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PARADOX Start

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PROBLEM AND PARADOX IN FOUNDATIONS OF DESIGN

by Ken Friedman

Considering foundations

The question of foundations in design is a key issue in design research. This question comes up repeatedly in different ways. The invitation to reflect on foundations in design is a welcome opportunity.

The invitation to this project involved two issues. One is a general response to the broader question of foundations. The other involves responding to issues raised in two papers by Wolfgang Jonas (1999, 2000, 2001b, 2001c).

This paper will outline a case for inquiry into the foundations of design. This paper states that design practice, design research, and design theory all have foundations. It will contest the idea that design is a groundless field. The paper will demonstrate the relevant foundations, and it will demonstrate the kinds of ground in which the field of design is anchored.

In doing so, this paper will approach broad, systematic questions first. Since several of Jonas's points address broad, systemic questions, it will address them in the context of related issues. Later, it will respond to other themes in Jonas's papers. The generative ideas in Jonas's papers have a useful role in the context of discovery. In responding to these, I distinguish between generative ideas and problematic assertions positioned in the context of justification.

Stating a case for the foundations of design and design research requires a brief review of three current debates.

The first debate involves the issue of progress in research and in practice.

The second debate involves systematic inquiry and the possibility of facts.

The third debate involves distinguishing among kinds of research, levels of analysis, objects and subjects of inquiry. I will summarize these here to explain my view of systematic inquiry.

Progress in research and practice builds on prior work. A field that can demonstrate progress in effect demonstrates that it has foundations.

A field of practice and a research discipline require systematic inquiry to solidify and develop foundations. Systematic inquiry is therefore a condition of meaningful foundations. Systematic inquiry is possible in meaningless ventures ú f.ex., games, puzzles, or play. Meaningful systematic inquiry must involve actual conditions or cases, that is, facts. Thus, any position on foundations in design must involve systematic inquiry and facts.

Systemic inquiry on the foundations of a research field requires distinctions among the kinds of research ú basic, applied, and clinical. It also requires distinctions among the levels of analysis and theorizing from individual cases to general laws or rules. Finally, systematic inquiry requires distinctions among the objects and subjects of inquiry. Meaningful systematic inquiry into the foundations of design requires a consideration of these distinctions.

It is necessary to examine these three sets of issues to play a fruitful role in the inquiry on "the basic PARADOX - foundations for a groundless discipline" (Jonas 2001a).

Progress in research and in practice

The concept of progress in research and practice is simple. The idea of progress is basic to what we know in most fields of human activity. It is so common that it has often been simplified into advertising for politicians and businesses. At the same time, progress is so visible in so many fields that it has often been taken for granted by philosophers and scholars. Some scholars and scientists ú Drucker, Innis, Schumpeter, or Varian, for example ú have focused on progress. In the philosophy of science, Imre Lakatos concentrated on the issue of progress in a series of articles and books that have profoundly influenced thinking in many fields.

Lakatos (1970: 132-177; 1978) focused much of his analysis of scientific research on the concept of the progressive research program. While the idea of constant progress as an ontological factor in science is largely mythical, epistemological progress is an important factor in any developing field.

According to Bunge (1999: 227), progress is the "process of improvement in some regard and to some degree." In epistemological terms, it involves an "increase in the truth, depth, coverage, and systemicity of the body of knowledge. Some of the means for epistemic progress are: increased accuracy of empirical data; substitution of theories for unrelated hypotheses, and mathematical models for verbal ones; replace of black-box theories with translucent-box ones; and interrelation or even merger of previously unrelated research fields" Bunge (1999: 227).

Building on Tore Kristensen's (1999: unpaged) model of a progressive research program for design research, I have identified eight characteristics of a progressive research program. These are:

1. Building a body of generalized knowledge,

2. Improving problem solving capacity,

3. Generalizing knowledge into new areas,

4. Identifying value creation and cost effects,

5. Explaining differences in design strategies and their risks or benefits,

6. Learning on the individual level,

7. Collective learning,

8. Meta-learning.

This model of a progressive research program is applicable to the practice of design as well as to design theory.

Progressive research programs within and across the fields of design involve at least four areas that must be linked in a virtuous circle.

These four areas of design research and design are:

1. Philosophy and theory of design

2. Research methods and research practice

3. Design education

4. Design practice.

Each of these fields of concern involves a range of concerns and programmatic development. These are:

Philosophy and theory of design

--Philosophy of design

----Ontology of design

----Epistemology of design

----Philosophy of design science

--Theory construction

--Knowledge creation

Research methods and research practices

--Research methods

--Research issues exploration

--Progressive research programs

--Development from research to practice

Design education

--Philosophy of design education

----Education based on research

----Education oriented to practice

--Rethinking undergraduate education

----Undergraduate focus on intellectual skills for knowledge economy

----Undergraduate focus on practice skills for professional training

----Undergraduate focus on foundations for professional development

--Rethinking professional degrees

---- Professional degrees oriented around intellectual skills

---- Professional degrees oriented around practical skills

---- Professional degrees oriented around professional development

--Research education

----Undergraduate and professional background for research education

----Research master's degrees

----Doctoral education

----Postgraduate training

--Continuing education

----Lifelong learning

----Partnership with design firms

----Partnership with professional associations

----Partnership with industry

----Partnership with government

Design practice

--Comprehensive practice

--Profound knowledge

--Practice linked to solid foundations in education and research

--Professional development

--Lifelong learning

The growth of design knowledge, the steady history of improvements in design practice, the dramatic development of design research, and the gradual development of design teaching, all indicate progress.

Progress is not uniform. Comprehensive progress is impossible. Nevertheless, there is relatively wide agreement in our field that we are meeting Bunge's (1999: 227) definition of progress as a "process of improvement in some regard and to some degree" in all four areas of design.

The state of physics in 1895 offers a good comparison for our field. Because we are a different kind of field, we cannot hope to make the fundamental progress that physics has made over the past 100 years. Even so, we can hope to grow if we focus on a progressive research program.

While most of us know the broad outlines of progress in physics during the past century, an even better comparison might be the programmatic development of mathematics. In 1900, David Hilbert gave a famous speech in which he outlined a series of important challenges for the growth of mathematics. He proposed a program of inquiry and research that he hoped would place mathematical knowledge on solid footing for the centuries to come.

Hilbert failed and succeeded in important ways. Hilbert's program offers three lessons for design.

