcomes that quite simply did not happen — especially when our best historians and our most trusted and secure compendia of reliable information that we denote as facts about the world provide for us the clearest story, the sharpest possible picture, of what “in fact” took place? If philosophical considerations or explicit philosophical thought is not in much (if any) evidence when we explore the record of what appears to have taken place in the realm of innovation, for example, how can we then defend or make use of the notion of what might have taken place if such philosophical thinking had ever occurred? This very serious challenge illustrates the utility of and reason for philosophical thinking; and, founded on innovation examples that are briefly reviewed in what follows, reflections on this sort of challenge wrap up the first part of this book.

We then move on to a somewhat finer level of detail. This takes place in later chapters that provide overviews of two more examples of innovation environments, projects and processes in the realm of contemporary leading-edge science. They are based on the author’s many years of work in both inventing and shaping, and also extensively co-managing two projects described. In essence, these two examples are founded on direct personal evidence and are offered here based on the argument of the strength of rationally justifiable representativeness (Guba and Lincoln 1981). Although eyewitness testimony is often subject to serious question (Loftus 1974; Tversky and Kahneman 1977), it can nonetheless have a very strong influence on juries’ deliberations, for example, and if carried out rigorously to avoid errors of misinterpretation as best as possible, can aid in filling out and adding value to what is generally thought to be the acquisition of a reliable and comprehensive picture. In this sense, the experiential examples are meant to add value through the critical descriptive and interpretive methodology applied in any case study (Yin 2009) and especially based on the procedures of synthesis and interpretation of “thick description” (Geertz 1973).

A connection is made between these examples in the following way: the two personal work-based examples in the second section are specific to the general case that is provided in the first. They thereby add to the illustrative power of the range of examples provided here. In this way, the absence of philosophical considerations and explicit philosophical thought in these examples is further revealed. Additional reflection regarding the challenge of this absence, or, if you prefer, the puzzlement of why this might be so — is offered.

We then move into the realm of complex adaptive learning systems, the conceptual home to all the examples provided (as well as those that are included later to
emphasize the point). How such systems and individuals in them do or do not learn, adapt, and shape their environments to survive and thrive is explored. Against this backdrop, the reasoning we appear to carry out to do our best innovative, scientific, and organizational work is further examined. Ideas are introduced addressing why conceptual slippage — for example, mistaking one form of reasoning or one framework for another, or coming to a conclusion or ignoring important aspects of what we are dealing with — can lead to powerful philosophical denial.

In the chapters that follow, further examples of absence and the denial of philosophy are provided, including work and thinking in the fields of quantum mechanics and the holonic enterprise. How innovation and organizational policy are understood also adds to our understanding of the emergence of such denial. Each example reinforces the motivation to determine why philosophical denial might be as common as it appears to be. This leads to consideration of the reasons for denial of evidence and reason in the working of religious faith. Faith in reason and non-reason is examined.

The final sections of this book suggest that the denial of philosophy may be a plausible indicator of slippage into non-reason, where the hard conceptual work of rigorous philosophical consideration is regarded as not only unnecessary but even threatening, or just plain unknowable. These are emerging conditions under which philosophical work is removed from consideration. Having developed a foundation for the final argument that the denial and loss of the work of philosophy is not desirable because it so severely limits reason, the book concludes by suggesting that philosophical awareness and the work of philosophy must be revitalized to eliminate or at least minimize denial. Additional rigorous investigation into the dynamics of philosophical denial — essentially, the pursuit of more focused and practically oriented philosophical work — is necessary, and is argued to be required in an absolute sense. The final statement of this book rests on the notion that to ignore the denial of philosophy is intellectually dishonest and morally reprehensible, and to perpetuate this denial through intellectual metadenial is fatal.
PROLOGUE

Let us examine an initial perspective on the work of science and the work of philosophy. We appear to have evolved a commonly held view that our products, processes and methods of thinking in science — including what we think of as innovation and the ways in which scientific work is organized and accomplished — are not only significantly different than the methods, processes, products and thinking of philosophy, but that they are vastly more preferable and desirable. Supported and nurtured by the best of our organizational efforts, we have learned well how to use and advance innovation and science. The rapid proliferation of benefits derived from the methods of science could not take place without innovation — the shaping of new ideas and things into novel products and ways of accomplishing what we see — being deeply integrated therein. In combination, innovation and science tangibly and reliably enrich our lives and our world (although most would admit this happens with a range of unanticipated outcomes and undesirable costs).

Where is philosophy today? Philosophy appears to many to have seen better days. It seems, at best, an aging handmaiden to science, or perhaps a poor and crippled stepsister; at worst, it is often seen as more of burden, a distraction, a waste of precious time and energy. Some who see philosophy this way claim it is useless, or a pleasant gloss on the real work of science (Weinberg, 1992; Feynman 2005); and, as we shall see, some even claim the extreme view that philosophy has finally expired and is no more (Hawking and Mlodinow 2010). Commenting on those who carry out philosophical work, Dyson (2012) observes that

“... [c]ompared with the giants of the past, (philosophers of the twentieth and twenty-first centuries) are a sorry bunch of dwarfs. They are thinking deep thoughts and giving scholarly lectures to academic audiences, but hardly anybody in the world outside is listening. They are historically insignificant. At some time toward the end of the nineteenth century, philosophers (and, presumably, philosophy itself) faded from public life. Like the snark in Lewis Carroll’s poem, they suddenly and silently vanished. So far as the general public was concerned, philosophers became invisible.”

And so, we can say, did philosophy itself. It may be the case that philosophers and the work of philosophy have become invisible, and it seems true that some claim that philosophy is useless or no longer exists — but others think differently. Collingwood (1936), Russell (1960) and Shapin (2008), for example, think that the work and value of philosophy are, today, what they always have been — essential for if not crucial to exploring the locus of unsolved (or perhaps even not-yet-identified) problems. However, when contrasted with the advances and outputs of science and innovation, philosophy as an approach to challenges or problems that are unclear or not yet well defined is increasingly seen by many to be of little if any value, if it seen at all.
Let us place these observations in context. We can state at the outset that we already have plenty of challenges that are both quite specific and extremely demanding, if not overpoweringly so. Most of our days are filled with such challenges. Compared to today’s methods and outputs of innovation and science and the organizations that permit such specific challenges to be addressed and valuable outputs to be achieved, philosophy (when it can be perceived, or if it ever enters consciousness) seems to contribute little. Today’s science and innovation and our ways of achieving their highly valued outputs and coming to their undeniably important accomplishments are often seen to be far more important and relevant than philosophical thinking, work and outputs — if such work and outputs are even considered. What, then, does philosophy do, if anything? The answer seems to be: not much, when the above comparison is made — indeed, in the current era, philosophy’s contribution seems almost infinitely small.

So, somehow — perhaps based primarily on what is collectively held to be the perceived worth of output — it has turned out that the thoughts, work, impacts, overall role and even the contemporary societal impacts of philosophy are today valued far less (perhaps infinitely so!) than what innovation and science seem to provide in all their realms, as we now understand them. Science and innovation today have a powerful and measurable ROI (return on investment), and have ascended to the most prominent and most favoured spot at the king’s table. If it is ever remembered today, philosophy no longer has even an old, cracked bowl that has been lost somewhere outside the back door of the servants’ kitchen. Although we might find reviews, reflections, summaries, and relatively meaningful stories about the earlier significance of philosophy that in its heyday helped us create, set useful directions for, and then boldly stride up the path to modernity, it seems that contemporary philosophical thought, awareness, and work, and thus the relevance of philosophy, have all but disappeared. Any work of philosophy is not in general circulation in a manner similar to the work of science and innovation.

This may be so; but why would this be? Is this a natural state of affairs — an inevitable result of how our contemporary thinking creates conceptual consistency (Ajzen 2002) while we have worked so hard to shape our modern work in the realms of science and engineering, of politics and our organizations (Kelly 1965)? Is this the course of events that must inevitably lead to the diminution and even the disappearance of philosophy from our world? Could this state of affairs have been different, and if so, would that sort of difference have made any difference (Bateson 1979)? Are science and innovation and the shaping of our organizations to move these fields forward so vastly overpowering in their impacts that nothing can compete? Is this why, in the current era, philosophy is relegated to the status of a “forgettable footnote”, something that philosophers address with other philosophers in closed-off rooms which, for the time being, the rest of us deem to be fine, as all right — just so long as whatever it is philosophers do back there doesn’t interfere with the self-shaping march of science and innovation? Why, if philosophy is remembered at all, is it simply a term for many that denotes a distant and perhaps pleasant if not
vaguely puzzling past — but has little if anything to do with our present goals, our
achievements, our ways of thinking about and shaping the world, and especially, our
ways of thinking about what is important? How should we deal with the challenge
of figuring out the relationship between innovation — that which we seem to value
so highly — and philosophy, the latter of which seems to be disappearing before our
very eyes? Should we even bother?
PREAMBLE

The pursuit of science, of innovation, and how we shape and run our organizations to promote these things can be viewed as a useful example of what we do well to achieve epistemic clarification and positive epistemic value — that is, we work hard and to the best of our abilities to clarify what we know, and in the same breath, to know what we clarify in order to achieve our ends. All of this is based on understanding. We are not much different from our forebears in this regard— as Plato observed, we are driven to understand — the drive to understand is “in our nature”.

As we have evolved and developed in scientific and philosophical realms, we seem to have come to our present status in order to build and hold what we take to be reliable and trustworthy accounts and beliefs about the objects of our attention, and thereby stand on what we conclude is the best foundation we can thereby muster. We do this with most of our other pursuits as well, as mentioned above, including science and innovation, including our organizations, through which we attempt to understand and run our efforts at effective policy — the formalization of best mechanisms and strategies for allocating resources and achieving our goals (Downey 1982). In all these realms, the goal of epistemic clarification is to achieve what we believe to be a reliable knowledge foundation, which we then employ with the goal in mind to optimize our decisions and improve our science, our innovations, our organizations, the outputs of our efforts, and thereby our well being. This fabric of our most reliable methods to building knowledge, understanding, and achievement is, of course, woven with the strands of what we hold to be reliable beliefs.

The evolution of science in concert with philosophy has been has been mapped by Collingwood (1960) and Russell (1996) from the time of the early Greeks to the first three decades of the 20th Century. The scientific method as we now understand it has gradually evolved as a strategy for rigorously assessing what we observe about the world, incorporating and comparing our observations with what we have already learned about that world, developing and testing hypotheses to verify or disconfirm what we think we have observed and thought about that world, and through this process of clarification, developing and establishing optimally reliable explanations and theories. When we carry out rigorous investigations in this manner, this leads us to think we have acquired enhanced knowledge of the objects of our attention in particular, and to feel both reasonably secure in that knowledge while at the same time recognizing the challenges offered by those things we do not yet know, or things of which we are not yet reasonably certain. Through this process of exploration and clarification, we then establish and hold reliable beliefs about what we take to be true in the world, to the extent that this ongoing process continues to confirm and help us understand what we discover, test and learn. This approach seems to have been very successful on all fronts where it is applied, in particular in the rise of science,
and this application seems universal. We have thus learned to reliably apply the scientific method to almost all that we investigate and do. Our innovations appear to be founded on this approach to exploration and knowing, and our organizations are built to support such rigorous processes of exploration and inquiry.

