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Creative Research

Description of Some Signposts to Unknown Areas

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When defining science as a problem-solving activity, philosophers of science have mainly focussed on "the context of discovery," the process of finding creative solutions to problems; and "the context of justification," the process of evaluating new theories. This article takes a step backward and proposes ways to find problems with a creative potential: (1) Seek areas of theoretical controversy—respect all the facts and set out to find a new theory that can encompass the contradictions; (2) move beyond the realm of research proper—take an interest in phenomena that do not fit into any established theory or frame of reference; (3) give a break to discovery by chance—be attentive and welcoming to unexpected results; (4) look for exceptions—treat them as first-class facts instead of ignoring them or trying to explain them away.

Scientific research is commonly regarded as requiring a creative turn of mind, but any habitual reader of scientific publications knows that it is seldom so. Science as a problem-solving activity has been the subject of much theorizing by philosophers of science. They have focused on the process of finding creative solutions to problems (the context of discovery) and on the process of evaluating new theories (the context of justification) (Darden 1980, 151). That leaves us with a gap concerning the very first step: how to find problems with a creative potential, that is, those that hit the center of unknown or badly understood areas. One crucial condition is the scientist's ability and willingness to feel puzzled or even confused, to take his confusion seriously, and to push it through his mental distillery in order to make it reappear as a poignantly stated problem. In this article I present some views on why creativity is so scarce and suggest some signposts that may guide the reader to the hiding places of interesting and productive problems.

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CONSENSUS AND CONTROVERSY

When I dig deep down into my past, back to the days when I was still a student, one of the books in the curriculum was on needs, drives, and motivation. In many ways it was an excellent book. The authors had set themselves the ambitious task of reviewing all the major theories in the field, and in the end pooled this mass of knowledge into a comprehensive theory of motivation. I studied the different theories with great interest but without being able to find any convincing hooks for a comprehensive theory. I looked forward to reading the last chapter, where the solution to the riddle would be disclosed. This is where my story about creative research starts: As I read the last chapter, I was surprised to notice that my concentration failed and my mind went AWOL. I turned to my usual tricks—underlining, making comments in the margin, taking notes. Fine. Except that the next day I could not remember anything about that last chapter. I reread it. Same story again. Why?

I think the answer is that the authors used a consensus-seeking approach on a subject on which there was no consensus. They presented a completely shapeless compromise among aspects of a very different nature and order instead of letting themselves be inspired by the theoretical and empirical diversity.

Theoretical disagreement can be seen as pointing to areas where facts may lead us in different directions. If the facts can stand closer scrutiny, the disagreement cannot be resolved by an "either-or" approach and not by an "a little of both" approach, but only from a new angle.

Take the earlier fierce and long-standing controversy regarding brain function. The tradition since the Middle Ages in exploring this subject was to locate every imaginable human "faculty" in its own strictly localized area of the brain. From the 1860s through more than the next half century, this paradigm was strengthened by some empirical evidence, but wishful thinking conquered scientific sobriety and the "maps" of the brain were sophisticated far beyond the firm ground of data. As there were logical and empirical reasons to doubt the validity of this approach, another school of neurologists worked with ideas of the brain as an undifferentiated entity. The breakthrough was finally made by the Russian neuropsychologist A. R. Luria:

> [T]his crisis compelled a search for new ways leading to the discovery of the true cerebral mechanisms of the highest forms of mental activity. . . . This task required the radical revision of the basic under-

> standing of the term "functions" on the one hand, and of the basic principles governing their "localization" on the other. (1976, 26)

The result of Luria's work is a theory that can encompass both sides: The different parts of the brain do have specialized functions, but there is no direct correspondence between specific brain functions and human behavior. Any human activity is dependent upon the simultaneous involvement of several brain functions, localized in widely different parts of the brain.

Here is my Important Lesson Number 1:

> There is nothing much to be gained in consensus. Seek the controversy, respect it, and set out to find the new theory that can encompass the facts in dispute.

If my advice is followed it will put an end to a popular fad, that of "Consensus Conferences," the results of which are exactly as shapeless as the last chapter in the book on motivation. I will advocate an alternative: the Dissensus Conference, which I believe would be far more productive as long as the participants are aware that they are handling explosives that go off under conditions of mutual disrespect but have an enormous creative energy under conditions of sincere curiosity.

THE S-CURVE OF KNOWLEDGE AND UNDERSTANDING

Another answer to the question of why research commonly is so uncreative is probably to be found in the standard norms for research quality. Those norms state that a research project should be based on existing theories and results and that preferably it should put another brick on the impressive building under construction. They also state the methodological sine qua nons. If you want a career in academia or rely on funding to support your research, you had better stick to those standard norms. If you are about to plan your Ph.D. dissertation, following those norms is vital, even if there also is a norm requiring originality. Originality is obviously a question of degree, and the degree allowed by the other standard norms is not impressive.