The first lesson is that great aspirations lead to significant progress, as Hilbert's program did.

The second lesson is that absolute progress is never possible. Mathematics made significant progress in the decades following Hilbert's challenge. In 1930, however, Kurt Godel destroyed any ultimate hope of placing mathematics on completely solid, consistent ground. He did it with the theory on the limits of logic and human knowledge that he published in January 1931.

The third lesson lies in philosophy. It is the lesson of human achievement in the face of our ultimate inability to achieve absolute knowledge. The years and decades since Godel rendered Hilbert's hopes impossible have seen some of the best and boldest progress in mathematics since Euclid the theorist and Archimedes the designer practiced mathematics.

During these years, mathematicians have solved fundamental theoretical and philosophical problems. They have contributed to rich developments in physics and the natural sciences. They have even shaped applications that make it possible for all of us to live a better daily life.

While design and design research may not give rise to the same kinds of work, most of us seem to agree that design can be an important field of research and practice. If this is true, then design and design research can make progress.

Jonas's article (1999: unpaged; 2001b: unpaged) is partly correct and partly mistaken in stating that design is "an interface discipline without progress." Design is certainly an interface discipline lodged between and among many disciplines. This condition can be demonstrated by numerous cases and examples. It is a mistake to write that the field is "without progress." This statement is a metaphysical proposition without empirical support.

It is clearly possible to demonstrate progress in design.

We know more about design theory and design practice than we did a decade ago. The state of knowledge has increased dramatically in many areas of design: interaction design, industrial design, materials design, nanotechnology, computer design, software design, interface design, design management and many other fields have shown increasing strength and improvement. Several fields of design research have blossomed for this first time during this period, including philosophy of science applied to design, and philosophy of design.

The working skills of the best designers in many fields are better than they once were. In all of the fields named above and many more, new methods and skills have emerged together with new technologies, new professional clusters, and new applications.

Improvements in education and pedagogical method make it possible to help design students learn more and better than ever before. The systemic quality of design education has improved, and the level of design education has increased. The past twenty-five years have seen a large-scale transition from vocation training to professional education. The past decade has seen the smaller but widespread development of design research.

Design is making progress as Bunge and most leading dictionaries define it. The field is undergoing "a forward or onward movement (as to an objective or to a goal), advance, gradual betterment" (Merriam-Webster's 2001: unpaged; Merriam-Webster's 1990: 940). If we can say that design is "moving or going forward," (Webster's 1913: 1145), then by definition, design is a field or a discipline making progress. (See also Shorter Oxford English Dictionary 1993; Wordsmyth 2001.)

None of these factors are uniform across the entire field, and no form of progress typifies all the instances and institutions to which it may apply in specific cases. This is no different for design than for any other field. Uneven development and distribution typify progress in all progressive fields.

Progress in research and in practice depends on prior art. This is another way of stating that progress requires foundations. If there is progress ú and there is ú there must be foundation. There is progress in design. QED: design has foundations.

The claim that design is a field without foundations is a metaphysical claim of the same nature as the assertion that design is a field without progress. Anyone who believes that there is no progress in design research or design practice may conceivably argue that the field has no foundations and no ground. The demonstration of progress in a field is a demonstration that foundations exist.

We will consider the meaning and nature of foundations in greater depth later.

At this point, we will consider systematic inquiry and the nature of facts as a condition for building on foundations.

Systematic inquiry and facts

The question of systematic inquiry and the possibility of facts is a recurring debate in design research.

While interpretations are finally subjective, facts exist independent of the inquiring subject. The kinds of inquiry we make determine our ability to access, describe, and understand facts.

One purpose of research is to identify, access, understand, and describe facts, as well as to interpret facts and their meaning to us. Another purpose of research involves exploring and interpreting ideas and issues including but not limited to facts. Many forms of research involve ideas and issues that are ú or may legitimately be ú independent of facts. These include art, literature, and music, as well as some forms of mathematics, logic, and philosophy. Design research involves both kinds of research. Some forms of design research involve facts. Other forms of design research are based on idea and interpretation.

To discuss design research as a whole, we must understand and discuss the possibility of facts independent of the observer. To assert that objective facts are possible perspective involves several premises. A substantial argument and demonstration can be made for each of these premises. This short outline states the basic issues:

(1) There is an objective world independent of human observation. (2) In principle, this world is open to human inquiry, description, and understanding. (3) Even though facts exist independent of human observation, we do not know all or any of these facts. We must seek facts out, describe them, analyze, and understand them. (4) One purpose of research is identifying, describing, analyzing, and understanding facts. (5) Research always has a cost in time and human effort. Identifying, describing, analyzing, and understanding facts requires time and effort. (6) While it is rare that a conscious agent purposely conceals facts, many facts are so inaccessible that we describe as hidden. (7) Anything that can be known must be known by a conscious agent. Only a human being can know something. (8) Human beings are fallible. Individual human knowledge is limited by the constraints of time, place, and person. Social knowledge is limited by still more constraints, including perspective, culture, traditions, and paradigms. The knowledge of any research field is a form of social knowledge. The knowledge of a research field is limited by prior knowledge, instrumentation, method, and many other factors. (9) Some facts that are not yet known can in theory be known. While it is difficult to gain the actual knowledge of these facts, this knowledge is theoretically possible. (10) Some facts cannot be known. This knowledge is theoretically impossible to obtain. (11) It is possible to be wrong about whether any specific fact cannot be known. It is possible to contest the case of any specific fact. It is nevertheless widely accepted on theoretical grounds (f.ex., Godel's theorem and Heisenberg's principle) that some facts in principle cannot be known. (12) The effort to determine what can be known in principle is a legitimate research goal. (13) The effort to determine what can be known in practice is a legitimate research goal. (14) The effort to

know specific facts is a legitimate research goal. (15) The effort to place specific facts in an appropriate theoretical context is a legitimate research goal. (16) Because fallible human beings conduct research, knowledge of the world and its facts is difficult. (17) Human beings are often mistaken in their findings of fact. When the facts are correct, they are often partially correct and partially incorrect. Even when facts are entirely correct, human beings must often choose among competing theories, principles, and explanations to account for those facts. (18) Research involves a wide range of activities that require us to explore the nature of facts, interpretation, and inquiry. These activities involve the content and the context of inquiry, as well as the frame of inquiry and the boundaries between various frames. (19) Research often requires us to change our view of any fact or range of facts.