The underpinnings of the pursuit of science as it is addressed here are generally applied to a wide variety of our endeavours. Indeed, it could be argued that we would not be able to successfully carry out most of what we do in the current era without the application of the methods of scientific inquiry as an expression of how we embrace, stand on and employ rigor to varying degrees of efficacy as we seek understanding of what we investigate and what we take to be more or less reliable knowledge of those things. This method is founded on epistemic clarification and thus the achievement of what we interpret as epistemic value — one might say it is founded on achieving value based on optimizing the processes of reliably knowing, and reliably knowing that we know.

Therefore it is no surprise to see major scientific projects founded on this approach to highly practical epistemic clarification, although — importantly in terms of what is addressed in this book — this may not in any way be an approach that is consciously revealed or in evidence. In other words, we may regularly engage in epistemic clarification to achieve what we have identified as our goals, but we may not be reflective about or aware of this mode of clarification. Regardless, it seems that any good inquiry and in particular good science is founded on this approach; conversely, bad science is not founded on this.

Against this backdrop, we now move on to Chapter 1 — an exploration of some representative examples of innovation achievements that allow us to commence exploration of a plausible model of innovation diffusion, and begin to focus on the fundamental questions addressed in throughout this book.
CHAPTER 1

BACKDROP WITH QUESTIONS

This first chapter introduces two examples of relatively recent innovation achievements — or, at least, powerful forward steps that could have brought us to such visionary achievements if circumstances had not evolved in the way they did. In each example briefly provided in this first chapter, the following is illuminated: [i] a vision to achieve something is identified, agreed upon, and made reasonably clear; [ii] the technologies and processes required to achieve that vision are thought about, some are brought into alignment and new ones developed, and then they are applied; [iii] the organizational constraints that define how those technologies and processes are to be researched, developed and made real are specified; and [iv] in the complex network of practical, political and organizational decisions about how to achieve the vision through such steps, at some point the project itself (or the steps being taken to achieve it) is altered for any number of reasons, and the original vision is never realized. Finally [v], any sub-benefits of steps [ii] and [iii] are extracted from the unrealized vision and differentially applied for purposes other than reaching the original vision.

1.1 Nuclear Power

In an interview regarding the science and engineering of advancing early nuclear reactor technologies and bringing the concepts into the development cycle to reach productivity quickly, Freeman Dyson is quoted by Ellerson (2009) as saying

“… [i]t’s much better to make mistakes early and learn from the mistakes than to try not to have mistakes at all. If you don’t allow people to make mistakes, then you don’t allow anything new.”

This is a significant observation. Dyson’s point here is that innovating in technological realms of any kind requires a freedom from overly restrictive regulation (and presumably overly-restrictive thinking) in order to achieve the vision originally supporting the new idea. With projects that are highly complex with leading-edge research and development, suggests Dyson, mistakes will be made. There are risks with unanticipated consequences emerging from mistakes especially if the technologies are extremely complex and have inherent dangers, as with nuclear power. However, Dyson is suggesting an intelligent, open-minded, very well-informed balance between “pushing the envelope” on novel research in order to achieve output objectives relatively quickly and in so doing permitting the necessary
research, development and innovation (RDI), versus being overly cautious and restricting and slowing down the innovation process to the extent that the product of that process is (potentially) never created and thus the benefits of the original vision are never realized. Dyson is not suggesting throwing caution to the winds; he is suggesting that we should realize innovation in new and challenging circumstances by applying the best of our intelligence to meet those challenges without being so cautious that the entire innovation process unnecessarily grinds to a halt.

Another way of thinking about what Dyson addresses here is the following: if an original vision powers an innovation process designed to bring that vision to fruition (or product to market), and if the process turns out to have increasingly complex challenges and features that were not initially anticipated such that those challenges and features become more important and more powerful than the original vision, then it could be very useful to revisit (and hopefully reformulate) the full scenario of the original vision, what was understood and anticipated as the routes and methods that would be followed to achieve that vision, and what has now been learned about those things that were not originally anticipated. The goal of such reformulation would be to find new ways of achieving the original vision (presumably to be more in accord with the original time-line and anticipated expenditures), rather than continuing to focus exclusively on and extending the time and the new efforts now being spent to deal with new complexities (which, without conscious awareness, essentially redefine the goals and solution processes so that they replace the original vision).

Dyson’s point with regard to the nuclear power question is that we now possess the necessary expertise and the scientific and technological savvy to make such complex products available in a relatively short time, and thus immediately support the original objectives of economic development plus meeting other usage needs. He also argues that environmental and public health concerns, although real and legitimate, are often subject to political manipulation that can distort public opinion about safety, for example, and that such distortion can have powerful effects on strategic plans for siting, implementation, security, and use (including eventual decommissioning).

However, if we become “hamstrung” with and spend increasing amounts of time and energy on what he characterizes as overcautious regulatory overkill, both the longer-term dollar costs and the overall time delays in bringing the desired innovation into useful production mean that the project fails because wrong thinking about innovation, and its contexts, costs and benefits has taken place. As he observes, rather than taking two years of development, the time spent balloons out to 20 years or more. Dyson’s argument is that the type of thinking that results in the condition of overcautious delay that permeates the original project and kills innovation and its desired outputs should be changed for the better.

One can therefore ask a number of interesting questions here: if we reflect on this example, what can we learn about improving our thinking about how to innovate more effectively? Is it simply a matter of better understanding the risks
of research and development in the realm of complex technologies, some of which may include important dangers if mistakes are made? Or is it a matter of better understanding of the cost-benefit ratios of fast action versus caution in a research, development and innovation context that we feel we understand? Is an obstacle to sensible, rapid innovation that we think mistakes are to be avoided and are seen as an unacceptable cost, rather than inevitable and providing learning opportunities and therefore improvements of process and benefits of product? Or are there other aspects of the complex web of new technologies that could be more sharply and perhaps definitively developed (with unanticipated spinoff benefits?) to alter the overall technological picture and thereby improve our understanding of actual risks — essentially “carving up” the project terrain into smaller challenges and tasks that have the largest effects on how we view the entire terrain of innovation? We can also ask if there are aspects of the innovation terrain, including the technologies and the political contexts, about which we are making restrictive assumptions when such assumptions are not supported by evidence. If we are dealing with unknowns (and it seems we most certainly are in the example that Dyson addresses), what are we presuming about what we don’t know?

It is important that we recognize that the questions related above are both scientific / technical, and philosophical in nature. That is, they are based, on the one hand, on the technical and scientific knowledge we have acquired, about which we tend to feel quite confident and sure. They also, on the other hand, address things about which we are not quite so sure, and where our understanding is not clear; they address both what we think we understand and what we know we understand, and they are exploratory and reflective in nature.

This mixture of what we think is clear and what we are not quite so sure about defines the full challenge terrain addressed by Dyson. When faced with such a mixture (which we can suggest is the usual state of affairs in almost any context), we tend to devote our focus to those things we think we know reasonably well — in particular, the technical and the scientific. In the current era, these are the realms where we tend to have relatively secure knowledge. The more philosophical questions, ones that address more general problems that are not so well defined, are not generally addressed. From the point of view of wanting to come to the “right”, “safest”, “most knowledge-based” decisions about nuclear power, for example — in particular, those residing in the technical and political realms, which are usually the realms where we focus most of our thinking and energies — we tend to rely on what we already know and believe we understand. If we are faced with less well-defined questions about things that we might agree are important, we still tend to focus, return to and rely on what we already know. The approach to decision-making regarding what to do, what sorts of risk to take, and thus the shaping of innovation direction, is therefore more aligned with what we think we know in our technical and scientific realms rather than questions about things we aren’t sure of, questions that are more open-ended, questions that tend to be more philosophical in nature.
1.2 Supersonic Transport

The recent history of commercial supersonic transport (Flight Archive 1969 [no author]) provides a perspective into understanding innovation and philosophy that is slightly different from that of the nuclear power story illuminated by Dyson. It has a stronger emphasis on international politics although concerns, ill-informed or otherwise, over environmental and public health impacts are comparable.

The vision for viable supersonic commercial transport emerged through the 1950s as research, development and innovation in the aerospace industry, especially in France, indicated that increasingly reliable technologies of supersonic flight were at hand. Based on mid 20\textsuperscript{th} Century technologies, visions of what sorts of commercial aircraft might be designed, built and put into service began to emerge. The politics of which countries could be the leading contenders in the competition to put supersonic commercial transports into the air travel market meant that the innovation capacities in necessary scientific, technical and engineering realms were identified and supported by those national governments (France, Britain, the US, and [at the time] the Soviets) already having the antecedent industry clusters capable of transforming such a vision into a viable product. Global economic conditions addressing commercial air travel from the 1960s and projected from that time into the 1990s suggested that the costs of developing and then operating a supersonic transport fleet were justifiable. We should note parenthetically that plans and projections made in the early and mid-1960s did not tend to include scenarios such as the oil crisis and the potential for critical, long-term global financial instabilities that would spill over into the 21\textsuperscript{st} Century.

Against the backdrop of international competition and combined political and economically determined alliances, the combined Anglo-French aerospace industry cluster successfully met the technical and engineering design and production challenges of the day, and brought a commercial supersonic product — the Concorde — into what primarily became the trans-oceanic air travel market. At the same time, the American aerospace cluster encountered a series of somewhat different competitive challenges and delays in the design, environmental impact, and the emerging fuel economy realms. Where the Concorde was put into service as a technically successful product generated from what evolved into a “forced marriage” of innovation initiatives between Britain and France and entered the market as a finished product just as fuel prices began to skyrocket, the American competitive market populated by the major North American aircraft manufacturers created design and implementation delays that permitted a stronger environmental lobby to seriously question both the utility and viability of a supersonic commercial domestic aircraft. The investments made by Boeing in winning the competition meant that company ran head-on into the rapidly changing circumstances of ever increasing operating costs and increasingly strident over-land environmental and public health impact criticisms and objections. The Concorde, as the successful technical product
in the global competition in the commercial supersonic transport sector, ended up occupying a relatively small market niche for elite travellers and was finally removed from service after a series of accidents, and similarly increasing operational costs and maintenance issues after years of service; the American SST was never brought beyond a middle-range design stage before government support was withdrawn and the reality of an inevitably unsuccessful economic return was obvious.

The technological innovation dimension of the commercial supersonic transport saga has resulted in significant improvements to aerospace technologies beyond the original SST vision. For example, the supercritical airfoil design was a major part of the project and has powerful effects on low-speed control as well as high-speed (but subsonic) fuel efficiency and quiet. This innovation has now been generalized to all modern subsonic aircraft, and is a specific example of the general case of the “spin-off” benefits of technological consequences of innovation resulting from an original visionary project that is, itself, unsuccessful.