In order to explain that, I want to introduce the S-curve of knowledge and understanding. The horizontal axis of the diagram depicts the amount of knowledge on a subject, represented by facts and theories. The vertical axis shows the amount of understanding. When

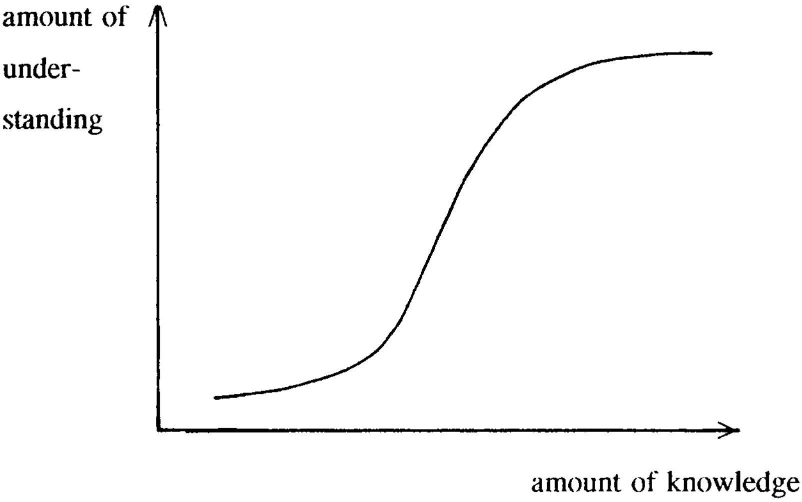

Figure 1: The S-Curve of Knowledge and Understanding

you start from scratch on a new subject, you have to consume a fair amount of knowledge before your understanding begins to form. That is the flat bottom part of the S-curve. Then follows a period where your understanding grows rapidly as you add more knowledge. You are on the steep slope of the curve. Later again, the well-known law of diminishing returns applies, as your attempts to add more knowledge only occasionally give you a new insight but usually just add details or nothing at all. This is the flat top part of the curve.

Here is my Important Lesson Number 2:

> Most research moves on the flat top part of the S-curve of knowledge, as this is a safe area where it is possible to fulfill the standard norms for research quality. The norm of originality is usually given credit by moving one step down on the steep slope but rarely to the middle part, let alone the flat bottom part. Those are the areas where truly creative research has its playing field.

Do not misunderstand me: My mission is not to kill research on the top part of the S-curve. Every map of a geographical area was originally a crude outline, drawn by an explorer. Today's detailed, precise maps are the result of subsequent diligent and competent filling out of details, a process that makes the maps usable. My mission is to revive the spirit of and respect for the explorers.

UNCERTAINTY

Why are the standard norms contrary to creativity? They are excellent for research at the top part of the S-curve, where the objective, key concepts, and methods are for a large part defined by a pre-existing structure. You move in a field of certainty, and if you stick to the standard norms you are almost certain to be able to put the missing brick into its proper place.

When you move down the slope of the S-curve, you move into uncertainty. (Whether you like it or not is a question of taste—some people cannot stand it; others are hooked on it.) This means that you are trying to grasp the nature of a phenomenon that is not very well understood. Until you have this elementary grasp, you cannot define it very precisely, let alone measure it. Your understanding takes shape, falls apart, and takes new shapes by using all kinds of information in every way you can get it, weighing it and judging its credibility and value. You may object that this is a pre-stage to research proper. If you believe that this pre-stage is unworthy of a serious scientist's precious time and craftsmanship, any frail embryo of understanding will rapidly suffer an abortion, provoked by questions from the repertoire of the standard norms, for example: "How are you going to measure that?" Save those questions until you have a minimum of firm ground under your feet, and then they are an excellent means of disciplining your wild hunches.

The issue of method in the process of creating an elementary understanding of a phenomenon will have to rely on the scientist's seriousness and curiosity, which may protect him against grave errors in choice and interpretation of facts. "Seriousness" and "curiosity," however, are defenseless and are easy prey to the wolves unless there is an unequivocal criterion of success. That is seldom the case on the slope of the S-curve.

I learned my elementary lessons of method when I was 15. As a florist's delivery girl I often had to find streets outside my home territory. Asking my way, the first thing I learned was to choose my sources of information. Men were quickly ruled out because they were apt to think that I asked about something quite different from what I actually did ask. Old people had too much time on their hands and would drown me in redundancy. My best informant was a woman with a pram, because she was in her local area and in a hurry to get on with her own business. She gave precise and quick information.