Many of the arguments against systematic inquiry rest on the notion that there are no facts external to the belief system of the scholar or scientist who selects among possible explanations.

For many, this argument began with Thomas Kuhn's (1962, 1970) description of revolutions in science as paradigm shifts. Kuhn was attempting to analyze the conditions of scientific progress. He never claimed that facts depend on the viewpoint of the scientist or the dominant paradigm of the community within which a scientist works. Kuhn never challenged the possibility of objective facts. Rather, he addressed the issue of how and why we understand facts or fail to understand them. This involves questions of search, discovery, and interpretation.

Kuhn's work has often been used, mistakenly, to support what is sometimes labeled as the constructivist view of science. Constructivism involves the argument that scientific knowledge is constructed rather than discovered. In its most radical form, there are no facts external to the observer. In this view, all knowledge and all facts are artifacts of the individual human being or the community within which that human being acts and thinks. This was not Kuhn's view.

Social factors do influence the development and interpretation of science. One of Kuhn's great contributions was a persuasive argument that helped shed light on scientific growth in contrast with the sometimes-simplified misconception of scientific method. His account of how social factors influence the way that scientists conduct research and interpret their findings addresses the importance of social factors in science. These factors do not influence the physical or natural objects of study, and Kuhn never claimed that they did. Social factors influence what we believe about wood, atoms, or the tensile strength of steel. These factors influence the nature of our understanding. These factors do not influence the nature of wood, atoms, or the tensile strength of steel.

Kuhn argued for a view of scientific knowledge based on many factors, including experimental evidence and robust theory. In Kuhn's (1977: 320-339) view, the relations between objectivity, value judgment, and theory choice requires a serious and subtle engagement with empirical reality. Kuhn asserts a world of objective facts. His inquiry addresses the ways in which we understand and intercept those facts. The social circumstances of science affect what can be seen as scientific knowledge, but they do not affect the objects of scientific inquiry, at least not in natural science.

Debates among design researchers often involve a naïve misreading of Kuhn's views. One example of this occurred in a 1999 thread on the DRS list where one scholar wrote (Moon 1999a: unpaged), "In Kuhn's argument for theoretical revolution, theories take on a different perception. Rather than contested theory being representative of inadequate knowledge, each represents the ideological preferences of the theoriser/s. Thus, is theory a true portrayal of the physical world, or the extension of some (abstract) belief system?" Kuhn did not write this last sentence. It is an immature scholar's view of Kuhn. Kuhn himself had a very different view, as his own words demonstrate.

First, Kuhn did not "[argue] for theoretical revolution." Kuhn (1962, 1970) described the process in which paradigm shifts led to what he termed scientific revolutions. A paradigm is not identical with a theory. Kuhn (1970: 175) uses the term "in two different senses. On the one hand," he writes, "it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, I denote one sort of element in that constellation, the concrete puzzle solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science." It is easy to see how the second sense of the word can be confused with theory, but a close reading of Kuhn's book shows that he hardly writes about theory at all. He is, rather, describing sociology of science.

Thomas Kuhn is one of the most cited and least read authors of our time. To read Kuhn is to enter a difficult world of subtle and challenging issues. Kuhn was a working physicist who moved into the sociology and history of science after he completed his Ph.D. in physics. The Structure of Scientific Revolutions was the fruit of two decades work teaching, doing research and thinking on the subjects he examined.

Kuhn (1970 198-9) explicitly DISAGREES with the common misunderstanding of his view. He never argued that: "the proponents of incommensurable theories cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to good reasons; instead theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached."

Kuhn plainly states that this is a misconstruction of his view. He did not see scientific theory as, "the ideological preferences of the theorizer." For Kuhn, the working physicist, theory was far more than the "extension of some abstract belief system."

Kuhn believed in the possibility of facts.

After Kuhn, the two most cited sources used to suggest the impossibility of objective facts are Paul Feyerabend (1975, 1993) and Peter L. Berger and Thomas Luckmann (1967). None of these three authors would support such a claim.

Feyerabend is an important thinker whose insights have shed useful light on knowledge claims and scientific method. Without addressing Feyerabend in full, it is worth noting the possibility that Feyerabend's work means one thing in terms of discovery and another in terms of justification. Feyerabend was a puckish and often whimsical thinker who loved to provoke debate. He seems often to have argued cases in which he did not entirely believe simply to stimulate thought. To the degree that Feyerabend's philosophy of science emerged from his life, Feyerabend's (1995) autobiography is a vital addition to his formal papers and publications. Reading Feyerabend's comments on Ernst mach and Felix Ehrenhaft demonstrates his view on the nature of facts: Feyerabend often questioned theory, but he remained convinced of the possibility of facts. Feyerabend (1993:147-158) dismisses the distinctions between discovery and justification, but he does so on the argument that methodological anarchy gets us closer to facts.

In contrast, Berger and Luckmann (1967) are not methodological anarchists. They are not even constructivists. Their book was a major breakthrough in the sociology of knowledge. It shed light on the processes by which social groups construct an understanding of reality. Those who have not read the book itself easily misread the title of this book. "The social construction of reality," is a pithy, poetic title. It is, however, shorthand for "the social construction of social reality." Berger and Luckmann distinguish between objective facts and the subjective interpretation of facts. In fact, they even distinguish between society as objective reality and society as subjective reality.

Some debates in design research seem to assert that systematic inquiry is impossible precisely because there are no objective facts. Since these arguments frequently refer to Kuhn, Feyerabend, Berger, and Luckmann, it seemed useful to contest the misuse of these authors here.

If there are no objective facts, it is difficult to see how it is possible to conduct meaningful systematic inquiry. If there are no objective facts, systematic inquiry may be no more meaningful than a Star Trek fanzine of alien life forms discovered by the Star Ship Enterprise. It is easy to understand that those who believe that facts are impossible to ascertain find the idea of systematic inquiry meaningless.

The issue is slightly more complex among those who believe that facts exist while arguing against systemic inquiry. Jonas's papers seem to be a case in point. In arguing for design as a field emerging from hybrid, indeterminate ground, Jonas (1999, 2001b) seems to argue against the value of systematic inquiry and for design as an emergent field of knowledge. Jonas's argument seems to propose a design field that responds opportunistically to each instance of design practice without foundations or progress.

Both foundations and progress require systematic inquiry. If design were nothing more than an opportunistic and emergent field that only responds to each specific instance, there would be no need for systematic inquiry in design or design research. Design is more than this.