Interestingly, however, we can return to the line of thinking about philosophical considerations brought to light with the first example about nuclear power provided earlier, as illuminated by Dyson. In the example of the commercial supersonic transport, we can see that a complex array of technical, scientific, engineering, and political variables were at play. These variables determined the course of events over decades that moved a concept to a contextually-driven vision, the vision into the realm of what was technically feasible and politically possible, from there into the realm of financial resourcing and business plan articulation that through obvious necessity over time had to be dramatically adjusted to accommodate large-scale global political changes that completely altered the fundamental economic terms of reference that could determine the viability of the entire project, to the final range of possibilities – and actualities. This entire process over many decades determined that the original vision of commercial supersonic transport might remain bright at a relatively high level of abstraction. However, when articulated into and through the intertwined technical, scientific and political realities that evolved over the same period of time, the translation of that vision into a sustainable product became impossible – for what we can understand as combined technical and political reasons.

In this section we have very briefly described a significant innovation process that lasted many decades, cost vast sums of money, affected some of the planet’s most important economic clusters and political alignments, and arguably affected millions of lives. Where, in this mix, as with the example in 1.1 of nuclear power, do we see explicit mention of the role and consequences of philosophical work? Such mention does not exist. Why is this the case?

1.3 Summary

The two brief examples in this first chapter have been provided to illustrate a plausible beginning model of innovation diffusion, where the complex of innovation
processes is differentially adapted and modified given a complex evolving and emerging network of technical, political, and organizational constraints.

It is quite clear that in reviewing the brief accounts of these two projects, at no stage of the scientific and engineering work carried out in the examples as they have been related — whether they achieved their original vision or not, or whether unanticipated problems or benefits resulted — do we find any evidence for magical or wishful thinking. This, we are most certain to agree, is a very good thing: excellent science and engineering and the very hard work necessary to carry them out could never be based on the wishes to achieve the goals of such projects. Designing, building, and flying supersonic aircraft or planning for and running nuclear reactors had best not be based on what we simply hope will happen.

The more important question that closes this chapter then becomes: in these examples, have we encountered any evidence for processes, effects or results of deliberative and explicit philosophical work? We can ask this question because neither this work and results of science and engineering nor that of philosophy contain magical or wishful thinking. Simply put, they cannot; if they did, they would not be science or philosophy. Good philosophical thought is as careful, disciplined and as rigorous as the best scientific thinking we can muster.

And yet, it seems we do not find evidence for explicit reflective philosophical thinking or its outputs in the two examples of leading-edge scientific and technical innovation that we have briefly recounted. Why is this? If we agree that magical and wishful thinking are not a part of the process of scientific innovation, for example, do we have a similar clear understanding and agreement that philosophical thinking should similarly be excluded? Is philosophical thinking in some way as silly or ill-founded as wishful thinking and magical fantasy? If we understand and know why magic and wishing should not be a part of the very serious projects such as those identified here, do we have a comparably clear understanding of why philosophical thinking and philosophical work would also be kept out? In focusing exclusively on the technical, political, and organizational components of innovation as it relates to the advance of science and the economy, is it possible that we have committed a grievous and potentially fatal philosophical error by blinding ourselves to the importance of the work of philosophy when we engage in the innovation and work of science, engineering, and politics?

In Chapter 2, we will begin the move to explore the author’s personal work experiences in leading-edge science as more proximate examples of demanding and rigorous enterprise that also appears to exclude explicit philosophical work. But before we do, we can raise a question that at this stage of the book that can only be defined as formative, as yet having no definitive answer, namely: if we were to re-think the variables contained in the technical, political, and organizational mixes that have begun to help us understand the innovation process, would explicit philosophical considerations have made any difference to the outcomes of these projects? And if so, how could this happen?

Let us reflect on these formative questions and move on to Chapter 2.
NOTE

1 Dyson (2006) clearly relates a semi-autobiographical story of working as a young man on combined tactical and strategic planning for Allied bombing runs flying from England over Germany in the Second World War. In his story, not only the decisions, but entire world views of his superiors were unfortunately based on self-reinforced incorrect assumptions that could only have been founded on what was arguably very bad philosophical thinking. In Dyson’s story, care and rigor were not applied to the factual information about the runs, and similarly not applied to underlying assumptions about that information, how it was achieved, and how it was used. Dyson’s story describes what can be thought of as magical and wishful thinking on the part of those who were responsible for thousands of Allied airmen’s lives. Dyson provides a clear example of where adherence to rigorous philosophical thought, carefully incorporated with technical, scientific and strategic military thinking, would have saved lives, reduced suffering, saved money, and potentially changed at least some outcomes of the war.
CHAPTER 2

A BACKGROUND TO AWARENESS

To this point we reviewed a range of variables in innovation based on leading edge science and engineering that met, or did not met, their original visions and goals. Through brief recounting of two examples, we considered aspects of their technical, political, and conceptual terrain. This has suggested that explicit philosophical thinking has generally not been a part of such innovation pursuits. In this way we can begin to consider whether philosophy appears to be regularly excluded from innovation.

This second chapter moves to the specific and personal example of the recent creation and activation of the Institute for Biocomplexity and Informatics (IBI) at the University of Calgary. This is the author’s personal experience. It is a story meant to describe the process and experience of putting a novel scientific institute together in a “classic” academic environment, and illustrates from a personal perspective that recent broad and deep experience at the leading edge of contemporary scientific investigation and innovation through the establishment of a novel scientific institute explicitly revealed nothing systematic or substantial about the philosophical underpinnings of this enterprise, or how serious consideration of philosophical issues or questions could ever have been an explicit component of the creation, implementation and growth of the IBI.

Coming to this realization based on this experience provided a strong motivation to explore why it seemed to be the case that philosophy in general or epistemic clarification in particular did not seem to be very much in evidence in the world of emerging science and innovation. This was the cause of much questioning and, indeed, some angst.

I should point out that in beginning to ask serious questions about the place of philosophy in the realm of innovation, I was greatly inspired by Collingwood (1960) who notes,

“[b]efore the nineteenth century the more eminent and distinguished scientists at least had always to some extent philosophized about their science, as their writings testify. And inasmuch as they regarded natural science as their main work, it is reasonable to assume that these testimonies understate the extent of their philosophizing. In the nineteenth century a fashion grew up of separating natural scientists and philosophers into two professional bodies, each knowing little about the other’s work and having little sympathy for it. It is a bad fashion that has done harm to both sides, and on both sides there is an earnest desire to see the last of it and to bridge the gulf of misunderstanding it has created. The
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bridge must be begun from both ends; and I, as a member of the philosophical profession, can best begin at my end by philosophizing about what experience I have of natural science. Not being a professional scientist, I know that I am likely to make a fool of myself; but the work of bridge building must go on.”

Having begun to think about the apparent absence of philosophy, and as I contemplated the possibility of “harm and bad fashion” coupled with the plausible need for bridge building between science and philosophy, I decided to explore this terrain to the best of my abilities. I therefore move forward here by relating the story of the Institute for Biocomplexity and Informatics not so much as a case study but as a review of a complex constellation of circumstances and events that caused me to puzzle ever more deeply over the bases for how we appear to think about science, innovation, organizations, and in particular, how philosophy fits into such thinking, if it does at all.

It is this story upon which I construct the later sections of this book where I raise and explore what I suggest are significant philosophical questions that hopefully can guide us to avoiding further harm and help to build Collingwood’s bridges. This then sets the stage for practical suggestions about embracing philosophical inquiry in the worlds of emerging science, innovation, and organizations in general.

2.1 – The Example of the Institute for Biocomplexity and Informatics

One afternoon in the first Winter weeks of January of 2004, from my home office in the shadow of Mount Rundle in Canmore, Alberta, I gave a “cold call” to theoretical biologist Stuart A. Kauffman who was then living in Santa Fe, New Mexico. He did not know me, and I did not know him. Kauffman had achieved some fame for his early work on Boolean networks, explorations of criticality, what is called “the inference problem” in Biology (attempting to infer network structure and dynamics from experimental data), and especially puzzling over problems of emergence, about how life came to be, and “what comes next”. Plus, he had successfully worked in applying complex adaptive system principles to business.

Based on what I knew of his story, I invited Kauffman to visit Calgary, Alberta, to be a catalyst, the main speaker — a speaker of some interest, reputation, and perhaps even providing some gravitas — in three local venues with which I had become increasingly familiar at the time, and which had an interest in the implications of novel, leading-edge science: academe (through the University of Calgary), business (through some major oil companies based in Calgary), and arts and management (through the Banff Centre, in Banff, Alberta). The invitation was conditional upon my being able to raise sufficient funds to cover the costs of travel and accommodation, and perhaps an honorarium — but I needed Stuart Kauffman to say “sounds good” before I began soliciting funds. He did; and so began my initial task.

There was a method to my madness. Kauffman had for years contributed seminal concepts to the field of complex adaptive systems (CASs), adding much to thinking
about emergence and evolution including the NK model in Biology (Kauffman and Weinberger, 1989) apparently generalizable to other fields. Indeed, two years later, he was to give the 2006 Herbert Simon award lecture entitled “Emergence” at the annual International Conference on Complex Systems (ICCS) conference (NECSI, 2007).

I had been working on the germ of an idea, bubbling away in the back of my mind, an idea of connecting with a scientist of Kauffman’s caliber and perhaps having that person serve as a focus for initial discussions about complex adaptive systems and their scientific, economic, and societal implications; and, if good fortune was to smile — with the addition of longer-term strategic planning, and acknowledgement of all the hard work that would entail — I thought that exposure through Kauffman to interesting ideas about complex adaptive systems (especially in Biology) might conceivably catalyze thinking among others in the Alberta venue about similar things so that new emerging science could be explored, and positive effects on the shaping of some aspects of medicine and perhaps even long-term science policy might result. I thought that this might formalize and help establish something truly new, interesting, different, useful, and even broadly beneficial in, and for, Alberta — to collaboratively lead exploration of the new field of Systems Biology.

As the last days of January 2004 winked out of existence, what that something could be was unknown. But the unknown can hold much promise. And I acted, thinking that taking risks by stepping into and exploring the unknown — proactively creating the future, on the fly — is necessary if interesting things are to be discovered, opportunities uncovered, and new achievements accomplished.

I knew very well that my thinking and initial steps to initiate exploration had precedent in the complex adaptive systems literature. I had read many of Kauffman’s Santa Fe Institute preprints, articles, and his books of the time — Origins of Order (1993), At Home in the Universe (1996), and Investigations (2000), for example, and along with many others by similarly thoughtful and creative people such as Prigogine (1985), Waldrop (1992), Lewin (1992), Holland (1998), and Casti (1995). I had been (and still am) very interested to learn whatever I am capable of learning about complex adaptive systems and especially the very interesting place that so many initial writers in the field denote with the term “The Edge of Chaos” — posited to be a critical phase transition in state space somewhere between reasonably predictable stability or even moribund inaction on the one hand, and utter chaos on the other where, apparently, systems (which we here assume spend most of their energy to compute in order to accomplish their work) do the very best job of computation with whatever it is they compute — where “very best” can mean a variety of things, such as “most efficient”, “most creative”, “most productive”, “most original”, and “most adaptable”.

It seemed plausible, and it was a very powerful early intuition to many, that rapid adaptation and learning and, therefore, achievement of competitive advantage and much innovation, lived on or somewhere very near this “Edge of Chaos”, and that this might apply to many types of systems. Even though we must be very cautious
about such things, intuition can often prove to be beneficial by pointing a possible direction for the development of good science. Even Tom Peters (1987) had started to wring a living out of the term which was rapidly finding its way into the management literature (Reed and Hughes, 1992) as well as much more broadly into the scientific (Strogatz, 2004); and, John Brockman (2009) creatively sliced the first word out of the term, boldly claiming it to describe his creation where he could organize and distribute the thoughts and writings of some of the most creative, inventive people on the planet.