Nevertheless, and especially if there was no pram pusher available, I made a test of validity by asking three different people. Usually they came up with three different answers. Taking the informants' individual credibility into account, I learned to construct a weighted median, even when my data said east, west, and south. I was in the fortunate situation that I had an indisputable criterion of success, which was an important part of my learning process. My main point is to illustrate how my motivation triggered my methodological adeptness.

Now let us move to the bottom part of the S-curve. Intellectual endeavors in this area are risky; it is here that you find the basis for new paradigms as well as wild products of fantasy. When you move into this area, you will be on your own, and your endeavor will almost certainly be met, at best, by condescension. If you insist, you will cause considerable disquiet, because you will challenge the existing view of the nature of the world. The penalty for that is death—socially in our modern enlightened society, physically in less civilized cultures. There is no guarantee that you are a Galileo who will be deemed right by future generations. Whether you like it or not, the great scientific challenges and the peak of intellectual excitement reside in this area.

Let me take an example, well knowing that I risk to lose my readers' respect. The subject of parapsychological phenomena divides people into believers and disbelievers, who are constantly at war. The core of the matter is hard to find, because the subject is fraught with charlatans, illusionists, sensation mongers, and hystericals. There also are ordinary, level-headed people who occasionally have experiences that may have a parapsychological nature. Often, they are reluctant to tell about them because they may find them hard to believe and because they do not want to acquire a reputation as charlatans. Even if believers and disbelievers are at war, most of them have one thing in common: Parapsychological phenomena are put in the realm of the mystical and supernatural.

I prefer to put those phenomena at the bottom part of the S-curve, where the question is whether they are "something" or "nothing." If they are nothing, I still find it extremely intriguing how illusions of those kinds are created. If they are something, I prefer to suppose that they are signs of as-yet-undiscovered laws of nature. (Is there any logical reason to take for granted that all the important laws of nature are already discovered?) When magnetic phenomena were first observed, they were regarded as mystical. It must have taken a lot of courage and creativity on the part of physicists who set out to explore magnetism systematically and put a scientific fence around it.

SERENDIPITY

Now a few words about serendipity: the case when important discoveries are made by chance. I want to turn your attention away from its element of pure luck and toward the key word "discovery," which results from a combination of attention and insight. If you set out to look for your lost boot, your attention may very well miss the gold mine. Or take the discovery of America. That was pure serendipity. But who discovered America?

The Vikings, who were the first overseas visitors from Western Europe to America, did not "discover" the new continent, as their visits had no consequences and were almost forgotten. Columbus? As we know, Columbus set out to find India by a western route. Accidentally, the West Indies are situated where he had calculated that India should be, and, understandably, he concluded that his mission had succeeded. He died in 1506, still believing that he had visited India, despite three more expeditions, two of which took him to the South American mainland, and despite Amerigo Vespucci's voyage in 1499, which convinced Vespucci that this was a new continent. In the discovery of America, we have to give credit to both Columbus, for his persistence in following his idea, and Vespucci, for his insight into the true meaning of the result.

A famous example of serendipity is the discovery of penicillin. In 1928,

> Sir Alexander Fleming at St. Mary's Hospital in England, [observed] that a growth culture of the pus-producing bacterium, Staphylococcus aureus, had disappeared in an area in which a green mold was growing . . . the organism that produced the substance . . . was a species of Penicillium. . . . Attempts to treat human infections with this material were not encouraging, however, because the substance was unstable and lacked potency; not until several years later did several workers at Oxford University examine the possibility that stable penicillin might be produced in large enough quantities to treat human disease. In 1941 the drug was used to treat serious infections . . . [but] World War II interfered with the large-scale manufacture of penicillin in Great Britain. (*Encyclopaedia Britannica* 1974a)*

Only around 1948 did the use of modern antibiotics begin to be widespread.

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*All excerpts from the *Encyclopaedia Britannica* are taken from the *Encyclopaedia Britannica*, 15th edition, © 1974, and are used here with permission.

Why, in 1927, did the original specimen catch Fleming's eye and interest, and why was it not just cleaned away as an instance of faulty procedure? How did his idea survive 10 years of meager results? As for research generally, one interesting question is how many serendipities are actually discovered and how many slip the researcher's attention or are shamefacedly hidden because they do not represent the original aim of the project?

Here is my Important Lesson Number 3:

> Regardless of whether your quest hits your goal, take a good look at where it actually took you. If you end up someplace you did not expect, do not choose the nondiscovery strategy, shamefacedly hiding your results. It may be healthy to remember Anthony Quinn's final line in *Zorba, the Greek*: "Did you ever see a more splendiforous crash?"