The argument can be made that design is far more than an opportunistic field. As a human practice, many forms of design are naturally opportunistic. As an evolving field, design is inevitably marked by emergent properties. These factors influence design. They do not constitute limits on design.

This brings us to the issues of the kinds and qualities of research.

Kinds of research

Systematic inquiry in a research field requires distinctions among the kinds of research ú basic, applied, and clinical. It requires distinctions among the levels of analysis and theorizing from individual cases to general laws or rules. Finally, systematic inquiry requires distinctions among the objects and subjects of inquiry.

While these are necessary to systematic inquiry, they do not bear directly on the issue of foundation. Rather than analyze each of these in depth, I will simply state that they exist and attempt to exemplify them before applying them to the question of foundations.

The three kinds of research ú basic, applied, and clinical ú deal with three different kinds of issues.

Basic research is a search for fundamental knowledge. This includes scientific principles of how things operate. It also includes forms of scientific inquiry that seek theories or laws explaining why things operate as they do, or even why they are as they are.

Applied research focuses on how to do things in general. Technology is the frame of applied research. Applied research often involves adapting basic principles to kinds of problems. This is what the term "classes of cases" means.

Clinical research is the examination of specific cases.

There is always some degree of overlap between any range of issues on a continuum. This is often the case in basic, applied, and clinical research. Even so, it is possible to offer examples that clarify distinctions.

Metallurgy and materials science study the properties of metals. These include studies into the nature and properties of steel. Engineering also includes some forms of basic research. Engineers often study the different kinds and properties of sheet steel.

Research into the specific properties and uses of a specific form of sheet steel constitutes applied research. Here, one does not seek general scientific principle. Neither does one solve specific case problem. Rather, one seeks a range of issues to which a specific sheet steel solution might be applied, and one studies the range of applications this specific sheet steel may have.

At a more specific level, learning how to bend a certain grade of sheet steel solves the problem of this specific application for all cases to which it may be applied.

Clinical research would involve designing, engineering, or manufacturing a specific artifact that requires bent sheet steel in its construction. Determining the grade and kind of steel required, and selecting among available sheet steels is clinical research for the specific case. Choosing among bending techniques and applying them to the specific case is clinical research.

The complex relationship between physics and ballistics will illuminate the distinctions.

Physics is the general study of a range of features and characteristics of the physical universe. Physics includes the study of motion, the properties of moving objects, and the relations between moving objects and the larger physical environment within which they move.

Ballistics can be found at the boundary of basic research and applied research. In general, ballistics is the study of bodies in flight. In this general sense, ballistics involves basic research. Some of Galileo's great contributions to physics involved ballistics.

When ballistics involves the study of a certain kind of problem used in technical applications such as gunnery, ballistics becomes a form of applied research.

In gunnery, one use of ballistics is the development of ballistics tables. Ballistics tables are matrix charts compiled specifically for a kind of weapon and a kind of ammunition, setting forth a range of parameters that allows a gunner to know what kind of performance to expect given a weapon, a load, wind factors, distance, elevation, and other such issues.

In the early days of artillery warfare, gunners hit their mark by ranging in. A gunnery master would fire a shot. A master gunner would use intuition and experience to determine the elevation and load of the first shot. Most of the time, the first shot fell short or went long. It was, in effect, a test shot.

Depending on how far long or short of the target the first shot hit, the gunners would range in, adjusting each succeeding shot until they were on target. When cannon were rare and relatively powerful, this was quite adequate. A medieval general who could arrive on location with six or eight cannon was a great power, and even a few cannon could win a war or break a fortress.

By the time of the Napoleonic wars, gunnery was far more advanced. Speed of response became an important factor in any battle where each side had hundreds of guns, all firing at one another. This was even truer of the first industrial war, the United States Civil War.

In the twentieth century, long distance artillery fire, timed barrages, and strategic plans requiring the use of weapons at great distance for tactical support made it impossible to rely on earlier forms of ballistics. There was neither time nor opportunity for the line-of-sight artillery command that was once required. While spotters, balloon observation, and other techniques were used to chart and range, the most effective means of artillery control took place on the input side. This involved ballistics tables.

Ballistics tables allow gunners to sight and shoot with reasonable accuracy without the tedious process of ranging. For each weapon, each load, and each series of conditions, a ballistics chart solves certain kinds of problems. Even so, what Clausewitz called "the friction of war" always takes hold and ballistics charts never do quite what they should. This makes ballistics charts the perfect example of a distinction between applied and clinical research.

The research used to set up a ballistics chart is applied research. One is not firing weapons in the heat of battle, but testing weapons, ammunition, and loads under controlled conditions. These conditions and the results of each set of parameters then established the factors for each chart. Each chart governs an ideal set of cases. The conditions of any battle determine the applicable case, and this is used to set up and fire a gun.

Firing in battle involves clinical research. Gunners use firing orders and the ballistics chart to determine how to aim and load. Artillery observers determine the accuracy of firing and report. Since battle rarely goes as planned, artillery commanders, gunnery officers, and gunners use observer feedback to adjust the ideal procedure to immediate needs.

The changing conditions of modern war created the need for ballistics tables. Ballistics tables are careful, precise, and accurate. The reality of war means that these tables rarely work as planned. The interplay between the results of applied research and feedback on the firing line demonstrates the distinction between applied research and clinical research. Both of these are far removed from the general laws of physics, though it has happened more than once that skilled technologists have used the laws of physics to advance the state of art in applied ballistics.

One can find equivalent examples in any field where we seek outcomes that affect or change the world around us.

There is generally a border zone between basic research and applied research, between applied research and clinical research. In this zone, some research may fulfill both basic and applied functions, or applied and clinical. Beyond this, information always travels among the kinds of research programs. Clinical problems suggest basic questions. Basic discoveries inform applications. Applications feed queries to basic research and to clinical research, as well as providing solutions to problems in each.

The interplay among these, as well as the distinctions, offer another reason for the vital importance of scholarly communication.

Research into foundations may involve any of these three kinds of research.

This is also true of the spectrum of levels on which analysis and theorizing can take place. These levels often coincide with the three kinds of research. Issues of the unit of analysis and the kind of theory to be developed come into play. Levels of analysis may study issues from individual cases to general laws, and they may produce findings on any level from a single-case description to broad gauge descriptions and law-like general rules.