What an exciting and promising place this edge appeared to be! What a challenge to learn more about and understand this phenomenon! What could we determine from studying the histories, behaviours and living actions of organisms and organizations that struggled — and evolved, failed, or succeeded to live another day — to survive and thrive, effectively partnering with other successes and on the way eliminating those who could not compete? Could we learn from the examples around us so that we, too, might live and dance more effectively on that edge (Mitchell, 1996)?

I dreamed of applying to organizations any underlying principles of “the very best job of (generic) computation”, of exploring that critical phase transition, of living on “The Edge of Chaos”, so they could become more productive, more adaptive, more innovative, less moribund, and skate along the edge but never fall into chaos. My first focus and motivation: leading-edge science in the realm of Systems Biology as an engine of societal innovation and benefit might be very well engineered and benefit strongly by closely studying and then taking such an edge into account.

This thought had come from the synthesis of a great deal of “breadth” reading I had been doing in the fields of Biology, Physics, Astronomy, Engineering, Medicine, and Computing Science — and other fields such as Economics, Management, Psychology and Anthropology. I was interested to learn what I could about what scientists, clinicians, inventors and practitioners from a wide range of fields were putting on the table in terms of new concepts, new ideas, new developments, new insights, and new directions, all of them up for review. I wanted to learn about their workspace, social, intellectual, emotional and communication efforts, ways of dealing with the unfolding of emerging new science, and how they moved their knowledge products forward, often aiming at commercialization. I had inferred that such advances were most likely reliable indicators of what might come about in broader societal and economic contexts — and if not “what”, then “where”. As Steve Jobs of Apple Computer was wont to say, we might in this way have signs to “the next insanely great thing” (Wolf, 2007).

But I interpreted Job’s words to apply well beyond what was to shortly emerge as his string of “iThings”. I was paying close attention to writers like Christensen (2003), Christensen, Roth and Anthony (2004), Utterback (1994), Hesselbein et al (2002), and Iansiti (1998) among many others who were making interesting comments about innovation writ large (especially against the backdrop of rapidly growing technologies), what it takes to innovate more, how to do so more effectively, and what problems often befall organizations when they innovate or attempt to do
so. The scientific research fields of Biology, Physics, Chemistry, Medicine and Computing seemed to be particularly active and fruitful, moving forward rapidly in many directions and on many broken fronts and, especially, those fronts were notably interactive — with the Institute of Electrical and Electronics Engineers (IEEE), for example, starting a new journal on Systems Biology (IEEE 2004) and new physical entities such as the Institute for Systems Biology (ISB) (2005) and Harvard Medical School’s Systems Biology Department (2006) rapidly taking shape with good funding, good business and research plans, good equipment and facilities, and especially, the pulling together of very, very good people.

While thinking about the “Edge of Chaos”, I also began thinking that, in some combinations, these vibrant fields, unfolding and labeled with increasingly flexible interdisciplinary meanings, might provide very fertile grounds for opportunity — both in terms of emerging new science itself by way of Systems Biology, and in terms of potential economic yields from the development of such new science.

I also learned of the funding agencies in the Province of Alberta. In accord with the four guiding “Provincial Priorities” in place at the time of energy and environment, health and medical technologies, agriculture and forestry, and information and communications technology (with nanotechnology as a fifth to follow shortly thereafter) (2006), such agencies received and reviewed research funding proposals from scientists, engineers, and medical specialists of all stripes here at home, or went further afield to recruit some of the planet’s best and brightest to come to Alberta to establish what one might call a “critical presence” and comprehensive strategic research and action in their fields of expertise — fields that, from the perspectives of elegant design of and impact on research, development, and economic growth, were (and are) aimed at being central to the engines of leading edge research, development, innovation, economic diversification and well-being, health and wellness, and business productivity.

Against the backdrop of these factors, a convergence of events unfolded. Some events were engineered locally; others were pushed forward by much larger forces. The emerging multi-faceted, interdisciplinary field of “Systems Biology” was nascent but beginning to burst at the seams, budding forth and being recognized on many university and science policy radar screens around the world, hammered out in a variety of different organizational shapes, sizes and configurations from malleable raw materials in Physics, Engineering, Biology, Chemistry, Computer Science, and Medicine. The rush to create a bold new future was on.

The small amount of funding I had been seeking materialized and, as planned, in mid-February 2004, Stuart Kauffman arrived in Calgary to speak about phase spaces, emergence, criticality and the frontiers of “New Biology” research in the three aforementioned venues. These talks had the desired effects, causing people to pause and reflect about this thing called complex adaptive systems, and especially how it could be seen in the emerging new field underpinning what was turning into the new science of Systems Biology. Many of the people who heard the messages and the questions wanted to know more. What did this mean? What were the
implications and potential opportunities? People began to pay attention. What was possible here? Interest began to build.

As part of a longer-term strategic plan to actively create a novel research space that blended new information technologies with the emerging field of Systems Biology, one of the Alberta funding agencies, the Informatics Circle of Research Excellence (iCORE), was poised to recruit a leader in this field and fund a unique renewable 5-year program — to be a kind of catalytic “seed” mechanism for creative ideas and leading-edge research in this burgeoning field. Leadership at the University of Calgary and in the Provincial government recognized a unique opportunity to launch a truly novel project that had the potential to strengthen this important part of emerging science, research, development and innovation at the University of Calgary, and thereby carve out a leadership position on the global stage of truly important new science in and for Alberta. The necessary initial alignments were made, a strategic recruitment plan was activated, and I was hired to create, shape and write the proposal in concert with Stuart Kauffman to spell out what a viable, unique, competitive, world-class Systems Biology project in Alberta would look like and could actually be.

The project name in the proposal became: “The Institute for Biocomplexity and Informatics” — a useful name in part reflecting the primary funding source as well as the requisite involvement of high-end computation to aid in solving some of the most daunting research problems appearing at the time in the emerging new Systems Biology field, problems that are best cast in the light of biocomplexity coupled with a strong computational challenge. We began to envision the new institute as being founded on bioinformatics, but this would be an institute “on steroids”. An initial focus, where advanced computational tools could be applied to real and increasingly difficult and novel biological and medical problems, was the controversial field of cancer stem cells, in tandem with regenerative medicine. Over the Summer and Fall, the proposal for the new institute grew, was repeatedly refined, was submitted, and then went forward through three levels of formal review — Provincial, National, and International. Then, in the last days of November, 2004 — just shy of 10 short months after the initial project idea had first seen the light of day and Kauffman first came to speak, and just three weeks before Christmas — the project was formally approved. Five years of funding for the proposed Institute were allocated by iCORE in conjunction with commitments from the University of Calgary and small amounts from other players. Kauffman was named as the iCORE Chair in Biocomplexity and Informatics and Director of the new Institute, while I was named as the Deputy Director.

January 01, 2005 dawned as the first official day for the Institute for Biocomplexity and Informatics at the University of Calgary. Housed at the University of Calgary, initially in one small office with a telephone and one computer, plus a growing plan for action and some truly radical scientific goals, we were at last real! An initial formative strategic plan for all aspects of Institute activity and growth was developed and put forward. An international network of collaborating scholars was
assembled and a variety of research thrusts, ranging from theoretical to experimental to simulation, were activated at various levels of intensity. A $2 million plan for the first of three laboratories was drawn up, space allocated, cutting-edge robotic equipment for high throughput screening and computer-mediated imaging was designed, custom-built and ordered, and further rounds of combined Federal and Provincial funding were orchestrated and acquired to pay for it all.

After almost 24 months, in the early Spring of 2007, the first of the three planned IBI labs was fully equipped, outfitted, and began operations. The original research goals of the institute began to be realized. Much collaborative network building on campus, across the Province, nationally, and internationally was undertaken to raise awareness of, explore, and participate in the emerging and rapidly growing and multifaceted interdisciplinary field of Systems Biology, with the new Institute having a unique initial focus on the dynamics of cancer stem cells and investigations into regenerative medicine. The Institute for Biocomplexity and Informatics became pivotal in the initial shaping of the new Canadian Association for Systems Biology. My role as Deputy Director, along with that of Director Stuart Kauffman, focused on strategic planning, lab completion, local collaborations, recruitment of new faculty, PDFs and staff, and extensive network and relationship building. After initial explorations with many vendors, industry relations with IBM became a strong focus that lasts to this day, with the view in mind to access and participate in the development of burgeoning High Performance Computing (HPC) essential for the novel research of the Institute for Biocomplexity and Informatics and its collaborators, especially in terms of simulation of and processing of increasingly large volumes of data collected from very complex systems ranging from genetic regulatory networks in cells to quantum effects at the atomic level.

By the Spring of 2008, the Institute had grown to five leading full-time faculty with world-class expertise in Systems Biology, Quantum Chemistry, and Computational Biology, plus almost 20 others who occupied PDF, graduate student, lab support and administrative roles. The Institute was at that time in the process of expanding to three fully equipped labs and a computational facility that already featured a 240-node quad-core cluster, and also had extensive grid computing access. By 2008, the publication record of Institute members since inception numbered over 100 articles and conference presentations, featured in leading journals and venues in Biological, Chemical, Physical, Computational, and Science Policy fields. The strategic development and business plans of the Institute were refined and greatly enhanced to step beyond the original focal areas of cancer stem cells and regenerative medicine to include investigation into advanced cell differentiation therapy, computational biology and computational chemistry, and newly-defined emerging fields denoted as “Atoms to Cells” and “The Physics of Life”, each of which presented novel scientific, technical, conceptual, and organizational challenges. Experimental results from the Institute began to show, for example, that cell types might correspond to attractors in a critical phase space, and that control of perturbations in that space could allow significant insights into the exceptionally complex terrain and specific
interrelated mechanisms of cellular differentiation. Based on such work, the Institute set itself to exploring the development of therapeutic drug leads to control and steer cell fates — which in turn can be thought of as major advances in understanding the ways in which networks of genetic switches and their products, hitherto unknown or inaccessible, could be medically manipulated to positively influence health and disease.

These are significant achievements that were accomplished in the slightly more than three years from the time that the Institute was formalized. By the Spring of 2008, the Institute for Biocomplexity and Informatics was approaching a turning point — renewal for a second 5-year period was contemplated and planned. An expansion plan to 10 or more faculty, a commensurate number of PDFs, graduate students, technical support and administrative staff, and a comprehensive and unique industry relationship with IBM as a potential supplier of High Performance Computing (HPC) and research involvement was sketched out. Necessary laboratory and office space, experimental equipment, and additional computational facilities were featured in this plan, and faculty pursued comprehensive funding possibilities. The anticipated products of such expansion and renewal included reaching significant experimental, theoretical and simulation goals and milestones more rapidly — and, of course, developing further research outcomes and moving strongly into intellectual property, health, and economic development realms.