EXCEPTIONS

My last theme is exceptions. Those are usually treated as non-facts or second-class facts. When an exception pops up in the course of scientific research, its fate is usually one of the following:

- It is regarded as the result of a methodological error and is treated as a non-fact, being discarded and kept apart from the true facts.
- It is ignored as a pure chance variation, resulting from uncontrolled variables of a kind that are impossible to eliminate completely even in very sophisticated experiments.
- The fact of the exception as such is recognized, "but it does not mean anything"; that is, it is not interesting and not to be taken seriously.

My proposal is that exceptions are to be treated as first-class facts. They are like surf on otherwise smooth waves, warning the sailor of hidden reefs or sandbanks. Exceptions tend to be as irritating to the scientist as underwater obstacles to the sailor. The sailor has to heed the warning if he wants to keep his ship clear; the scientist need not. Exceptions can be hints to where established theories and rules have more or less serious limitations, or to where they do not apply at all, calling for a search for competing—possibly still undiscovered—rules. Exceptions must, of course, be scrutinized closely to exclude the effect of methodological errors, but if this judgment is passed too rashly important facts may be wasted (although your peace of mind is saved).

To illustrate my point, let me remind you of a classical example: the discovery of the principle of vaccination. "[Edward] Jenner, even as an apprentice [around 1770], had been impressed by the fact that a person who had suffered an attack of cowpox—a relatively harmless disease that could be contracted from cattle—could not take the smallpox" (*Encyclopaedia Britannica* 1974b). Jenner stuck to his idea for more than 25 years, and finally in 1796 he made his breakthrough, testing his procedure of vaccination on *one* person.

> In 1797, [he] sent to the Royal Society a short paper describing his results, but the paper was refused. . . . In 1798, having added further cases, [he] published privately a slender book entitled *An Inquiry into the Causes and Effects of the Variolae Vaccinae*. . . . The reaction to the publication was not immediately favourable. . . . [but] the procedure rapidly proved its value, and Jenner became intensely active, promoting the cause of vaccination. . . . Jenner not only received honours but also aroused opposition and found himself subjected to attacks and calumnies. (*Encyclopaedia Britannica* 1974b)

The discovery of vaccination was triggered by a fact that only a keen eye and a quick mind would register. Systematic epidemiological research would probably miss it, as it would require singling out persons who had had cowpox as a special category for analysis. Another crucial point is that Jenner judged the observation to be important enough to merit further attention. He went on working for his idea despite resistance from influential parties in the medical society.

The case of Semmelweis, who discovered the causes of puerperal fever and introduced antiseptic prophylaxis, shows how strong repression and denial of obvious facts can be. Throughout Europe in the 1840s, maternity hospitals had mortality rates from childbed fever as high as 25% to 30%. Working as an assistant at the obstetric clinic in Vienna,

> Semmelweis proceeded to investigate its cause, over the strong objections of his chief, who, like other continental physicians, had reconciled himself to the idea that the disease was unpreventable.
>
> Semmelweis observed that among women in the first division of the clinic, the death rate from childbed fever was two or three times as high as among those in the second division, although the two divisions were identical with the exception that students were taught in the first and midwives in the second. . . He concluded that students who came directly from the dissecting room to the maternity ward carried the infection from mothers who had died of the disease to healthy mothers. He ordered the students to wash their hands in a solution of chlorinated lime before each examination. Under these procedures, the mortality

> rates in the first division dropped from 18.27% to 1.27%. (*Encyclopaedia Britannica* 1974c)

Semmelweis's superior was critical of the significance of his discovery because he failed to understand it. From 1849, Semmelweis worked in Hungary, where his ideas were accepted, and his measures promptly put an end to an epidemic of puerperal fever at the St. Rochus Hospital in Pest, reducing the mortality rate to 0.85%, contrasting the "normal" rate in Prague and Vienna of 10-15%. In 1861, he published his principal work. The general reaction among prominent obstetricians and medical societies abroad was adverse. "The years of controversy gradually undermined his spirit. He died in 1865, 48 years old" (*Encyclopaedia Britannica* 1974c).

Exceptions take you down the slope of the S-curve into uncertainty, or perhaps all the way to total bewilderment at the bottom. You have to face the possibility that a well-established rule begins to crumble at the edges or suffers a devastating earthquake. If you believe that creative research is a worthwhile goal, that is the price you have to be prepared to pay.

This is my fourth and last Important Lesson:

> Look for exceptions and treat them as first-class facts that can guide you down to the creative areas of the S-curve—and be prepared to fight.

In this article I have touched on the area of finding creative questions, illustrating my points with a few exceptional and dramatic examples. Do they "prove" anything at all? Wartofsky (1980, 6) offers the following anecdote. One story concerning Norbert Wiener has it that he was asked the question, "On how many instances would you be willing to base a generalization?" He is purported to have answered, "Two instances would be nice, but one is enough!"

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