In any field that involves both scientific or technical issues, as well as human beings, it is important to distinguish between objects of research and subjects. Any phenomenon may be an object of inquiry. Any conscious agent may be a subject of research.

Later in this paper, I will discuss the reasons that make it helpful to distinguish between subjects and objects. This bears on the question of agency, and the often-problematic issue of a field in which questions from natural science and technology confront issues in social and behavioral science or the arts and humanities.

Models and foundations

At this point, I will turn to a few central issues in Wolfgang Jonas's (1999, 2000, 2001b, 2001c) two papers. These papers raise many interesting issues and point to intriguing sources.

The logic of discovery involves the free play of the mind, and any place may be a good place to begin. Jonas appeals to metaphor and challenges existing constructs in his 1999 paper titled "On the Foundations of a 'Science of the Artificial'." and his 2000 paper titled "The paradox endeavor to design a foundation for a groundless field."

In a response that is already well over the requested length, it is difficult to respond comprehensively to these two papers. One reason for this is Jonas's purposeful decision to structure one paper in an unusual combinatorial sequence that allows 362,880 readings. Another is Jonas's love of swampy ground.

To set a stage for absolute free play, Jonas leaves concepts, issues, and problems ill defined. In some cases, he does not define them at all. This requires a respondent to establish definitions to undertake a response.

An example of this occurs where Jonas (1999: unpaged; 2001b: unpaged) argues that design is "an interface discipline without progress." This statement is offered without a clear definition, though there are implicit definitions and questions. Before challenging the assertion that design is a discipline without progress, therefore, I defined progress.

In their construction and appeal, these papers constitute a first step in a problem solving process. They open the problem space.

Because they avoid clear definitions and clarified statements, they do not address the problems they raise. Rather, they offer reflection or rumination on ideas and concepts to be found in the "the hybrid swampy region of artifacts and social phenomena is the fertile soil for knowledge creation" (Jonas 2000: unpaged; 2001c: unpaged). In this sense, these suggestive and poetic papers seem to address the possibility of puzzles without accepting the challenge of deeper problems.

They do not point toward solutions. This seems to be a purposeful strategy: the paper can be read 362,880 different ways. It also consistent with what I read as Jonas's philosophy. If design had no foundations and makes no progress while solutions always emerge from ill-defined, swampy ground, clear definitions would serve no purpose.

I take a different position. I accepted the invitation to this symposium to address questions, to develop well-formed responses, and to point toward useful answers. Developing answers requires us to develop a richer understanding of the problems we address.

Solutions emerge from good problem statements. It is difficult to develop solutions without understanding problems.

These papers outline problems and issues without defining them. Opening the problem space allows us to reflect. Closing the problem space through robust definitions allows us to begin the search for solutions.

For most of the past decade, I have been engaged in the work of understanding and defining the foundations of design. Articles that describe outstanding designed artifacts establish a theoretical foundation that permits evaluation on more than taste alone while analyzing success or failure in terms of practical outcomes (Friedman 1991a, 1991b.) Articles that describe the general condition of design, specific conditions of design issues, or specific conceptual tools offer clear definitions and systematic analysis of consequences (Friedman 1993, 1998; Friedman and Ofstad 1994)

A series of research reports and articles of the past half decade explores issues in the four major areas of what I have defined as a progressive research program for design: philosophy and theory of design, research methods and research practice, design education, and design practice. Each of these articles approaches a selected theme by systematically examining concepts, issues, and conditions; by establishing explicit premises and adducing their conclusions; by basing premises on careful empirical evidence or clearly reasoned deduction; by analyzing historical factors and issues and by tracing changes from past to present. At each point, I offer substantial documentation with carefully developed references and citations.

The findings in these articles (Friedman 1997a, 1997b, 1999a, 1999b, 2000a, 2000b, 2000c, 2000d, 2000e, 2001a, 2001b; Friedman and Ainamo 1999) offer substantial evidence for foundations of design.

What are foundations?

Foundations can be understood in at lest two major ways. One involves the historical basis of a phenomenon. The other involves its philosophical foundations in epistemological or ontological terms.

In Merriam-Webster's, the relevant definitions label a foundation as "the act of founding," "a basis (as a tenet, principle, or axiom) upon which something stands or is supported <the foundations of geometry> <the rumor is without foundation in fact>," "an underlying base or support; especially: the whole masonry substructure of a building," and "a body or ground upon which something is built up or overlaid" (Britannica Online 2001: unpaged)

The 1913 edition of Webster's Revised Unabridged Dictionary also defines the word in both senses: "1. The act of founding, fixing, establishing, or beginning to erect. 2. That upon which anything is founded; that on which anything stands, and by which it is supported; the lowest and supporting layer of a superstructure; groundwork; basis. Behold, I lay in Zion, for a foundation, a stone . . . a precious corner stone, a sure foundation. Is. xxviii. 16. The foundation of a free common wealth. Motley" (Webster's [ARTFL] 1913: 590)

Wordsmyth (2001: unpaged) offers these definitions and synonyms: "1. the base or basis of something such as a process, substance, structure, or opinion. SYN: base1 (1), basis (1), substructure, underpinning (1), groundwork, pedestal (2), reason (1), rationale (1), footing (1), footer (1) SIM: ground1, bed, bottom, support, justification, cause, root1, source, origin, principle DEF: 2. the act of establishing or originating. SYN: founding {found2 (vt 1)}, establishment (1), institution (5), creation (1) SIM: installation."

Bunge distinguishes nicely between historical foundations in the sense of historical roots and philosophical foundations as a secure platform of knowledge. Bunge describes four kinds of philosophical foundation. Two of these are relevant to design and design research, those dealing with epistemology and the foundations of science: "A Epistemology. The source, root, or basis of all knowledge. Although every research project starts from some body of knowledge which it does not question, some such presuppositions may be questioned in a different project. So, there are foundations but they are not necessarily final. . . . . D Foundations of science. Every factual theory has foundations constituted by postulates or theories of three kinds: philosophical, mathematical, and specific. For example, the foundations of quantum mechanics consist of large areas of classical mathematics (such as abstract algebra and analysis), which in turn are based on ordinary predicate logic; philosophical principles such as that of lawfulness; and specific postulates such as the Schrodinger equation together with semantic assumptions that endow mathematical formalism with physical content" (Bunge 1999: 105).