2.2 – The Institute for Biocomplexity and Informatics as a Complex Adaptive System

The Institute for Biocomplexity and Informatics as an initiative at the University of Calgary had been designed, resourced and activated to generate leading edge research in Systems Biology, initially addressing the complex of variables making up genetic regulatory networks of cancer stem cells. The institute leveraged standard, well-established discipline-focused resources, structures and processes, and simultaneously developed novel, adaptive, increasingly productive and highly proactive exploratory configurations of research and development derived from globally emerging interdisciplines comprising the Systems Biology realm. As such, the Institute for Biocomplexity and Informatics explored and actively participated in the development of Systems Biology as a new field of science and actioned many directions and roles in integrating that developing new field into a well-established scientific institution, understood here both in terms of field of disciplines (and interdisciplines), and as a functioning agent of advanced research, change, and education — the bricks, mortar, programs and people that comprise a university.

The story provided here about how the IBI came to be and how it evolved is an interesting complex adaptive systems case. The obvious simple reason why this claim can be made is that the Institute for Biocomplexity and Informatics as an organizational phenomenon was characterized not only by a history of instantiation and resourcing of a core idea moving into and through stages of ongoing development,
diversification and growth — following a path of innovation — but also a massive
proliferation in the number of variables and interrelationships among those variables
that, following some very simple basic design principles, by their very existence
defined through an iterative process what that organization could emerge to be. This
way, the new institute could determine its overall evolutionary and developmental
direction, processes, and products. In other words, with initial simple constraints,
the number of variables and their connections in the system we can denote as
“The Institute for Biocomplexity and Informatics” is large, is complex and highly
dynamic, is growing, and although many individual components can be known,
and known in some detail, the full extent and nature of their richness, interactions
and products, and the full system of which they are a part, could not have been
and will remain unknowable in toto. This is analogous to any system comprised of
especially unknowable numbers of elements and interrelations, where many of the
elements can be understood in relative detail both individually and in classes, but
how they interact and evolve at different levels of organization, at different rates and
at different scales, and how emergent properties of the larger system evolve, cannot
be fully known or predicted by their individual various smaller-scale behaviours.
To use Kauffman’s and Darwin’s combined thinking, it is not possible to know in
advance what will emerge from the processes of emergence, or to know with any
precision how that process will take place.

2.3 – The Institute for Biocomplexity and Informatics and Emergence

At this juncture it is useful to examine the concept of emergence, as the design of the
Institute for Biocomplexity and Informatics appears to revolve around enhancement
of heuristic processes amongst new interdisciplines that could over the long-
term hopefully be advanced and loosely-coordinated to help define the relatively
new field of Systems Biology. The concept of emergence is used to describe the
appearance of macro-level patterns, structures or system properties where such
features are generated by the dynamical properties of and interactions among system
elements and components at the organizational micro-level. The term is commonly
used today in this way by complexity theorists (see Holland 1996; Waldrop 1992)
and emergence concepts are broadly utilized in economic theorizing and systems
biology (see Kauffman / et al [2000, 2006, 2004]), but was first addressed by the
ancient Greeks (see Nicolis and Prigogine 1989; Goldstein 1999). It seems to be a
useful term to describe both the intended overarching purpose of and operational
design parameters of the Institute for Biocomplexity and Informatics, designed to
seed innovation.

How can this utility be explicated? Examples of what are thought to be emergent
phenomena abound and will help inform the example of the Institute for Biocomplexity
and Informatics. In gross analytic terms we can easily recognize that our thinking
of what constitutes an “ant hill”, “hornet nest”, “lizard colony”, “school of fish”
or “bird flock” connotes emergent dynamical properties of the unimaginably large
numbers of interactions and transactions occurring among all individual members or agents of the community of interest (see Anderson, Theraulaz and Deneubourg 2002; Eberhart et al 2001; Jacobs 2001). A multiscale view of this dynamic would range from the ecological system within which the community exists, to the evolution of that community, to individual members and their components, all the way to the quantum interactions among the assemblies of atoms and the subatomic particles that comprise those components. If turtles are the equivalent of emergent phenomena, then it certainly appears that it is, indeed, “turtles all the way down” (or up, as the case may be).

But we focus on our communities of interest. We seek to understand their patterns, flows, functions, adaptations, and other capacities. Because we have learned so well to be reductionist in our scientific thinking, we recognize, classify, explore, represent, experiment with and simulate the relatively simple operational programs that determine individual agent behaviours in these aggregates; and, as we increasingly recognize the systems and patterns of interactions among both individual agents and aggregates, we attempt to run increasingly complex simulations of those aggregates. We have concluded that emergent properties of such aggregates and their n-tuples are based on the complex dynamical interactions and interrelationships among their component parts, however we classify them. But we also recognize that we cannot predict what those emergent properties will be even with the most highly-detailed knowledge of massive numbers of individual agents, their subsets, or (at least at this time), the best of computational power to assist us with those simulations.

This is in keeping with Tasaka’s (1999) observation that “something is lost when an object is reduced to its component parts”. Another way of making this point is: if we analyze an individual ant down to the smallest detail (perhaps at the level of bioelectrochemical neuronal pathways and processes that comprise an individual ant’s nervous system, so that we know in great detail the individual “agent ant” program) — and even if we also know that many millions of such individual ants make up what we see as the ant hill — by virtue of relying only on the detailed individual neuronal mapping which we have successfully carried out, and even when we develop and apply massively parallel processing power, we cannot then predict or know the macro-scale behaviours, features, characteristics, and capacities of the ant hill (see Eberhard and Kennedy 2001). In other words, standard reductionist analysis and synthesis founded on what Schwarz (2002) calls the “empirico-analytical paradigm”, although undeniably helpful, will not permit a full understanding or prediction of the products of emergence. The only way we can know something about what may emerge is to run individual agent programs together in relatively large numbers in parallel on massively parallel systems, so that their aggregate behaviours become autocatalytic and generative — that is, so that they produce macro-scale behaviours, features, and characteristics that are not in any way written into or known in advance as a part of the micro-scale programs (see Theraulaz and Deneubourg 2002; Holland 1992).
Emergence has been considered an essential concept in fields of inquiry where many antecedent elements interact in novel ways to generate new consequences or phenomena, things that could not be predicted in advance by virtue of what was known about the constituent parts. Emergence has therefore been commonly viewed in terms of the unfolding macro-scale dynamics of interactions among relatively large numbers of simple system components – the formative interplays among the parts and the whole that generates new features, characteristics, or behaviours that could not be known or determined beforehand, and for which we may not even now have adequate models to permit comprehensive understanding of such phenomena (see Cilliers 2002; Gervasi and Prencipe 2003; also noted by Este and Kauffman 2005).

If we think of how the Institute for Biocomplexity and Informatics has been described here, we may usefully ask if it is an emergent phenomenon, or, as such a thing is sometimes called, “an emergent”. If it was, how would we know? Goldstein (1999) suggests that all emergent phenomena demonstrate:

- radical novelty — that is, the phenomenon has properties not previously observed, and which could neither be predictable nor deducible from lower, micro-level components
- coherence / correlation — that is, the phenomenon has properties that maintain their identity over time, and correlate lower, micro-level components into a higher, macro-level unity
- global / macro organizational level — that is, the observed behaviour(s) of the phenomenon occurs at the macro, not the micro level
- dynamical — that is, the phenomenon is not predetermined, but arises in terms of new attractors in dynamical systems comprised of dynamically interacting components
- ostensive — that is, the phenomenon shows itself and is recognized

Goldstein also suggests that when viewed through the lenses of complexity theory, emergent phenomena also demonstrate:

- non-linearity — that is, beyond the notion of non-linear positive and negative feedback loops, they also include “small cause, large effect” non-linear events
- self-organization — that is, beyond the notion of simple self-regulation, they also refer to creative, self-generated behaviours that seek adaptation
- beyond equilibrium (multi-, non-, or far from equilibrium) — that is, beyond the notion of homeostasis or ‘equifinality’, to include amplification of random events and dissipative structures in far from equilibrium conditions
- attractors — that is, beyond simple system equilibrium, to include dynamical attractors as features of complex state spaces where concepts such as fitness landscapes successfully account for dynamical system behaviours

The above frameworks provided by Goldstein strongly illuminate the major features and characteristics of the Institute for Biocomplexity and Informatics that permitted
innovation in Systems Biology to take place, both organizationally and scientifically. These reflect not only the IBI’s design parameters, but also its unfolding actions and activities, engineered to uniquely advance the field of Systems Biology — where the founding core assumption has been that a solid base of consistent inventiveness coupled with knowledge in the fields comprising Systems Biology will dependably yield unknowable processes and products, leading to new knowledge and new discoveries.

Under this illumination and having closely examined the concept of emergence and emergent phenomena as a feature of complex adaptive systems, we can take the framework of theoretical characteristics of such a system and use it as a lens through which to view the Institute for Biocomplexity and Informatics. It appears clear that the IBI as an organizational phenomenon did not operate solely in accord with a linear strategic or operational plan — that is, it was not in its early stages entirely algorithmic. The Institute for Biocomplexity and Informatics was a formal creation of algorithmic mechanisms at the University of Calgary in partnership with iCORE, but one cannot step back and examine a comprehensive dynamical “wiring diagram” of the early IBI that determined what it would be, or read a full and complete itinerary of its unfolding journey as though one were following a recipe or a train schedule at the beginning of its journey. Although there was a basic architectural plan for the Institute for Biocomplexity and Informatics, it was a description of very general structure and processes only, a setting of initial conditions. The initial “org chart” which most administrators seek as the organizational holy grail of explanation, control and accountability revealed very little of the complexity of relationships and interactions that connected the people, the thinking, the activities, the physical spaces, the research processes, the swirl memes and idea generation, the eureka moments and their outcomes, of the IBI.

The Institute for Biocomplexity and Informatics began as a single but very rich idea, and was transformed by strategic opportunity and vision to understand genetic regulatory networks especially of cancer stem cells into a novel entity designed to enrich and add value to the larger institution, the field of Systems Biology, and the Province of Alberta as a whole — both its people in terms of health and wellness, and its economy in terms of novel employment and diversification. As a networked complex adaptive system created to be an integral part of conjoined and much more stable and predictable entities, the IBI as described was comprised of non-linear and linear components, was both organized and self-organizing and adaptive, functioned well beyond equilibrium but depended on equilibrated elements, and featured interacting dynamical attractors on a variety of interconnected and evolving fitness landscapes. This set of claims can be supported by the evidence of how, for example, new faculty were hired based not on only how particular knowledge and skills would fill a predetermined “slot”, but how such “slots” would also of necessity be defined, developed, shaped and articulated based in part on what skills and visions new faculty would bring, combined with the evolving vision and purpose of the Institute for Biocomplexity and Informatics and its efforts to both fit within and act as a major change agent in the understanding of genetic regulatory networks, and therefore the creation and evolution of Systems Biology.
This is in accord with the unknowability of the emergent dynamical multi-scale structures and processes of a complex adaptive system, even where many of the details of the smaller scale system components can be relatively well known — the equivalent of ensuring, for example, that budget management strings do what they are supposed to do, that coffee is always available in the lunchroom, that the bills are paid, or that there are adequate supplies of reagents in the lab. The business plan for the Institute for Biocomplexity and Informatics did indeed have standard, recognizable metrics and was comprised of definable elements, such as a budget that could be broken down into individual line items, and research and development time lines defined by goals, structures and functions, all of which could in general terms be predictably mapped and activated, plus many of the smaller details which could be intimately known — but, the patterns and flows of the emergence of those elements when the system was in action resulted from a combination of and interrelations among powerful and unpredictable blends of algorithms and heuristics, many of which were and remain unknown.