In the strict sense of Bunge's definition, it may not be possible to establish an epistemological foundation for design. The possibility of foundationalism is limited in a field that is involves both art and a science in both a research discipline and a professional practice. This is further complicated by the fact that design is rooted in several fields and exists on a transient boundary or interface among them. Many forms of useful design knowledge simply cannot be anchored in a firm basis or foundation. In describing most fields, Bunge (1999: 105-6) notes "Foundationalism may be traced back to the confusion between psychological or historical root or source, and foundation proper. Thus, the historical root of geometry was land surveying, but any geometry has a purely conceptual foundation, which includes logic. According to ratioempiricism, there are no ultimate foundations of knowledge matters of fact, for sometimes research starts from observation, at other times from theory, and at still other times from combining hypothesis with data, or from questioning philosophical presuppositions. It is only when a body of knowledge has been transformed into a theory (hypothetico-deductive system) that one can raise the problem of its logical organization or foundation."

Design has one set of foundations in the many historical roots from which it developed. Design has another set of foundations or bases in the ideas or goals toward which any of several communities of designers ú practitioners, researchers, educators, or scientists ú strive. While a goal is not a foundation, it does provide a psychological platform, and it forms a kind of philosophical basis, reason, or foundation in that sense.

Clearly, there can be no perfect and unshakeable foundation of knowledge for a field such as design. In a sense, this makes little difference. The notion of stable foundations for any field is becoming difficult to imagine. Godel's theorem suggests that even such a thoroughly axiomatic field as mathematics cannot be established on perfect and secure foundations. For example, Hersh (1998: 22) argues against the foundationist positions in mathematics on the basis that each is fallible, and none can account for the existence of rival positions. In contrast, Hersh argues for a humanist position that acknowledges mathematics as a human construct that involves many kinds of ideas and issues, including aspects of the foundationist positions that each idealizes one form of mathematical truth.

In arguing for foundations, therefore, I do not assert the existence a stable anchor for all design knowledge. Rather, I point to foundations three senses. One is the historical sense. The next is the philosophical sense of a basis in goals and purposes. The third involves the multiple senses of the kinds of knowledge, theory, and practice that different forms of design and design research may engage.

Three kinds of foundation for design

Given the complexity of design as a field, I will not propose a foundation in a comprehensive or stable sense. In fact, any of several kinds of models may constitute reasonable and adequate foundations. The sense in which they form a foundation must be defined for each.

Saying that design may have several kinds of foundation is a necessary consequence of s hybrid field.

Like many emerging systems, the design process arises from multiple points in specific instances. As a social or intellectual system, it can be described as a complex adaptive system.

Without making impossible claims to absolute truth for any specific foundation, this paper has demonstrated reasonable evidence for different kinds of foundations.

The evidence and history of the different foundations on which we draw in different ways makes it problematic to describe design as a field with no foundations and no progress. In much the same way, it is difficult to describe design as a groundless field.

Resolving issues of definition and description do much to remove seeming paradoxes and puzzles.

If it is the case that design is a hybrid field ú and the evidence suggests that it is ú then design can occupy several states at any one time, while serving as a forum of different kinds of activities. While some of these activities must obviously be at variance with one another, variance does not mean contradiction. There is no reason that design cannot take several shapes, permitting several kinds of approaches.

One seeming paradox involves the question of whether we discover the terrain of design, develop it, or design it. The answer is all three. These do not contradict each other.

Most terrains that humans inhabit partially existed before humans came to them, and since design works in and on the material and physical world, this is true of the terrain of design. Our pre-human and proto-human ancestors designed and manufactured artifacts before the human species evolved in its current form. To some degree, the terrain of a design process existed before we did, along with the physical and material world within which design activities take place. The terrain of design certainly exists before any one human being or any living community enters it.

Humans develop nearly every terrain that they inhabit. This reasonable and common statement requires no definition or demonstration. It is possible, nevertheless, to provide both.

Saying that we design the terrain is another way of saying that we develop it or change it from one state to another. This is merely a matter of clarifying language.

It is fair to say that we discover the terrain, that we develop it, and that we design it. There is no paradox here.

One of the values of clarity is that it resolves minor puzzles to leave the ground open for genuine problems. Seeking foundations involves addressing a serious and interesting range of problems.

Some of these problems are serious and so fundamental that we may never solve them. We will solve other problems on a temporary basis, and the steps toward solution will raise new problems. We may well solve other problems in a deep and serious sense. This search is the condition of progress.

It is the willingness to seek foundations and clarified understanding that enables us to make progress as individuals and as a field.

Descriptive models and reality

One of the poorly articulated debates in our field involves the difference in approaches to knowledge among practitioners and scholars or scientists. I have addressed these issues at length in earlier papers (see especially Friedman 1997a, 1997b, 1999a, 2000b, 2001a, 2001b). While I will not address the central themes in this debate here, it is worth noting the way in which several features of the debate seem to arise in the question of foundations.

The arguments against the concept of foundations are intuitionist in nature. They are asserted without demonstration. While assertions of belief are valid as statements of belief, they are not valid as factual statements or as truth claims other than the truth of the believing condition.

In many ways, these assertions resemble the articles of belief that form the foundation of guild mysteries and the spiritual or psychological secrets involved in the higher levels of the craft guilds.

Reasoned argument from evidence is distinct from the issue of using scientific criteria for reasoned argument. Scientific debate is one form of reasoned argument. Many forms of debate are reasoned even though they are not scientific. Reasoned argument from evidence is used in philosophy, the humanities, history, mathematics, and other fields. It is possible to ask for reasoned argument from evidence without locating design in the context of science.

While I have argued that SOME forms of design practice, design research, and design education are -- and should be -- treated as forms of science, this is not the case for all forms of design practice, research, or education. In contrast, ALL design activity requires reasoned thought. Ideas, issues, and inspiration often begin with intuition. This is the context of discovery. They must finally end in reason. This is the context of justification.

Design involves solving problems external to the designer. This is a contrast with those forms of free art in which artists solve the problems they find interesting. Some aspects of design resemble free art at certain stages in the problem solving process. Some aspects of design even become a form a free art when designers address free artistic problems as part of a larger program or project. This is a specific case of design activity, and it is generally a small part of the larger work of design.

While solving problems effectively raises the issue of elegant solutions and artistically satisfying solutions, this is a general condition of problem solving in all fields. It is a separate issue than the artistry involved in free art.

In discussing the design process, we discuss solving problems external to the designer. To the degree that design involves solving problems external to the designer, design implies an empirical world and the possibility of evidence separate from the designer's taste, opinion, or judgment. This, in turn, means that design at some point requires reasoned argument.

In the next phase of "the basic PARADOX," I will query a number of specific issues and claims. At this moment, I simply note that some of the claims I have read so far are intuitive assertions without demonstration. Some of these assertions may be valid or reasonable, but they require reasoned argument from evidence to warrant reasonable consideration in a debate.

It seems to me that there are genuine problems involved in the issue of foundations. Some problems allow solutions. Some may not. Some of the questions here may involve basic problems. Nevertheless, I see no basic paradox. Carefully reconstructed arguments and reasoned analysis will probably untangle many pf the issues that seem to be paradoxes so far. Only after this careful work is done will we know if there is a "basic paradox," or not. So far, the work of critical inquiry has not been undertaken. In its place, we find the first steps of intuition and assertion, that it, we see ideas being generated without being tested.

A metaphor and a model: two clarifications

Jonas's idea of a "hybrid swampy region of artifacts and social phenomena [as] the fertile soil for knowledge creation" is reasonable. It can be supported by evidence in knowledge creation and problem solving research, and by an analogy from biology.

Jonas's hybrid swampy region is a breeding ground and a context for physical experiments, thought experiments, and sense experiments. This kind of hybrid ground can be described in terms of the wetlands areas between dry land and ocean that serve as breeding ground for evolution and for the transmutation of life forms.

Problems arise in the idea that many seem to feel follows from this, that results emerge in some natural, evolutionary sense from the hybrid swampy ground. The is only partly true. When it is true, there is a high price to be paid. In a biologically rich dynamic system, there are many more opportunities for evolutionary dead ends than for successful mutation.

At several points in his papers, Jonas describes design as an art. The wastefulness of the evolutionary process has dramatically different implications in art and in design. There must be and there is greater latitude for mistakes and transgressions in the world of the arts than in the immediate and results-oriented world of design, business, or social policy.

A healthy art world may be a world in which there is always more bad art than good. This may not be the case for design.

In design, our ability to reason, to draw on experience, to analyze likely outcomes, to conduct thought experiments, and to use other forms of experiment along with argument from evidence can help us to avoid predictable problems.

The death rate of species in natural evolution is stunning. The hybrid swampy ground is a rich source of inspiration, but we can do better than simply depending on hybrid soil for its admitted fertility.

Many designers seem to design by randomly generating hundreds of ideas, filtering them through a conscious series of preference criteria heavily influence by intuition and personal taste. This emulates the process of evolution. In one way, it works. It is instrumental in realizing designed artifacts. The problem is that these artifacts often fail to solve the problems for which they are designed. The high cost of random development and natural selection is revealed failed development and extinct lines. The evidence of new product failure is clear.

One study suggested that of new product ideas that move beyond the proposal stage, 57% achieve technical objectives, 31% enter full-scale marketing and only 12% earn a profit (Mansfield et al. 1971: 57). According to some experts, over 80% of all new products fail when they are launched, and another 10% fail within five years (Edwards 1999, Lukas 1998, McMath 1998).

Planning and study can never yield perfect results. Even so, they do yield improvements. Both a design science approach and other forms of reasoned argument improve the design process.

Some leading designers now enhance their success rate by using scientific method and other forms of articulated problem-solving process. Effective problem solving seems to require a generic model in which designers explore and examine possible problems, identify and describe problems, explore and examine possible solutions, then select among solutions for best first to he real problems. This requires clarity, and clarity is difficult if we simply accept the design process as an emergent, opportunistic outcome in ill-defined, swampy ground.

The hybrid swampy ground is a rich metaphor for the nature of design as a complex adaptive system. It is insufficient for a comprehensive understanding of design. The apparent paradox of foundations on swampy ground is readily resolved if we understand that the hybrid swampy ground exists as one context of discovery without describing the entire terrain of design.

The second clarification involves models of the design terrain.

In describing several models of the design terrain, Jonas (2000: 44, 2001b. unpaged) states that "contemporary theory designs present us with geometrical and structural imagery, but without naming their contingency: Archer's triangle of humanities (words) - sciences (numbers) - design (artefacts); Dilnot's (1999) triangle of aesthetics - technology - ethics with design in a reconciling function; Haavisto's Yin-Yan model; Friedman's (1999) [KF: This reference appears as 1999b in the reference list below, but the model cited by Jonas does not appear in the article cited.] pentagon of social sciences - technology - art - natural sciences - humanities, or his (2000) [KF: This reference appears as 2000e in the reference list below. The cited model is found in this article.] hexagon. From this emerges the phenomenon of oscillation between disciplinary fantasies of omnipotence and impotence: is design an agent of reconciliation, a gapfiller, or is it simply trying to find its humble niche?"

This description of models of the design terrain is problematic. Any model or diagram must have some geometric or structural form. The issue is how the author uses structure and form in relation to content. Jonas seemingly describes a series of geometrical solids or Platonic shapes without describing content or function. This seems to be the kind of argument one sees in those marvelous books that bring together entirely different kinds of structures or figures based solely on their surface form or numerical properties. Models of a terrain or field have content.

The content of a model determines its structural use and the degree to which it reasonably reflects the object of inquiry whose model it is intended to be. In the philosophy of science, this would be the contingency that Jonas requires (Audi 1995: 158; Blackburn 1996: 80, 257; Bunge 1999: 49; Lacey 1996: 68, 213-218). There are other meanings of the term contingency, but these are irrelevant here.

The model that Jonas describes (2000e: 11) is a labeled as a "model of the field of design." It specifically names the elements of the terrain, dividing them into two domains, "domains of theory," and "domains of practice and application." The article in which the model appears describes the elements of the model in term of their content and structural use. It links them to an earlier model (2000e: 10) labeled "domains of design knowledge." This model specifies and clarifies the content of the large-scale structural elements that constitute the domains in the model of the field of design.

The model of a social or intellectual system is somewhat abstract. Of necessity, it cannot have empirical or factual content in the same way that a model of a steam engine has specific working parts or the way that the model of a house has structural elements. Nevertheless, the model of the field of design has content in the form of well-understood domains, and these are linked to specific subject field descriptions in a second model that describes domains of design knowledge.