The emergence of large-scale, relatively stable features of the IBI may have been constrained in structural and process dimensions by the overall system architecture of the host university and cast in general terms by the primary funding agency of the time (with many established and practiced hierarchical control systems and lines of authority, for example); however, the Institute for Biocomplexity and Informatics was at the beginning of its life intentionally designed, created and activated as a unique unit operating within, yet quite different from, its well-defined host framework. It was not “just another” department or center, but a formative interdisciplinary research entity that (although described by words that seem simple at first glance) does not to this date have a simple, clear and succinct definition that would place it in the same class as other well-institutionalized structures, processes and functions. The Institute for Biocomplexity and Informatics as a new entity was not so much a “skunk works” along the lines of the first days of ARPA (the Advanced Research Project Agency; see Herzfeld 1998) but more like a multiply task-focused early era Aspen Institute (see Hyman 1975) or Santa Fe Institute (see Waldrop 1992). The initial design parameters of the IBI were intentionally not set to be the equivalent of a new department at the University of Calgary following well-established design and operational principles, but as a unique “innovation engine” intended to enrich and diversify the functions and outputs of the institution’s research tasks.

The initial design parameters of the Institute for Biocomplexity and Informatics intentionally imposed fewer constraints than are found in well-defined and established organizations. However, in its initial configuration, the IBI was embedded structurally and functionally in, and accountable to, its host organization, the university, and its primary funder, iCORE. It was then steered by multiple emerging decisions that can be seen as “loosely-coupled” more along the lines of improvisational jazz (see Weick 1976; De Pree 1993) than strict lines of command for decision and action. Some might feel uncomfortable with a description of a system intentionally set near the edge of chaos necessitating some of what has been termed “muddling through” (see Lindblom
1959) or dealing with “wicked problems” (see Rittel and Webber 1973) but this seems to be quite a normal phenomenon in the realm of establishing an organizational function explicitly designed to collaboratively invent and shape something new and important — in particular, make major contributions to a new field of science — that happens to be just beyond and outside what is predictable and known.

This case therefore allows us to understand how an initial set of embryonic ideas has, through what we can think of as an organic blend of what I here call shaping pressures and opportunity selections, created a highly complex set of evolving, interdependent and interrelated investigative, research, development, information management, and product generation functions. These functions were and continue to be a three-way combination of technical (in the sense of the sciences that are here combined, plus the tools that allow both the sciences and the organization itself to exist), political (in the sense that discretionary decisions and prioritization about resource allocation determine who is to be afforded certain opportunities, all of this in relation to the organization’s, others’, and their own interests), and conceptual (in the sense of how well or poorly the technical and political variables are understood in relation to each other, and how all of this exists in relation to what we have come to understand as emerging new science in the propositional and prescriptive knowledge contexts (see, Mokyr 1999; Kauffman, Logan, Este et al 2008; Tichy 1983).

If we think about the technical, political and conceptual classes of “shaping pressures”, it is interesting to briefly categorize the extant factors of project creation, shaping, and evolution in this case — essentially, classes of variables that defined and continue to define the terrain of complex conditions, elements and processes that steer and advance the Institute for Biocomplexity and Informatics. For example, we have the fact that iCORE existed in 2004 and was poised to commit, and that Stuart Kauffman was at that time sufficiently motivated and interested to become available and chose to be involved; we have emerging decisions for action (for example, university and Provincial leadership deciding in a very short time frame to create a new entity and make funding available to make it happen — itself a radical move); and we have shaping pressures (for example, a broken front of support from components of the larger research community where Systems Biology and bioinformatics initiatives were also underway and contained in orchestrations from the university and from iCORE on the one hand, plus differential levels of interest and support from other components of the university on the other, adhering more strongly to conformational pressures). Beiner (1983) has suggestions about what factors might be at play in this sort of situation that has potential for both great advance as well as conflict, while Wolfram (2002) provides us with yet another example of an effort to break through conformity pressures; and we can usefully note that others have been down this road some considerable time before us. For example, we can recall Machiavelli (1513; 1992) who, almost 500 years ago, observed:

“… there is nothing more difficult to execute, nor more dubious of success, nor more dangerous to administer than to introduce a new system of things.”
Machiavelli presaged what Thagard and Shu (2001) have denoted as “incommensurability” — namely, the lack of isomorphism between the elements of the “old structure” and the “new structure”, and therefore the assumed inability of the successful blending of the old with the new that would allow maintenance of and consistency among all conceptual relations.

Even with clear scientific and economic goals in mind and the engineering of the convergence of opportunities to make things happen, systemic change through the development of novel efforts to explore and move a field of inquiry forward is often resisted (either successfully or not) where such new things perturb that system, yet such moves are often accepted (either successfully or not) where new things enhance or add value to that system (Hesselbein and Johnson 2002; Kawasaki and Moreno 1999; Kelley 2001; Kelly 1998). This is where the evolutionary terrain of critical combinations of shaping pressures and opportunity selections is developed in the 3-way wrestling match among technical, the political, and conceptual variables. In the case of the IBI, the potentials remain and in fact are being broadened, and useful productivity continues to emerge. It is helpful to recall that the New Penguin Dictionary of Science (2002) defines heuristic as “(a)n intelligent trial-and-error approach as opposed to a rigid algorithmic method. Heuristics are used in computer programs which can learn from experience.” The Oxford Companion to Philosophy (2002) states that an heuristic is “(c)onducive to understanding, explanation, or discovery.”

Earlier in this section, I made the comment that leading-edge science might be very well engineered and benefit strongly from closely studying and then taking the ideas and models of “the edge of chaos” into account. If we think this to be so, I believe that in examples like the Institute for Biocomplexity and Informatics, we can only get to and then have a hope of successfully navigating the edge of chaos to advance science in a non-incremental fashion by standing on the excellent algorithmic foundation we have created, and use that as a solid base from which to launch the best exploratory heuristics we can muster — to engage in the best abductive reasoning possible (see Peirce 1934; Harman 1965; Aliseda 2006; Thagard and Shelly, 1997; Magnani 2000), and at the same time be driven by the call to maintain perspective (see Schwartz 1991). The Institute for Biocomplexity and Informatics — still very young, still in its early formative stages, still very much an “emergent” — is a very good example of precisely this exercise.

2.4 – Where is Philosophy in the Example of the Institute for Biocomplexity and Informatics?

In doing this sort of thing I believe we are creating the future epistemology of science (see Cetina 1999). However, in terms of addressing the vanishing and denial of philosophy, I am not sure that an example such as that of the Institute for Biocomplexity and Informatics does anything more substantial than illuminate the fact that we do not know nor might we even be aware of what we are doing in the epistemic realm,
and that in this example virtually nothing was in evidence having to do with explicit discussion about how the IBI as an agent of institutional and scientific change could play, plays or has played a role in exploration or explication of the philosophy of science, or how considerations of the philosophy of science played into how the Institute for Biocomplexity and Informatics was first conceptualized and then actualized. This was the core of commencement of my deep personal awareness of the absence of explicit philosophical thinking, questioning or reflection in what was ostensibly an innovative, leading-edge scientific enterprise. This growing awareness was surprising given the relative significance of the scientific and medical goals being undertaken in the IBI, the complexity of organizational arrangements that were engineered to render it possible, the apparent valuation of the enterprise in terms of budget and in particular the commitment of large portions of people’s professional and personal lives, and the extent and nature of what could be accomplished if the scientific and medical goals of the Institute for Biocomplexity and Informatics were to be achieved. I began to think very seriously about why philosophical questions and considerations were not in any way a palpable part of this enterprise.

So, a question: how did I know to do this? It seems in the personal example of co-creating and building the Institute for Biocomplexity and Informatics that I encountered some initial indicators that a very interesting new terrain did indeed lay ahead, and I saw that its exploration was not only promising, but essential. If the goals of the institute could be met even only partially as it was developed, significant advances in both the understanding and clinical realm of human health and disease would be possible. To use the idea of how we see indicators of actions that may be afoot in our nearby environment, and where we might wish to go to learn, survive and thrive, one might say that I saw fresh tracks along the riverbank and decided to act on what I inferred from them. Let us look back at the starting point of this history to determine where and how I moved into a realm of philosophical questioning and awareness.

Through three decades of reflective practical experience, professional practice, and scholarly pursuit, I found that I shared a number of assumptions with many others (ranging from academic and professional colleagues to business associates and students, from family members to casual acquaintances and even to some authors) about innovation and science; and, through much of this time, I found myself to be more or less confident of and secure with many of these assumptions. They seemed to consistently explain experience (and even the history and interpretations of others’ experience); and holding them — essentially as lenses through which to view the terrain of unfolding and emerging new things and new science — appeared to permit knowing what innovation and new science were believed to be, and how to act appropriately in relation to them: after all, it seemed that most others both felt and believed the same as I did. I knew of conformity enforcement but did not think that what I was experiencing had much to do with that. I learned to handily use the aphorism of “if the only tool you have is a hammer, everything starts to look like a nail”, and even turned its nested questions inwards. However, I did not initially feel
much in the way of motivational stirrings to clear away the conceptual overburden
to the extent that my absolute presuppositions could either be explored or eventually
revealed.

For example, I was aware of well-studied problems that are reported to emerge
with “groupthink” (Janis 1972), but happily, the shared beliefs about innovation
and science in which I was immersed when the first glimmers of thought about the
potential of an Institute for Biocomplexity and Informatics appeared did not seem to
give evidence of those problems. Writings about science policy, for instance — even
those that acknowledged “fuzziness” and “unanticipated outcomes” — appeared to
assume a kind of “perfect rationality” on the part of policy actors along the lines of
what is so well-liked and powerfully advanced by traditional market economists
(see Friedman 1990). I read organizational management stories about prescriptive
strategies and tactics that could be employed by almost anyone to guard against
poor performance or mediocrity (at best) or wastefulness and destruction (at worst):
that is, we could benefit greatly from “a whack on the side of the head” (von Oech
1992), “thinking outside the box” (Eisner 2005), employing “TRIZ” (Altshuller
2005), or “managing at the edge of chaos” (Peters 1987). I read “first blush” stories
about complexity (Hall 1991; Lewin 1992); these suggested that dynamics having
to do with complex adaptive systems could be transferred back into the human
organizational realm (Auyang 1998; Lissack 1999), and as is the case with much of
memetic infection due to the undeniable attractiveness of concepts to which we are
attracted to support choices about what we think we know, became common currency
powering a growing interdisciplinary feeding frenzy, where leading proponents
nipped off fresh buds and supped, refreshed and energized, “at the edge of chaos”.

This was an exciting state of affairs, but at the time I did not yet find myself asking
deeply reflective questions about why these things appeared to be so. Articles and
communiqués of all kinds proliferated having to do with complexity and complex
systems, and how challenging such systems were to formalize and to understand;
yet these same writings suggested how natural it was to apply to common, everyday
experience as well as the challenges of understanding systems of systems previously
thought to be intractable. New ideas about complex organizations and systems of all
types were being developed, and what appeared to be fundamental new knowledge
about complexity and complex adaptive systems was being rapidly generated. This
new knowledge could describe and explain and perhaps even create a new scientific
discipline that would allow us to understand so much that had previously been
unknown, and apparently unknowable, about the networks, systems and dynamics
that seemed to comprise so much of our complex world. Such a possibility was very
exciting. Prescriptions about how to think about these new ideas, implicit in the
articles and communiqués, flowed freely; prescriptions about how to influence and
affect organizations and systems of many different kinds and how to improve their
“yields” (especially knowledge yields related to them) were issued at a rapid rate by
well-recognized and memorable men (not too many women) whose excitement about
their claims often overshadowed almost everything else, although it was assumed to
be the case that, being very good scientists, they all adhered to the highest standards and evidence and rigor.

Based on such reassurance, I became convinced that it was possible to safely assume that following such prescriptions would permit all desirable results from a multiplicity of efforts to generate the desired new ideas, new things, come up with new solutions, make more money, solve apparently intractable problems, train better and smarter people, create indispensable new knowledge and “a culture of innovation”, and especially, strengthen and add to the national economy. With such prescriptions, it became evident that what was being addressed, and the suggested methods for solution, were virtually certain. Naysayers were nowhere to be seen. Algorithms for success were ours to be had at every turn, and the benefits from them lay well within reach! Like the siren’s call, this was terribly exciting and more than promising – almost hypnotic. Questions I might have had about how to innovate, how to create benefits, how to make innovation happen, and even what constituted this thing called innovation, all had the potential to be answered!

However, over a relatively short time, the excitement began to fade. I quickly realized that the circularity of holding and reinforcing shared assumptions maintained against a backdrop of parallel assumptions about exercising necessary and sufficient critical thinking was, indeed, a problem; and, I realized, it was a crucial problem at that. My thinking about finding the keys to innovation in emerging science through the study of complex adaptive systems had been my initial motivation to pursue studies in this area; but, I found that holding and reinforcing these assumptions did not lead to satisfactory knowledge of what innovation is, how it (or, whatever we think innovation might be) can be enhanced, or (in the case of where my interests were at the time focused) how new science emerges. Assumptions such as those identified in the previous paragraphs continue to be held by many to this day, of course, and coupled with this, innovation and new science appear to continue; but, it remained unclear (and remains so to this day) if I, or anyone else for that matter, actually understood, or understands, what we claimed or assumed to be so.

What I am here calling “core shared assumptions” about innovation and emerging science included the following: that we know what innovation is (or, put less positively, that what we think we know about innovation is good enough — it is adequate — so there is no need to know any more than we do); that innovation — whatever it might be, or whatever it is that we are referring to with the term “innovation” — consistently appears to generate some levels of enhanced economic outputs for the benefit of all or at least those who are the engineered beneficiaries, and is therefore obviously desirable, even if we don’t understand what innovation is, or how it accomplishes what we think it does; and, finally, the “good enough” of both incremental and radical innovation adequately explains how we create new emerging science — the assumption being that we don’t need to have full, complete and comprehensive understanding just so long as it does what we generally hope that it should. By holding such shared core assumptions (even if they are remarkably similar to a house of cards) over a period of time, and as long as the house did not
collapse, it became possible to successfully claim expert and reliable knowledge about innovation, the emergence of new science, and the relationship between the two. Articles were published, conferences attended, and news items even appeared in the popular press. Holding and reinforcing such assumptions allowed the claim to be sustained, and sustaining the claim allowed the assumptions to be reinforced. An engine of self-perpetuating claims and assumptions was created and was itself sustained. In this state, on one day, a new prescription could be written about what to do to create innovation; if the system receiving the prescription could not make use of it, or use it well, a further prescription on the day following was all that was needed. Fine-tuning what to do seems a perfectly fine thing if the assumption is made that what is being fine-tuned is understood even if it isn’t, and if no deep questions are ever asked about the system that has been created to support the assumptions.

A surprise can be detected here. I am reporting the surprise in this story about my own growing awareness because I suspect it was pivotal to my shifting focus from wishing to study innovation and the emergence of new science, to realizing that to address these things I must first explore the field of epistemic clarification in order to know, or at least hope to know, what I was aiming to address.

I began to think about how to carry out such clarification. First it became apparent that the condition and state of affairs described above does not generally seem by many to be a problem or a suspicious nest of circular reasoning. Instead, it seems to be (and, I think, we prefer to think of this as) a description of a well-integrated system of beliefs about whatever the object of our attention happens to be — even if some system components are acknowledged to be poorly understood. A doctor might prescribe one treatment, and then another if the first doesn’t seem to work; the range of possible causes for observed symptoms or evidence upon which to base a diagnosis are founded on assumptions about which species is being examined and treated. In other words, innovations we generate and emerging science at which we arrive through what we understand to be the best available rigorous means are to be fundamentally trusted. This, after all, seems to be the best we can do; and, it even seems that from time to time we do better. Standing on this ground, the assumptions we have held to get to that ground are repeatedly verified and generally understood as being correct. We therefore believe that what we assume is correct, and in so doing, we do not find ourselves very often investigating the deepest of our basic assumptions about these things.

In such an exercise, we also want to believe that our beliefs are founded on exploring and then knowing systems that are logical and integrated and therefore reasonably predictable, and thus almost always capable of description by algorithm. We tend to approach the objects of our study — even if they confront us with such intractable complexity that we call them “hairballs” or, as one wag recently termed them, “the ridiculome” (Kaern 2005) — as not being fundamentally chaotic and random and therefore unpredictable and incapable of algorithmic description. We approach such very difficult problems with the assumption that they are amenable to simplification, reduction, and thus representative systematic understanding. We
also want to believe that our beliefs about these things are, themselves, an example of such an integrated, predictable system that can, if required, be described by laws, for example. It is a very hard problem to understand something that is chaotic — but integrated systems, we think and tend to believe, are systems that can be reduced to the point of being understood with necessary and sufficient effort, clarity, discipline, and focus; and, the basis of understanding is knowledge, as complete and integrated as possible, and logic that is tight and unassailable. Such understanding reveals reliable knowledge of system components, their behaviours, and the relationships among them — and can even lead us to discover fundamental laws about such systems.

We tend therefore to think of the most valuable pursuits of knowledge as being, quite literally, systematic; and that that knowledge so derived, too, must be systematic. After all, given our history (essentially since Galileo, as outlined by Russell [1996] and Collingwood [1960]) of seeking, seeking to understand, and then successfully illuminating integrated systems, the behaviours of the components of those systems, and today even more about systems of systems, our shared core epistemology about things like science (indeed, what we hold to be true about science itself) is that, all around us — and even we — are comprised of integrated systems, and systems of systems that we continue to explore, discover, and understand. Through science and its systematics we can identify, reduce and even eliminate magic; we can see, understand and grasp the elements of the world and the extent and nature of their relationships; we can understand and stand in awe of our world, and reach out to see and perhaps even move to distant worlds beyond.

This is heady stuff. It is no wonder that in the history of science there has been a long battle between magical and scientific thinking, still underway in some quarters. We can also happily report that our history demonstrates systematic scientific activity to be, in general, a successful enterprise. The basis for such thinking is the rigorous exploration and establishment of a system of assumptions that lead us to reliable knowledge. In other words, the state of holding assumptions and claims provides reliable evidence of the rigorous pursuit of understanding of systems.

At this point I came to recognize that I was confidently standing on a strong historical foundation of the logic of rigorous scientific inquiry, discovery, definition, and presumed integration. The unspoken assumption was that we were dealing with systems — linear ones palpable through reductionist science, and complex non-linear ones beginning to be illuminated by the ongoing study of whole systems and their dynamically interacting parts. But no matter how comfortable I felt about my claims, or how integrated and well thought-out I believed my shared assumptions about such systems to be, I continued to return to a question — a sure sign that the state of affairs was not satisfactory, even if I was not entirely sure why. In holding core shared assumptions and making what appear to be logically integrated claims, the question came down to the following: did I in fact understand the objects of those assumptions and claims, and what I could hold on to as their interrelationships? I began to reflect on other questions: how would I know whether I had achieved
understanding, or had not? Would I have any idea of what “understanding” might be? What types of thinking should I pursue to approach such questions of knowing?

I reflected on my environment of colleagues and what they claimed to believe and think about their assumptions. Although I could feel secure with what I had embraced as my assumptions and was warmly reinforced by others holding what appeared to be the same or similar assumptions, although I thought that those assumptions did in fact permit me to engage in what appeared to be meaningful and rewarding intellectual, economic and emotional exchanges and pursuits about innovation and emerging new science, and although I could make decisions and take actions yielding consequences that appeared to demonstrate and confirm a logical and perhaps even desirable causal relationship between those decisions and consequences, I found that being simply confident and secure, and reassured and reinforced by colleagues, was inadequate. The answers to my questions continued to be “I am not sure; I don’t know”. I could not say that I understood innovation and whatever constituted its relationship to emerging science.

A short while ago, while revisiting some of the references for this book, I had a powerful insight. Like the familiar ingredients we find in the fridge and to which we return to combine into a good homemade soup, these references have been frequently visited, discussed, contemplated, and digested. They feel now more like parts of a comfortable and extended conversation with old friends than anything else. As I again reviewed what they had to say — “Does science need philosophy?” (Murcho 2006), “Philosophy and the front line of science” (Pernu 2008), “Reshaping Reason” (McCumber 2005), “Re-engineering Philosophy for Limited Beings” (Wimsat 2007), plus a host of others — I realized that before me were ingredients that brought me to the following: in general, we all like to know what is going on around us, and, in particular, what will end up happening. But it had never fully dawned on me in quite the way it did on that morning just how impatient and even desperate we can be (and often are) for assurances, if not promising signs of predictability and even what we think could be certain, both about what we are doing and what will both confront us in and define the future. This realization was a new window into the dynamics of what seems to be our shared goal of wanting to be as sure as possible that we know what is going on, and what will happen in the next second, the next quarter, and maybe even the next century.

Of course there’s nothing wrong with knowing what is going on, wishing to be reasonably sure about aspects of the future, and having predictability to the extent that we can engineer such things in whatever realms we happen to find ourselves. It can be plausibly argued that our ancestors most likely evolved these behaviours and the mental capacities to go with them as essential survival skills. I suspect we are here because these sorts of things worked reasonably well for our predecessors; they have been passed down to us and improved upon, at least somewhat, to allow us to reach today. Without them, we’d never be able to develop, use or understand any kinds of rules or patterns, recipes or formulae, instructions or assembly guides, or feel secure with predictable behaviours and optimally dependable systems — we would have
nothing algorithmic and would, therefore, not know algorithms. It is very difficult to envision a world without some degree of patterned predictability and what are then reasonable expectations based on the algorithms we develop to understand and predict those patterns, just as it is not easy to envision a world without any predictability at all: we appear to live in a world with an interactive, changeable and dynamic mixture of the two. We can best describe this type of system as complex and adaptive. It is also a system that learns from its experiences and by extension necessarily has a memory, a recall function, and the ability to use its memory as a source of recursive input that can be blended with ongoing experience for the purposes of analysis, evaluation and synthesis that allows us to achieve comparative, reformulative and adaptive outcomes.