To a topographer, a donut, a teacup, and a wrench may all be described as a torus. The abstract mathematical description of topographic shapes is based on limitless plastic deformation. This is permissible if we overlook the function, content, and meaning of the objects being described, considering only their abstract, topographic description. This happens when Jonas considers these models as geometrical and structural images without attending to their specific and detailed content. By stripping them of content and divorcing them from the careful context of the article in which they appear, Jonas reduces them to abstract images in much the same way that an art critic or a drafting teacher may describe a skyscraper, a single brick, and a loaf of bread as having the same shape. While I understand the reasoning behind this description, it is the description and not the model involves abstract geometry and a structural image.

The model has been given a structure to express its content. The content can also be stated in words. The model has a specific shape to express a dynamic relationship between domains. Placing domains of theory and practice across from each other links them in a dynamic dyad.

The model to which Jonas refers is easy to describe, picture, and construct. I will describe this model in geometric terms. It is easy to reproduce with a quick sketch.

Draw a circle or pie chart. Bisect the circle with a horizontal line. Draw six equal triangles on the circle so that three triangles are above the horizontal line and three below.

Use a dotted line to extend the horizontal bisecting line to the right and left of the circle. Above the dotted line, inscribe a caption to denote that the three triangles above the horizontal line represent "domains of theoretical study." Below the dotted line, inscribe a caption to denote that the three triangles below the horizontal line represent "domains of practice and application."

The triangles represent six general domains of design. Moving clockwise from the left-most triangle above the horizontal line, these domains should be labeled as (1) natural sciences, (2) humanities and liberal arts, (3) social and behavioral sciences, [shifting below the line] (4) human professions and services, (5) creative and applied arts, and (6) technology and engineering.

The nature of design as an integrative discipline places it at the intersection of several large fields. In one dimension, design is a field of thinking and pure research. In another, it is a field of practice and applied research. When these applications are directed to solving specific problems in a specific setting, design involves clinical practice and clinical research.

Design is a field that may involve any or all of these domains, in differing aspect and proportion depending on the nature of the project at hand or the problem to be solved.

The placement of domains across from each other along the horizontal axis suggests dynamic relationships among specific fields of theory and application. The domain of the natural sciences is closely linked in dynamic interaction with technology and engineering, the domain of humanities and the liberal arts with the creative and applied arts, the domain of social and behavioral sciences with human professions and services.

The model distinguishes between and among domains for the purpose of explanation. In the reality described by this conceptual model, both design practice and design theory can be found at the center of the model. For any given project, a differently shaped territory inscribed on the model will represent design. This shape is often fuzzy or ambiguous. This territory may engage any or all of these domains in differing degrees and proportions.

This model of the design terrain does not describe disciplinary fantasies of omnipotence or of impotence. It permits difference and variation according to specific circumstances located in the context of each specific project or program. Design is something an agent of reconciliation. It occasionally fills gaps. The niche filled by the design process is sometimes humble and sometimes grand. Depending on the specific context and purpose, design may be any of these things, and it often possesses other qualities.

This model describes design as a system of complex adaptive processes functioning in a network of activities. It seems to me that this model meets Jonas's goal of locating design in a realistic and fluid context. In overlooking the content of the model to see it as a geometrical formalism, Jonas has overlooked the very qualities he seeks in a model. It seems to me we may be closer here than we seem to be on the formal surface of the debate.

Conclusion

I agree with Jonas on many fundamental issues. In certain important regards, we say exactly the same thing. For example, Jonas (2000: 44, 2001c: unpaged) writes that " 'fundamental' issues comprise [such] meta-subjects as: analytical and systemic thinking, associative power, synthetic, generative and evaluative competencies, communicative skills." I have stated these ideas (in Friedman 2000e. See also 1997,a, 1997b, 2000a, 2001b). While my language is slightly different, the point is the same, and I agree completely with Jonas on these issues.

On other issues, we agree, but with a difference in interpretation. For example, Jonas (2000: 44, 2001c: unpaged) sees no "stable disciplinary core but a fluid network of 'chunks of ideas,' re-established in communicative feedback at the interface between the contextual and the artifactual." In contrast, I see a multiple series of cores: the core of each designer's work ú or the core of issues for any design researcher ú depend on his or her starting point and focal point. Despite the difference in emphasis, however, I, too, state, "The design process has no center. It is a network of linked events. Systematic thinking makes the nature of networked events clear" (Friedman 2000e: 11)

The difference in metaphor is significant. I believe that the quest for explicit definitions and foundations give one greater purchase on the possibility of analytical and systemic thinking, issues that Jonas and I both believe to be fundamental to design. A network is always based on series of linkages ú in this case, a fluid series of ever-changing linkages. These yield a changing series of emergent properties. This does not mean that a network lacks foundation or basis.

Jonas argues, "Networks do not have foundations." I see this as an assertion that he has yet to demonstrate. In contrast, the significant research program pf of complexity theory demonstrates that complex adaptive systems and networks do have foundations. Networks have distinct foundational qualities that emerge in the properties revealed by each system. Networks realize and reveal their qualities in different ways than other kinds of systems do.

In some ways, we clearly disagree. I call for clarity and explicit description. Jonas seems to believe that metaphor best describes the qualities of celebrates the hybrid swamp. I believe that this does a disservice to the swamp. If it were impossible to describe the wetlands, the science of biology could not exist. The science of complexity and the concept of complex adaptive systems allow us to describe a hybrid swampy environment without reducing its richness. This requires greater and more explicit descriptions, not less.

I believe that foundations exist. Jonas does not. Without arguing for comprehensive axiomatic foundations or monolithic platforms, I assert that design involves many kinds of foundations. Understanding these foundations is a central issue for design research. Describing them and rendering them accessible for the process of design has been part of the clear progress our fields and disciplines have made during the past decades.

There may well be issues and processes that cannot be described and problems that may never be solved. Part of the scholar's work is to examine issues, processes, and problems carefully enough to know when this is the case and when it is not. One of the qualities of great scholars and great fields is the willingness to struggle with tough problems long after others give up hope on solving them.

I am convinced that we will discover ways to work on many foundational issues and process. I am convinced that during the next few decades, we will solve problems that now seem far more difficult that they are simply because we have not yet developed a language through which we can address them. This is an assertion, not a fact. I assert it nevertheless, and I take the intuitive position that this is so.

If I am wrong, we will discover that I am wrong by carefully working our way through these issues, processes and problems. If the work of the last decade is any indication, we will make some interesting and useful discoveries along the way, developing and designing solutions for rich problems as we do.

-- Ken Friedman

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