Thinking this way about the “learning” function of uncertainty (non-algorithmic processes) in complex adaptive systems in the context of and in relation to our apparent need to find more predictability and certainty (algorithmic processes) may be very useful. We may plausibly describe the necessary and sufficient conditions for the success (e.g., survival) of all complex adaptive systems in terms of the relative longevity of a dynamic, integrated balance of blended certainty and uncertainty that supports learning in that system. Lack of success (e.g., non-survival) would be described by a state of affairs that is not dynamic but frozen and/or not blended or balanced (that is, not correlated nor recursively functional, and/or without positive and negative feedback among components and actions); or, conversely, a state of affairs that is chaotic with nothing that we would be able to refer to as structure or memory whatsoever. From this it is not a great stretch to suggest that only with such a mixture of integrated dynamic predictability and unpredictability, recursively supporting system learning, do we have the successful abilities to create, solve, invent and evolve. This, in turn, describes in general terms a complex adaptive system; and thus, by extension, an ecology of complex adaptive systems.

When it comes to what we think of as innovation and the pushing of boundaries for enhanced adaptability and proactiveness, our species may entertain expectations and hopes, and develop and make good use of the algorithms we create, but it seems we very much need to understand the role of and wisely move away from a narrow focus on predictability. These would be part of the necessary and sufficient action conditions to support the above-mentioned integration. We cannot be driven by any desperation or impatience for a solution having the characteristic of some certainty (such as learning, or reaching the goal of integration); to successfully achieve this, we must move into the unknown where the environment, writ large, is almost guaranteed to be non-algorithmic. Some would add that we need to be very deliberative in all of this, and shouldn’t rush. The argument here is: integration (and by extension, the learning that is necessary to support integration) takes time. It isn’t possible to push the river.

Therefore it seems that we need to make good use of what we already know as a useful foundation, seek out and embrace the opportunities, challenges and flexibility offered by the uncertainties of what we do not know, and move forward by not denying new things or our awareness of them, and at the same time not desperately
hanging what we take as answers, plausible or not, around our necks. We may find that our claims of having found the desired integration in our complex system serve not as badges of discovery, achievement and solution, but as millstones, the masses and limits of which may prevent any further useful exploration of the balanced dynamical integration of certainty and uncertainty, and thus severely restrict learning.

The main point here isn’t particularly new — it sounds similar to the exhortation, which in at least the past decade or so has become the tired old adage, to move and think “outside the box”, as previously mentioned. But the insight about this point — more a question, really — has to do with the philosophical foundations of what we unconsciously do, presumably to be safe from or at least reduce the perceived or anticipated risks of moving away from what we think of as certain: I think this is where we encounter a very important point of choice. At this point we can explore the unknown with all attendant risks of unintended outcomes and promises for what is desired. It seems we need to acknowledge that the exploration of the unknown might even lead to changing our beliefs. This is risky. If we know how to “intelligently take risks” to explore the unknown which by definition is uncertain, we can enhance our chances of benefit. But at the same time we can increase our chances of either damage or failure.

To avoid either or both less desirable of these outcomes, we can of course at this point choose to not explore these possibilities. If we make the choice to not explore, we do not expose ourselves to a new range of experiences. But if we choose to experience nothing new, we have the potential of falling into denial and thus not even knowing that this has taken or is taking place.

My claim at this point, therefore, is this: our sense of security and comfort with what we believe to be the desired predictability is rooted not only in what we believe to be reliable knowing as we develop and follow life’s algorithms that for the most part bring us to what we intend (a reasonably healthy thing), but unfortunately is also based on the denial of uncertainty or perhaps the denial of what will turn out to be counter to what we had wished in the first place. This, in sum, restricts and even shuts off the opportunities offered by exploration — which I here claim isn’t quite so healthy. The reason for characterizing the denial of uncertainty (or, the denial of opportunity to explore and enhance what we know) as unhealthy is that such denial severely limits or even eliminates possibilities, options and choices. Such denial is the self-imposition of limits on what we take to be knowledge, and the concurrent restriction of action and thought that limits learning. An integrated part of this claim is that it seems we are not generally willing to take much action that we interpret as stepping outside of our known algorithms, thereby risking what we believe to be our security and comfort in what we have come to hold as what we take to be justified true belief (Gettier 1963).

What we think of as denial is not uncommon. For example, it is useful to note that recently, in the more popular press, denial has been addressed and explored at least to an introductory level (Carey 2007), and Ariely (2008) has attempted to illuminate the terrain of predictable irrationality. Denial presents serious challenges in the
realms of medicine and public health (Nature Medicine 2006) and group behavior (Janis 1972; Surowieki 2005) A system of beliefs powered by and based on denial of exploration, and therefore a restricted flow of new information, presents serious problems for learning, understanding and adaptation. Why would we choose to restrict our learning, when learning seems to be the basis for achieving understanding and, therefore, the core of adaptability and the engine of proactivity?

More questions began to emerge. What would be the mechanisms, the philosophical implications, and any plausible evolutionary advantage for the existence, reinforcement and perpetuation of denial? I realized I was beginning to think about what might be thought of as the interlocked dynamics of epistemic clarification and epistemic denial, and, especially, the relationship between the two.

These thoughts reminded me of Philip Armour, the computer scientist who writes about how we approach the task of writing computer code (Armour 2000). Writing code is one of those things that is an attempt to achieve, or at least maximize, algorithmic certainty — and Armour correctly suggests this is not simple or easy. Although computer code can be readily written by knowledgeable and talented people, it is clear that one really ought not write code that doesn’t work! He argues that the challenges of writing good code are not merely technical. Although highly skilled and gifted programmers can focus their energies and efforts in apparently superhuman ways to accomplish exceptionally complex and demanding code-writing tasks, we run into problems aiming at algorithmic certainty because we tend to forget that the product of writing computer code is not the code itself, but the knowledge we create to understand and then make use of the code in the ways we had originally intended. In other words, the goal of writing code is not the code itself, but the consequences of the applications of that code. When we forget the real purpose of writing computer code and focus entirely on developing algorithmic perfection, we write code for code’s sake and for the sake of coding, not for understanding and then using the code in the best ways possible.

As confirmed by those who have been in the field for decades (Goebel 2006) we fall in love with the code and the act of coding, and forget what the algorithm is for. There is no question that one can be very good at writing code qua code. But this illuminates the equivalent of the “W5” problem — working extremely hard to clearly identify and drill down into the details of the who, what, where and when (the W4), and paying very little if any attention to the fifth “W” — the “why”. Of course we realize that we need to do very, very well with regard to the “W4” (Wilson, no date).

This tends to be so in any realm we care to examine. We require and work hard to develop exquisitely crafted and sharply refined technical knowledge and skills; and, we simply could not achieve what we aim to do — write excellent code, or push the boundaries on science and engineering to successfully build large and complex devices and projects such as the Large Hadron Collider or the Square Kilometre Array, for example — without them. But without the fifth “W”, we do not think well, or at all, about using the excellent code in the way we intended. We forget the deeper reasons for doing what we do.
2.5 – Conceptual Slippage

I claim we are engaged in denial when this takes place. As I have suggested elsewhere (Este 2007) this “conceptual slippage” is a serious problem because, being so enamored with the process of coding and the perfection of code, we find we don’t even know that we don’t know — and this is where we can be certain (if you will pardon my use of the term) that anyone who suggests we don’t know what we’re talking about doesn’t know what they’re talking about!

This is understood to be a worldview of intentional obscurantism, known in some places as “proud ignorance”. I think there’s a strong parallel here for a better understanding of innovation in general — that is, standing on what we know (or at least think we know), and then pushing the boundaries and building into what we don’t (at least not yet).

No one likes to think that they might be proudly ignorant. It is no surprise that most people don’t like to talk about this possibility very much, and commonly become annoyed and even angry when the question is raised, especially if it persists. This has caused me to wonder if we actually can think about this prospect in any deep fashion when we do think about it, in the most productive of ways. I am optimistic and would like to think that we have the possibility of doing so. This has a direct parallel in the realm of innovation where, in general, people claim that we simply need more of it, and don’t tend to think much further than that — we don’t need to know what it is, really — we just need more. We just need to do more “innovation things”, and perhaps do those things faster and better; we have confidence that whatever we do will then produce more of it. Policy specialists are “certain” about how to go about creating policies and shape the policy processes that generate policies and thus generate a range of outcomes, and similarly those who are specialists in innovation are “certain” about advancing what they think is innovation, and especially about pushing and pulling various policy levers to get more of it, adjusting this and fine-tuning that to get to what they think is an optimal state of affairs. But, contrary to their claims of understanding and being able to make use of policy and innovation mechanisms, they don’t talk a great deal about the knowledge required to understand and then make use of policy or the policy process in the way we hope or intend, or how it might be that comprehensively understanding innovation might help us be better at innovating, rather than simply trying to get more of the same. We become very skilled and knowledgeable about how to push certain levers, turn certain dials, write certain code — but that may be all we end up doing well.

As I gave more extensive thought to having worked so hard to collaboratively build a novel scientific enterprise (the IBI, described earlier), I felt uncomfortable about not being able to adequately answer some of my beginning questions about shared assumptions. I began to think that what I had assumed was a solid foundation for an innovative scientific enterprise might actually be founded on serious philosophical questions that had fallen victim to considerable conceptual slippage, that this might not be generally known, and that in general we might not even be aware that
important aspects of building new science were not addressed. At the same time, as I began to think about how and why we appear to limit our thinking that we do in our efforts to carry out and accomplish many complex things, I realized in ways that I hadn’t realized previously that not pursuing what we are unsure about appears to be the main reason why, in the policy realm, so many policies have unintended outcomes (that is, the code might work some of the time but doesn’t do all the things you had hoped; it may do nothing close to what you had hoped; it may disrupt or destroy what you already have; or, it may do nothing at all). I think this is why innovation as we tend to think about it today primarily has to do with “valorization” and “return on investment”, and very little if anything with the knowledge that we create (and need to create) to understand as many dimensions as possible of this thing we call innovation. Philosophical considerations that could help us with such challenges are left unaddressed.

In the next section I will delve more deeply into why we would find ourselves in this kind of situation, and what we might do to clarify and perhaps even improve it. At this point of attempting to grasp and understand at least some initial parts of philosophical denial, we now step into exploring the first stage of “the puzzlement.”

NOTES

1 My role was later refined to that of Director of Operations.

2 “Abductive reasoning accepts a conclusion based on the grounds that it explains the available evidence. The terms was introduced by Charles Sanders Peirce to describe an inference pattern sometimes called ‘hypothesis’ or ‘inference to the best explanation.’” (The Oxford Companion to Philosophy 2nd ed, p1). Also see Papineau (1996) and Harman (1965, Philosophical Review, 74, The inference to the best explanation). “In general, there will be several hypotheses, which might explain the evidence, so one must be able to reject all such alternative hypotheses before one is warranted in making the inference. Thus one infers, from the premise that a given hypothesis would provide a ‘better’ explanation for the evidence than would any other hypothesis, to the conclusion that the given hypothesis is true.”